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Funding Contagion through Common Owners

Abstract

Funding contagion is the impaired ability of a firm to raise external funds when negative shocks hit other firms under the same owner. We study this possibility with pairs of private firms in unrelated industries that share a large common shareholder. We find that a firm's debt growth and financial leverage go down when the partner firm experiences negative shocks. Our results are consistent with creditors contracting the credit supply because of cash-flow cross-pledging between related firms. Funding contagion increases when control rights are strong, and the credit market is less developed.

Control rights allow firms to reallocate resources across projects, divisions, and business units (Stein, 1997; Shin and Stulz, 1998; Whited, 2001; Maksimovic and Phillips, 2002; Matvos and Seru, 2014). A consequence of these reallocations is that conglomerates and multi-establishment firms propagate shocks across their divisions. For instance, negative shocks to one division reduce investment and employment in other divisions (Lamont, 1997; Giroud and Mueller, 2015, 2019). We study whether the propagation of shocks across business units can go beyond pure internal reallocation. In particular, we study whether shocks to one unit can also make capital raising more difficult in other units.

We refer to *funding contagion* as the impaired ability of a firm to raise external funds when a related firm is hit by a negative shock. Funding contagion is important to understand the interplay between internal reallocation and external capital markets. Hann, Ogneva, and Ozbas (2013) show that the average cost of external funds is lower in conglomerates because of the cross-pledging of cash flows between units, i.e., because the income produced by one unit is (implicitly or explicitly) pledged to repay the financial obligations of other related units. We argue that, to the extent that creditors factor in cross-pledging, a negative shock to one firm reduces the funds that other firms under the same owner can get from creditors.

To study funding contagion, we focus on firms linked through a common owner rather than units or divisions within a conglomerate. In conglomerates, capital raising is done in a centralized manner with a common balance sheet for all units. The common balance sheet absorbs both the direct effect of the shock and the funding contagion toward other units, making it hard to isolate the contagion effect. While the investment response of each unit is often observable in conglomerates and has been documented by previous literature, the individual funding response is not. In our setup, instead, each unit of observation is a firm with its own balance sheet. While joint ownership is the defining feature both in conglomerates and in our setting, we can observe how individual balance sheets respond to shocks to other firms under the same owner. In other words, the key advantage of our setting that allows us to identify funding contagion is that we study the transmission of shocks *across* firms, rather than *within* firms in conglomerates or multi-establishment firms.

Our empirical strategy identifies, as cleanly as possible, funding contagion through common owners. Using *Amadeus*, a comprehensive sample of European private firms, we select shareholders that simultaneously have equity stakes in just two (non-financial) firms.

This is the simplest possible setup to explore contagion. The key identification challenge is to find pairs of firms that, although related through a common shareholder, are not related in other ways that also transmit shocks, e.g., customer-supplier links (Acemoglu, Carvalho, Ozdaglar, and Tahbaz-Salehi, 2012). For this purpose, we look at pairs of firms that operate in different industries and that are not vertically integrated. We exploit shocks that can be transmitted by the owner but are not caused by the owner herself. These hand-collected shocks represent deep industry downturns related to input and output prices or to regulatory changes.

Our main regressions study the outcomes of one firm when the partner firm is hit by a negative shock. We saturate the regressions with industry-by-year fixed effects, which implies that, within each industry and year, we identify the extent of contagion solely through the identity of the partner firm. After also accounting for firm fixed effects and lagged size, we find that a shock to the other firm's industry reduces asset growth by a cumulative 6.6% over three years. This investment response is consistent with the previous literature on conglomerates (Lamont, 1997, among others).

The key test of *funding contagion* is to examine the capital structure of the unaffected firm. We find that debt growth falls by 8.6% when a negative shock hits the partner firm. Since the effect on debt growth is larger than on asset growth, leverage significantly falls by 1.2% after a shock to the partner firm. This is primary evidence for funding contagion since leverage would remain constant or increase otherwise. For instance, debt does not change in the case that the unaffected firm only deviates internal resources (i.e., cash holdings) to help the affected firm. The unaffected firm could even increase its demand for credit to help the affected firm, resulting in higher leverage. Instead, we find that debt and leverage fall. Our evidence is consistent with creditors contracting credit supply as the partner firm is hit by a negative shock. Cross-pledging implies that the future cash-flow stream from both firms is relevant to assess the debt capacity of the two firms under a common owner. A negative shock to one firm reduces the joint cash-flow stream and, hence, reduces the funds that the firm not directly affected by the shock can raise from creditors.

We examine whether the strength of the results is related to the two main ingredients behind funding contagion. First, ownership stakes must be high enough to yield significant control rights. That is, the owner must have the ability to decide what to do with the assets of

each firm (Grossman and Hart, 1986). Consistent with control being a defining feature of the mechanism, we find that funding contagion weakens if a stake of 20% or more is absent in any of the firms. Hence, the transmission is related to common owners, i.e., those with significant control rights, and not to any common shareholder.

The second necessary ingredient for funding contagion is financial constraints, which should be pervasive in this setup. Private firms have limited access to external financing since they have not yet tapped public equity markets (Farre-Mensa and Ljungqvist 2016). Financial constraints are important for the mechanism because there should be no contagion if each firm can smooth shocks by raising enough capital from financial markets. Within our sample, we find that funding contagion is stronger in countries with relatively low credit market development. Overall, the heterogeneity related to control rights and financial constraints is consistent with the idea that funding contagion occurs through common owners rather than through other channels.

Cash-flow cross-pledging is the baseline case considered in the literature (e.g., Tirole, 2006). However, collateral cross-pledging, where hard assets instead of income are pledged, can also play a role in our results. We argue that cash-flow cross-pledging is the more natural explanation for two reasons. First, the industrial shocks that we study are more likely to represent shocks to cash flows rather than to collateral, which is often not industry-specific (e.g., real estate). Second, we find that when a high-profitability partner is hit by a shock, funding contagion is stronger, while the opposite happens when a high-tangibility partner is hit. This suggests that a reduction in collateral value is less likely to be responsible for the contraction in credit supply.

Finally, we address alternative hypotheses through a placebo test. One preeminent alternative explanation is that both firms in the pair are affected simultaneously because the demands for their goods and services are correlated, and not because they share a common owner. We form a placebo sample by looking for standalone firms that match the treated firms (i.e., firms that receive a shock to their partner firm) in terms of industry, country, year, size, and other firm characteristics. These placebo firms should have a similar demand for credit as the treated firms in our sample. At the same time, a standalone firm cannot be affected by shocks to another firm with the same owner because, by construction, their owners do not have other firms. In our test we examine whether the placebo firms reduce

their debt growth and leverage in response to shocks to their made-up partners (i.e., the partner firms of the actually treated firms). In line with our identification strategy, we do not find any significant effects for placebo firms. This confirms that the most likely explanation for our results is a credit supply reduction, not a reduction in credit demand due to correlated shocks across industries.

We contribute to the literature that studies the reallocation of resources across business units (see Stein, 2003, for a survey). Owners, or managers endowed with the control rights that owners delegate, can directly reallocate capital across units. The empirical literature is centered on the role of these internal capital markets for investment (Lamont, 1997; Duchin and Sosyura, 2013; Matvos and Seru, 2014; Giroud and Mueller, 2015, 2019). However, less emphasis has been placed on studying the effects of internal reallocation for external capital raising (Gopalan, Nanda, and Seru, 2007; Hann, Ogneva, and Ozbas, 2013, are among the exceptions). We provide novel evidence that credit supply adjusts in response to negative shocks to partner firms. In particular, when a negative shock hits a firm, other firms with the same owner find it harder to raise financing. This shows that the owner's consolidated financial position influences each firm's debt capacity, or that there is cross-pledging between firms.

Our results also speak to the recent literature on networks as a propagation mechanism for shocks (Acemoglu, Carvalho, Ozdaglar, and Tahbaz-Salehi 2012). The networks that we study are very simple (only two firms). Simplicity is precisely what renders our contribution, namely, to be able to empirically identify funding contagion through common owners abstracting from other confounding factors (e.g., input-out linkages). While we identify the effects in a simple context, the transmission mechanism that we document is potentially at play in more complex ownership networks such as business groups (Khanna and Yafeh, 2007). Achieving identification in those settings would be more challenging for at least two reasons. First, in business groups, there are links between many more firms and potentially important feedback loops. Second, large business groups can have general equilibrium effects (Almeida and Wolfenzon, 2006), which we can put aside when studying small firms. Besides being an interesting laboratory, small private firms are in and of themselves important for aggregate employment and wealth (Hurst and Pugsley, 2011;

Lyandres et al., 2019), and account for a large fraction of business cycle fluctuations (Gertler and Gilchrist, 1994).

Our setup of two-firm networks is similar to the one used by Larrain, Sertsios, and Urzua (LSU, 2019). LSU study the effects of leaving a two-firm network to identify the overall advantage of network affiliation. LSU conclude that this advantage is related to the cross-pledging of hard assets since they find that the debt capacity of a firm that splits from a group is reduced more strongly when they lose a high-tangibility partner. Our paper, instead, focuses on funding contagion with a sample of firms that are still paired. By definition, contagion from one network member to another has to be studied while both members are part of the network, and not after they split. In short, funding contagion cannot be studied within LSU's setup of firms that leave a network. Moreover, our results point towards cash-flow cross-pledging, instead of collateral cross-pledging, as the main mechanism behind funding contagion when firms are part of the same ownership structure.

The rest of the paper is organized as follows. Section 1 develops the main hypotheses and explains the empirical design of our tests. Section 2 describes the firm-level data and the identification of the industrial shocks. Section 3 shows the main results and the mechanisms, together with auxiliary results. Section 4 concludes.

1. Conceptual Framework

a. Hypotheses Development

Consider a simple model of investment with financial constraints in the spirit of Stein (2003). There are two firms $i = 1, 2$ with a common large shareholder. For the purposes of the model, we assume that the large shareholder controls the investment policy of both firms. This is the maintained assumption throughout the theoretical analysis, although empirically, we test for a breakdown of the mechanism when control is absent. For simplicity, investment I_i can be described as a one-period project that produces cash flows through a standard concave function $F(I_i)$. Cash flows are discounted with the risk-adjusted rate R_i . The cash-flow rights of the owner are a fraction α_i of each firm. The objective function of the owner is therefore $\alpha_1(F(I_1) - R_1 I_1) + \alpha_2(F(I_2) - R_2 I_2)$.

The financial constraint can be written as $I_1 + I_2 = w_1 + E_1(w_1, w_2, \theta) + w_2 + E_2(w_1, w_2, \theta)$, where w_i represents the internal resources in each firm and the function $E_i(w_1, w_2, \theta)$ represents the external funds that each firm can raise. Internal resources can be cash flows or hard assets to use as collateral. The external funds function simply captures the imperfect substitutability between internal and external funds, and hence the failure of Modigliani-Miller's irrelevance theorem in our setup. The parameter θ summarizes the financial frictions in the market. For example, θ can be the multiplier on internal resources as in $E_i(w_1, w_2, \theta) = \theta(w_1 + w_2)$.

Notice that the financial constraint is a joint constraint, meaning that funds can be moved across firms. For example, the owner can use the internal funds of one firm to fund the investment of the other firm. Our main departure from standard models in the literature is that we consider separate external funds' functions for each firm. We allow each one to be a function of the internal funds of both firms, so there is cross-pledging in our formulation (Tirole, 2006, chapter 4). Cross-pledging refers to the situation where the internal resources of one firm are (implicitly or explicitly) pledged to repay the financial obligations of the other firm. Basically, creditors consider the consolidated position of the owner when determining the supply of external funds to each firm. Cross pledging implies that the first derivative of $E_i(w_1, w_2, \theta)$ with respect to the internal resources of the other firm is positive ($E'_{ij} > 0$).

The first-order condition for the optimization problem is $\alpha_i(F'_i - R_i) = \mu$, where μ is the shadow price of the financial constraint. If firms are not financially constrained ($\mu = 0$), then each firm invests at its first best level ($F'_i = R_i$), and there is no cross-firm dependence despite common ownership. If firms are financially constrained ($\mu > 0$), then firms do not invest at their first-best level. The owner still invests up to the point where the marginal benefit of investment in both firms is the same, i.e., $\alpha_1(F'_1 - R_1) = \alpha_2(F'_2 - R_2)$.

We examine the effect of a shock to the internal resources of firm 2 (w_2) on the investment of firm 1 (i.e., asset reallocation). We can show that:

$$\underbrace{\frac{\partial I_1}{\partial w_2}}_{\text{asset reallocation}} = \underbrace{\frac{1 + E'_{22}}{1 + \frac{\alpha_1 F''_1}{\alpha_2 F''_2}}}_{\text{pure reallocation}} + \underbrace{\frac{E'_{12}}{1 + \frac{\alpha_1 F''_1}{\alpha_2 F''_2}}}_{\text{funding contagion}} > 0 \quad (1)$$

In words, a negative shock to the internal resources of firm 2 reduces investment in firm 1, even when the internal resources of firm 1 (w_1) or the relevant discount rate for firm 1 (R_1) remain constant. Asset reallocation has two components that we label pure reallocation and funding contagion. Pure reallocation refers to the fall in investment to accommodate the direct fall in the internal funds of firm 2 plus the fall in the external funds that firm 2 can raise ($1 + E'_{22}$). Even if firm 1 can raise the same amount of external funds as before (if $E'_{12} = 0$), the owner forces firm 1 to reduce its investment so the investment of firm 2 does not fall as much. Funding contagion, the second term in equation (1), refers to the additional drop in investment that is caused by the reduction in the external funds that firm 1 can raise.

There is asset reallocation ($\partial I_1 / \partial w_2$) if there is funding contagion ($E'_{12} > 0$), but there can be asset reallocation without funding contagion. Hence, evidence of asset reallocation (i.e., to find an effect of a shock to firm 2 on the investment of firm 1), by itself does not imply nor contradict funding contagion. To really test for funding contagion we need to look beyond investment and study funding variables (i.e., directly looking at $E'_{12} > 0$). Similarly, evidence for funding contagion does not exclude the existence of pure reallocation.

The owner can reallocate funds between firms because of the control rights granted by ownership (Grossman and Hart 1986). This is expressed in the joint financial constraint for the two firms. As noted by Stein (2003), firm 1 can be forced to accommodate the shock to firm 2 and invest less than it would invest as a standalone firm. By the same token, firm 2 can get additional financing to smooth its own shock when outside funding is limited.¹ If control rights are weak or if they disappear, then the link between the two firms should weaken or disappear too. Control is best characterized as a dichotomic variable: the owner either controls the investment policy or not. Still, within our setup, we can simulate the absence of joint control by studying the case where $\alpha_2 \rightarrow 0$. As implied by equation (1), in this case the investment in firm 1 does not respond to shocks in firm 2. In other words, the links between both firms are severed when joint control is absent.

As emphasized by Kaplan and Zingales (1997), the response of investment in equation (1) is mediated by the curvature of the production function. The curvature reflects how costly

¹ The derivative of investment with respect to a firm's own internal resources is analogous to the cross-derivative in equation (1). Interestingly, the effect of own funds is smaller than in a standalone firm, precisely because of the smoothing effect produced by the second firm. See also Giroud and Mueller (2019) on this point.

it is for a firm to distort investment away from the first best. If firm 1 is small, which suggests high F_1''/F_2'' , then the response of firm 1 to shocks on firm 2 is weak. In this case, firm 1 is so constrained that adjusting its investment plan to accommodate the other firm's shock is too costly for the owner.

It is interesting to compare our setting of one owner and two firms with a conglomerate of two fully-owned divisions ($\alpha_1 = \alpha_2 = 1$). In this case, the financial constraint can be written as $I_1 + I_2 = w_1 + w_2 + E(w_1, w_2, \theta)$. Notice that there is only one external funds' function $E(w_1, w_2, \theta)$ since, in a conglomerate, there is only one vehicle for raising capital. Investment is still observed individually for each division, but not capital raising. The equivalent to our previous equation (1) is now:

$$\frac{\partial I_1}{\partial w_2} = \frac{1 + E_2'}{1 + \frac{F_1''}{F_2''}} > 0 \quad (2)$$

The impact on investment encompasses both pure reallocation and funding contagion as before. However, now there is only one sensitivity of external funds to shocks (E_2') because capital raising at the conglomerate level subsumes the two effects that before we identified separately ($E_2' = E_{12}' + E_{22}'$). The advantage of observing separate vehicles for raising capital, i.e., separate balance sheets, is that we can identify funding contagion independently since we can measure cross-effects (E_{12}'). That is, we can see if shocks to a firm affect the partner firm's ability to raise external funds.

b. Empirical design

The combination of strong control rights and financial constraints suggests that private firms are a good laboratory to test these hypotheses. Private firms usually have concentrated ownership, which results in strong control rights. Private firms have not yet accessed public equity markets, so they face more obstacles to raising equity. Often, they do not have a debt rating, which is another proxy for financial constraints (Kashyap, Lamont, and Stein 1994; Faulkender and Petersen 2006).

Besides being an interesting laboratory, small private firms are an important part of wealth, employment, and business cycle fluctuations. Equity in small private firms represents close to one-third of business owners' wealth (Lyandres et al., 2019). At the aggregate level,

controlling stakes in companies represent the second-largest source of wealth after real estate. More than 25% of owners have two or more small businesses in their portfolio (Lafontaine and Shaw, 2016; Lyandres et al., 2019), which suggests that contagion across firms with common owners has the potential for aggregate implications. In terms of their relevance for the macroeconomy, firms with fewer than 20 employees represent approximately 90% of firms, and 16% of total U.S. employment (Hurst and Pugsley, 2011).² Finally, small firms are more sensitive to recessions and hence contribute more to business cycle fluctuations (Gertler and Gilchrist, 1994).

We start with pairs of private firms that share a common large shareholder, and that operate in different industries. In this sample, we run a regression like the following:

$$Y_{it} = \beta \text{OtherShock}_{it} + \delta' X_{it-1} + \tau_{jt} + \mu_i + \epsilon_{it}. \quad (3)$$

We regress Y_{it} , firm i 's outcome in year t , on the variable OtherShock_{it} that captures negative shocks to the internal resources of the *other* firm in the pair. These shocks are defined at the industry-year level considering the industry of the other firm in the pair. X_{it-1} denotes lagged firm size.³ We proxy for investment opportunities of the own firm by saturating with industry-by-year fixed effects, τ_{jt} , at the three-digit SIC level. The industry-by-year fixed effects absorb the effect of shocks to the own industry. Hence, the identification of OtherShock_{it} comes solely from variation in the partner firm within each of the approximately 2,880 industry-year bins in the data. That is the most stringent test of contagion in our setup, and hence it is the baseline specification throughout the paper. Time-invariant unobservable characteristics are captured by firm fixed effects μ_i . Standard errors are double clustered at the level of the owner (firm-pair) and the industry of the own firm.

² See reports of the U.S. Small Business Administration at www.sba.gov.

³ Our baseline regression specification is parsimonious in the sense that it only controls for firm size, but we believe that, given the data constraints, it is the most appropriate one. The traditional control variables in capital structure papers are firm size, the market-to-book ratio or Tobin's q , profitability, and tangibility. Since the firms in our sample are private firms we do not have data for the market-to-book ratio or Tobin's q . We add industry-year fixed effects to capture swings in investment opportunities at the industry level. Profitability is only available for 20% of the firm-year observations (see OROA in Table 1). Hence, it is impracticable to include profitability in the main specification. The results go through when we control for tangibility, even if we run the risk of overcontrolling and losing observations. Lemmon, Roberts, and Zender (2008) point out that firm fixed effects (which are included in our specification) capture a lot more variation in leverage ratios across firms than traditional control variables.

The main challenge is, of course, the identification of the effect of $OtherShock_{it}$. Changes in the internal resources of the second firm can correlate with unobservable variation in the investment opportunities of the first firm. In other words, even if there are no financial constraints or control rights, we can see the internal resources of the second firm moving together with the investment or financing decisions of the first firm. Our empirical strategy is designed to suppress several sources of co-movement and alleviate endogeneity concerns. We focus on three sources of co-movement. First, since both firms share a common owner, a mistake or bias in the owner's expectations can reduce internal funds and investment in both firms, even without financial constraints. Second, correlated industry shocks can also make the internal funds in one firm, and investment opportunities in the other firm, move together. This is the case if both industries are connected, for example, through input-output links. Third, if firms face correlated demands, shifts in demand can impact the two firms simultaneously, regardless of their ownership links or financial position.

Our empirical design handles these sources of co-movement in different ways. First, to abstract from owner or firm-specific shocks we consider only industry-wide shocks. These shocks can be traced back to known sources (e.g., input prices) and leave out idiosyncratic elements of each firm or pair of firms. Unlike other work, we do not use direct changes to internal funds at the firm level, such as cash flows or sales growth, as possible shocks.

Second, in order to abstract from industry co-movement, we focus on pairs of firms in industries that are not related by input-output flows. The unrelatedness of both industries allows us to distinguish our setup from the propagation of shocks through customer-supplier links (Acemoglu, Carvalho, Ozdaglar, and Tahbaz-Salehi 2012; Barrot and Sauvagnat 2015). At the same time, it distinguishes our setup from propagation within multi-establishment firms (Giroud and Mueller 2019), where establishments usually operate in the same industry. We only look at pairs of firms, so the constraint of unrelatedness is not too binding. For instance, it is much harder to find sets of 3 or 4 firms that are industrially unrelated and that have a large common shareholder at the same time. Also, the transmission of a shock in a pair can only go from one firm to the other. With multiple firms, the owner can choose which other firms to use as a buffer for the shock, creating selection problems that muddle the experimental design.

Third, we explore the independence of our results from several demand channels through a placebo exercise. Both firms in the pair can be affected simultaneously because the demands for their goods and services are correlated, and not because they share a common owner. We form a placebo sample of firms that should have a similar demand for credit as the treated firms in our sample (i.e., similar to firms that face a shock to their partner firm), but that are not paired with any firm, i.e., they are standalone firms. We then see whether the placebo firms are affected by the shocks to our treated firms. If co-movement is due to correlated demands, then the shocks should also affect placebo firms. However, if co-movement is due to funding contagion, then placebo firms should not be affected by the shocks, because there cannot be contagion if these firms are not paired with other firms. This placebo test, combined with tests that exploit variation in financial constraints and control rights, allows us to attribute our results to a credit supply contraction instead of a reduction in credit demand.

Our identification strategy is in the spirit of Lamont (1997), who studies the effect of oil shocks on non-oil segments in conglomerates. Our approach has several advantages with respect to Lamont (1997). First, and most importantly, we deal with firms instead of segments, plants, or divisions of a conglomerate, which allows us to compute independent funding effects for the different business units. Second, we have variation in control rights within our sample, which means that we can test for the importance of control for the transmission mechanism. In contrast, full control is the only case in conglomerates. Third, segments in a conglomerate have other margins of integration besides the common owner (e.g., joint distribution channels, centralized HR, and R&D departments). The focus on firms allows us to emphasize ownership as the defining element of the transmission mechanism rather than these other margins of integration. Fourth, by focusing on common owners of just two firms, we avoid dealing with selection issues present in large conglomerates, such as the choice of which business unit to use as buffer for the shock. Fifth, we focus on small firms, which makes it easier to discard general equilibrium effects. Conglomerates and multi-establishment firms can have general equilibrium effects at the level of regions or even countries (Gabaix, 2011).

It is worth emphasizing the differences and similarities in empirical design with respect to LSU. LSU study firms that split from their groups (“treated” firms) and compare them to firms that still belong to a two-firm group (“control” firms). We only study firms that belong

to a two-firm group. The different focus of the two papers can also be seen in the variable of interest in the right-hand side of the regression. In LSU, the variable of interest is a dummy for affiliation to a group. In our setting, the variable of interest is the sensitivity to the $OtherShock_{it}$, and there is no variation in affiliation.

The similarity is that both papers use the $OtherShock_{it}$, although for different purposes. LSU use the $OtherShock_{it}$ as an instrument for group affiliation. The $OtherShock_{it}$ identifies exogenous group splits (1st stage of their instrumental variables setup), and ultimately it allows the authors to estimate the causal impact of group affiliation (2nd stage of the instrumental variables setup). Consequently, the reduced-form regression in LSU looks at first sight like our equation (3). However, the treatment of interest and identification strategies are different. The identification assumption in LSU is that the $OtherShock_{it}$ does not have a direct effect on the own firm, except for the fact that groups are more likely to split when hit by the $OtherShock_{it}$. The entire effect of the $OtherShock_{it}$ is assumed to be in the extensive margin of affiliation. In our setting, instead, the own firm and the other firm are still together, and direct transmission of the $OtherShock_{it}$ towards the own firm is the focus of study. As we show later, we can check within our setup the types of firms for which the identification assumption of LSU is more appropriate.

2. Data

a. Firm-level data

We get firm-level data from *Amadeus*, a database assembled by Bureau van Dijk that provides both accounting and ownership information for private firms in Europe. *Amadeus'* accounting data include balance sheets and income statements that can be easily accessed through WRDS. The ownership data cannot be directly downloaded, so we obtain it from *Amadeus'* DVDs at the yearly frequency. The ownership information includes the names of large shareholders and their stakes. Our sample begins in 2005 since *Amadeus* dramatically increased its coverage of private firms in the early 2000s. Regulatory requirements in Europe imply that ownership structures, together with abbreviated balance sheets, are available for every firm (Bernard, Burgstahler, and Kaya 2018). Income statements are available less frequently (e.g., Germany and the U.K. do not require their disclosure below a certain level of

firm size). The data goes up to 2014 since we get the ownership data only with a few years of delay. All the firms in our analysis are private firms.

We select individuals or families with stakes in just two firms.⁴ We require these firms to have no direct ownership stakes between them, nor with any other firm. The full description of our sample selection criteria is in Appendix A.1. Some examples can illustrate the type of firms and shareholders that we study. Steven Jones-Blackett has 49% of Wendy Jones-Blackett Limited (assets €0.54 MM), a firm in commercial printing, and a 5% stake in Metachem Diagnostics (assets €0.71 MM), a biotech distributor. Frank Balke has 100% stakes in two German firms: FRABA Fliesenbau GmbH (assets €0.26 MM), a firm in painting and paper hanging, and ASS Sicherheit GmbH (assets €0.54 MM), a firm that provides security services. These examples make it clear that we deal with small firms run mainly by owners. Some of these firms also have smaller shareholders, probably due to family or business connections.

As explained before, a key element of our identification strategy is to drop pairs of firms in well-integrated industries. Following Fan and Goyal (2006), we construct a measure of vertical integration based on the U.S. input-output matrix. Using this matrix, we compute the fraction of input (output) that an industry acquires (sells) from (to) other industries. Then, for each pair of firms, we compute the average of what each firm's industry sells to the industry of the other firm, and what it buys from the industry of the other firm. As is standard in the literature, we drop the pair if this average is larger than 1% (Ahern and Harford, 2014; Hoberg and Phillips, 2016). In the same line, we also exclude pairs where both firms are in the same three-digit SIC code. After imposing these restrictions, and if we focus on firms with non-missing book assets and asset growth, we have data for approximately 71,000 firms and 248,000 firm-years.⁵ This sample is the baseline sample that we use throughout the paper.⁶

⁴ We speak interchangeably of individuals and families because entire families are sometimes identified as shareholders in the data (e.g., in some French firms). However, most of the data is organized around individuals.

⁵ We also develop a measure of integration based on Eurostat's input-output tables for the European zone. These data are reported at the NACE2 level. The higher level of aggregation implies that integration measures increase mechanically, and therefore we drop more firms when applying our main restriction on integration (i.e., dropping firms in the same industry or with integration above 1%). Our results remain strong in this reduced sample.

⁶ The different focus of this paper and LSU can be seen in the different samples under study. LSU study close to 3,800 firms that leave a two-firm network. The "treatment" under study is leaving a network. LSU compare the treated firms with other 3,800 control firms that are always affiliated to a two-firm network. In the current paper we study approximately 71,000 firms that are always affiliated to a network. Hence, the 3,800 treated firms of LSU are not part of our sample; only the 3,800 control firms are potentially here. Overall, the possible overlap between the two samples is at most 5% of the firms studied in the current paper ($\approx 3,800/71,000$). The firms in

Figure 1 illustrates the distribution of firm pairs across industrial segments. The x-axis represents the SIC code of the first firm in the pair, and the y-axis represents the SIC code of the second firm. As explained above, pairs in related industries have been removed from the data. We find that the remaining pairs are not clustered in any combination of industries, which is also a check that the cleaning procedure for relatedness is effective. Figure 1 is consistent with owner preferences for diversification (Lyandres et al., 2019). However, the rationale behind this diversification does not seem to be systematic since we observe firm pairs all over the industrial spectrum. As emphasized by Hurst and Pugsley (2011), non-pecuniary motives (e.g., “doing something new,” or “lifestyle”) are the most important reasons for starting small businesses. These motives can potentially explain the scattered distribution of firm pairs that we see in Figure 1.

Observations are well distributed across years (see Table A.1 in the appendix for data coverage). We cover 10 Western European countries, with a focus on Germany and the U.K. Some of the cross-country differences in coverage are related to market size and reporting standards for private firms.⁷ In terms of industries, we have more observations in services, as can be expected for small firms, but overall the sample is well balanced across sectors. SIC 6 (finance) includes firms that provide finance-related services such as accounting or real estate management. Financial firms or intermediaries in the traditional sense are not present in our sample. SIC 9 (government) includes services such as security for government infrastructure.

Table 1 shows summary statistics for the main variables in our analysis. The size distribution is highly skewed. Mean (median) assets are €8.82 MM (€0.18 MM). Asset growth is our main proxy for investment since CAPEX is typically missing. Asset growth, which includes growth in fixed assets, inventories, accounts receivables, and working capital broadly speaking, is also a better measure of investment for firms in services. Debt growth is the percentage change in the book value of total debt, and leverage is book debt over assets. Debt includes all types of liabilities: bank loans, trade credit, factoring, leases, and any non-bank private debt, such as loans from business development companies, credit unions, fintech lenders, and others. Debt issuance (retirement) is a dummy for cases where the annual change

LSU are also smaller than our firms. Average firm assets are €1.5 MM in LSU (see their Table 2), while they are €8.8 MM in our sample (see our Table 1).

⁷ Appendix A.1 explains the reasons behind the country coverage of our main sample. The availability of ownership data represents the main restriction.

in debt over lagged assets is greater (smaller) than 5% (-5%) (Leary and Roberts, 2005). Net debt issuance is equal to 1 for large issuance, -1 for large debt retirement, and 0 otherwise (i.e., the difference between the dummies for issuance and retirement).

Amadeus also reports information on the names of banks with which a firm has a loan each year. There is no information on the size of loans or the interest rate. Small firms, like the ones in this sample, are only required by law to report abbreviated notes to financial statements, which typically do not include information on banking relationships. Data providers in each country complement balance sheet notes with other public and private sources (e.g., credit registries, courts, etc.) to get a more complete picture of credit relationships. Still, missing reports can partly explain the low average number of relationships (0.67) in our sample. Moreover, non-bank credit relationships that are crucial for small businesses (e.g., trade credit, leases, etc.) are typically left out. Due to these reporting issues, we only use banking relationships as a secondary data source (appendix A.2 explains in more detail the coverage of the banking data). We focus on breaking banking relationships, which happen with a frequency of around 13%. This variable is less likely to be affected by the absence of coverage since the banking relationship has already been reported in the past.

We note that the mechanism in our model can be applied broadly to any credit relationship and not only to banking relationships. Non-bank lending is also affected by the main idea behind funding contagion, namely that the credit provider considers the consolidated position of the owner when extending credit to any of her firms. Credit providers of all types constantly screen their clients and gather information to assess their risk. Hence, funding contagion may still operate in the absence of banking relationships.

The mean (median) number of employees is close to 13 (4), which shows again that these are small firms. The mean (median) operating return on assets ($OROA = EBIT/assets$) is 4% (2%), but it is missing for many firms in the sample given the reporting standards for income statements. Mean (median) sales are 1.91 (1.42) times assets, suggesting that many of our sample firms are in services. The low mean (median) tangibility of 25% (13%) points in the same direction.

The average ownership stake is relatively high (61%), which can be expected from the focus on private firms. As seen in Figure 2, the distribution of stakes has clusters around 50% and 100%. The median stake is 50%. There are still many smaller stakes, which provide

interesting variation for our tests. For example, approximately 15% of firms have a shareholder with a stake of 20% or less.⁸ Large changes in owner stakes are very infrequent (1% of observations), as shown by the average of the dummies for stake increases and decreases when these changes are larger than 5% in absolute value.

b. Industrial shocks

The identification of shocks follows LSU. We proceed in a reverse engineering fashion by identifying candidate shocks from the stock returns of listed firms. Although the firms in our sample are not listed, they are likely to be affected by the same shocks. For instance, many small firms provide services to the listed firms in their industries. We first form four-digit SIC industrial portfolios in each European stock market. We then select periods where the 6-month returns on these portfolios fall below the 5th percentile of the empirical distribution. From this list of candidate shocks, we select poor returns that affect a given industry across Europe. That is, we make sure that shocks are not driven by a few firms. Then, we check by hand, in the press or analyst reports, the likely cause of the negative returns. We only use periods of poor returns where we can confidently pin down the potential source of the shock, which we classify in terms of input or output prices (e.g., materials, commodity prices, etc.), and regulation (e.g., safety laws, tobacco laws, etc.).

After merging the shock database with our main database (which is at the three-digit SIC level), we have 255 industry-year shocks for 116 unique three-digit SIC codes.⁹ The shocks that we identify are deep downturns. The average industry return in a shock-year is -30% lower than the sample average (see Table A.2 Panel B in the appendix). Even after accounting for year and industry fixed effects, the average return in a shock-year is -18%.

In order to provide context, we discuss some examples of shocks and the firms they hit. David B. Sharples owns two U.K. companies that are hit at different points in this sample period. The first one (Holdsworth Chocolate Limited) operates in SIC 206 (chocolate and cocoa products) and experienced a negative shock in 2009 due to the strong increase in the cost of cocoa and sugar. The second one (Bond Printing Associates) operates in SIC 275 (commercial

⁸ An additional difference with the sample in LSU is that we include shareholders with small equity stakes. The shareholders in LSU have a median stake of 100%, as seen in their Table 2. Shareholders with smaller stakes allow us to test whether significant control matters for shock transmission, or if any ownership link suffices.

⁹ Details on the aggregation of shocks to the three-digit level are explained in Table A.2 Panel A of the appendix. Appendix A.3 describes more examples of shocks in our sample.

printing) and suffered from the rise in the price of ink during 2011-12.¹⁰ These are two examples of input shocks. Other shocks refer to output prices or regulations. For instance, recycling companies in SIC 505 (metal service centers) did poorly because of low output prices when commodity prices fell in 2014-15. Similarly, companies in SIC 382 (measuring and controlling instruments) experienced a regulatory shock that increased production costs with the harmonization of weighting instruments across Europe.

For our identification strategy, we need shocks to be exogenous to the firms under study, which is more likely to be the case with small firms, but we do not claim that shocks are exogenous to market forces in general. Naturally, input and output prices can be related to macro crises and aggregate cycles. For example, a spike in the price of gold represents a shock to a gold-intensive industry such as jewelry. In our sample period, the price of gold can increase due to uncertainty around the European banking crisis and not because of a pure demand or supply effect of gold (e.g., mine closings in Africa). This is not a problem for our empirical design since we compare a firm paired with a jewelry firm with a firm paired with another firm in a different industry. If the price of gold is simply a reflection of macro uncertainty, it will similarly affect both firms (i.e., the one paired with a jewelry firm and the one paired with a firm in a different industry). In that case, we would not find a significant coefficient on $OtherShock_{it}$ in equation (3). Our identification assumption is, therefore, that the $OtherShock_{it}$ does not *differentially* affect both firms, except for the links that firms have to their partner firms.

An example from our sample can help to illustrate how the identification strategy works. In the years 2007-2013, Claudia Berlin had stakes in two firms, Creba Haus GmbH, a real estate management company (SIC 653), and Envitex Textile Industrieprodukte GmbH, a manufacturer of industrial filters for air pollution (SIC 281). In the years that followed the financial crisis of 2008-2009, real estate managers were struggling to produce cash flows (e.g., high vacancy rates), while Envitex's industry enjoyed a healthy profitability of around 9%. Despite the strong profitability, Envitex had negative asset and debt growth. For example, in

¹⁰ David B. Sharples and Genevieve E. Sharples each owns 50% of Holdsworth Chocolate Limited. David B. Sharples and Robert H. Stead each owns 49% of Bond Printing Associates. Hence, David B. Sharples is the common shareholder between both companies.

2011, its debt contracted 27.6% against a growing and profitable industry. Our regression model in equation (3) basically tests whether this behavior generalizes to a large sample.

In the bottom part of Table 1, we report summary statistics for dummy variables that take a value of one the year an industry suffers a shock and zero otherwise. The average of each dummy corresponds to the frequency of being hit by a shock. About 11% of the firm-year observations correspond to years when a shock hits the firm's own industry. Similarly, 11% of the observations correspond to years when a shock hits the industry of the other firm in the pair. These numbers imply that not all firms are hit by shocks, either to their own industry or to the industry of the other firm in the pair. Also, it implies that shocks are more narrow than global recessions and aggregate cycles.

Figure 3 shows the frequency of shocks by industrial code. To simplify the picture, we aggregate to the one-digit SIC level, but shocks are identified at a more granular level. More than 30% of the firm-years in SIC 1 suffer a shock to their own industry, which is close to three times the sample average. This could be related to our procedure for pinning down shocks. However, notice that while *own* shocks are more frequent in SICs 1 through 3, there is no cluster of *other* shocks in any industry. This last comparison is the relevant one since we get identification from *other* shocks rather than from *own* shocks. As can be seen in Figure 3, it is not the case that firms in any industry are more frequently paired with other firms that are hit by shocks. Hence, the distribution of *other* shocks is consistent with quasi-random assignment, which is what matters for our empirical design.

We further validate our measure of shocks by presenting regressions that relate firm-level investment to shocks. In the first three columns of Table A.3, we present regressions without industry-by-year fixed effects, so the coefficient on $OwnShock_{it}$ can be identified in conjunction with the coefficient on $OtherShock_{it}$. Consistent with the prior literature, negative shocks to the firm's own industry and to the industry of the other firm in the pair reduce asset growth.¹¹ The coefficient on $OtherShock_{it}$ is very stable across specifications

¹¹ The coefficient on $OwnShock_{it}$ is slightly larger than the coefficient on $OtherShock_{it}$. According to the model, both effects should be symmetric. In practice, one could expect the magnitude of the coefficient of $OwnShock_{it}$ to be larger because of endogeneity: future investment opportunities go down, as well as own internal funds, when there is a shock. However, there could also be a selection effect since own shocks are not evenly distributed across industries (Figure 3). It is plausible that industries more frequently affected by own shocks are better at dealing with them and suffer a smaller impact on investment. Hence, the relative magnitude of the coefficients of own and other shocks is empirically an open issue.

regardless of the level of aggregation of the industry-by-year fixed effects, which eventually absorb the own shock.

Our shocks focus on extreme negative events. The industry Tobin's q , which is widely used in the literature on internal capital markets, is perhaps more appropriate to estimate the response to average shocks. In Table A.3 Panel B in the appendix, we present regressions of asset growth on the Tobin's q of the own industry and the Tobin's q of the *other* industry (Shin and Stulz 1998; Rajan, Servaes, and Zingales 2000). We find that both q 's enter the regression significantly and positively, in line with the previous literature. Still, our shocks are economically and statistically significant regardless of the inclusion of q 's (see column 8 of Table A.3 Panel B). Measurement error and the consequent distortion in statistical inference are a disadvantage of q (Whited, 2001). Our shock variables deal with measurement error by focusing on a subset of shocks with well-identified sources.

3. Results

a. Funding contagion

In Table 2, we show our main tests of funding contagion. The idea is to see whether shocks to a partner firm reduce the ability of a firm to raise external funds. Because of adjustment frictions, the funding response can be spread out in time. Hence, we consider if there is a shock to the industry of the other firm in that year or in any of the previous two years. We combine this three-year period into a single dummy, *OtherShock 3p*, equal to 1/3 each one of the three years and 0 otherwise. The advantage of this normalization is that the coefficient on *OtherShock 3p* can be understood as the cumulative effect over the three-year period that follows a shock. Hence, it captures the medium- or long-run effects of the shock in an easily interpretable way.¹²

¹² Alternatively, one could run a conceptually similar regression by including three different dummies (one for each year) and report a test for the sum of the three coefficients. Table A.4 in the appendix reports the results using dummies for the years before and after the shock. There is no anticipation before the main year of the shock, which is reasonable given that shocks are identified from stock prices that move ahead of real variables such as asset growth. This absence of a secular decline in the firms that subsequently experience other shocks is consistent with the parallel-trends assumption of the differences-in-differences setup. There is some persistence as indicated by the coefficients on the years following the main year. *OtherShock 3p* captures the period between $t=0$ and $t=+2$. The coefficients on *Other Shock 3p* in Table 2 do not match the sum of the coefficients for $t=0$, $t=+1$, and $t=+2$ reported in Table A.4 since there is overlap in the event windows for some shocks.

As a baseline for comparison, the first column in Table 2 reports the regression for asset growth. While not a test of funding contagion per se, asset reallocation is a necessary, though not a sufficient, condition for funding contagion (see our equation 1). The coefficient on *OtherShock 3p* shows a cumulative fall in asset growth of 6.6%, which suggests that shocks to partner firms have economically meaningful effects.¹³

In the rest of Table 2, we study funding-related variables. The null hypothesis is that there is no funding contagion, i.e., $E'_{12} = 0$, which cannot be tested with asset growth. Under the null we should not see reductions in debt growth or leverage, but we can still see reductions in asset growth due to pure reallocation.

External funds come from two main sources: debt and equity. In column 2 of Table 2-A, the coefficient on *OtherShock 3p* implies a reduction in debt growth of 8.6%, which is approximately 30% larger than the coefficient of *OtherShock 3p* on the asset growth regression (column 1). For column 3, we define debt growth with asset base as the change in debt over lagged assets, so the numerator is the same as for asset growth.¹⁴ In a firm with 50% leverage, and if all the effects on the right-hand side of the balance sheet are symmetric, the effect on debt growth with asset base should be exactly half of the effect on asset growth. Instead, in column 3 we find that the coefficient on *OtherShock 3p* is 3.5%, or more than half of the 6.6% effect on asset growth. The immediate consequence of a stronger fall in debt than in assets is that the leverage ratio goes down by 1.2% with shocks to the industry of the other firm (column 4). This reduction in leverage is comparable to the effects of traditional capital structure determinants (e.g., a 10% decrease in tangibility or a 1% increase in profitability; see Frank and Goyal, 2009).

We interpret these results as evidence in support of funding contagion. It is hard to explain the more than proportional fall in debt —and the associated fall in leverage — in a model without funding contagion (i.e., if $E'_{12} = 0$). We consider below three counterfactual

¹³ The coefficient on the three-year shock dummy in Table 2 is larger than the single-year shock dummy shown in Table A.4 of the appendix (column 4, Panel A) suggesting there is persistence instead of reversal after the first year of the shock. A quick reversal in asset growth would imply a smaller coefficient on the three-year dummy than on the single-year dummy.

¹⁴ From the accounting identity, asset growth is equal to debt growth plus equity growth when both are measured with asset base: $\frac{\Delta A}{A_{t-1}} = \frac{\Delta D}{A_{t-1}} + \frac{\Delta E}{A_{t-1}}$.

scenarios without funding contagion. Regardless of reallocation between firms, these alternative cases cannot account for our findings.

Consider first the case where firm 1 supports firm 2 simply by substituting assets. For instance, firm 1 can reduce current assets (cash holdings, inventories, accounts receivables) and issue an inter-company loan to firm 2 (Gopalan, Nanda, and Seru, 2007; Buchuk et al., 2014). In this case, the debt and leverage of firm 1 stay constant, and there is only a change in the composition of assets. Therefore, asset substitution cannot explain the fall in debt and leverage that we find.

Consider next a case where firm 1 supports firm 2 by rearranging its own capital structure. Support can be given through extraordinary dividends (Gopalan, Nanda, and Seru, 2014), which implies that equity in firm 1 goes down as retained earnings are distributed. Assets also go down, as cash holdings or other assets are liquidated to pay dividends. Debt stays constant, and therefore leverage increases in firm 1. Leverage also increases if, instead of extraordinary dividends, firm 1 raises more debt from creditors and issues an inter-company loan to firm 2.

A third and subtler alternative is that the reduction in the equity of firm 1 due to the distribution of retained earnings prompts a response from lenders who cut credit to that firm. This can happen, for example, if creditors lend in direct proportion to the firm's equity and they do not consider the position of the owner in other firms. However, the reduction in debt would leave leverage constant instead of falling.

Overall, in these counterfactual scenarios, debt either increases, remains constant, or falls proportionally with assets, and leverage never falls. Only funding contagion, which entails a contraction in credit supply as lenders factor in the cross-pledging between firms, can generate the patterns we observe in the data.

The difference between the coefficients for *Other Shock 3p* in the asset growth and debt growth (with asset base) regressions allows us to perform a back-of-the-envelope calculation of how much asset growth would fall under the null of no funding contagion. Given that the coefficient is -6.6% in the asset growth regression and -3.5% in the debt growth regression, then asset growth would fall by only 3.1% ($=6.6\%-3.5\%$) in a world without funding

contagion. This is not a formal test of funding contagion, but a way to gauge the relative importance of funding contagion for asset reallocation.

In columns 5 through 7, we explore large changes in funding by studying the effects of other shocks on the dummies for debt issuance and retirement. These large funding events are informative, as they represent more active financing decisions. We find that the frequency of large debt issuance falls by 7.3% with other shocks. In terms of economic magnitude, this effect is equivalent to a quarter of the sample frequency of large debt issues (see Table 1). Regarding the frequency of large debt retirement, we find that it increases by 4.9%. Overall, the effect of other shocks on *net* debt issuance (=issuance-retirement) is -12.1%.

Consistent with the inability of the firm to issue or roll over debt, we find that banking relationships are affected by negative shocks to the partner firm (column 1 Table 2-B). The effect on the destruction of banking relations over three years is 11%. Given the importance of banking relationships in the flow of soft information, particularly for small firms (Petersen and Rajan, 1994), these results can explain why it is harder to raise additional debt. They also speak against an alternative hypothesis where the owner voluntarily decides to cut back on debt to control the increasing risk of her portfolio of firms. Risk aversion could explain the debt reduction, but an owner is unlikely to voluntarily break banking relationships and waste the accumulated goodwill with creditors.

Finally, we study if external equity (the second source of external funds) reacts to shocks to the other firm (columns 2-4 Table 2-B). An increase in external equity would dilute the owner, and a decrease in external equity would increase owner concentration. We find a negative but economically small and statistically insignificant effect on the owner's stake. Over three years, owners reduce their stake by just 0.023% (column 2). We do not find evidence of a higher frequency of large changes in the owner's stake (columns 3 and 4). Hence, funding contagion works mainly through debt rather than external equity, at least in our sample.

b. Control rights

Control is a crucial element behind funding contagion. Without substantial control rights, i.e., without the ability to dispose of a firm's assets, the links between firms are severed. Ownership stakes of 50% or more are typically considered to grant full control over a firm's financial and investment policies. The literature has also considered lower thresholds. For

instance, La Porta, Lopez-de-Silanes, and Shleifer (1999) use cutoffs of 10% and 20%. These cutoffs typically ensure that the shareholder has the right to a seat on the board of directors. To explore the effects of control, we split the sample into three subsamples. In the first subsample, which represents about half of the observations, the shareholder has stakes of 50% or more in both firms. We call this the “full control” sample. In the second subsample, which represents approximately 25% of the observations, the shareholder has a stake of 20% or more in both firms (but never 50% or more in both). This allows us to see if the transmission mechanism is present without a dominating stake of 50%, while still retaining a significant stake of 20% or more. Stakes of 20% or more are often observed in firms owned by several business partners with explicit or implicit shareholder agreements. We call this the “partial control” sample. The third subsample includes the rest of the firm pairs where any of the two stakes falls below 20%. In this last sample, significant control is absent from at least one firm, so we call it the “no control” sample.

The effect of shocks to the other firm’s industry on asset growth is large and significant in the full and partial control subsamples but smaller and insignificant in the no-control sample (columns 1-3 Table 3-A). Comparing across columns with the full-control sample, the effect on debt growth (9.3% in column 4) is larger than the effect on asset growth (6.4% in column 1). Leverage consequently goes down by 1.2% (column 1 Table 3-B). Also, net debt issuance is 12.4% less likely, and breaking banking relationships is 14.3% more likely in the full control sample (columns 4 and 7 Table 3-B).

The focus of our hypothesis is on the funding variables. The coefficients for debt growth (with or without asset base), net debt issuance, and banking relationships are always smaller (in magnitude) in the partial-control and no-control samples than in the full-control sample. Furthermore, the effect of the other shock on leverage is statistically significant only in the full control sample. Hence, funding contagion is stronger in firms where the large shareholder has full control, which is precisely what one would expect if the owner were the culprit of contagion.

Although ownership stakes may still be large (say 5% or 10%), the effect of the other shock on asset growth, debt growth, and leverage weakens when significant control rights are absent, as in the no-control sample (columns 3, 6, and 9 Table 3-A, column 3 Table 3-B). This

is most likely the case because other shareholders have stronger control rights in the firm.¹⁵ This last result represents an important falsification test for the theory and for our identification strategy since the effects are weak precisely when control is weak. In this respect, our results are unique in comparison to the literature on shock transmissions within ownership structures. This literature is focused on conglomerates with fully owned subsidiaries, and hence 100% ownership is the sole case under study (Lamont, 1997, Giroud and Mueller, 2019, and others).

c. Financial Constraints

Financial constraints represent the second essential element behind funding contagion. While we expect small private firms to be constrained on average, there can also be variation in financial constraints within the sample. One obstacle to measuring financial constraints is that standard proxies, such as the indexes of Kaplan and Zingales (1997), or Whited and Wu (2006), require more data than what is available for private firms. It is also unclear whether these indices apply to private firms because they are calibrated to public firms. Moreover, the relevant constraint in our setup is a joint constraint so that any individual firm characteristic can be a poor proxy for the financial constraints faced by the pair of firms.

In this context, we proxy for financial constraints at the market level using the total availability of credit in the economy. There are two advantages of measuring financial constraints at the country level. First, unlike firm-level characteristics, they are exogenous to the firms under study. Second, they are less subject to Kaplan and Zingales (1997)'s critique. Kaplan and Zingales (1997) argue that derivatives of investment-cash-flow sensitivities (like our equation 1) with respect to measures of financial constraints (θ in the model) have ambiguous signs. The source of the ambiguity is the non-monotonicity produced by the curvature of the production function $F(I_i)$. By averaging across firms in an entire market, we can smooth out possible non-monotonicities.

In Table 4, we sort firms according to the degree of development of the credit market (measured as private credit to GDP). The effect of shocks to the other firm's industry on asset

¹⁵ In line with the relevance of other shareholders, in unreported tests we study the effects of the number of shareholders, and whether the firm is a family firm or not. We define family as two or more shareholders with the same last name. The effects of the other shock are slightly stronger when there are more shareholders involved in the firm, and when the firm is not a family firm. This suggests that the owner has more effective control when the rest of the ownership structure is more dispersed.

growth is larger in magnitude and only significant in countries with low credit market development (-6.6% vs. -2.7%). The difference is even stronger with debt growth, which falls by 9.4% in less developed credit markets compared to 2.5% in more developed markets. The effects on leverage, net debt issuance, and breaking banking relationships go in the same direction. Overall, differences in market development are consistent with financial constraints as an important element for funding contagion.

d. Firm size

Beyond market characteristics, firm-level variables are often used as proxies for financial constraints. Although size is a standard predictor of financial constraints, the evidence in the previous literature comes mostly from comparing *public* firms of different sizes (e.g., Hadlock and Pierce, 2010). It is less obvious that size can be a good proxy for financial constraints within our sample of private firms. Regardless of empirical applicability, firm size also has opposing theoretical effects. One interpretation is that, if individual size proxies for financial constraints, then we should find that small firms suffer the most when a shock hits their partner firm. However, our model suggests the opposite in equation (1): small firms adjust less to their partner's shock, because it is too expensive for them to cut investment given their high marginal returns. The key difference in predictions arises because we assume a joint financial constraint in our model, while the standard argument considers a firm-level financial constraint.

In order to study which effect of firm size prevails in our sample, we split firms into "large" and "small" with respect to median assets. Large firms are 20 times larger than small firms, as judged by the median assets in each sample. Admittedly, these represent small and very small firms in comparison to the general population of firms in the economy. The results for asset growth by sorts of firm size are reported in Panel A of Table 5. We find that small firms are not affected by shocks to the other firm's industry (column 1), and hence, the contagion that we see in previous tables is towards the relatively large firms in our sample (column 2). In columns 3-4, we consider pairs of small or large firms. When both firms are small (column 3), the effect of the other shock is small and insignificant (2.8%). When both firms are large (column 4), the effect is large and strongly significant (4.7%). The results for debt growth in Panel B basically mirror the results for asset growth in terms of significance, but the coefficients are larger in magnitude. The results for debt growth with asset base (Panel

C) and leverage (Panel D) confirm that funding contagion happens to the large firms in the sample. These results are consistent with the interpretation of our model, where large firms are more likely to accommodate shocks since the returns to investment are lower for them.

Table A.5 showcases the heterogeneity related to firm size in a more compact way. We define the dummy *Small Pair* as equal to 1 when the size of the combined firms is below the sample median. We then interact this dummy with *Other Shock 3p*. Columns 1 through 4 present a positive and significant coefficient on the interaction term. The magnitude of the interaction-term coefficient is similar to the main coefficient on *Other Shock 3p*, but with the opposite sign. This implies that total funding contagion (the sum of the interacted and un-interacted coefficients) is basically zero in small pairs. Overall, large firms, rather than small firms, are hit hardest since they behave as shock absorbers in the pair.

The analysis so far does not consider the possible impact of shocks on firm survival. In other words, we have focused, as in our model, on the impact of shocks on the intensive margin (growth rates), and not on the extensive margin (survival). The lack of response from small firms implies that there is little adjustment on the intensive margin, but not necessarily on the extensive margin.

To study the extensive margin, we define a dummy for the last year that a pair of firms appears in the sample (*Pair dissolved*), either because one or both firms later disappear or because of a change of owners.¹⁶ In Table A.5, columns 5 and 6, we present results from a linear probability model using the dummy pair dissolved as the dependent variable. We find that the *Other Shock 3p* increases the likelihood of disaffiliation by 3.2% (column 5). However, we also find that the coefficient on *Other Shock 3p* interacted with *Small Pair* is strongly positive (15.4% in column 6) and larger in magnitude than the coefficient on *Other Shock 3p* alone (-3.6% in column 6), which corresponds to the effect on larger firms. Hence, other shocks increase the probability of disaffiliation, but only for small pairs. Since small firms show less (or no) evidence of funding contagion, the increased probability of disaffiliation is unlikely to explain our main results on funding contagion.¹⁷

¹⁶ We do not include the last year of the sample in this calculation because these are censored observations.

¹⁷ The identification strategy of LSU is consistent with the exit response of small firms, combined with the absence of an effect on asset growth (i.e., columns 1 and 3 in Panel A of Table 5). Using a sample of small firms comparable to the sample of small firms in this paper, LSU argue that shocks increase the likelihood of breaking up firm pairs without a direct effect of those shocks on the investment of the other firm.

e. Cash-flow and collateral cross-pledging

Cash-flow cross-pledging is the baseline case of cross-pledging considered in the literature. In this case, the income of one firm is (implicitly or explicitly) pledged to repay the financial obligations of another firm. Collateral cross-pledging refers to the case where the hard assets -and not the cash flows- of the firm are cross-pledged. Lamont (1997) suggests that this can be an alternative explanation for the negative impact of oil shocks on non-oil segments. LSU (2019) provide evidence in favor of collateral cross-pledging since they find that the debt capacity of a firm that splits from a group is reduced more strongly when its former partner firm is a high-tangibility firm.

In principle, both types of cross-pledging can be captured by the cross-derivative E'_{12} in our model. This derivative captures changes in the funding capacity of firm 1 when firm 2 receives a shock, and this shock can be to cash flows or to collateral values. The nature of the shocks that we study empirically is key for the interpretation of our results. The industrial shocks are more likely to represent shocks to cash flows rather than to collateral values. Collateral values (e.g., real estate prices) are typically not dependent on the shocks to a particular industry because they are not specific to that industry. Hence, we believe that cash-flow cross-pledging is the more natural explanation. However, we study both types of cross-pledging now since collateral cross-pledging is also a possibility.

It is worth noting that the role of collateral is more complicated in our setup than in LSU (2019). In LSU (2019), the firm loses access to all the collateral provided by the former partner firm. In their case, the argument does not hinge on the marginal loss of collateral value. In our setup, where firms are still together, collateral is not lost. Therefore, there can be two effects. On the one hand, if collateral loses value with the shock, having a high-collateral partner leads to higher funding contagion. On the other hand, if the collateral value is not too sensitive to shocks, having a high-collateral partner can help dampen the shock and mitigate funding contagion. We note that, although there is ambiguity regarding the predictions of collateral cross-pledging, there is no ambiguity in the predictions of cash-flow cross-pledging.

The challenge is to devise a test that allows us to disentangle both types of cross-pledging empirically. With this in mind, in Table 6, we add to our baseline specification interactions of the dummy *Other Shock 3p* with indicator variables for cases where the other

firm has above-median profitability (OROA) and above-median tangibility. The idea is that, if cash-flow cross-pledging is important, then having a high-cash-flow partner affected by a shock should increase the severity of funding contagion since the cash flows of the partner firm are an integral part of the joint pledgeable income. Similarly, if collateral cross-pledging is important, then having a high-tangibility partner affected by a shock should increase the severity of funding contagion. We note that our tests control for the interactions between size and the shocks to mitigate the concern that firm size could also proxy for collateral.

As seen in Table 6, the interaction of the *Other Shock 3p* and *High Other OROA* is negative and significant for all funding variables (columns 2-4). The most telling result is for leverage (column 4) since it summarizes all changes in the capital structure. When a high-profitability partner is hit by a shock, contagion is stronger, and debt capacity goes down. The negative interaction with profitability is qualitatively similar when using other profitability measures such as the operating margin or the cash-flows-to-sales ratio. The interaction of *Other Shock 3p* and *High Other Tangibility* is, instead, positive and significant for debt growth and debt growth with asset base (columns 2 and 3). This suggests that collateral serves to dampen rather than to amplify shock transmission. The effect of the interaction with tangibility on leverage (column 4) is not statistically significant. Overall, our results side with cash-flow cross-pledging more than with collateral cross-pledging as the mechanism behind funding contagion.

f. Identification threats and alternative hypotheses

We first study threats to our identification strategy that come from subtler customer-supplier links. To this end, we keep pairs of firms with zero integration in an input-output sense (previously, as in the literature, we kept industries with integration below 1%). We can see in Table 7 that the effects of the other shock on asset and debt growth are even stronger in this subsample (compare the coefficients in columns 1 and 3 of Table 7 with the coefficients in columns 1 and 2 of Table 2). In the same spirit, we further drop firms if they are in the same two-digit SIC industry (previously, we dropped firms in the same three-digit SIC industry). As seen in the even-numbered columns in Table 7, the results remain very similar.

A second possibility is that correlation in the demand for goods and services explains the co-movement between firms. For example, industries may have high demand complementarity (e.g., landscaping services and fertilizers). Demand correlation can also

follow from aggregate wealth effects. A negative shock to a key industry (e.g., oil in Norway) can generate large wealth effects, causing a fall in demand in otherwise unrelated sectors of the economy. If demands are correlated, a firm can reduce asset and debt growth while the other firm's industry is experiencing a negative shock, even in the absence of funding contagion. In financial terms, we should worry that the demand, and not the supply of credit, is moving in tandem for the two firms.

To address the hypothesis of correlated demands, we propose a placebo test.¹⁸ Pairs in our original sample are formed by the "own firm" and the "other firm." The own firm is the treated firm in our experiment since it is potentially affected by the shock to the other firm. Not all pairs in our original sample receive shocks; hence, not all pairs are treated. According to Table 1, only 11% of firm-year observations are treated. Untreated pairs are not part of the analysis that follows. For each own firm, whose pair eventually receives a shock during the sample period, we find a suitable match for the first sample year and track it over time. We call the matched pairs placebo firms.

Placebo firms are selected in the following way. We start by assembling a sample of standalone firms, i.e., firms whose owners do not have any other firms. We then look at each country-industry-year cluster where treated firms are first observed in our sample. There are 2.6 million standalone firms in the same country-industry-year clusters as our treated firms. Approximately 75% of the country-industry-year clusters with a treated firm have more than 1,000 potential control (standalone) firms. Close to 25% of the country-industry-year clusters have more than 10,000 potential control (standalone) firms. We further refine the sample by searching, within each cluster, for the standalone firm with the most similar assets and ownership stake to the treated firm. Given that this search algorithm is computationally demanding (in particular because of the continuous variables size and stake), we perform the search in two steps. First, we split the standalone sample into 50 buckets along the distribution of assets and stake. For example, assume that we have a treated firm from SIC3 999, Germany, in 2010 with €2.5M in assets and an owner with a 75% stake. The potential placebo firms for the treated firm also belong to SIC3 999, Germany, and the year 2010, but they are all standalone firms. Furthermore, these potential placebo firms belong to a size bucket that contains €2.5M in assets and a stake bucket that contains the 75% stake. Second, within the

¹⁸ We thank the anonymous referee for suggesting this test.

refined sample of placebo firms that consider size and stake buckets, we identify the nearest neighbor match for each treated firm. Hence, for each treated firm, we pick one placebo standalone firm. We also consider a second, more stringent, matching criterion where we add age and leverage to the previous matching variables.¹⁹ By definition, the standalone firms cannot receive a shock from the “other firm” in the original pair since they are not really paired with any firm. However, they could, in theory, have correlated demand shocks given that they belong to the same industry and country, share similar firm characteristics, and operate during the same period.

The placebo test consists of running our main regression in the sample of placebo firms. With our first matching criterion, we have 17,257 firm-year observations, or 5,156 placebo firms times approximately 3 years each firm (there are 640 firm-year observations with the second criterion). The *Other Shock 3p* dummy corresponds to the years when the “other firm” in the original pair received shocks. In the regression with the placebo firms, the *Other Shock 3p* dummy captures the difference in the dependent variable when comparing placebo firms in the years when they supposedly receive shocks from their made-up partners with placebo firms in the years when their made-up partners do not receive shocks. The other shock should be completely irrelevant, given that placebo firms do not have a real partner firm.

As seen in Panel A of Table 8, other shocks have no effect on our main outcome variables of asset growth, debt growth, debt growth with asset base, and leverage. None of the coefficients for the *Other Shock 3p* is statistically significant. The coefficient sign often flips between positive and negative across the table. For example, leverage, which summarizes the effects on capital structure, shows a positive instead of a negative response to the placebo shocks.

In Panel B of Table 8, we show regressions using the subset of originally treated firms for which we find a placebo firm in Panel A. The goal is to show that the absence of significant

¹⁹ In Table A.6 in the appendix we show summary statistics for the eventually treated firms (own firms) and placebo firms according to two matching criteria. Panel A reports averages of firm characteristics when the matching is based on industry, country, year, size, and ownership stake. For Panel B we add leverage and firm age to the matching variables. There are 5,156 control (placebo) firms according to the first criterion in Panel A, and 296 control firms according to the second and more stringent criterion in Panel B. There are fewer firms in the second case because, as we add more variables to the matching criterion, many clusters of potential placebo firms end up being empty. As seen in both panels, there are no significant differences in characteristics between the treated and placebo firms. Economically speaking, the differences are also negligible.

coefficients in Panel A is not an artifact of the smaller sample. Despite the similar number of observations in Panels A and B, we find strongly negative and significant coefficients in Panel B for the originally treated firms. Overall, the placebo test speaks against correlated demands and in favor of our identification strategy.

g. Other outcomes

In Table 9, we study the components of asset and debt growth. We first decompose asset growth into current asset growth and fixed asset growth to understand the adjustment of different asset classes.²⁰ These variables are constructed using lagged total assets as the numerator to ease comparisons across them. In columns 1 and 2, we find that other shocks have a weaker effect on fixed asset growth (coeff. -0.015) than on current asset growth (coeff. -0.046). In columns 3 and 4, we further decompose current asset growth into cash growth and non-cash current asset growth (i.e., growth in inventories and accounts receivables). We find that the effect is stronger on non-cash current asset growth (coeff. -0.032) than on cash growth (coeff. -0.021), which implies that shocks to the other firm are not simply accommodated by drawing down on cash holdings. The fact that shocks have more pronounced effects on the non-cash component of current assets is consistent with the idea that financial constraints have a large effect on inventory investment (Dasgupta, Li, and Yan 2018; Kashyap, Lamont, and Stein 1994) and trade credit (Jacobson and von Schedvin, 2015). On the liability side, the effects also point to a stronger effect on current debt growth than on non-current debt growth (columns 5 and 6). Current debt growth is often related to working capital needs rather than long-run financing, which is more likely associated with hard assets. This result is also consistent with cash-flow cross-pledging rather than with collateral cross-pledging.

Employment hoarding and training is an important form of investment for small firms (Sharpe, 1994; Bernstein et al., 2018). Small firms also account for a disproportionate fraction of total employment in the economy, so understanding employment response to shocks is important for aggregate labor dynamics. In Table 9 (column 7), we see that $OtherShock_{it}$

²⁰ The balance sheet identity is $TotalAssets_{it} = CurrentAssets_{it} + FixedAssets_{it}$. We define current asset growth as $(CurrentAssets_{it} - CurrentAssets_{it-1})/TotalAssets_{it-1}$, and fixed asset growth as $(FixedAssets_{it} - FixedAssets_{it-1})/TotalAssets_{it-1}$.

implies a reduction in employment of 4.1%, significant at the 1% level. Given the decrease in asset growth, these results suggest complementarity between capital and labor.

We finally explore the impact on profitability. We find that shocks to the other firm's industry led to a cumulative drop in OROA of 1% over three years (column 8 Table 9). The effect is, however, statistically insignificant. In column 9 of Table 9, we use the level of EBIT to focus on the numerator of OROA without the confounding effect of assets (denominator), which are also affected by the other shocks (i.e., through a reduction in asset growth). We find that other shocks reduce EBIT by close to €17,000 over three years, or about 5% of the EBIT of the average firm in the sample ($€352,000 = 4\% \text{ OROA} \times €8.82 \text{ MM assets}$). This suggests that firms reduce their profits on average, albeit not in a statistically significant way. The lack of significance can be rationalized as the result of two opposing forces. On the one hand, the average profitability of the firm goes up when the firm cuts relatively poor marginal projects (e.g., stopping service to marginal clients). On the other hand, the profitability of assets goes down when sales go down. The lack of statistical significance can also be related to the smaller sample size, given that disclosing income statements below a certain firm size threshold is not required in many countries.

4. Conclusions

In this paper, we study funding contagion, which is the impaired ability of a firm to raise external funds when a negative shock hits another firm under the same owner. The effects of adverse shocks on investment across divisions in conglomerates or across establishments in multi-establishment firms are well known in the literature (e.g., Lamont, 1997, Giroud and Mueller, 2019). However, funding contagion across those units has not been documented before. One of the reasons is that the individual capital raising of divisions or establishments operating under a common balance sheet is not observable. The ability to estimate effects across different balance sheets, i.e., across different firms, is an advantage of our approach.

Our empirical setup is designed to identify the presence of contagion and the underlying mechanisms as cleanly as possible. To this end, we focus on pairs of firms that share a large common shareholder and operate in unrelated industries. Firms in unrelated industries help us to rule out input-output linkages by design. Owners with stakes in just two

firms help to circumvent selection effects that would be present in more complex ownership structures. Our sample consists of private firms, which tend to be small and financially constrained, and where owners have substantial control rights. All these ingredients make private firms more likely to experience contagion through common owners.

Our evidence is consistent with firms having a harder time raising external funds when negative shocks hit other firms under a common owner. We find that debt growth falls by more than asset growth, and, consequently, that leverage goes down in a firm as shocks hit the partner firm. These results suggest that creditors factor in the cross-pledging of cash flows between firms when supplying credit to firms under a common owner.

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Figure 1: Pairwise distribution of firms' industries

This figure shows the distribution of firms according to their three-digit sic code and the three-digit sic code of their partner firm. Pairs in related industries (including the same industry) have been removed. The size of the crosses indicates the number of firms in any given SIC/other-SIC coordinate. Smaller crosses represent a single pair of firms; mid-sized crosses represent between two and five pairs of firms; larger crosses represent more than 5 pairs of firms in a coordinate.

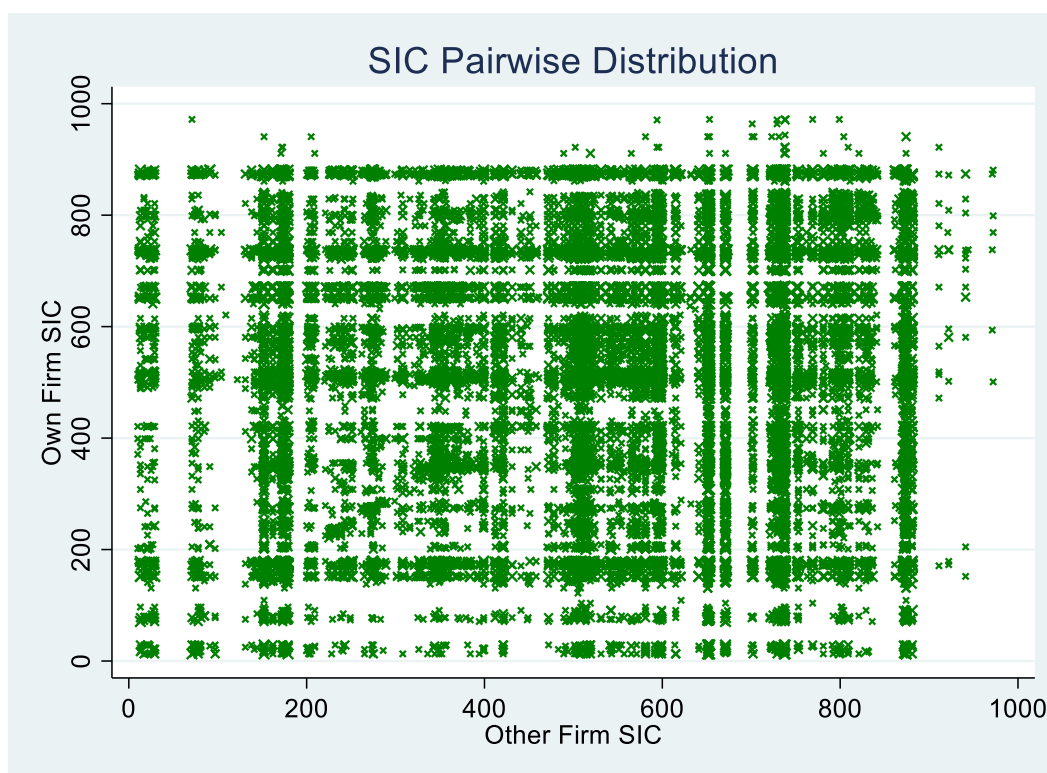


Figure 2: Distribution of ownership stakes

This figure displays the frequency of stakes of large shareholders in our sample.

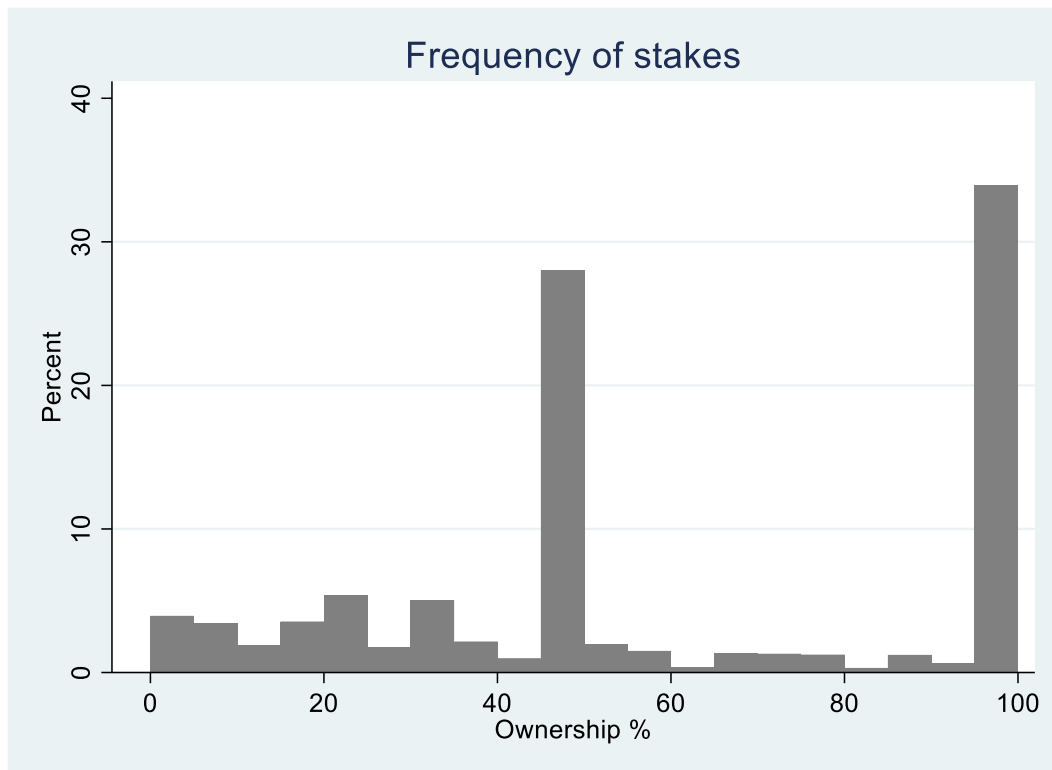


Figure 3: Distribution of shocks by industry

This figure displays the frequency of shocks by the one-digit SIC code. *Own Shock* represents shocks to the industry of the *own* firm and *Other Shock* represents shocks to the industry of the *other* firm with a common owner. Both shocks are defined at the three-digit SIC code level and later aggregated to the one-digit SIC level solely for the purpose of this figure.

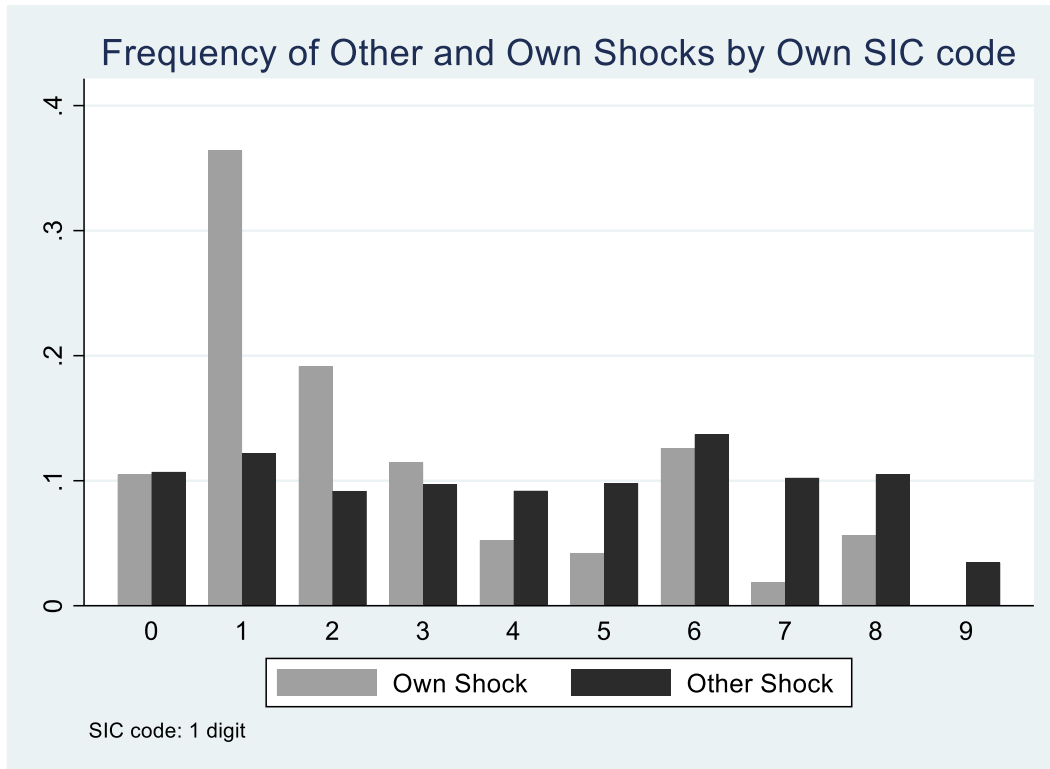


Table 1. Summary statistics

This table presents summary statistics for our sample. Firm-level characteristics include firms' financials, ownership, and banking relationship variables. *Assets* are measured in millions of Euros. *Assets (Debt) growth* is the difference between a firm's logarithm of book assets (debt) and its lag. Debt issue (retirement) is a dummy variable that takes a value of 1 if a firm changes its debt in a year by more than 5% (less than -5%) of its lagged assets, and 0 otherwise. Net debt issuance is debt issue minus debt retirement. *Leverage* is the book value of debt over assets. *Banking relations* is the number of credit relationships a firm has in a year. *Breaking bank relations* takes a value of 1 if a firm in a year discontinues a relation with a bank, and 0 otherwise. *Employment* is the number of firm employees. *OROA* (operational return on assets) is EBIT (earnings before interest and taxes) divided by assets. *Sales/Assets* is the ratio of sales over assets. *Tobin's q (industry)* is the median *q* of a firm's industry in their country, where the industry is measured at the two-digit SIC code level. *Tangibility* is defined as fixed assets over total assets. *Stake (%)* is the shareholder's ownership stake in the firm. Stake increase (decrease) is a dummy variable that takes a value of 1 if the shareholder increased (decreased) its stake in the firm's equity by more (less) than 5% (-5%), and 0 otherwise. *Own Shock* takes a value of 1 if a firm operates in a year and industry where we identify an industrial shock, and 0 otherwise. *Other Shock* takes a value of 1 if the *other* firm with the same owner operates in a year and industry where we identify an industrial shock, and 0 otherwise. Financial variables are winsorized at the 1% level and growth rates at the 2.5% level.

	Mean	P10	P25	P50	P75	P90	Total
Assets (Mill Eur)	8.82	0.01	0.04	0.18	0.73	2.32	248,940
Asset growth	0.05	-0.33	-0.12	0.00	0.12	0.41	248,940
Debt growth	0.08	-0.44	-0.18	-0.00	0.19	0.63	197,333
Debt issue	0.27	0.00	0.00	0.00	1.00	1.00	211,278
Debt retirement	0.29	0.00	0.00	0.00	1.00	1.00	211,278
Net debt issuance	-0.02	-1.00	-1.00	0.00	1.00	1.00	211,278
Leverage	0.52	0.02	0.16	0.55	0.85	0.97	216,980
Banking relations	0.67	0.00	0.00	0.00	1.00	2.00	241,396
Breaking bank relations	0.13	0.00	0.00	0.00	0.00	1.00	241,396
Employment	13.34	1.00	2.00	4.00	12.00	30.00	64,266
OROA	0.04	-0.10	-0.00	0.02	0.09	0.22	50,793
Sales/Assets	1.91	0.11	0.61	1.42	2.55	4.17	56,794
Tobin's q (industry)	1.25	0.89	0.99	1.15	1.44	1.75	244,244
Tangibility	0.25	0.00	0.02	0.13	0.40	0.79	198,195
Stake %	60.98	16.66	35.00	50.00	100.00	100.00	248,940
Stake increase (>5%)	0.01	0.00	0.00	0.00	0.00	0.00	248,940
Stake decrease (>5%)	0.01	0.00	0.00	0.00	0.00	0.00	248,940
Own Shock	0.11	0.00	0.00	0.00	0.00	1.00	248,940
Other Shock	0.11	0.00	0.00	0.00	0.00	1.00	248,940

Table 2. Funding contagion through common owners

Panels A and B present estimation results that examine the relation between shocks to the *other* firm and outcomes of the *own* firm in the year of the shock and the next two years. In all columns, the main explanatory variable is *Other Shock 3p*, which takes a value of 1/3 during the year of the shock in the industry of the other firm and the two years after that. Panel A presents results for asset growth, debt growth (with and without asset base), book leverage, and the dummies for debt issuance, retirement, and net issuance. Panel B presents results for banking relationships and ownership stakes. Standard errors are adjusted by heteroscedasticity and two-way clusters at the owner and own industry levels. Significant at the *10%, **5%, ***1%.

Panel A							
VARIABLES	(1) Asset growth	(2) Debt growth	(3) Debt growth (asset base)	(4) Leverage	(5) Debt issue	(6) Debt ret	(7) Net debt iss.
Other Shock 3p	-0.066*** (0.017)	-0.086*** (0.032)	-0.035*** (0.011)	-0.012*** (0.004)	-0.073*** (0.019)	0.049*** (0.018)	-0.121*** (0.034)
Log(assets) (t-1)	-0.125*** (0.006)	-0.137*** (0.007)	-0.060*** (0.003)	0.001* (0.001)	-0.054*** (0.003)	0.068*** (0.003)	-0.122*** (0.006)
Observations	248,666	194,071	208,401	213,920	208,401	208,401	208,401
R-squared	0.428	0.334	0.346	0.925	0.384	0.369	0.302
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year FE	3-dig	3-dig	3-dig	3-dig	3-dig	3-dig	3-dig

Panel B				
VARIABLES	(1) Breaking bank rel.	(2) Ownership %	(3) Stake incr. (>5%)	(4) Stake decr. (>5%)
Other Shock 3p	0.110*** (0.011)	-0.023 (0.234)	-0.003 (0.003)	-0.001 (0.003)
Log(assets) (t-1)	0.021*** (0.001)	-0.093*** (0.012)	-0.001*** (0.000)	0.000 (0.000)
Observations	241,119	248,666	248,666	248,666
R-squared	0.583	0.980	0.293	0.312
Firm FE	Yes	Yes	Yes	Yes
Industry-year FE	3-dig	3-dig	3-dig	3-dig

Table 3. Funding contagion through common owners: Sub-samples according to control rights

Panels A and B present results that examine the relation between shocks to the *other* firm and outcomes of the *own* firm in the year of the shock and the next two years, according to the owner's control rights. The full control sample considers pairs of firms where the owner holds an equity stake larger or equal than 50% in each firm. The partial control sample considers pairs of firms where the owner holds an equity stake larger or equal to 20% in each firm but does not have stakes larger or equal to 50% in both firms. The no control sample considers pairs of firms where the owner holds an equity stake smaller than 20% in one or both firms. Standard errors are adjusted by heteroscedasticity and two-way clusters at the owner and own industry levels. Significant at the *10%, **5%, ***1%.

Panel A									
VARIABLES	(1) Asset growth	(2) Asset growth	(3) Asset growth	(4) Debt growth	(5) Debt growth	(6) Debt growth	(7) Debt growth (asset base)	(8) Debt growth (asset base)	(9) Debt growth (asset base)
Other Shock 3p	-0.064*** (0.023)	-0.081*** (0.028)	-0.034 (0.027)	-0.093** (0.046)	-0.066* (0.039)	-0.075 (0.047)	-0.039*** (0.014)	-0.038** (0.017)	-0.005 (0.016)
Log(assets) (t-1)	-0.136*** (0.007)	-0.116*** (0.006)	-0.125*** (0.008)	-0.154*** (0.009)	-0.125*** (0.006)	-0.128*** (0.008)	-0.067*** (0.003)	-0.055*** (0.003)	-0.057*** (0.004)
Observations	136,814	66,875	44,076	109,260	50,791	32,925	116,478	54,717	36,123
R-squared	0.429	0.441	0.468	0.326	0.364	0.387	0.342	0.368	0.408
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year FE	3-dig	3-dig	3-dig	3-dig	3-dig	3-dig	3-dig	3-dig	3-dig
Sample	Full control both	Partial control in one or both	No control in one or both	Full control both	Partial control in one or both	No control in one or both	Full control both	Partial control in one or both	No control in one or both
Panel B									
VARIABLES	(1) Leverage	(2) Leverage	(3) Leverage	(4) Net debt iss.	(5) Net debt iss.	(6) Net debt iss.	(7) Breaking bank rel.	(8) Breaking bank rel.	(9) Breaking bank rel.
Other Shock 3p	-0.012** (0.005)	-0.015 (0.009)	0.000 (0.011)	-0.124*** (0.045)	-0.115** (0.052)	-0.094* (0.055)	0.143*** (0.017)	0.076*** (0.016)	0.041** (0.020)
Log(assets) (t-1)	0.003*** (0.001)	-0.000 (0.001)	-0.001 (0.001)	-0.137*** (0.008)	-0.109*** (0.006)	-0.123*** (0.008)	0.021*** (0.001)	0.021*** (0.001)	0.018*** (0.001)
Observations	119,310	56,350	37,204	116,478	54,717	36,123	132,866	64,317	43,012
R-squared	0.931	0.917	0.925	0.297	0.328	0.371	0.605	0.577	0.568
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year FE	3-dig	3-dig	3-dig	3-dig	3-dig	3-dig	3-dig	3-dig	3-dig
Sample	Full control both	Partial control in one or both	No control in one or both	No control in one or both	Full control both	No control in one or both	No control in one or both	Full control both	No control in one or both

Table 4. Funding contagion through common owners: Sub-samples according to credit market development

Panels A and B present estimation results that examine the relation between shocks to the *other* firm and outcomes of the *own* firm in the year of the shock and the next two years, according to the credit development of the country where the pair-of-firms operate. Credit development is measured by the ratio of private credit to GDP (source: *World Bank*). Columns (1), (3) and (5) present results from the subsample of countries with above-the-median credit development and Columns (2), (4) and (6) present results from the subsample of countries with below-the-median credit development. Standard errors are adjusted by heteroscedasticity and two-way clusters at the owner and own industry levels. Significant at the *10%, **5%, ***1%.

Panel A						
VARIABLES	(1) Asset growth	(2) Asset growth	(3) Debt growth	(4) Debt growth	(5) Debt growth (asset base)	(6) Debt growth (asset base)
Other Shock 3p	-0.027 (0.023)	-0.066*** (0.024)	-0.025 (0.045)	-0.094** (0.043)	-0.001 (0.014)	-0.041** (0.016)
Log(assets) (t-1)	-0.312*** (0.011)	-0.084*** (0.004)	-0.720*** (0.023)	-0.109*** (0.006)	-0.150*** (0.007)	-0.048*** (0.002)
Observations	119,569	121,199	76,935	111,351	89,875	112,588
R-squared	0.528	0.357	0.456	0.271	0.444	0.277
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year FE	3-dig	3-dig	3-dig	3-dig	3-dig	3-dig
Sample	High credit develop	Low credit develop	High credit develop	Low credit develop	High credit develop	Low credit develop

Panel B						
VARIABLES	(1) Leverage	(2) Leverage	(3) Net debt iss.	(4) Net debt iss.	(5) Breaking bank rel.	(6) Breaking bank rel.
Other Shock 3p	-0.004 (0.008)	-0.016*** (0.005)	-0.052 (0.050)	-0.151*** (0.052)	0.016*** (0.006)	0.062** (0.028)
Log(assets) (t-1)	0.004* (0.003)	0.000 (0.001)	-0.319*** (0.017)	-0.101*** (0.005)	0.000 (0.000)	0.035*** (0.001)
Observations	93,255	114,323	89,875	112,588	119,567	121,192
R-squared	0.921	0.933	0.417	0.243	0.526	0.613
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year FE	3-dig	3-dig	3-dig	3-dig	3-dig	3-dig
Sample	High credit develop	Low credit develop	High credit develop	Low credit develop	High credit develop	Low credit develop

Table 5. Funding contagion through common owners: Sub-samples according to firm size

Columns (1)-(4) present estimation results for Equation (3) for different sample splits. Columns (1)-(2) present results for sample splits based on the *own* firms' size (above and below the sample median). Columns (3)-(4) present results based on firm pairs: the *own* and the *other* firm are above (large both) or below (small both) the sample median of size. The dependent variable is *Asset growth* in Panel A, *Debt growth* in Panel B, *Debt growth (asset base)* in Panel C, and *Leverage* in Panel D. Standard errors are adjusted by heteroscedasticity and two-way clusters at the owner and own industry levels. Significant at the *10%, **5%, ***1%.

Panel A	(1)	(2)	(3)	(4)
VARIABLES	Asset growth	Asset growth	Asset growth	Asset growth
Other Shock 3p	-0.019 (0.019)	-0.094*** (0.017)	-0.028 (0.027)	-0.047*** (0.018)
Log(assets) (t-1)	-0.321*** (0.010)	-0.091*** (0.004)	-0.331*** (0.012)	-0.086*** (0.004)
Observations	124,011	124,175	67,960	60,210
R-squared	0.509	0.439	0.527	0.521
Panel B	Debt growth	Debt growth	Debt growth	Debt growth
Other Shock 3p	-0.014 (0.029)	-0.126*** (0.029)	-0.061 (0.064)	-0.094*** (0.033)
Log(assets) (t-1)	-0.588*** (0.047)	-0.110*** (0.004)	-0.619*** (0.053)	-0.096*** (0.003)
Observations	84,745	108,807	42,379	49,526
R-squared	0.373	0.371	0.433	0.456
Panel C	Debt growth (asset base)	Debt growth (asset base)	Debt growth (asset base)	Debt growth (asset base)
Other Shock 3p	0.001 (0.009)	-0.059*** (0.012)	-0.004 (0.019)	-0.040*** (0.013)
Log(assets) (t-1)	-0.149*** (0.007)	-0.048*** (0.002)	-0.148*** (0.008)	-0.040*** (0.001)
Observations	98,545	109,333	51,045	49,965
R-squared	0.411	0.344	0.437	0.434
Panel D	Leverage	Leverage	Leverage	Leverage
Other Shock 3p	-0.006 (0.007)	-0.017*** (0.005)	-0.009 (0.013)	-0.013** (0.006)
Log(assets) (t-1)	0.003* (0.002)	0.001 (0.001)	0.002 (0.002)	-0.000 (0.001)
Observations	101,433	111,969	53,013	51,828
R-squared	0.924	0.911	0.919	0.896
Firm FE	Yes	Yes	Yes	Yes
Industry-year FE	3-dig	3-dig	3-dig	3-dig
Sample	Small Own	Large Own	Small Both	Large Both

Table 6. Funding contagion through common owners: Cash-flow vs. collateral cross-pledging

The present estimation results that examine the relation between shocks to the *other* firm and outcomes of the *own* firm in the year of the shock and the next two years, according to the tangibility and profitability of the other firm. High Other Tangibility are firms with above-the-median own tangibility, defined as the ratio of fixed over total assets. High Other OROA are firms with above-the-median own profitability, or cash-flow generating ability, defined as OROA. We use as additional controls the interaction of Other Shock 3p with dummies for small own and small other (below the median in terms of assets). Standard errors are adjusted by heteroscedasticity and two-way clusters at the owner and own industry level. Significant at the *10%, **5%, ***1%.

VARIABLES	(1) Asset growth	(2) Debt growth	(3) Debt growth (asset base)	(4) Leverage
Other Shock 3p	-0.108** (0.045)	-0.156** (0.072)	-0.059** (0.028)	-0.006 (0.012)
Other Shock 3p x High Other Tang	0.026 (0.040)	0.163*** (0.060)	0.055** (0.024)	0.014 (0.020)
Other Shock 3p x High Other OROA	0.020 (0.040)	-0.140* (0.071)	-0.079** (0.032)	-0.047** (0.022)
Observations	47,449	36,149	37,483	39,175
R-squared	0.524	0.464	0.461	0.901
Firm FE	Yes	Yes	Yes	Yes
Industry-year FE	3-dig	3-dig	3-dig	3-dig
Size x shock controls	Yes	Yes	Yes	Yes

Table 7. Funding contagion through common owners: Dropping finer margins of integration

This table presents additional results using the specification presented in Table 2. In odd-numbered columns, we replicate the main specification, but exclude pairs of firms in industries with a measure of vertical integration between 0% and 1%. We had previously excluded pairs of firms in industries where the measure of vertical integration was above 1%. In even-numbered columns, we additionally exclude pairs of firms in the same two-digit SIC code. We had previously excluded pairs of firms in the same three-digit SIC code. Standard errors are adjusted by heteroscedasticity and two-way clusters at the owner and own industry levels. Significant at the *10%, **5%, ***1%.

VARIABLES	(1) Asset growth	(2) Asset growth	(3) Debt growth	(4) Debt growth	(5) Debt growth (asset base)	(6) Debt growth (asset base)	(7) Leverage	(8) Leverage
Other Shock 3p	-0.072*** (0.019)	-0.066*** (0.019)	-0.093*** (0.034)	-0.086*** (0.033)	-0.035*** (0.012)	-0.034*** (0.012)	-0.015*** (0.004)	-0.016*** (0.005)
Log(assets) (t-1)	-0.123*** (0.006)	-0.123*** (0.006)	-0.135*** (0.007)	-0.135*** (0.007)	-0.059*** (0.003)	-0.059*** (0.003)	0.001 (0.001)	0.002* (0.001)
Observations	218,264	209,361	172,180	165,379	184,452	176,985	189,152	181,477
R-squared	0.435	0.434	0.337	0.336	0.350	0.350	0.929	0.929
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year FE	3-dig	3-dig	3-dig	3-dig	3-dig	3-dig	3-dig	3-dig
Excl. 0%<Int. <1%	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Excl. Same SIC2	No	Yes	No	Yes	No	Yes	No	Yes

Table 8. Placebo regressions using matched standalone firms

Panel A presents regression results on a placebo sample of standalone firms. We match firms from our main sample whose partner eventually receives a negative shock to standalone firms. We perform two matching procedures using the nearest-neighbor approach within industry, country, and year. One matches on the owner's stake and firm size (Panel A, Table A.6) and the other matching additionally uses firm age and leverage (Panel B, Table A.6). After firms are matched on their initial year, we follow the pair over time, allowing us to assign the shocks of the partner firm from our main sample to the matched standalone firm. Columns 1-4 (5-8) presents the results for the first (second) matching procedure. Panel B uses the subset of treated firms that have a matched standalone firm in Panel A. The goal is to show that even in this smaller sample, treated firms behave similarly as in the main sample. Standard errors are adjusted by heteroscedasticity and two-way clusters at the owner and own industry level. Significant at the *10%, **5%, ***1%.

Panel A: Matched Placebo (Standalone) Firms								
VARIABLES	(1) Asset growth	(2) Debt growth	(3) Debt growth (asset base)	(4) Leverage	(7) Asset growth	(8) Debt growth	(9) Debt growth (asset base)	(8) Leverage
Other Shock 3p	-0.022 (0.033)	-0.009 (0.027)	-0.010 (0.022)	0.004 (0.013)	-0.014 (0.165)	0.056 (0.119)	0.033 (0.092)	0.044 (0.051)
Log(assets) (t-1)	-0.118*** (0.004)	-0.073*** (0.003)	-0.058*** (0.003)	0.003** (0.001)	-0.170*** (0.019)	-0.089*** (0.024)	-0.063*** (0.016)	0.013 (0.012)
Observations	17,257	17,255	17,255	16,116	640	640	640	633
R-squared	0.438	0.365	0.362	0.896	0.657	0.594	0.589	0.935
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year FE	3-dig	3-dig	3-dig	3-dig	3-dig	3-dig	3-dig	3-dig
Sample	Placebo firms	Placebo firms	Placebo firms	Placebo firms	Placebo firms	Placebo firms	Placebo firms	Placebo firms
Matching	Nearest neighbor	Nearest neighbor	Nearest neighbor	Nearest neighbor	Nearest neighbor	Nearest neighbor	Nearest neighbor	Nearest neighbor
Matching by	Stake, size, year, industry, and country	Stake, size, year, industry, and country	Stake, size, year, industry, and country	Stake, size, year, industry, and country	Stake, size, leverage, age, year, industry, and country	Stake, size, leverage, age, year, industry, and country	Stake, size, leverage, age, year, industry, and country	Stake, size, leverage, age, year, industry, and country

Panel B: Treated Firms with Matches in Panel A								
VARIABLES	(1) Asset growth	(2) Debt growth	(3) Debt growth (asset base)	(4) Leverage	(7) Asset growth	(8) Debt growth	(9) Debt growth (asset base)	(8) Leverage
Other Shock 3p	-0.058** (0.029)	-0.054** (0.025)	-0.048** (0.023)	-0.015 (0.011)	-0.399** (0.183)	-0.370** (0.176)	-0.336** (0.169)	-0.111** (0.049)
Log(assets) (t-1)	-0.116*** (0.004)	-0.080*** (0.004)	-0.072*** (0.003)	0.002 (0.001)	-0.168*** (0.025)	-0.123*** (0.027)	-0.118*** (0.027)	0.017* (0.009)
Observations	20,246	19,782	19,782	19,183	719	706	706	698
R-squared	0.511	0.516	0.551	0.901	0.652	0.662	0.667	0.930
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year FE	3-dig	3-dig	3-dig	3-dig	3-dig	3-dig	3-dig	3-dig
Sample	Treated firms	Treated firms	Treated firms	Treated firms	Treated firms	Treated firms	Treated firms	Treated firms
Matching	Nearest neighbor	Nearest neighbor	Nearest neighbor	Nearest neighbor	Nearest neighbor	Nearest neighbor	Nearest neighbor	Nearest neighbor
Matching by	Stake, size, year, industry, and country	Stake, size, year, industry, and country	Stake, size, year, industry, and country	Stake, size, year, industry, and country	Stake, size, leverage, age, year, industry, and country	Stake, size, leverage, age, year, industry, and country	Stake, size, leverage, age, year, industry, and country	Stake, size, leverage, age, year, industry, and country

Table 9. Funding contagion through common owners: Other outcomes

This table presents estimation results for different firm-level variables using the specification presented in Table 2. First, we decompose asset and debt growth using lagged assets as a common base to ease the comparison of coefficients in terms of the magnitudes. Column (1) presents results using *Fixed Asset Growth*, and Column (2) presents results using *Current Asset Growth*. Columns (3) and (4) present results for the two main components of *Current Asset Growth* as dependent variables: Column (3) presents results using *Cash Asset Growth*, and Column (4) presents results using *Non-cash Current Asset Growth*. Columns (5) and (6) present results decomposing *Debt growth (asset base)*: Column (5) presents results for *Non-current debt growth* and Column (6) for *Current debt growth*. Column (7) presents results using the logarithm of employment as the dependent variable. Column (8) presents the results for *OROA*. Column (9) presents results for *EBIT* in Euros. Standard errors are adjusted by heteroscedasticity and two-way clusters at the owner and own industry levels. Significant at the *10%, **5%, ***1%.

VARIABLES	(1) Fixed asset growth	(2) Current asset growth	(3) Cash growth	(4) Non-cash current asset growth	(5) Non-current debt growth	(6) Current debt growth	(7) Log(Empl)	(8) OROA	(9) EBIT
Other Shock 3p	-0.015*** (0.004)	-0.046*** (0.015)	-0.021*** (0.007)	-0.032*** (0.009)	-0.009* (0.005)	-0.019* (0.010)	-0.041*** (0.014)	-0.010 (0.009)	-16,828 (13,314)
Log(assets) (t-1)	-0.015*** (0.001)	-0.113*** (0.006)	-0.028*** (0.003)	-0.043*** (0.002)	-0.012*** (0.001)	-0.052*** (0.003)	0.009*** (0.001)	-0.001 (0.001)	284 (696)
Observations	248,662	248,662	206,227	199,738	208,401	208,401	59,551	48,375	48,375
R-squared	0.373	0.398	0.284	0.303	0.187	0.306	0.977	0.597	0.818
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year FE	3-dig	3-dig	3-dig	3-dig	3-dig	3-dig	3-dig	3-dig	3-dig
Sample	All	All	All	All	All	All	All	All	All