Firm Networks in the Great Depression*

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Abstract

We study how firms facing working capital constraints allocate resources across their constituent establishments in response to local shocks in the context of the Great Depression. Using data from the Census of Manufactures consisting of establishments linked to their parent firms, we find the employment in establishments in a multi-plant firm is more correlated with local retail sales than that of single plant establishments. Furthermore, in a long difference specification, we show that establishments are affected by shocks to the local credit supply of the other establishments that make up the same firm. These results show the important role of firms in the geographic propagation and amplification of local shocks.

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1 Introduction

Firms play a critical role in the allocation of resources in the aggregate through their decisions of how to allocate resources to their individual business units. These decisions can have important implications for long-run growth through the choice of which projects to invest in. These decisions can also have important consequences in the short-run through the choice of how to respond to local shocks faced by individual units or establishments. In particular, these decisions can affect the geographic contour of recessions. We empirically establish the central role played by these firm-level decisions in the understanding the Great Depression.

As motivation, consider the following concrete example: the Alpha Portland Cement Company. One of its nine constituent establishments was located in Alabama and another in Illinois. During the Depression, there were region-specific shocks such as the banking panic in Chicago of July 1931. This event presumably affected the Alpha establishment located in Illinois directly. The question we address empirically is whether the other Alpha establishment in Alabama was also affected by this event through *spillovers* stemming from its connection to the Illinois establishment. If so, the natural followup question we investigate is whether the response of Illinois Alpha establishment to these Illinois specific shocks was *amplified* relative to a similar establishment located in Illinois but not part of a firm spanning multiple regions.

We build a model of a firm's decision to allocate resources across its constituent establishments when facing a working capital constraint. The working capital constraints limits the total wage bill of a firm to a function of the revenue earned by each establishment. We allow for the possibility that revenue from different establishments have 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 given to local wholesalers. In this case, establishments in a given region might be exposed to changes in the local availability of funding for 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. The existence of the network allows the firm to exploit local shocks by transferring resources between establishments located in different regions.[1]

We derive two sets of comparative statics results from the model. The first set compares

[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.

the response of single establishment (SP) establishments to ones that are part of a multi-plant (MP) firm. The second set of comparative statics regards spillovers of shocks to other establishments in the same firm. We consider comparative statics in response to two different shocks: (1) investment opportunity and (2) cashflow. The first type of shock changes the optimal ratio of labor across 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 changes the total amount of resources a firm has access to while leaving this optimal ratio fixed. As compared to investment opportunity shocks, we find that cashflow shocks always generate a positive correlation in employment across establishments making up the same firm (Giroud and Mueller, 2017).

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 establishments at this time. In addition, a number of these industries had firms with large networks of establishments. Following the setup of our model, our focus here is on "horizontal" networks of multiple establishments that are part of the same industry selling a similar product. There are, of course, other types of network structures such as one where an establishment in a firm 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. As an example, the cement industry is comprised of firms operating establishments producing nearly identical products to satisfy local demand. In this industry, the average number of establishments operated by the three largest firms to operate was more than ten.

Our empirical approach estimates the difference between the response of MP and SP firms in quarterly employment in reaction to changes in the local economic environment. We focus on changes in local demand as measured by an index of retail sales. The work closest to our is by Giroud and Mueller (2017), who study the role of these networks in response to housing price shocks in the Great Recession. Following the work of Mian et al. (2013), they interpret these shocks as local demand shocks. We exploit the geographic variation in the local availability of credit during the Depression, variation not readily observable in the Great Recession. This allows us to contribute to the large literature that has attempted to identify the effect of local credit shocks in the Great Depression.[2]

[2]For example, Calomiris and Mason (2003) and Lee and Mezzanotti (2017) both identify negative effects

We find that employment at MP firms is more correlated with demand conditions than for SP firms. The point estimates range from a quadrupling of the sensitivity of SP firms to a 1/3 more sensitive depending on the specification. We then examine whether these differences in sensitivity spillover to the other establishments that make up a particular firm located in distant regions. If firms reallocate funds across their establishments in response to local conditions, this should be reflected in spillover effects on the employment of other establishments unless the firm's supply of funds is perfectly elastic. For example, consider a firm with establishments in two separate regions with one of those establishments subject to a local demand spike. Given our previous results, the establishment in the region with relatively lower demand should see its employment fall. We document precisely this effect for changes in demand. The effect of "other" demand shocks is of roughly the same magnitude as the direct effect. We quantify the statistical significance of these effects by constructing a permutation test in which we randomly assign establishments to firms and reestimate the model. We interpret our empirical results as evidence of spillover effects that are present in MP firms.

We provide another set of long difference specifications that uses finer geographic variation at the county-level. We construct two measures of changes in local credit supply between 1929 and 1933. The first following Lee and Mezzanotti (2017) (minus) the ratio of the number of bank failures between 1929 and 1933 relative to the number of banks in 1929. The second measure *Deps* is the symmetric percentage change in deposits over this same period. We construct "other" measures of this aggregating over establishments that make up the same firm. We find similar to the higher frequency specifications that other credit conditions spillover inside of the firm with magnitudes similar to the direct own effect. This provides additional evidence for the role of these within firm networks of establishments.

Our paper also relates to a literature in corporate finance that studies the functioning of firms' 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).[3] Recently, the literature has examined to what extent these internal capital

from local credit market breakdowns. Benmelech et al. (2017) use variation in when a particular firm's long-term debt matures to isolate the effects of credit availability on employment.

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

markets affect how firms respond recessions. For the case of the Great Recession, Matvos and Seru (2014) estimate a structural model to account for how internal capital markets can be a substitute for external ones. Kuppuswamy and Villalonga (2010) document a fall in the diversification discount, which they interpret as evidence of an increase in the efficiency of internal capital markets. Rudolph and Schwetzler (2013) find a similar result looking across the world. Santioni et al. (2017) study the value of these networks for Italian firms during the recent Euro crisis and Almeida et al. (2015) for Korean *chaebol* in the Asian financial crisis of 1997. One limitation of all this work and much of the empirical literature is the reliance on Compustat segment data to identify the extent of a particular firm's operations. As pointed out by Villalonga (2004), these self-reported "segments" not infrequently conflict with segments as categorized by the Census Bureau.

2 A Model of Firm Networks

In this Section we introduce our model of firm networks in partial equilibrium. Firms operate establishments in different regions. Each establishment has a constant returns to scale technology in labor. The firm overall faces a working capital constraint that limits the total wage bill of the firm to a function of the firm's total revenue.

2.1 Setup

Demand and Production — We assume there is local demand for non-tradeable differentiated goods indexed by ω produced by monopolistically competitive establishments operating in region i. 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.[4] 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 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.

Let w_i be the wage rate in region i and define $z_i = \frac{\sigma-1}{\sigma}\tilde{z}_i$ as the *investment opportunity*

[4]This type of demand curve could be micro-founded with a Dixit-Stiglitz household demand structure. In ongoing work, we are developing a fully general equilibrium model that models the labor supply decision of households and imposes market clearing in the labor market to determine wages. This richer model allows us to explore the quantitative implications of firm networks. For this paper, we are only interested in establishing some qualitative results.

shock. Then profits are $\pi_i(l_i) = \frac{\sigma}{\sigma-1} z_i l_i^{\frac{\sigma-1}{\sigma}} - w_i l_i$. It is easy to check that the first best level of labor input is $l_i^{FB} = \left(\frac{z_i}{w_i}\right)^{\sigma}$. This is why we call z_i an investment opportunity shock since the ratio of first best labor inputs across regions i and j is a function of 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 s_i for the firm's liquidity position overall. Our earlier discussion the financing institutions at the time motivates this assumption that the firm faces different degrees of pledgeability across its constituent establishments. In addition, we establish a number of empirical regularities that are consistent with this assumption.

If the firm owns N establishments,[5] its working capital constraint is

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

The firm maximizes total profits

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

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

$$\kappa_i \text{MPL}_i = w_i$$

where $\text{MPL}_i = z_i l_i^{-\frac{1}{\sigma}}$ 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

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

establishment's profits and its free cashflow. [6] We can then rewrite the firm's problem as

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

subject to

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

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

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.[7] 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 $\alpha_i(0) = \frac{w_i^{1-\sigma} z_i^{\sigma}}{\sum_{i=1}^{N} w_i^{1-\sigma} z_i^{\sigma}}$. In this case, whether a firm is liquidity constrained overall depends on a weighted average of κ_i where the weights depend on both z_i and w_i.

2.2 Labor Choices When the WCC Binds

SP Firm Case — We derive the second best outcome in the case of a single establishment firm operating with a binding WCC. In this case, the optimal labor choice is the one that sets the free cashflow equal to 0:

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

[6] Note that it will never be optimal for a firm to set $l_i < l_i^{Max}$ since in this case, increasing l_i would increase both free cash flow and profits of establishment i.

[7] As we discuss below, we also do this to draw a parallel to a related paper (Giroud and Mueller, 2017) that studies the effects of "cashflow" shocks.

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 our benchmark elasticities with respect to the investment specific shock z and the cashflow shock, κ.

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

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

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

This defines a negative relationship between labor input at establishment i, l_i, and that at some other establishment $-i$, holding fixed the remaining establishments' labor inputs.[8]

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

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

We can rewrite this in units of the first best labor input, $\bar{l} = l/l^{FB}$, and the FOC (eqn. 5) becomes

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

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.[9] In the case when there is no cross-subsidization meaning for all i, $\mathrm{FCF}_i(l_i) = 0$, and, therefore, $l_i = l_i^0$, MP establishments will operate just like SP establishments in terms of their responses to local

[8] 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.

[9] 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 $-\mathrm{FCF}_i(l_i^{FB}) > 0$ 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 $-\mathrm{FCF}_i(l_i^{Max}) < 0$ 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.

investment opportunity shocks.

2.3 Comparative Statics

We now derive comparative statics with respect to "own" conditions z_i and κ_i as well as with respect to "other" conditions z_{-i}, κ_{-i}. To start, Figure 1 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 change only the WCC curve is affected and rotates about the no cross-subsidization point. Figure 2 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 multiplier on eqn. (4), the working capital constraint. It summarizes the link between the establishments that makeup a firm and 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:

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

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.[10] 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

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

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.

[10] In Appendix A, we collect all proofs and derivations for the model.

The fact that α_i changes with λ introduces complications into the comparative statics that we address by focusing on the case when the WCC is just binding. 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 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.

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

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 the link between establishments imposed by a binding WCC with the firm "spreading" the effects of the shock across all of its constituent establishments.

This result is similar to the one in Giroud and Mueller (2017).[11] 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 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 relative labor demand in our case (eqn. 7). For us, the allocation of labor across establishments also depends on the relative values of pledgeability.

3 Data

We use establishment-level data from the Census of Manufactures (CoM) covering 25 industries from 1929, 1931, 1933, and 1935.[12] This data source provides a detailed picture of manufacturing establishments over the course the first half of the Depression.[13] 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.

We will instead focus on quarterly establishment-level employment as our dependent variable.[14] The CoM asked for breakdown of employees into wage and salary earners. For the former category, the CoM from the Depression furthermore asked for this count on a monthly basis. We aggregate this to the quarter to smooth out some of the high frequency fluctuations. As discussed in Vickers and Ziebarth (2018), this distinction between salary

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

[12] 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).

[13] 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.

[14] This is similar to the dependent variable in Giroud and Mueller (2017).

versus wage earners is similar to the modern CoM distinction between non-production and production workers. 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.

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 these financial variables. The only sources from this time period we are aware that have this type of information are from Dun and Bradstreet and Moody's. Hansen and Ziebarth (2017) use the first source to study bankruptcy during the Depression. While not limited in terms of the size and kind of business covered, these records on their own do provide not any information beyond estimates of net worth and credit ratings. Benmelech et al. (2017) use data from Moody's that has information on the composition of a firm's debts outstanding. They study the effects on employment of having to refinance during the Depression. The drawback of their dataset is that it only covers the largest firms since only the largest firms will have bonds trading and this is a requirement for being included in the Moody's handbook. Our dataset, on the other hand, will cover *all* establishments within an industry subject to a very minor minimum revenue requirement.[15]

While originally collected for a variety of purposes, we would argue that the sample overall as shown in Table 1 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%) both non-trivial fractions.[16] Second, we have a variety of types of industries from "high tech" ones of the day such as aircraft and radios to durables selling to other businesses such as cement and steel to non-durables selling to final consumers such as ice cream and manufactured ice. Finally, Benguria et al. (2020) conduct some formal tests 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 overly representative of counties with higher bank failure rates or larger declines in retail sales between 1929 and 1933. On the other hand, counties with higher sample coverage do tend to be (slightly) more Democratic in terms of presidential vote share, black, and illiterate.[17]

[15] To be sure, the Moody's dataset is not limited to only manufactures.

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

[17] The appendix also discuss the geographic coverage of the sample and provides some additional checks

Unlike the modern CoM, the Census Bureau at this time did not provide establishment or firm identifiers. This makes it difficult 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. Because of this, 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.[18] For firm identifiers, we use the name of the parent company listed on the establishment schedules.

Given the limitations in the linking process, it is important to identify what types of linking errors will bias our results and how. To look ahead, our regressions will be repeated cross-sections using quarterly variation in employment as the dependent variable and conditions of other establishments making up the same firm as the key independent variable. So an error in linking an establishment *within* a particular year to its parent firms can potentially generate biases in the regressions.[19] 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. 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.[20]

One limitation of the constructed firm identifiers is that they identify establishments *within a particular industry* comprising a firm. Because we do not have the whole universe of manufacturing establishments, we are not able to identify establishments owned by a particular firm that fall outside of our industries of interest. 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. 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

on the quality of the data.

[18]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.

[19]Errors in linking firms over time will potentially affect the estimated standard errors since we will cluster on this variable. However, these errors will not affect the point estimates.

[20]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.

Ford will not be in our sample. We do not think this 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 1 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 0% in macaroni all the way to 72% in rubber tires. The range is even larger if we consider revenue percentages that range from 0% in macaroni to almost all 97% in soap. The point is that for all industries except macaroni, there is variation *within* the industry that we can exploit to identify the effects of firm networks. 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 size between these two types of firms, a question we return to below.

One final point to keep in mind is that the industries differ in their degree of "aggregation." The Census Bureau at the time did did not use a detailed hierarchal 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 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 things. 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.[21]

[21]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.

4 Comparing MP to Non-MP Establishments

Given that our empirical strategy will (partly) rely on comparing MP to non-MP establishments, one might wonder whether the comparison between these two types of establishments is a good one. Any ways in which MP establishments differ from non-MP establishments beyond simply being part of a 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 observables such as revenue, employment, labor productivity, and the labor share. Figure 3 shows the differences in mean 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. Not surprisingly given Table 1 across all industries except malt, MP establishments are larger in terms of employment and revenue. The magnitude of these differences is about 0.5 standard deviations with motor vehicle assemble, cigars, and radio equipment being outliers with differences larger than one standard deviation.

On the other hand, along the labor share 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 3 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 technology. 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 and establishments. On the balance, we find a similar pattern with higher labor productivity for MP establishments though for 40% of industries, we actually find the opposite.[22]

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. For ice, the horse power 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

[22] 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.

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 an MP firm.

Finally, we examine the distribution of geographic locations of MP to non-MP establishments by Federal Reserve district. Figure 4 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 national ratio. 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, it is clear that in any Federal Reserve district, there are both MP and non-MP establishments. This will allow us to identify the effects of firm networks using *within* region variation rather than having to use between variation comparing one region with only MP establishments and another with only non-MP ones.

5 Empirical Specification

Our empirical strategy uses geographic and temporal variation in local demand conditions to identify the effects of firm networks. Besides the work of Giroud and Mueller (2017), who also emphasize the geographic dimension, most of the literature on internal networks considered another dimension of internal networks, the set of "segments" a firm operates. Relative to this "segments" approach, there are a number of benefits to our geographic strategy. First, as pointed out by Villalonga (2004), measurement error due to the self reporting of these segments in the Compustat data, one of the most popular datasources for these studies, can severely bias results. An additional difficulty in working with segments is the fact that, in many cases, how well one segment does directly affects the performance of other segments. For example, Microsoft Office and Windows are treated as separate segments are hardly independent in how well they do. It seems nearly impossible to identify a shock that will solely effect the demand for Windows and not also affect the demand for Office. Besides demand complementarities as in the Microsoft case, there could also be production complementarities where one establishment or segment of a company produces a key input for another part of the company.

By focusing on the case where a firm's internal network consists solely of geographically dispersed establishments producing similar products, we eliminate these possible sorts of spillovers between establishments. There still might be spillovers stemming from the fact that all establishments within an industry tend to co-move together due to, say, variation in key input or output prices.[23] It is also important to keep in mind that our sample includes industries that are vertically integrated like automobile manufactures, but we only focus on the piece of those manufactures that is horizontally integrated, the final assemblers in the case of automobiles. Instead, for us, the only reason why shocks to a particular MP establishment in one region should spill over to another establishment that is part of the same firm in another region is through the firm's internal network.

Although our outcome variable, establishment-level employment levels, is available at the quarterly level and is geographically coded to counties, the data on retail sales and the state of the banking sector is less precise. When working with data from this period, there is a tradeoff between geographic detail and sampling frequency. Datasets with smaller geographic units tend to have lower frequency variation. Data on retail sales is available by quarter, but only geographically at the level of the Federal Reserve district. For this variable, we employ a fixed effects regression by quarter. By contrast, measures of the health of the financial system, in particular bank closures and suspensions, are available at the county level but only at an annual frequency. For this shock, we use a long difference specification, comparing outcomes in 1929 and 1933. We describe each specification in more detail below.

5.1 Retail Sales

Our first specification uses data on retail sales at the Federal Reserve region level and a quarterly frequency. Details regarding the construction of this retail sales index are discussed in Park and Richardson (2011). We are aware that this is a relatively coarse geographic unit. Our regressions attempt to measure spillovers from conditions faced by *other* establishments comprising the same firm. To do this, we need to construct a measure of "other" conditions that summarizes all these other establishments. The idea of examining how "other" conditions spillover inside of a firm is similar in spirit to the specification of Giroud and Mueller (2017). In the case of a firm with more than two establishments, we have to decide on how to weight the local conditions at the various establishments that make up a particular firm. For each measure, we construct an employment-weighted average of the measure for regions where other establishments part of the same firm are located.[24] Define the weighted "other"

[23] We will address this concern through a placebo test that we discuss in detail later.

[24] We also have robustness checks where we equally weight all establishments and another where we weight based on an establishment's revenue.

measure X_{it}^{Other} for some measure of local economic conditions X as

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

where Rev_j is the revenue of establishment j and the sum is over all establishments in firm f except for establishment i.

With this we estimate on the set of establishments that are part of a multi-plant firm, the following regressions:

$$L_{it} = \beta_{Retail}^{\text{Own}} \cdot Retail_{it}^{\text{Own}} + \beta_{Retail}^{\text{Other}} \cdot Retail_{it}^{\text{Other}} + \epsilon_{it}.$$

We will run the spillovers regressions on just the set of MP establishments. We also experiment with specifications where we drop all MP firms where all of the establishments are located in the same Federal Reserve district.

5.2 Banking Conditions

Our specifications use relatively coarse geographic variation at the Federal Reserve region level but relatively high frequency variation at the quarterly frequency in a panel setting. In this section, we consider the "opposite" case where we estimate regressions at the relatively fine geographic level of the county with low frequency variation in the form of changes over 4 years in a cross-sectional setting.

Define the symmetric percentage change in employment between 1929 and 1933 for establishment i:

$$\Delta^{Sym} E_i = 200 \frac{E_{i,1933} - E_{i,1929}}{(E_{i,1929} + E_{i,1933})}.$$

The symmetric percentage change allows us to include establishments that exit over this period assigning them a value of -200. For the independent variables, we draw on a study done by the FDIC that collected information on bank failures and suspensions as well as deposits and loans tied up in those banks over this period at the county-level.[25] We construct two measures for the own change in local credit supply between 1929 and 1933. The first measure $BankFails$, following Lee and Mezzanotti (2017), is (minus) the ratio of the number of bank failures between 1929 and 1933 relative to the number of banks in 1929.[26] The second

[25]This study was retrospective in the sense that it covered years before the FDIC was in existence.

[26]Note that this is not exactly the fraction of 1929 banks that failed if there is new bank entry over this period.

measure *Deps* is the symmetric percentage change in deposits over this same period. Note that an increase in either of these variables is associated with an increase in local credit supply. Following Giroud and Mueller (2017), we then construct the "other" values of these variables using 1929 employment as the weight.

We estimate the following specification on the set of establishments in 1929 that are part of a MP firm:

$$\Delta^{Sym} E_i = \beta_X^{\text{Own}} \cdot \Delta X_i^{\text{Own}} + \beta_X^{\text{Other}} \cdot \Delta X_i^{\text{Other}} + \nu_i,$$

where X is either $BankFails$ or $Deps$. By using the symmetric percentage change, the sample here is balanced since all establishments in 1929 that are part of a MP firm will have a value of $\Delta^{Sym} E_{it}$ assigned to them even those that exit.[27] Note that using the symmetric percentage change as the dependent variable, in principle, would also allow us to include establishments that *entered* over this same period. We do not, in the end, include these because it is not clear what value of the other conditions to assign them. For one, using 1929 values does not make sense since, by assumption, entering establishments were not in existence in that year. So to make the regressions as transparent as possible, we have decided to exclude entering establishments. We also restrict attention to the industries with local demand.

It is interesting to compare our specification to that in Giroud and Mueller (2017). They estimate to what extent local shocks to housing prices are "insured" against by firms' internal networks. Their approach is to use variation across regions in the run-up in housing prices before the 2008 Financial Crisis to identify local demand shocks. The idea following Mian et al. (2013) is that these run-ups were followed by equally sharp reversals, and that these declines depressed local demand and negatively affected non-tradable employment. We consider a similar setup here but instead focus on local credit supply shocks. Like housing prices in the Great Recession, we would argue following a long literature going back to Friedman and Schwartz (1971) that a good portion of the bank failures during the Depression were due to panics and not related to fundamentals. We can also compare our specifications to those in Lee and Mezzanotti (2017). Relative to their work, we use symmetric percentage changes in employment at the establishment-level to handle entry and exit versus log changes by industry in theirs. Also, their results are at the MSA level and ours are at the county.

[27] Because over 50% of our sample exits between these 4 years, we consider in the appendix some specifications that focus on exit as the dependent variable.

6 Results

6.1 Sensitivity to Other Local Economic Conditions

Table 2 reports the results of the regression including both own and other conditions. restricting attention to MP establishments. We find that an establishment in a particular location responds to conditions of establishments in other locations that are part of the same firm. In particular, the response is of the same sign of the response to a shock in its own region. If demand is relatively high for other establishments, employment is higher. For the results in Column 1, which include Federal Reserve specific seasonal trends, industry specific seasonal trends, and Federal Reserve district time fixed effects, the other effect about 1/4 of the own effect.[28] Column 2 shows that we cannot identify these spillovers in a fully saturated model. Column 3 shows that there are spillovers when we restrict attention to local industries as defined above. In this specification, the other effect is as large as the own effect, though only one of the two effects is statistically significant at the 10% level.

This is suggestive evidence the changes in retail sales act more like a cashflow shock than an investment opportunity shock. Cashflow shocks, as opposed to investment shocks, always generate a positive correlation in employment across establishments within the firm. If it were the latter, we would expect to see a negative effect of economic conditions in other regions on an establishment's own employment. It is difficult to directly interpret the magnitude, but relative to the direct effects, these spillovers appear economically meaningful .

Table 3 reports the results from equally weighting establishments in constructing other conditions. Results are qualitatively similar, which suggests that the choice of weighting the shocks to other establishments within the same firm is not crucial in generating the results. Besides MP firms that span multiple Fed districts, there is a group of firms and establishments that are located in one Fed region. One may think that this group is not a fair comparison since for this group, a shock in the local region affects all the establishments at the same time independent of any internal capital market effects. So we consider a specification where we drop all of these establishments and firms. Table 4 reports the results when we drop all the firms with all of its establishments in a single Fed region. The results from this specification are again quite similar

[28]We have found little evidence that these own and other sensitivities vary these 4 years.

6.2 Long Differences

As noted above, we employ a long difference specification to examine the effect of a credit supply shock. In this specification, we restrict attention entirely to establishments producing local goods. Table 5 shows the results of these specifications. While the own effects of both of these credit variables are only marginally statistically significant, all the estimates are economically significant. To interpret the magnitudes, consider two establishments similarly situated with the only difference between the two between a difference in the other conditions of one standard deviation. In this case, if we use the bank survival measure, the difference in the symmetric growth rate in employment between these two establishments would be $8.2 = 0.525 * 15.57$ percentage points. For the deposits measure, this difference would be $7.92 = 0.198 * 40$ percentage points.

Understanding how local credit conditions spilled over inside of a firm requires understanding how credit mattered at this time. 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). Under the investment theory, local businesses would 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 investment spending are located in the same region. Ben Bernanke made exactly this point in commenting on Cole and Ohanian (2000) on pg. 260: "[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 people like Lee and Mezzanotti (2017) observe these links between local credit and local economic outcomes?[29] We would argue that this reflects the trade credit channel where banks play an important role in facilitating purchases of goods by local wholesalers. In fact, these so-called "real bills" were an important form of collateral at the discount window. This is the way in which firms' internal networks may be useful because internal financial resources can be used to substitute for a lack of external credit.

[29] Their results, which depend on the financial dependence of an industry, cast doubt on a version of the Friedman-Schwartz hypothesis that emphasizes the effects on bank failures on consumers and the ability to tap their deposits.

These results highlight a potential concern with number of recent papers (Ziebarth, 2015; Lee and Mezzanotti, 2017; Mldjan, 2018; Quincy, 2019) that have attempted to identify the effects of the banking panics during the Great Depression on economic outcomes using quasi-experimental setups. An underlying assumption in these papers is that there are no spillovers from the "treatment" units to the "control" units. This is plausible when considering whether banking outcomes in one county, say, spillovers onto outcomes in another county because of the fragmentary nature of banking markets at the time. Our results highlight another way in which banking panics in one area can have spillover effects on other areas through the existence of these firm networks. Put slightly different way, these networks provide a micro-foundation for how local shocks can end up looking like an aggregate ones. It is an empirical question to what extent this channel has affected the estimates from this literature, which we leave for later work.

6.3 A Permutation Test

We run a permutation test to quantify the uncertainty in our estimates. To do this, we randomly assign establishments to firms, calculate other conditions with this random assignment, and then rerun our spillover 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 establishment identifiers be denoted by E and the set of firm identifiers F. Define the assignment $f : E \to F$ as the (surjective) function that assigns a firm identifier to each establishment identifier. We then draw a random permutation of the establishment identifiers σ and apply this randomly generated assignment rule $f \circ \sigma$. With this artificially generated dataset, we rerun our main specifications and collect the point estimates on the own and other effects of retail sales. We repeat this process 250 times and compare our actual estimate to the distribution of generated effects.

This test can be thought of as a joint hypothesis test of the quality of the firm matching and the existence of firm network effects. If our firm matching was no better than random, then our estimated effect should not be unlikely given the counterfactual distribution of estimated effects. If we assume that the errors in our firm matching are relatively small, then we think of this comparison 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 this generated density of treatment effects to the estimated one.[30] In our case, we think of the firm network as the treatment that we will

[30] 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,

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 cases, in our case, there are many plausible counterfactual distributions that could be constructed. 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 locations of establishments within firms. For example, if an industry was composed of two firms with two establishments each with an establishment in both region A and B, then this balancing requirement would impose the restriction that any artificially generated firm would have an establishments in both regions. In the other direction, one can imagine, for each iteration, drawing a number of firms and a distribution of establishments across those firms completely at random. We chose what we consider a middle route in terms of the constraints we impose in constructing the randomly generated distributions. We require the marginal distribution of establishments per firm to be the same in each generated sample. On the one hand, we want to rule out what we think our implausible counterfactuals such as an industry made up of one firm owning all the establishments. At the same time, we do not want to rule out counterfactuals that change the distribution of establishment observables across firms.[31]

Figure 5 shows the results of this test. We plot the distribution of generated regression effects based on 250 simulations as well as the actual estimates for own and other demand conditions from Column 1 of Table 2. For the own sensitivity, the actual estimate is roughly centered in the middle of the simulated distribution, but even the smallest value is greater than 0. Note 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. For the other sensitivity, the actual estimate is greater than all the simulated estimates, which for the most part tend to be *negative* rather than positive. This shows that these estimates are economically and statistically significant.

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 cases since we do not require the generated distribution of the other variable to have the same marginal distribution as the actual one.

[31]It also is difficult to operationalize this case since it requires specifying a metric that defines the distance between the empirical distribution of observables and the counterfactual one. Of course, this can be done, but, in our view, it would require specifying a whole host of parameters that we have no good way to calibrate.

7 Conclusion

"Big" firms matter, and they matter in particular for business cycle fluctuations. For example, in 1937, 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 that year (Hausman, 2016). So understanding business cycles and, in particular, the Great Depression is in many cases not about looking for aggregate shocks but particular shocks to "systemic" firms and understanding how those shocks propagate within the firm.

We considered one dimension of bigness as defined by whether a firm owns multiple establishments. To study the effects of these networks of establishments in the Depression, we collected a an establishment-level dataset from the Census of Manufactures and linked establishments to their parent firms. We then documented that employment at MP firms was more correlated with local "demand" conditions as proxied by a retail sales index but less so with regional Fed discount rates. In addition, we found that shocks tended to spillover between establishments part of the same firm located in different regions as a function of differences in the pledgeability of revenue.

Going forward, one salient feature of the Depression was the synchronicity in, for example, manufacturing employment across regions of the county (Rosenbloom and Sundstrom, 1999). In ongoing work, we are working to extend the static model in this pattern in a dynamic and quantitative direction. The goal is to quantity what fraction of the spatial correlation in economic outcomes over this period is due to these firm networks.

A Proofs

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

Proof 1 *To show this, we start with the FOC for l_i*

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

Multiply both sides by l_i and factor out a κ_i to get

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

Now sum over i and use the fact that $\sum_{i=1}^{N} \kappa_i y_i = \sum_{i=1}^{N} w_i l_i$ to get

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

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.

We can now prove the result in the paper for the optimal labor choice as a function of λ.

Proof 2 *The FOC for l_i can be written as*

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

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

A.2 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 $\alpha_i(\lambda) = \frac{w_i l_i}{\sum_{i=1}^{N} w_i l_i} = \frac{w_i^{1-\sigma} z_i^{\sigma}(1+\lambda\kappa_i)}{\sum_{i=1}^{N} w_i^{1-\sigma} z_i^{\sigma}(1+\lambda\kappa_i)}$ where we noted the dependence of α_i on λ. 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 the value for λ and we will use it to derive the comparative statics. We can then use the implicit function theorem to calculate the derivative of λ with respect to the various parameters a_i, 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 $B = \sum_{i=1}^{2}\kappa_i(1-\kappa_i)\left(\frac{w_i^{1-\sigma} z_i^{\sigma}}{w_{-i}^{1-\sigma} z_{-i}^{\sigma}}+1\right)+\sigma(\kappa_1-\kappa_{-i})^2 > 0$. So if κ_i increases, the financing constraint is relaxed and λ falls. If a_i increases, the financing constraint is relaxed if and only if $\kappa_i > \kappa_{-i}$, i.e. establishment i's are 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 a_i

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

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.

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Table 1: Relative Importance of MP Establishments by Industry

Industry	Percentage in an MP firm of... Revenue	Employment	Establishments
Beverages	22	17	13
Ice cream	52	49	25
Ice, Manufactured	66	60	50
Macaroni	0	0	0
Malt	21	28	32
Sugar, Cane	26	38	13
Sugar, Refining	59	59	52
Cotton Goods	59	59	45
Linoleum	19	25	29
Matches	48	47	29
Planing Mills	17	17	10
Bone Black	73	69	65
Soap	96	95	65
Petroleum Refining	81	82	54
Rubber Tires	92	90	72
Cement	68	72	61
Concrete Products	18	15	9
Glass	60	55	37
Blast Furnaces	78	75	58
Steel Works	86	84	64
Agricultural Implements	75	76	14
Aircraft and Parts	33	37	18
Motor Vehicles	80	70	32
Cigars and Cigarettes	52	41	27
Radio Equipment	38	42	8

Notes: These numbers are percentages of industry totals in 1929 by MP status. The "Establishments" column is the percentage of establishments that are part of an MP firm.

Table 2: Sensitivity to Other Economic Conditions

	Log Wage Earners			
	(1)	(2)	(3)	(4)
Own Retail Index	0.426***		0.131	
	(0.090)		(0.118)	
Other Retail Index	0.100**	0.002	0.116*	0.059
	(0.042)	(0.044)	(0.060)	(0.064)
Fully Saturated?	No	Yes	No	Yes
Industries	All	All	Local	Local
Observations	72131	71629	28549	28526

Notes: These data are at a quarterly frequency. The retail index is defined at the Federal Reserve district. The base specification includes Federal Reserve specific seasonal trends, industry specific seasonal trends, and Federal Reserve district time fixed effects as well as district, industry, and year fixed effects. The "Fully Saturated" model includes fixed effects for all possible interactions between Federal Reserve region, month, year, and industry. Standard errors are robust. We restrict attention to establishments that are part of a MP firm.

Table 3: Sensitivity to Other Economic Conditions:
Equal Weights

	Log Wage Earners			
	(1)	(2)	(3)	(4)
Own Retail Index	0.348***		0.131	
	(0.090)		(0.114)	
Other Retail Index	0.187***	0.114**	0.122**	0.104*
	(0.043)	(0.044)	(0.051)	(0.053)
Fully Saturated?	No	Yes	No	Yes
Industries	All	All	Local	Local
Observations	72684	72182	28721	28699

Notes: These data are at a quarterly frequency. The retail index is defined at the Federal Reserve district. The base specification includes Federal Reserve specific seasonal trends, industry specific seasonal trends, and Federal Reserve district time fixed effects as well as district, industry, and year fixed effects. The "Fully Saturated" model includes fixed effects for all possible interactions between Federal Reserve region, month, year, and industry. Standard errors are robust. We equally weight establishments in constructing the other variables. We restrict attention to establishments that are part of a MP firm.

Table 4: Sensitivity to Other Economic Conditions:
Drop All in One District

	Log Wage Earners			
	(1)	(2)	(3)	(4)
Own Retail Index	0.416***		0.184	
	(0.114)		(0.151)	
Other Retail Index	0.167***	0.140**	0.119*	0.128
	(0.051)	(0.055)	(0.071)	(0.082)
Fully Saturated?	No	Yes	No	Yes
Industries	All	All	Local	Local
Observations	46984	46406	17827	17805

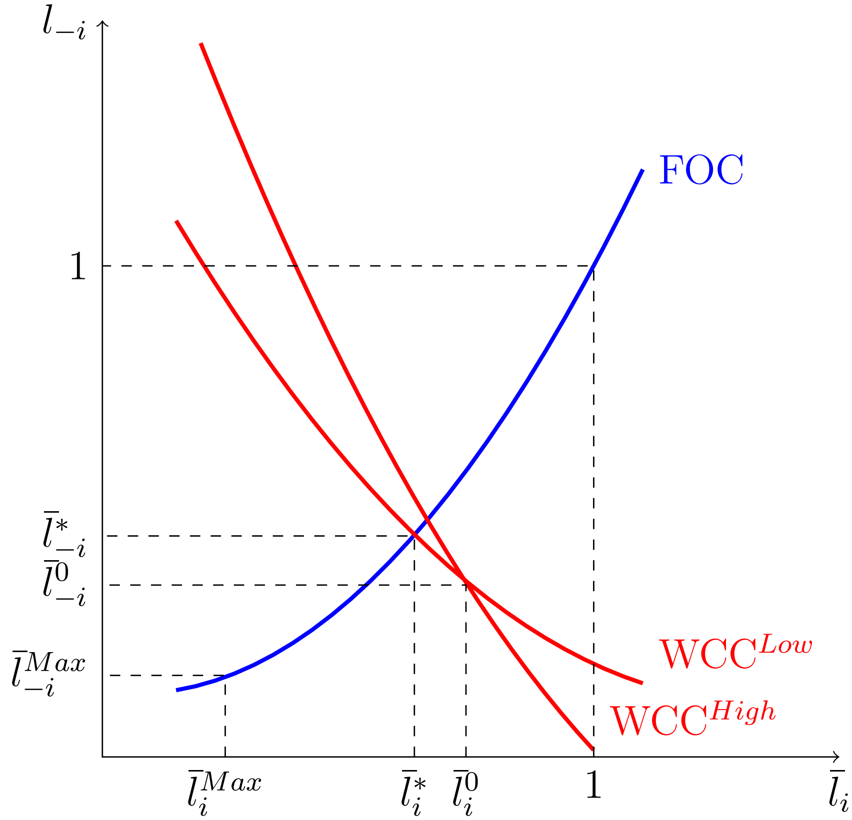
Notes: These data are at a quarterly frequency. The retail index is defined at the Federal Reserve district level as is the discount rate. The variable MP is an indicator for whether an establishment is part of a multi-plant firm. The base specification includes Federal Reserve specific seasonal trends, industry specific seasonal trends, and Federal Reserve district time fixed effects as well as district, industry, and year fixed effects. The "Fully Saturated" model includes fixed effects for all possible interactions between Federal Reserve region, month, year, and industry. Standard errors are robust. We restrict attention to establishments that are part of a MP firm. We also exclude firms that have all of their constituent establishments located in a single Federal Reserve district region.

Table 5: Long Differences of Banking Outcomes on Employment

	Symmetric % Change in Employment			
	(1)	(2)	(3)	(4)
Own % of Banks that Survive	0.176	0.154		
	(0.119)	(0.117)		
Other % of Banks that Survive		0.525**		
		(0.228)		
Own Symmetric % Change in Deposits			0.120**	0.109**
			(0.053)	(0.054)
Other Symmetric % Change in Deposits				0.198*
				(0.110)
Observations	1947	1930	1949	1933

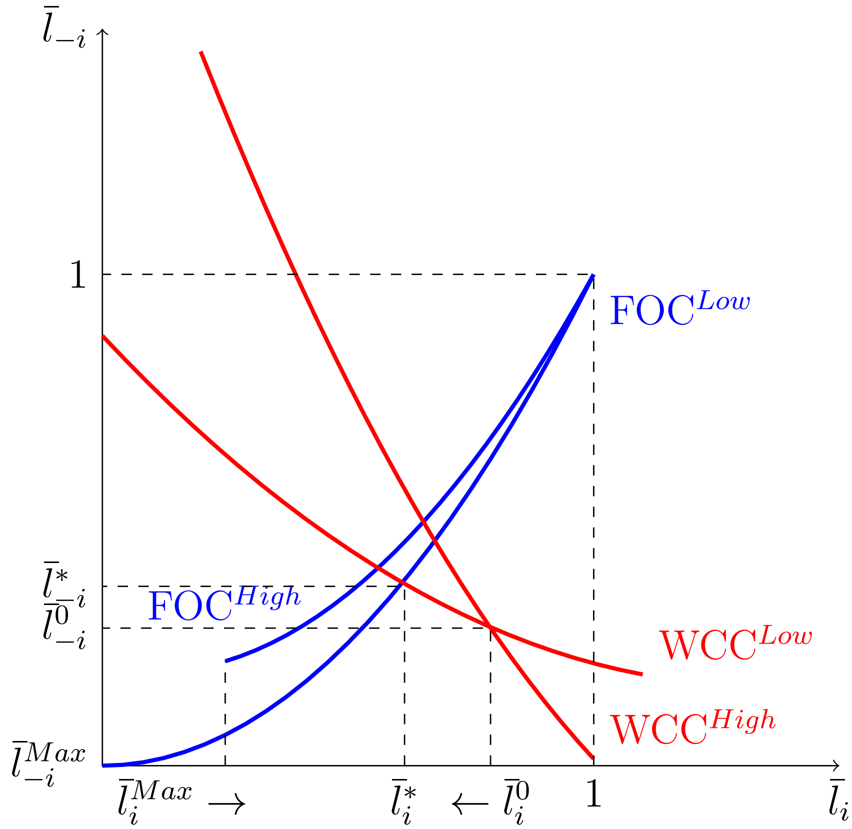
Notes: The sample is all establishments that are part of a MP firm in 1929 in local industries. Following Giroud and Mueller (2017), we use employment weights to construct an average of the changes in local banking outcomes for all other establishments in a given firm. The percentage change for employment and in deposits is the symmetric change between 1929 and 1933, which treats cases of 0s in a symmetric way for the purposes of calculating a percentage. Standard errors are robust.

Figure 1: Comparative Statics of an Investment Opportunity Shock



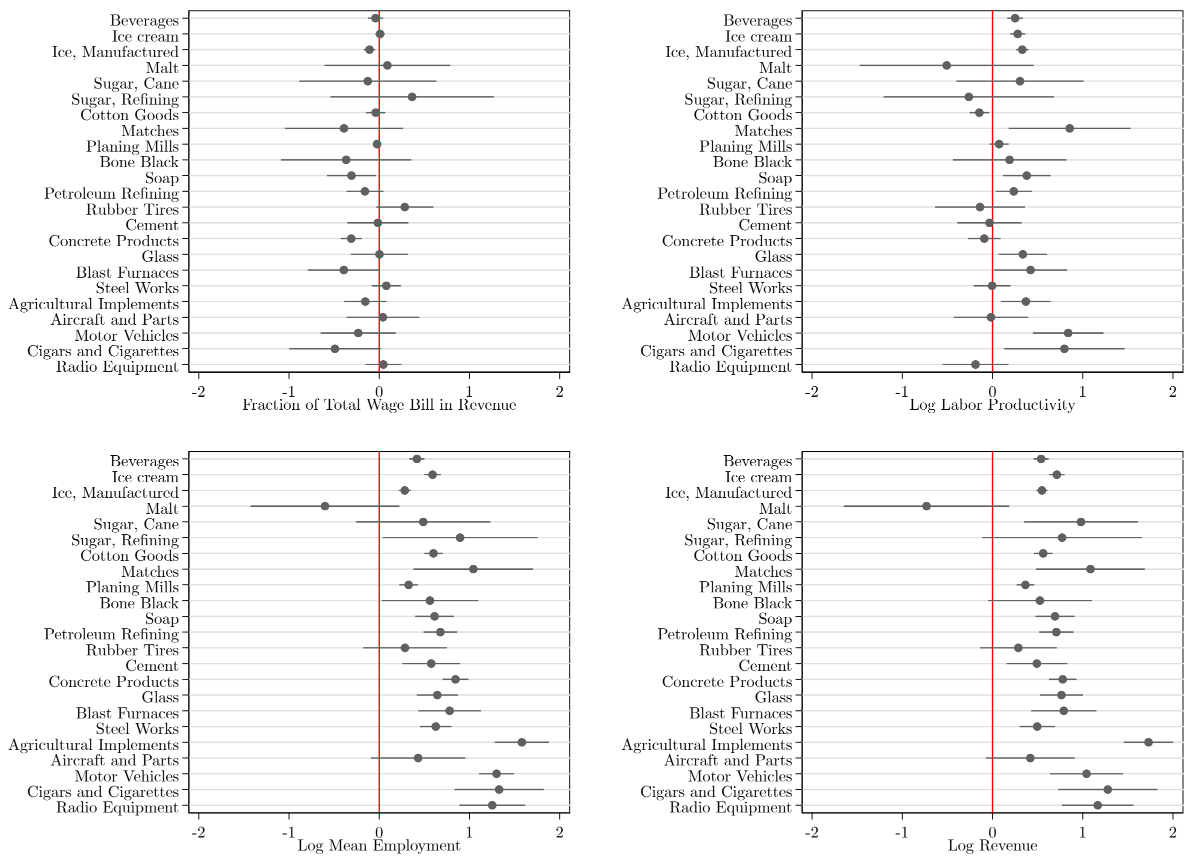
Notes: A positive investment opportunity shock in region i is an increase in a_i. Establishment i is subsidizing establishment $-i$ at the initial optimal choice. The WCC constraint rotates about the point $(\bar{l}_i^{Zero}, \bar{l}_{-i}^{Zero})$ where neither establishment generates any cashflow. The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

Figure 2: Comparative Statics of a Cashflow Shock



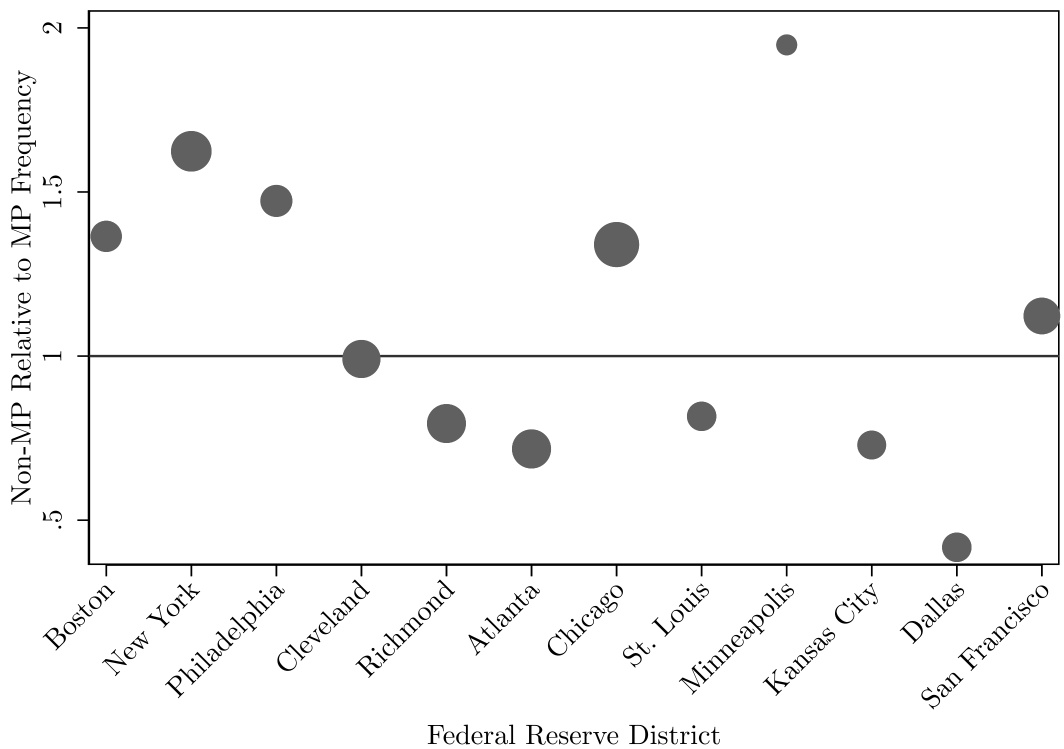
Notes: A positive cashflow shock in region i is a decrease in κ_i. Establishment i is subsidizing establishment $-i$ at the initial optimal choice. The WCC constraint rotates about the point where establishment i generates no cashflow. There are two effects on the FOC curve. First, the domain over which it is defined shrinks since $\bar{l}_i^{Max}$ increases. In addition, for all values of $\bar{l}_i$ where both FOC curves are defined, the new FOC curve is higher.

Figure 3: Comparison of MP to non-MP Establishments in 1929



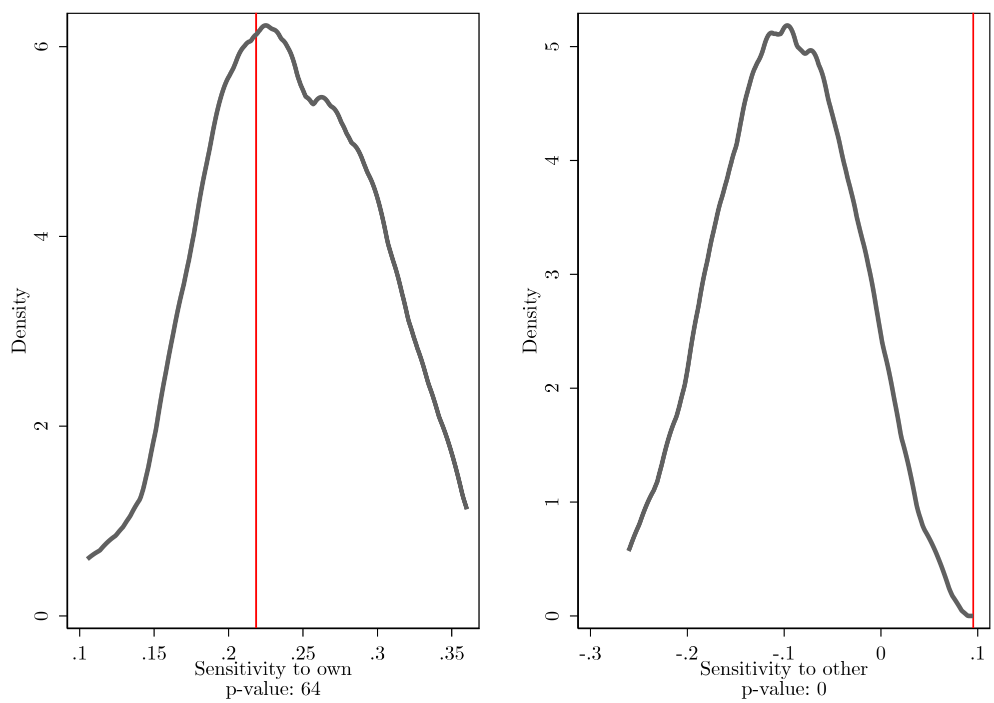
Notes: The figure reports the mean difference between MP and non-MP establishments. Labor productivity is measured as the ratio of total revenue relative to number of wage earners. Mean employment is monthly employment averaged over 1929. Each variable is log transformed besides the fraction of the total 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 robust.

Figure 4: 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 5: Permutation Test of Sensitivity to Local Demand



Notes: This is for the sample of MP establishments. Regressions include Federal Reserve district fixed effects and industry specific seasonal trends as well as an indicator for MP status. Standard errors are robust. This figure compares the actual estimates denoted by the red line with estimates using "simulated" firms. Employment is used as the weight variable to construct the other variable.