The effects of business group affiliation: Evidence from firms being "left alone"

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Abstract

We study a sample of business groups composed of two firms in unrelated industries in the period 2009-2013. Some of these groups split up during this time, leaving firms as standalone. We instrument for the stand-alone status using shocks to the industry of the *other* firm in the group. We find that firms becoming standalone reduce their leverage and investment. This evidence is consistent with the idea that affiliation to a group eases credit constraints. The effects are more pronounced when the other firm has high tangibility, which is consistent with cross-pledging, and when the firm operates in a debt-dependent industry or in a country with low domestic credit. In line with "socialism" in internal capital markets, firms more affected by becoming standalone are firms with previous poor performance and high leverage relative to their industry peers, and firms with low Tobin's Q relative to the other firm in the group. We do not find a significant effect of becoming standalone on performance.

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Business groups –sets of firms with the same controlling shareholder—are common around the world, from Asia to Europe and Latin America. The literature has identified bright and dark sides to business groups (Khanna and Yafeh, 2007). The bright side lies in the potential ability of business groups to overcome market frictions. For instance, firms with limited access to external financing can benefit from the support of the rest of the group when they suffer negative cash-flow shocks (Gopalan, Nanda, and Seru, 2007; Khanna and Yafeh, 2005). The dark side refers to the potential misallocation of capital across firms within the group. Business group can engage in internal cross-subsidies by which some firms receive more funding than what their investment opportunities deserve, while other firms receive less. Stein (2003) describes these situations as corporate "socialism". The misallocation can be severe and take the form of negative NPV investments, such as pet projects of the controlling shareholder or the outright expropriation of minority shareholders (called "tunneling" in Johnson, La Porta, López-de-Silanes, and Shleifer, 2000, or Bertrand, Mehta, and Mullainathan, 2002). Recent research suggests that the advantages of business group affiliation could outweigh the disadvantages (Masulis, Pham, and Zein, 2011), but the issue is far from settled.

The debate remains open partly because the identification of a causal effect of group affiliation has proven to be elusive. The affiliation to a business group is clearly an endogenous decision. For example, one could argue that weak firms need to be in business groups to survive, or instead, that good firms end up forming business groups around them. Even the position of a firm within groups is not random according to Almeida and Wolfenzon (2006a), and Almeida, Park, Subrahmanyam, and Wolfenzon (2011). The previous literature has dealt with the selection of firms into business groups, but it has not estimated the effect of being "treated" with a business group affiliation. Identifying exogenous variation in business group affiliation is, first and foremost, hard. For example, the formation and split of groups is often mingled with political events, regulation, and financial crisis (see Kandel, Kosenko, Morck, and Yafeh, 2015; Khanna and Yafeh, 2007; Perez-Gonzalez, 2015). Ultimately, exogenous variation is crucial to get good estimates of the effects of business group affiliation.

We propose a novel identification strategy that allows us to estimate the effect of business group affiliation. Our identification strategy is based on two building blocks. First, we study the effects of leaving a group or becoming a stand-alone firm. Arguably, a firm in a business group is inherently different from a stand-alone firm so one cannot use the latter as a control for the former in the cross section. By focusing on a particular firm that changes its status, and hence delving into within-firm variation, one can partially attack the endogeneity problem and omitted variable bias that affect cross-sectional comparisons.

Unfortunately, studying firms that change status is only a partial solution because firms that become standalone are far from a random sample. One could argue that firms leave business groups precisely when group affiliation destroys value. A similar problem occurs when studying conglomerate spinoffs (see Gertner, Powers, and Scharfstein, 2002). For this reason our identification strategy needs s second crucial ingredient. In a nutshell, we focus on firms that are "left alone" rather than in firms that "become standalone". More precisely, we instrument for the stand-alone status using negative shocks to the industry of the *other* firm in the group. Shocks to the other firm can make the other firm either disappear or force a sale to different owners, leaving the firm under study as a standalone not by choice but arguably by chance. In order to make this mechanism believable we focus on groups with only two firms and firms in unrelated industries. In groups with more firms, the link between shocks to other firms and becoming standalone probably involves selection, i.e., choosing what firm to let go. Also, if the industries of both firms are closely integrated, or worse if both firms are in the same industry, one can expect contagion to the firm under study and hence the exclusion restriction in an instrumental variables setting would likely be violated. The second part of our identification strategy is reminiscent of Lamont (1997) who studies the effects of oil price shocks on non-oil segments of large conglomerates. Similar to Lamont (1997), we focus on industrial shocks that can be cleanly identified as commodity shocks (e.g., milk, aluminum, pulp, etc.) or regulatory shocks (e.g., tobacco industry).

One could argue that forced splits are traumatic events that naturally leave a mark on firms. Hence it is perhaps not surprising that the effects of "being left alone" differ from other instances where firms become standalone, potentially by choice. The marriage analogy would be that we compare becoming a widow by accident with all divorces. This is true, but at the same time it is not a criticism of the approach, but the heart of the identification strategy we are trying to pin down. The fact that being in a relationship with another firm through a common controlling shareholder matters for firm outcomes is precisely evidence that group affiliation is relevant.

Our data come from the universe of European private firms in *Amadeus* for the years 2009-2013. A particular advantage of *Amadeus* is that we have information on ownership structures, and hence within this universe we can identify pairs of firms with a common controlling shareholder. In IV regressions that use shocks to the other firm as instrument for stand-alone status, we find a strong negative effect of becoming standalone on leverage and asset growth. Our results suggest that credit constraints, and hence investment, are indeed different in groups than in stand-alone firms. In other words, ownership structure has real effects unlike a Modigliani-Miller type of setup where ownership structure should not matter at all: the identity of a firm's controlling shareholder matters, and the identity of the other firms with the same controlling shareholder also matters.

Tirole (2006) argues that putting both firms under a common roof can increase financing with respect to the case where both firms remain standalone because there is cross-pledging. The assets, or the cash flows, of one firm can be used as collateral for the other. Consistent with this idea we find that the fall in leverage and asset growth is more pronounced when the other firm contributes with more tangible assets to the group. The evidence of cash-flow cross-pledging, which should decrease with an increase in the sales correlation of the two firms in the group (Hann, Ogneva, and Ozbas, 2013), is weaker. Also pointing towards credit constraints is our finding that the results are stronger among firms in debt-dependent industries (in the style of Rajan and Zingales (1998)'s financial dependence), and in countries with less developed banking systems as measured by the ratio of domestic credit to GDP.

These findings are evidence in favor of what Stein (2003) calls a "more-money effect". Firms in business groups have access to more funds, which results in higher leverage and higher investment. However, more funds do not necessarily add value. In fact, they can be harmful if the allocation of capital within business groups is affected by the inefficiencies described in the literature on the dark side of internal capital markets (Rajan, Servaes, and Zingales, 2000, Scharfstein and Stein, 2000). Consistent with "socialism" in these internal allocations, we find that the firms that reduce leverage more strongly as they become standalone are precisely those that were more likely to overinvest with subsidized financing. In particular, we find that firms that had initial profitability below their industry peers and initial leverage above their industry peers see their leverage ratio fall by more. Also, firms that had lower Tobin's q than the other firm in the group also experience a stronger fall in leverage. Given the interplay of the "more-money effect" and corporate "socialism", it is perhaps not surprising that we do not find an overall effect of becoming standalone on profitability.

Our paper is mainly related to the literature that estimates the costs and benefits of being affiliated to a business group. There is now ample evidence that groups have a financial advantage. For example, Gopalan, Nanda and Seru (2007) show that intra-group loans in India reduce the likelihood of bankruptcy in affiliated firms (see also Buchuk, Larrain, Muñoz, and Urzua, 2014, for intra-group loans in Chile). Gopalan, Nanda and Seru (2014) find that groups use the dividends of some firms to fund investment in others. Almeida, Park, Subrahmanyam, and Wolfenzon (2011) find that groups acquire capital-intensive firms, which are typically more financially constrained (see also Bena and Ortiz-Molina, 2013). Almeida, Kim, and Kim (2015) show that Korean groups were able to sustain the investment of high-growth firms during the Asian crisis through cross-firm equity investments. The other side of this financing advantage is that the same mechanisms are often used to tunnel funds out of affiliated firms instead of investing in profitable projects. Jiang, Lee, and Yue (2010) argue that Chinese groups use intra-group loans to extract cash from firms. There is also evidence of inefficient use of dividends (Faccio, Lang, and Young, 2001), mergers and acquisitions (Bae, Kang, and Kim, 2002), private equity placements (Baek, Kang, and Lee, 2006), and dilutive equity offerings (Atanasov, Black, Ciccotello, and Gyoshev, 2010).

Most of this literature deals with large groups. The groups that we study are simple or textbook examples of business groups, probably quite different from the complex structures seen in Sweden, Korea, Brazil, or other countries. Small groups help us to better understand the behavior of more complex groups since the underlying incentives are analogous in both cases. More importantly, our results represent arguably cleaner estimates of the causal effect of business group affiliation, which is much harder to have with large groups. Another advantage of focusing on small groups is that large groups bring the added complexity of general equilibrium effects, since they often represent non-trivial parts of the economy (see Almeida and Wolfenzon, 2006b; and Morck, Wolfenzon and Yeung, 2005), while such complexities are absent in small business groups. For example, the split of large groups can have implications for market power and equilibrium prices (e.g., Perez-Gonzalez, 2015). Finally, tunneling, which is a concern in large groups, is less of a possibility in the small groups that we study because the stakes of controlling shareholders are typically very high (above 90%) and therefore minority shareholders are too small to mater. This also implies that the typical agency problem between a market of dispersed shareholders and a CEO is absent from our sample.

Our paper also contributes to the literature on internal capital markets that studies the allocation of capital within firms, mostly (although not exclusively) using data on U.S. conglomerates. There are theoretical underpinnings for bright and dark sides to internal capital markets. On the one hand, conglomerates may be better at picking winners (Giroud and Mueller, 2015; Khanna and Tice, 2001; Stein, 1997). Also, even if there seems to be a drop in productivity in conglomerates, their behavior may still be profit-maximizing (Maksimovic and Phillips, 2002). On the other hand, rent-seeking behavior from plant managers and corporate socialism in the form of redistribution between firms can distort the allocation of capital (Matvos and Seru, 2014; Ozbas and Scharfstein, 2010; Rajan, Servaes, and Zingales, 2000; Scharfstein and Stein, 2000). Although similar mechanisms apply to the allocation of capital within business groups, there is one main difference between conglomerates and groups that we exploit to our advantage. Firms in business groups are separate corporations with their own capital structure, which is something that we cannot observe across segments or plants in a conglomerate. The fact that we can measure capital structure effects implies that we can test the mechanism more precisely, instead of relying only on outcome variables such as investment.

Finally, our paper speaks to the recent literature on networks as propagation mechanisms for shocks. Networks in general, beyond ownership networks or business groups, are ubiquitous in the corporate world. For instance, the literature studies customer-supplier links (Acemoglu, Akcigit, and Kerr, 2015; Acemoglu, Carvalho, Ozdaglar, and Tahbaz-Salehi, 2012; Barrot and Sauvagnat, 2015), banking relationships (Khwaja and Mian, 2008), networks in boards of directors (Khwaja, Mian, and Qamar, 2011), and so on. A common theme in this literature is that networks transmit, and therefore amplify, shocks that affect in a direct way only a subset of the members of the network. In our setup, shocks that are specific to an industry are propagated to unrelated firms that share owners with the affected firms, with the consequent lower growth and change in capital structure.

The rest of the paper is organized as follows. Section 1 explains our empirical design, including the main data and the industrial shocks that we use as part of our identification strategy. Section 2 presents the main results. Section 3 explores different mechanisms. Section 4 concludes.

1. Empirical Design

1.1. Identification strategy

Imagine that we start with a sample of firms associated in pairs or small business groups. In this sample we study the effects of losing the affiliation to the business group, i.e., the "treatment" consists of becoming a stand-alone firm. "Control" firms are those firms that do not become standalone. The fundamental economic problem that we face is that the treatment does not arrive randomly. In general, firms -or their controlling shareholders- choose to become standalone. Hence, estimating the causal effect of becoming a standalone is very challenging.

We can think of the following regression setup. We regress firm outcome Y_{it} on the dummy $StandAlone_{it}$ that takes a value of 1 if firm i is a standalone in year t, and 0 otherwise. We control for shocks to the same industry ($OwnShock_{it}$), and lagged firm characteristics included in the vector X_{it-1}. Time-invariant unobservable variables that could explain the selection into business groups are captured by firm fixed effects μ_i. For example, some could argue that firms in particular industries are more prone to form groups. Finally, we also control for time fixed effects τ_t:

$$Y_{it} = \beta\ StandAlone_{it} + \gamma\ OwnShock_{it} + \delta' X_{it-1} + \mu_i + \tau_t + \varepsilon_{it}. \qquad (1)$$

In essence, regression (1) is a before-and-after comparison for those firms that receive the treatment. Control firms allow for better estimates of the time effects τ_t and the δ coefficients. The null hypotheses is that $\beta = 0$ since in a Modigliani-Miller type of world the ownership status should not matter for firm outcomes such as capital structure or investment policies. The problem is that the typical OLS estimate of β does not correspond to the causal effect of being standalone. For example, unobserved productivity contained in ε_{it} affects the outcome Y_{it}, but also the decision to become standalone, so the standard exogeneity assumption of OLS does not hold. In short, we need an instrument for $StandAlone_{it}$ if we want to talk about causality.

The instrument that we propose consists of industrial shocks to the *other* firm in the group. The stylized idea behind our instrument is depicted in Figure 1. Think of a group controlled by owner A that has two firms, Firm 1 and Firm 2. Firm 1 receives a severe negative shock and shortly afterwards the firm is sold to another owner (owner C) or disappears. Alternatively (not shown in the figure as possibility), Firm 2 is sold to a different owner. The shock, therefore, has the consequence of leaving Firm 2 as a standalone. This change of status for Firm 2 is precisely the crux of our identification strategy. As control firm we use Firm 3 in a different business group under owner B. Firm 3 comes from a one-to-one matching procedure that we explain later on. The first stage regression is then:

$$StandAlone_{it} = \theta\ OtherShock_{it} + \pi\ OwnShock_{it} + \rho' X_{it-1} + \mu_i + \tau_t + \vartheta_{it}. \quad (2)$$

We estimate (2) with OLS, hence this represents a linear probability model since $StandAlone_{it}$ is a dichotomous variable.

Like any instrument, $OtherShock_{it}$ has to obey the exclusion restriction and it has to be relevant (as opposed to weak). We note two things in terms of the exclusion restriction. First, the instrument is at the industry level and not at the firm level, so it is easier to argue that variation is exogenous to the skills or preferences of the controlling shareholder. For example, a measure of shocks based on the earnings of the other firm would most likely be contaminated with these skills or preferences. Also, since the instrument affects an entire industry it is harder to argue that our results come from, say, the smaller firms within each industry. For our identification strategy the key is that the shocks are exogenous to the business group itself, but we do not argue that shocks are exogenous to market forces in a general sense. (For example, the housing crisis in this period is likely to be endogenous to the previous financial crisis). Second, we take business groups with two firms in unrelated industries as measured by coefficients of the input input-output matrix as we explain later on. If the industries were related, one could argue that spillovers other than through the ownership structure could explain away our results (e.g., through customer-supplier links).

In order to avoid the case of a weak instrument, we focus on groups with only two firms. In large groups the link between a shock to one firm and another firm becoming standalone is more tenuous or, if not, potentially endogenous. For instance, think of a group of 4 firms that suffers a shock that bankrupts one of the firms and leaves 3 firms in the group. If one of those 3 firms later on becomes a standalone, it is harder to argue that the decision is forced from outside and not the consequence of selection.

1.2. Industrial Shocks

The shocks in $OwnShock_{it}$ and $OtherShock_{it}$ are identified in the following way. We proceed in a reverse engineering fashion by first identifying candidate shocks from the returns of listed firms. We then clean this list of candidates by checking the nature and source of the shock in the press and analyst reports. We prefer to be conservative, in the sense that our final sample only contains shocks that we can identify with confidence and precision.

We start by computing six-month rolling windows of stock returns for four-digit SIC industrial portfolios in each European stock market in the last decade. Industry portfolios are value weighted. Within this universe we select the returns that belong to the lowest 5% of the distribution and during the years that are relevant for our business group data. This gives us a sample of 5,648 six-month returns. From this sample we first check that the fall in returns is not driven by idiosyncratic shocks to a few firms in the industry, but that the shock is a sufficiently widespread phenomenon. Then we check by hand, in the press or analyst reports, the type of shock that likely causes the negative returns. In about a fifth of the cases (1,045 observations) we are able to pin down the source of the shock, from commodity-related shocks (e.g., metals, grains, livestock, and others) to regulatory decisions (e.g., safety laws, tobacco-related laws, etc.). Since we use rolling windows, many of these returns in the lowest 5% correspond to observations in adjacent months. Overall, we identify 359 country-industry-year shocks, out of which there are 322 commodity shocks and 37 regulatory shocks. Only 10 are country-industry specific shocks, and the rest affect an entire industry in all of Europe. Table 1 (Panel A) summarizes our selection process.

Figure 2 shows two examples of shocks in our sample. The first example is a regulatory shock that hit the games and toys industry in June 2009. The trough for six-month returns of about -25% was observed a couple of months afterwards. The shock was related to safety regulation for toys and it affected the entire industry in Europe. A second example corresponds to the poor returns of the prepared meats industry in late 2011-early 2012. The returns of almost -20% go together with a 42% price decline in the price of lamb in 12 months.

The housing crisis –decline in construction and real estate— that affected Europe during this period propagated to 7 four-digit SIC codes, representing 21 SIC-year shocks out of the 359 shocks from Table 1.

1.3. Data Description

We obtain firm level data from *Amadeus*, the database assembled by Bureau van Dijk that provides both accounting and ownership information on private and public firms in Europe. *Amadeus*' accounting data includes balance sheet and profit and loss numbers that can be easily accessed through WRDS. The ownership data cannot be directly downloaded and we obtain it from *Amadeus*' DVDs at a yearly frequency. This data includes the names of shareholders and the ownership stake they have. It also includes information on the type of shareholder and therefore whether it is a family, an individual owner, a publicly listed firm or another type of corporation.

We collect data for 16 Western European countries from 2009 until 2013 — the coverage prior to 2009 is not good enough for many of the countries in our sample. Since firms in Europe have minimum reporting requirements, *Amadeus*' coverage is almost equivalent to the universe of firms. There are, for instance, 8.6 million firms in 2011, out of which 1.3 million are directly controlled by families or individuals with an ownership stake larger than 50%. A crucial step for our identification strategy is to focus on business groups controlled by either families or individuals and that are composed of *only* two firms incorporated in the same country. All these firms are privately held. An example would be the group controlled by Sergio Traversa in a village close to Vicenza, located between Milan and Venice. In 2010 he has controlling stakes in a company that produces cushions and fabrics for garden furniture (Lollo Due SRL) and also in Ongaresca Societa' Agricola SRL, a vineyard.

We refer to firms that eventually transit to standalone as treated firms and to firms that always belong to a two-firm business group as control firms. Treated firms in our sample become standalone the year when there is no other firm in the sample where the same controlling shareholder owns more than 50%. This can happen if there is a sale to different owners (of any of the two firms), or if the other firm in the group goes bankrupt (this last possibility only happens rarely).

Having identified which firms belong to two-firm business groups, we drop groups that have firms in well integrated industries. Following Fan and Goyal (2006) we construct a measure of vertical integration based on the input-output table for the U.S. Using this table we compute the fraction of input (output) that an industry acquires (sells) from (to) the other industries. Then, for each business group we compute the average of what the industry of each firm sells to the industry of the other firm, and what it buys from the other industry. If this average is larger than 1%, then we drop the group. In the same line we also exclude groups were both firms are in the same 3-digit SIC industry. These two restrictions mean that the groups we consider are basically composed by firms in unrelated industries. Overall, a little less than 1% of the firms in *Amadeus* (e.g., 81,275 firms in 2011) conforms with the criterion of belonging at some point to a business group with two firms in unrelated industries.

To further ensure the quality and homogeneity of the data we apply several additional restrictions. First, we restrict the sample to firms that during their first year in the sample are part of a two-firm business groups (i.e., we drop firms that were not originally in a two-firm business group and transit into a business group during the sample period). Second, we drop firms with less than four years of data and firms with annual asset growth below -90% and above 200%. Third, we ensure that we compare firms that become standalone with firms that stay part of a two-firm business group that are as similar as possible. To do this we perform a one-to-one propensity score matching, based on two-digit SIC code classification and size (book assets) in their initial year in the sample. Figure 3 shows the distribution of firms by size in both groups of firms. As expected from the matching procedure, both distributions are basically overlapping.

Our final sample consists of 3,843 firms that eventually transit to standalone, representing 16,105 observations; and 3,843 firms that always belong to a two-firm business group, representing 15,762 observations. Although our final sample is perhaps not small in size, it only represents a small fraction of the original *Amadeus* universe. We have to discard millions of firms, even losing all observations from four countries (Sweden, the Netherlands, Belgium, and Switzerland) in order to satisfy the strict selection criteria that are necessary for our identification strategy.

Table 1 (Panel B) shows the industrial shocks previously identified that we are able to match to the business group data.[1] We consider a match if there is a firm in our business group data for that country-industry-year combination, and also up to two years later. We look up to two years after the shock because it is likely that divestiture decisions or bankruptcy take time to materialize. We are not able to match all of the shocks because not all industries with shocks are represented among business groups. Out of the initial 359 shocks, in 121 cases we have firms in the same industry of the shock, and in 119 cases we have firms in other industries but paired with those firms directly affected by the shock.

A concrete example of our identification strategy would be the following. In 2009, Johannes Backenecker had two firms, owning 100% of each: Bernhard Upmann Verpackungsmaschinen, a manufacturer of calibration, weighting and packaging machines; and Backenecker Liegenschafts-verwaltungs, a real estate agency. In November 2009, the European Commission issued a directive regarding maximum possible errors of measuring instruments, which lead to poor returns in this industry. In 2010, the manufacturing firm was sold to three partners, each of them owning 33.33% of the firm. Thus, from 2011 onwards the real estate agency operated as a stand-alone business under Johannes Backenecker, who

[1] Given the SIC-code availability in the business group database, we aggregate shocks from the 4-digit up to the 3-digit SIC-code level in the merged sample. We initially look at shocks at the 4-digit level so our understanding of the shocks (their nature, source, duration, etc.) is better.

kept a 100% stake. In a nutshell, our identification strategy is focused on the impact of becoming standalone in this second firm and others like it.

Table 2 describes the composition of our sample by year, country, and industry. Out of the 16,105 observations for treated firms, 3,507 correspond to firms facing an industry shock to their companion firm (i.e., the other firm controlled by the same group), and these are relatively spread out throughout the sample period. Although the sample is not perfectly balanced in every single dimension, it is relatively well balanced. Some differences can be expected because of the size of different countries. For instance, Spain is a much bigger market than Portugal. Other differences are related to the regulatory standards for reporting data on private firms. In terms of geographical distribution, Germany has the largest representation, followed by Italy, Norway, Austria, Spain, and the U.K. Table 2 also shows that there is no cluster of observations in one particular industry. SIC 5 (wholesale and retail trade) has the largest share but it is still less than 25% of the observations.

Figure 4 shows the distribution of observations according to the industry of both firms in the business group. We split observations into pairs with shocks to the industry of the other firm and pairs with no shocks. The purpose of the figure is to show that there is no specific cluster of observations, with or without shocks, in particular industrial segments that could bias the results later on. For example, it seems hard to tell a selection story along the lines of "firms in industry X are typically paired with firms in industry Y that received more shocks in this sample period."

Table 3 show summary statistics for the main variables in our analysis. Panel A shows firms that eventually become standalone (treated firms) and Panel B shows firms that remain in groups throughout the period (control firms). We have between 12,000 and 16,000 observations for each variable in each panel, except for OROA (operating return on assets=EBIT/book assets), for which we have only about 6,000 observations. Firm characteristics such as size (book assets in million EUR), leverage, tangibility, and others are remarkably similar on average across treated and control firms. The stake of the controlling shareholder is generally above 95%, so minority shareholders are almost non-existent in this sample. Averages for the dummy variables representing shocks correspond to the frequency of being hit by a shock. Therefore, between one-fifth and one-quarter of the observations are hit by shocks, for both treated and control firms.

We also compute characteristics of the pair of firms in each group. All characteristics are computed as ratios of the characteristic for the other firm over the characteristic for the firm under study (i.e., other/own). For instance, relative tangibility is the ratio of PPE (property, plant and equipment) of the two firms in the group. Relative Tobin's Q and relative OROA are computed analogously. Sales correlation is computed from U.S. data as the correlation coefficient between sales growth of the industries of the two firms in the group. Relative-to-pair characteristics are also very similar on average across treated and control firms. This means that the characteristics of the business groups that split up do not differ significantly from the business groups that stayed together. For example, it is not the case that groups that split up had on average larger differences between their firms in terms of tangibility, Tobin's Q, OROA, or sales correlation than other groups.

Finally, we show industrial and country characteristics of the groups in our data. Rajan and Zingales (1998) measure the external dependence of an industry as the average fraction of investment that is not financed with internal cash flows (everything measured for U.S. industries). They compute equity dependence as the average fraction of investment that is financed through equity. We compute debt dependence as the difference between external dependence and equity dependence. Again, we do not find differences in these characteristics at the industry level or the country level (domestic credit over GDP) across treated and control firms.

2. Main Results

Table 4 shows the results from our first stage (for the subsequent leverage regressions) where the dummy variable for stand-alone status is the dependent variable (equation (2) above). The explanatory shocks correspond to indicator variables that take a value of one the year of a given shock and the two following years, and zero otherwise. We include an indicator variable for shocks in the industry of the *other* firm in the group (the instrument) and a different indicator variable for shocks in the same industry (not an instrument). The other controls (X_{it-1}) are lagged log assets of the firm, lagged tangibility, and the Tobin's Q of the industry. The estimated coefficient for $OtherShock_{it}$ ranges between 0.0889 and 0.1120, which implies a sizeable impact of shocks to the other industry in the likelihood of becoming a standalone. For comparison, the coefficient on the own industry shock is only slightly higher between 0.0902 and 0.1285. The large F-statistics confirm that the instrument is strong. The last three columns of Table 2 show a placebo test where we generate a random 0-1 variable with the same mean as $OtherShock_{it}$ in the sample. The placebo shocks have no significant effect on stand-alone status, and the F-statistics are all below one (i.e., the placebo "instrument" is weak as expected).

Table 5 (Panel A) shows the results for the second stage (equation (1)) of the IV estimation. The effect of becoming standalone on leverage ranges between -0.0866 and -0.1034, which implies that the reduction in leverage is approximately one-third of the standard deviation of leverage in sample (see Table 3). The different specifications in Table 5 vary according to whether we use or not the control variables in X_{it-1}, or if we restrict the sample for availability of these controls. Including these control variables reduces the sample size from 27,000 to 19,000 observations approximately, but the coefficient on $StandAlone_{it}$ remains significant and of similar magnitude. Irrespective of the specification, the effect is significant at least at the 5% level. This means that the effect is quite robust since the standard errors typically increase in IV regressions. The effect on asset growth –our proxy for investment– goes between -0.1562 and -0.2239, again always significant at the 5% level at the least. Hence, the effect of becoming standalone on growth is stronger than in leverage since it represents about three-quarters of the standard deviation of asset growth in the sample (see Table 1). The effect on growth discards a mechanical decrease on leverage that could happen if firms pay debt with the proceeds of the sale of the other firm (technically, the controlling shareholder would need to increase equity and use this to pay debt). If that were the case, why grow less in the firm where the controlling shareholder has just increased her investment?

The OLS regressions (Panel B in Table 5) basically show that there is no impact of becoming standalone on either leverage or asset growth. OLS estimates are likely to be biased against a negative effect on leverage and growth because many firms become standalone intentionally when they have little to lose. In other words, many firms select themselves into the stand-alone status. The IV procedure allows us to isolate the firms that receive the treatment unintentionally, hence we say that these firms are "left alone" more than that they "become standalone". In other words, the IV procedure allows us to estimate the causal effect of business group affiliation, unlike other papers that deal with the selection of firms into business groups.

Our experimental design rests on several assumptions that we now explore in more detail. First, given that our empirical strategy is an IV strategy on top of a differences-in-differences setup, our analysis is valid to the extent that treated and control firms share similar or parallel trends before the event. This would imply that post-event differences are not produced by pre-event differences. We explore this issue in an event-study fashion in Figure 5.

Panel A in Figure 5 shows our main result for leverage. We define year zero as the year when a firm becomes standalone, and we plot leverage before and after this event. Given that our IV strategy uses shocks to other firms as instrument, we differentiate between treated firms that faced shocks to the other firm and treated firms that do not. Since not all firms have data for the entire window, we first compute differences in leverage between two consecutive years for each firm. We do the same for the control firm that is paired with each treated firm at the beginning of the sample. We then take the average of these differences across all firms in each event year and for each subsample of firms: firms that become standalone and did not face shocks to the other firm, firms that become alone and faced shocks to the other firm, and control firms with and without shocks. Finally, we add back the initial level of leverage to each subsample. The parallel trends are clear before year zero; the negative slope is consistent with an overall decline in leverage in European firms during the sample period. However, it is also clear that treated firms with shocks to the other firm in the group have a break in that trend (a stronger reduction in leverage) after becoming standalone. We do not see the same reduction in other stand-alone firms. In fact other stand-alone firms and control firms behave very similarly.

Panel B in Figure 5 illustrates the result for asset growth. We present results only starting in year -1 since we have few observations of growth rates for year -2 (they imply having data for year -3). Again, the parallel trend before becoming standalone is clear in the figure, as well as the downward break in years 2 and 3 for treated firms that receive a shock to the other industry. In a nutshell, Figure 5 illustrates the reduced form version of our results. It is clear that the firms affected by a shock to the other firm in their group are those that experience a stronger reduction in leverage and growth.

Second, there may be a more subtle critique regarding the exclusion restriction. One could argue that $OtherShock_{it}$ belongs in our equation (1) on its own merit, and not only as a reduced form of the effect it has on firms through the standalone status. Ultimately, the exclusion restriction cannot be directly tested. However, we can show some suggestive evidence that it is unlikely that $OtherShock_{it}$ has a direct effect on firms' outcomes for our sample of small business groups.

In Figure 5 we also show the leverage and asset growth dynamics for *control* firms, i.e., firms that never split from their groups. If $OtherShock_{it}$ has a direct effect on firms' outcomes other than through changes in business group affiliation, then we should also expect to see differences among the firms that never split. In particular, those firms that are hit by a shock to the other firm in the group should display differences in growth and leverage when compared to firms that did not receive a shock to the other firm. We find, however, that when there is a shock to the other firm in the group, leverage is very similar to when there is no shock, and this difference is not statistically significant. By the same token, differences in asset growth for year t=1 and t=3 are not statistically significant when comparing control firms with and without shocks to the other industry. In year t=2 we see that firms hit by shocks to the other industry have higher growth, instead of lower growth. Albeit transitory, this effect, if anything, would bias the results against our main findings.

Although not shown in Figure 5, shocks to the same industry have a strong effect on control firms - in contraposition with shocks to the other industry. There is a statistically significant increase in leverage by 3.7% (=62.1%-58.4%, t-stat=-5.57) and a strong reduction of 1% on asset growth (1.9% v. 0.9%, t-stat=-1.78) when firms receive a shock to their own industry. Overall, this is consistent with own industry shocks being detrimental to firms, but shocks to the industry of the other firm in the group having no direct effect in firms' outcomes.

One important message from Figure 5 is that shocks to the other industry do not have a relevant effect on firms that never split, which goes in favor of the exclusion restriction that we assume. Our interpretation is that, for the sample of small, private firms that we study, the effects of shocks on group affiliation are decisive for future financing and growth, while other potential channels, such as intra-group lending and equity flows, are not. These other channels are more likely relevant in the context of large business groups and conglomerates, and potentially smaller shocks (relative to firm size) that can be easily smoothed. For small firms, group affiliation is arguably of great importance when facing financial intermediaries, in particular banks, and asking them for credit. We explore these credit constraints in more detail in the next section.

3. Mechanisms

In this section we explore heterogeneity in the treatment effect and ancillary predictions that help us to better understand the main results. We first study reasons for why (or when) should credit constraints be less binding in groups vis-à-vis stand-alone firms. By pinning down the mechanisms behind credit constraints we can be more confident about the interpretation of the main result. Second, we explore firm characteristics that are typically related to inefficient cross-subsidies, or "corporate socialism", in order to get a sense of the capital misallocation within business groups. The overall effect on firm performance depends on the interplay of lifting credit constraints and the potential inefficiencies in investment. We borrow from the literature on internal capital markets (see Stein, 2003, for a survey), which although focused on conglomerates, can guide de discussion for capital allocations within groups as well.

3.1. Cross-pledging and Credit Constraints

Business groups can have a financing advantage due to cross-pledging between the different firms in the group (Tirole, 2006). There is a "more money effect" when seeking financing in a group (Stein, 2003): total financing is more than the sum of what each firm can get on its own. A firm in a group can lift some of the credit constraints that affect it by using the assets or cash flows of the other firm as collateral. From the contractual point of view, the controlling shareholder can leave the assets of one firm (e.g., real estate), or her shares in that firm, as collateral for the debt of the other firm.

In the tables that follow we split our data according to the median of each characteristic as threshold for the high and low subsamples. In Table 6 we split firms according to the potential for cross-pledging. For Panel A we use the relative tangibility of the two firms in the group. A firm that becomes standalone when the other firm added relatively more tangible assets to the group should suffer more in terms of debt capacity, and consequently growth. In a sense this firm reduces its debt capacity and investment because it losses the financial "subsidy" it was receiving from the other firm. We find that our results are stronger, both in terms of magnitude and statistical significance, in the sample of firms that lost a high-tangibility partner. Notice that the first stage is similarly strong in both subsample of firms with high and low tangibility partners, so the lack of an effect in the low-tangibility sample is not due to a selection problem, i.e., that only firms with high-tangibility partners are affected by the shocks in our instrument.

In Panel B of Table 6 we split the sample according to the sales correlation of the industries of both firms in the group. The total cash flow of the group should be more stable as this correlation decreases, and financing capacity should increase (see Tirole, 2006, chapter 4 for a formal model). Therefore, a firm that had a low-correlation partner in the group should be more affected in its financing capacity when becoming standalone. We do not find evidence supporting this hypothesis in the data. In fact, the decrease in leverage is marginally stronger among firms in the high-correlation sample instead of the low-correlation sample. Overall, we find evidence of asset cross-pledging, but not of cash-flow cross-pledging. We believe that part of the reason is that industrial correlations are only a proxy for the cash-flow correlations of the firms in a group, while relative tangibility is measured more precisely.

Cross-pledging relaxes credit constraints, which should be particularly important in industries that naturally rely more on debt. In fact, we find in Table 7 that our results come mostly from firms in debt-dependent industries. In Panel A we find that the coefficient on $StandAlone_{it}$ in the high debt-dependence sample is almost three times as big as the coefficient on the low debt-dependence sample (-0.1261 v. -0.0460). The coefficient on the asset growth regression is also stronger and more statistically significant in the high debt-dependence sample (-0.1882 v. -0.1128). In Panel B we find, instead, that it is not the high equity dependence industries that suffer the most, but the low equity dependence industries, which serves as a sort of placebo test to really attach our results to credit constraints and not some generic external dependence (reported for completeness on Panel C).

Cross-pledging should also be particularly important in less developed credit markets, where collateral is crucial to obtain debt financing. In Panel D of Table 7 we split our sample between countries with more and less developed credit markets as measured by the ratio of domestic credit to GDP. The results, as expected, are stronger in the sample of less developed credit markets. The reduction in leverage is larger in magnitude (-0.1354 v. -0.0436), as well as the reduction in asset growth (-0.2230 v. -0.0752).

3.2. Corporate "Socialism"

Relaxing credit constraints is potentially good if firms underinvest when compared to their first best. However, relaxing credit constraints can also have counterproductive effects if firms in groups invest in poor projects that credit markets would otherwise not fund. Stein (2003) calls this corporate "socialism" since firms in groups can be internally subsidized when compared to market allocations. The prediction for our setup would be that those firms that are subsidized in groups should be hurt the most as they become standalone.

In Table 8 we explore several candidates for being subsidized if there is corporate socialism. In panel A, we split firms using their performance (OROA) relative to industry peers in the beginning of the sample. We find that the reduction in leverage is seen mostly in firms with low initial performance. In fact, firms with high performance relative to their industry see their leverage ratios increase after becoming standalone, which suggests that they were instead subsidizing other firms. The differences in terms of asset growth go in the same direction, but the coefficients are not statistically significant in either sample. Notice, though, that the sample of OROA is smaller than our main sample, so the sample split is based on fewer observations. In panel B, we split firms according to their initial leverage ratio relative to industry peers. We find that the reduction in leverage is stronger among the high-leverage firms. One interpretation of this last result is that the leverage ratio of those firms in a group was subsidized above the level allowed by the market. We do not find significant results for asset growth in these subsamples.

In Table 9 we split firms according to their characteristics relative to the other firm in the group. This is a measure of relative standing within the group and not the industry as in the previous table. As in Rajan, Servaes, and Zingales (2000) we split the sample according to the divergence in Tobin's q between the industries of both firms in the group. These authors find that the degree of cross-subsidization is stronger when the divergence in Tobin's q is greater. Our results point towards a similar mechanism, since the reduction in leverage after becoming standalone is stronger in those firms where the Tobin's q of the other firm in the group was higher. However, we do not find a similar effect in terms of asset growth, nor when splitting the sample according to the relative OROA of both firms in the group (Panel B).

3.3. Average Effect on Performance

As emphasized by Stein (2003), the prior theoretical literature does not give a strong prior as to what the average effect on performance of becoming standalone should be. This is because of the interplay of the bright and dark sides of internal capital markets. By becoming a standalone a firm may lose access to financing, but at the same time it may cut value-destroying investments and gain focus. In Table 10 we show that the effect of becoming standalone on OROA is not statistically significant. The first-stage is as strong as in our main results regarding leverage and asset growth, so we cannot blame weak instruments for this lack of significance. It is true that the OROA sample is smaller than our main sample since OROA is missing in many cases. Still, given the theoretical ambiguity it is perhaps not surprising that we do not find a strong effect on performance.

4. Conclusions

We study the financial and real effects of being affiliated to a business group using a sample of groups composed of two firms in unrelated industries in the period 2009-2013. Some of these groups split up during this time, leaving firms as stand-alone. In the spirit of Lamont (1997), who studied the investment of non-oil firms as response to oil shocks affecting conglomerates, we instrument for the stand-alone status using shocks to the industry of the *other* firm in the group. In order to make the identification strategy believable we look at groups that have firms in unrelated industries. Otherwise the industrial shock to the other firm could easily affect the firm of interest and violate the exclusion restriction in the instrumental variables setting.

We find that firms becoming standalone reduce their leverage and investment. This evidence is consistent with the idea that affiliation to a group eases credit constraints. The effects are more pronounced when the other firm has high tangibility, which is consistent with cross-pledging, and when the firm operates in a debt-dependent industry or in a country with low domestic credit. In line with "socialism" in internal capital markets, firms more affected by becoming standalone are firms with previous poor performance and high leverage relative to their industry peers, and firms with low Tobin's q relative to the other firm in the group. We do not find a significant effect of becoming standalone on performance. Our results showcase that there are bright and dark sides to internal capital markets, so it is not surprising that the on-average effect on performance is close to zero.

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Figure 1

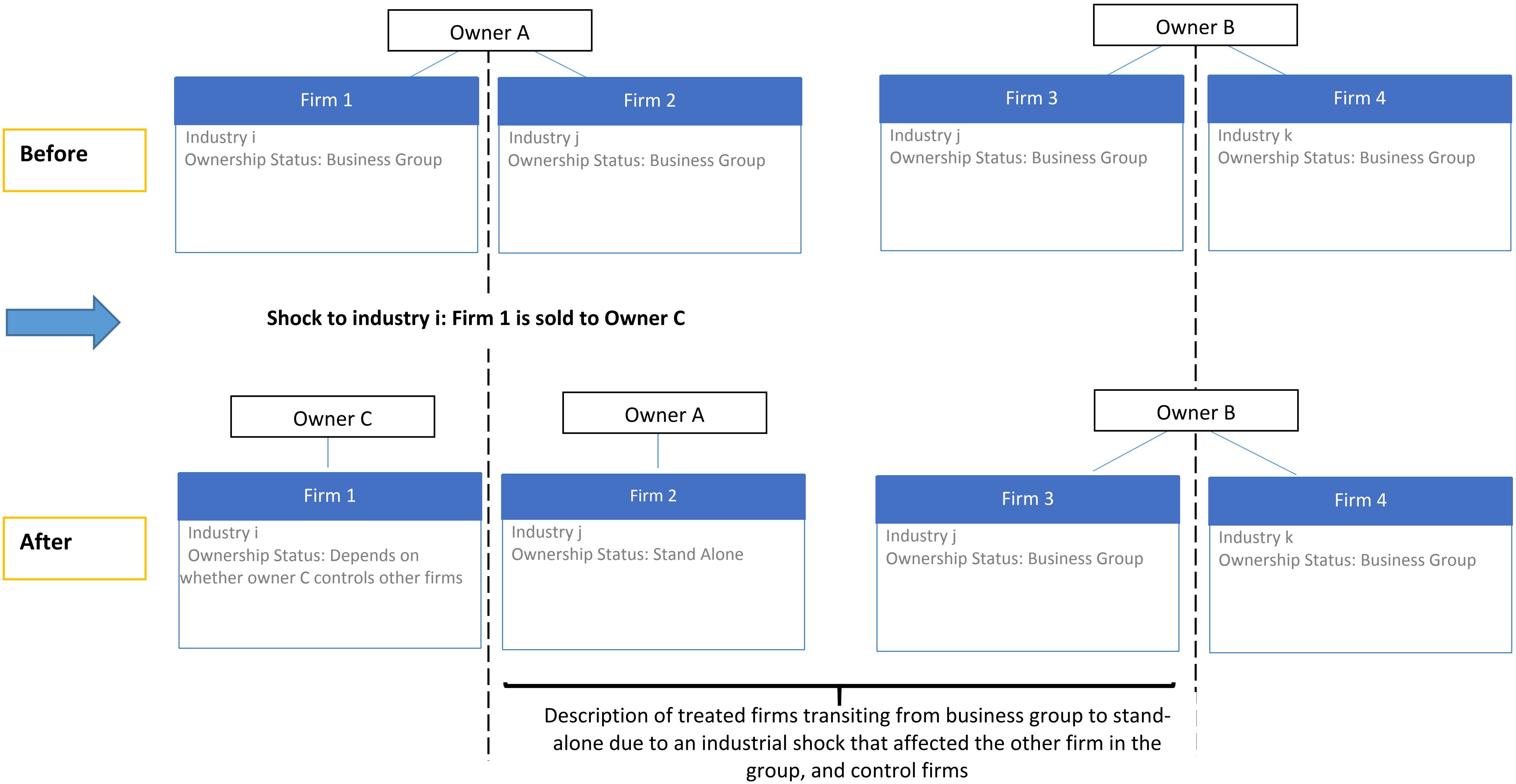
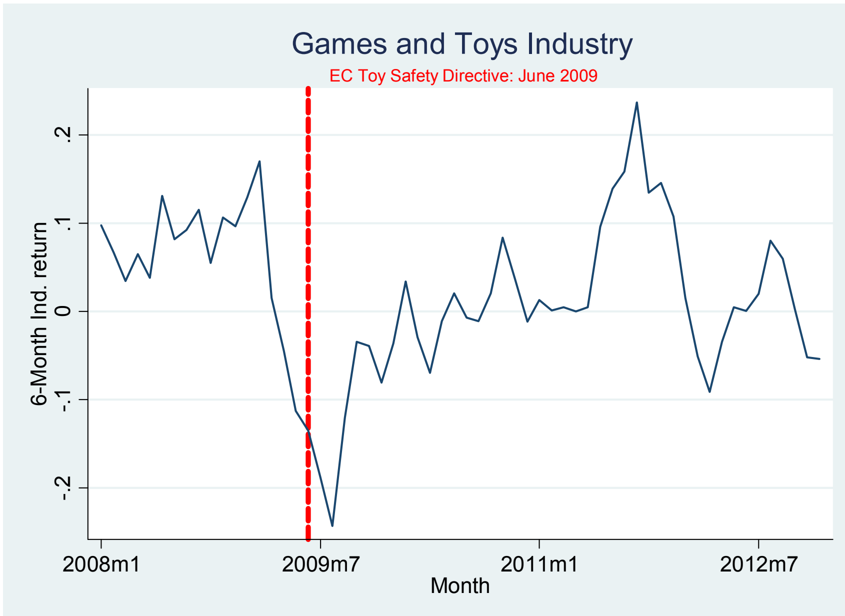

Owner A
Owner B
Firm 1
Industry i
Ownership Status: Business Group
Firm 2
Industry j
Ownership Status: Business Group
Firm 3
Industry j
Ownership Status: Business Group
Firm 4
Industry k
Ownership Status: Business Group
Before
Shock to industry i: Firm 1 is sold to Owner C
Owner C
Owner A
Owner B
Firm 1
Industry i
Ownership Status: Depends on whether owner C controls other firms
Firm 2
Industry j
Ownership Status: Stand Alone
Firm 3
Industry j
Ownership Status: Business Group
Firm 4
Industry k
Ownership Status: Business Group
After
Description of treated firms transiting from business group to stand-alone due to an industrial shock that affected the other firm in the group, and control firms

Figure 2

This figure shows the evolution of returns for firms in Europe in two industries (continuous line) and the time of an exogenous event (long dash line) that affected those industries. Returns are computed as 6-month rolling window weighted averages of returns. Panel A shows the evolution of returns for the Games and Toys industry. The event is a toy safety regulation enacted by the European Commission in June 2009. Panel B shows the evolution of returns for the Prepared Meets industry. The event is a twelve-month price decline in lamb.

Panel A



Panel B

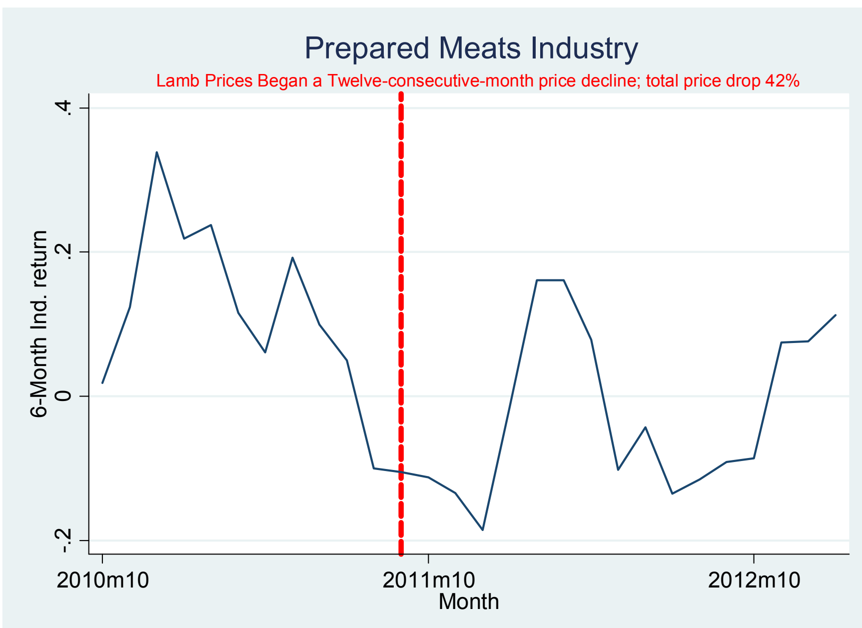


Figure 3

This figure shows the kernel distribution of assets for two set of firms: Firms that belonged to a 2-firm business group during the whole sample period; and firms that belonged to a 2-firm business group at the beginning of the sample, but is left alone as the other firm in the group was sold, or disappeared.

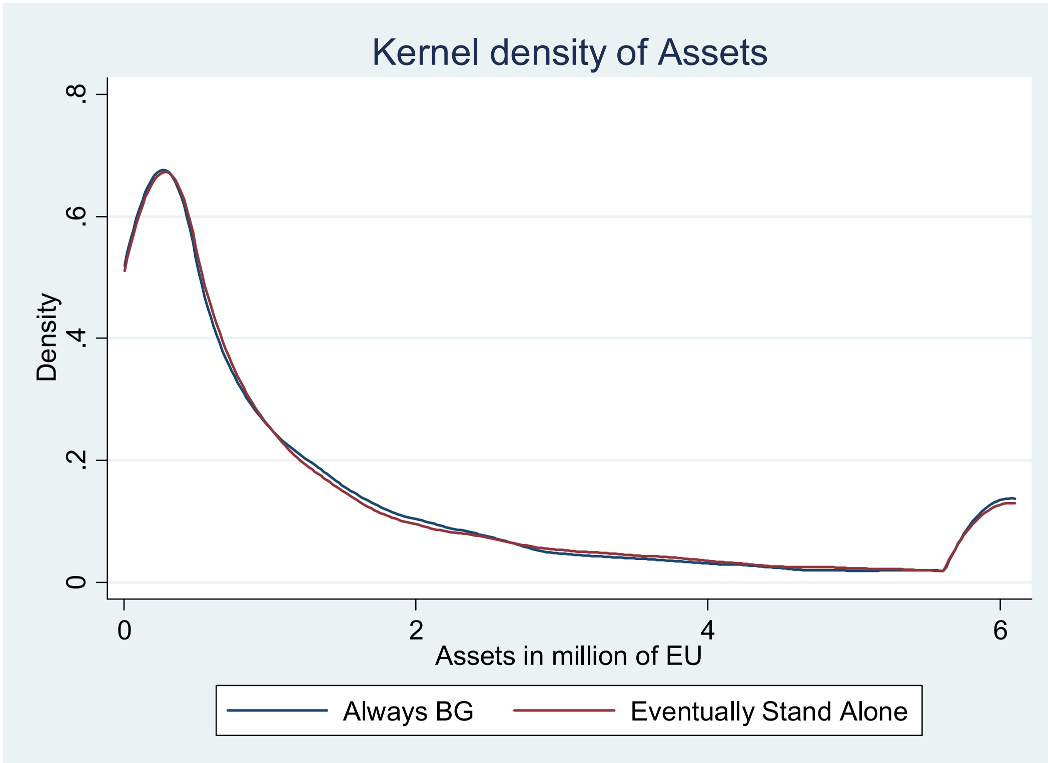


Figure 4

This figure shows the distribution of firms belonging to 2-firm business groups in unrelated industries, according to their 3-digit sic code and the 3-digit sic code of its companion. Triangles represent firms whose companion firms did not receive an industrial shock. Crosses represent firms whose companion firms did receive an industrial shock. The sizes of the figures indicate the number of firms in any given sic/sic-other coordinate. Smaller figures represent a single firm; mid-sized figures represent a 2-5 firms; larger figures represent more than 5 firms in a coordinate.

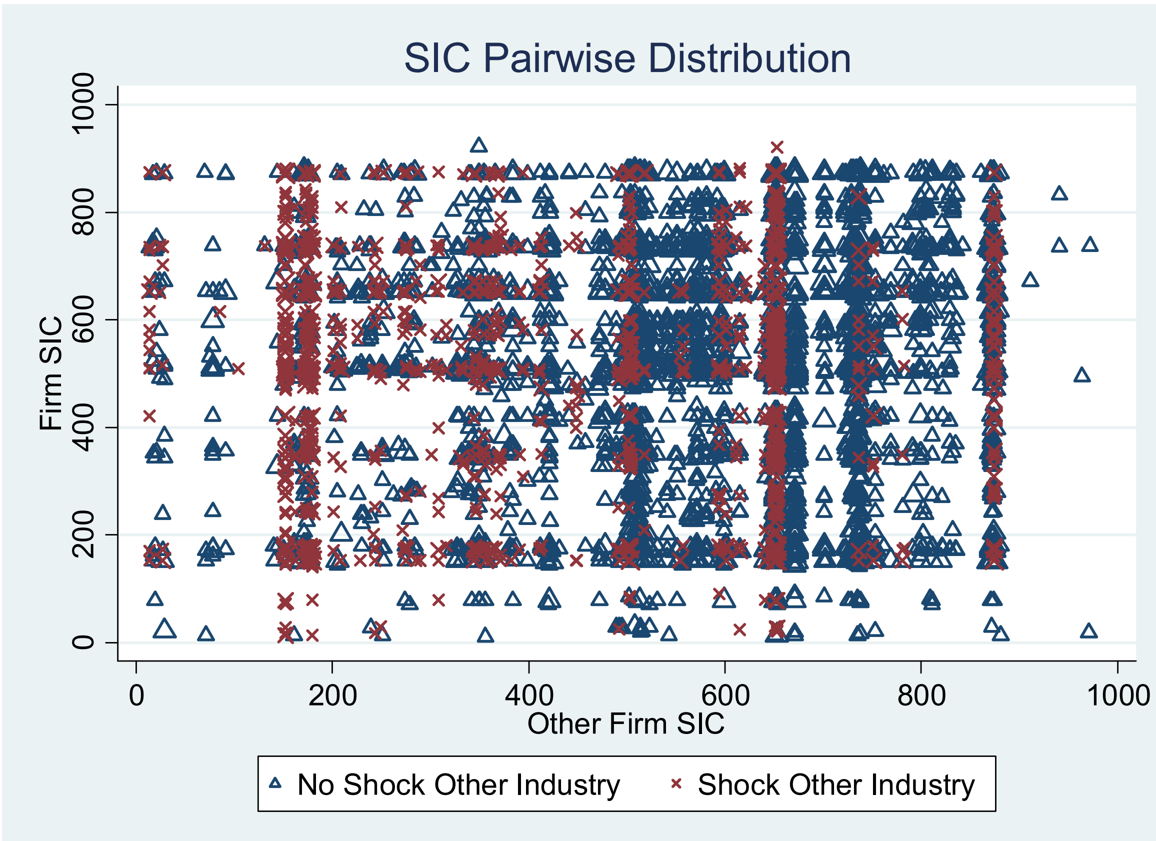
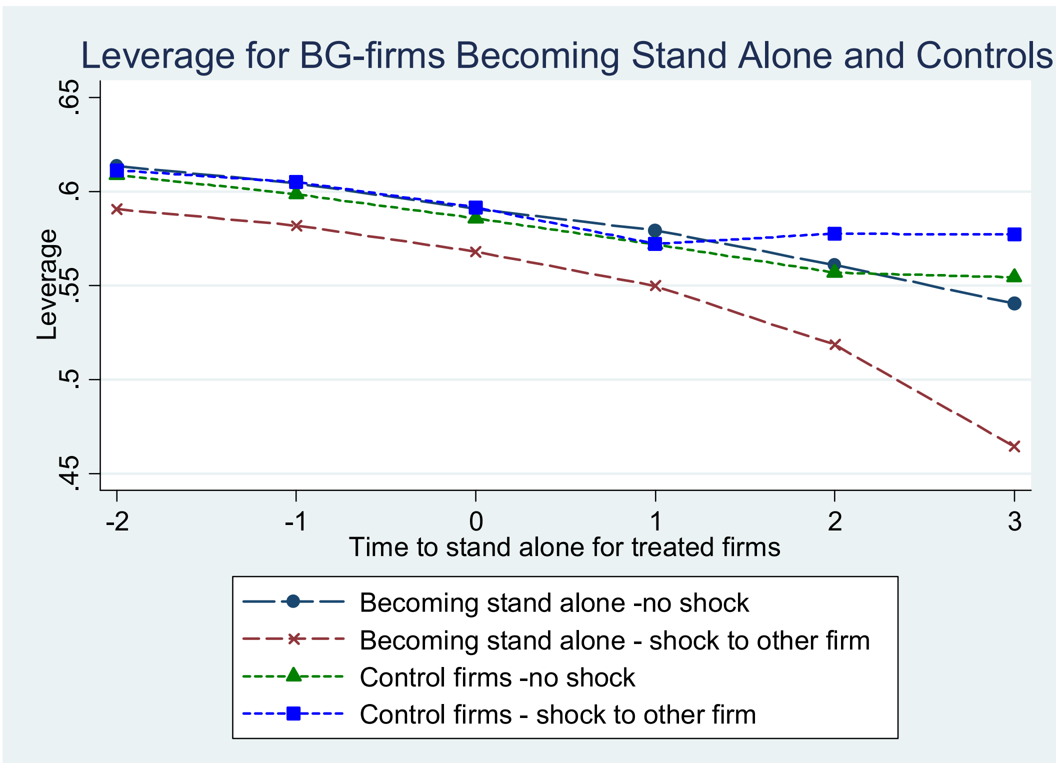


Figure 5

This figure shows the evolution of leverage (Panel A) and Asset Growth (Panel B) for firms the sample, split into 4 categories: Treated firms with pre-treated other shock (i.e., firms that initially belonged to a 2-firm business-group that eventually operate as standalone; and that prior to their transition their companion firm received an industry shock); the reminder of treated firms; control firms (i.e., firms that always belonged to a 2-firm business group) affected by a shock to their companion firm; and the reminder of control firms. For each treated firm we compute the years relative to stand-alone status, which is represented by 0. We use a matched control for each treated firm, so we are also able to graph the evolution of control firms relative to the years to becoming stand-alone of treated firms. Although our sample consists only of 5 years, some firms have 3 years of stand-alone status as they transit early into stand-alone –during their second year in the sample. To emulate within firm differences, we compute the average change in firm leverage (asset growth) between years (e.g., we compute the difference between the second and first year after stand-alone status, and compute the average). We use as starting point the average of firms' first leverage (asset growth) observation in the sample. In Panel B we start from t=-1, rather than t=-2, as asset growth is computed as the difference in logarithm of assets between two periods, thus we lose the initial observation.

Panel A



Panel B

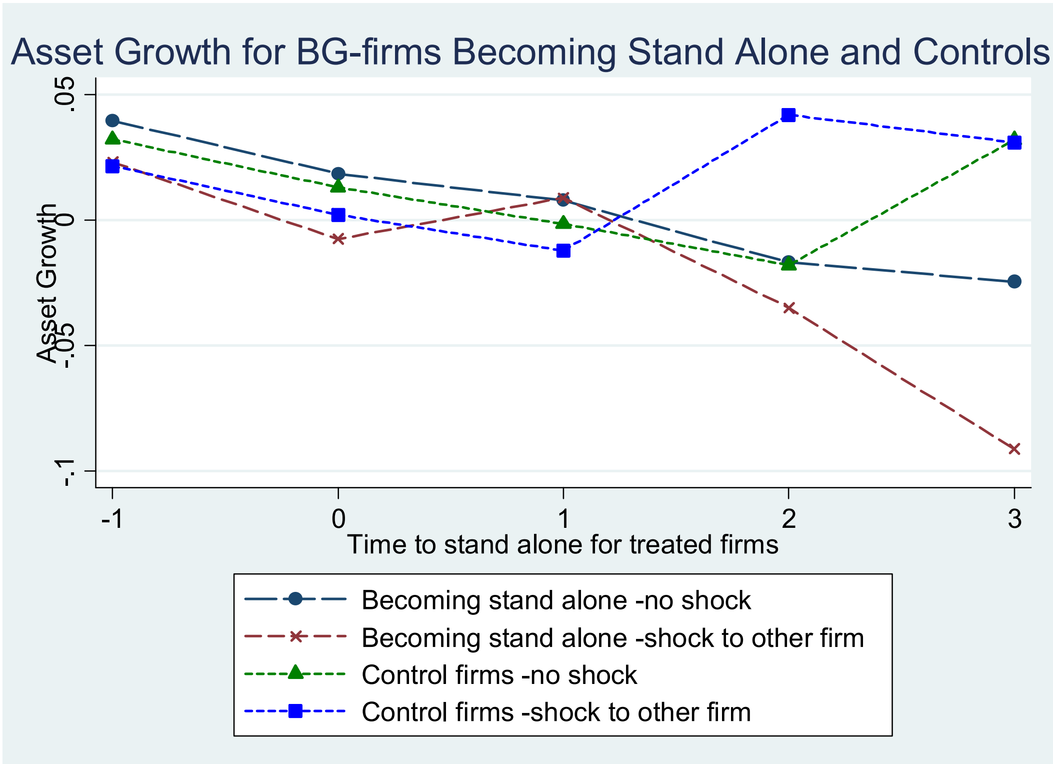

Asset Growth for BG-firms Becoming Stand Alone and Controls
Asset Growth
.05
0
-.05
-.1
-1
0
1
2
3
Time to stand alone for treated firms
Becoming stand alone -no shock
Becoming stand alone -shock to other firm
Control firms -no shock
Control firms -shock to other firm

Table 1

This table summarizes the data collection process of the shocks database (Panel A) and how it maps into our final sample of business group firms (Panel B). Panel A starts with the number of low-return episodes at the country 4-digit-sic code level for which investigated the presence of exogenous shocks and ends with the number of unique industry-year shocks identified. Panel B begins by describing how many of the shocks match firms in our sample according to their industry and year. Given the granularity of our shocks and sic-code availability in our business group database, we aggregate shocks at the 3-digit sic-code level in the merged sample. The panel finally describes the number of observations the shocks in the own and other industry of the focal firm represent, once we define them as 3-year events. We define shocks as 3-year events, starting the year of the shock and up to 2 years later, as shocks can motivate business group firms to split during the year of the shock or later.

Shocks

Panel A - Shocks Data Construction		**Panel B -Shocks in the BG data**	
# of sic country six-month-returns in the lower 5% distribution of returns	5,648	# of unique shocks -Own industry	121
# of negative return spells related to shocks	1,045	# of unique shocks -Other industry	119
# of shocks	359	# of obs. affected by own industry shocks -defined as 3-year events	7,746
# of commodity shocks	322	% of obs. affected by own industry shocks -defined as 3-year events	24.3%
# of regulatory shocks	37	# of obs. affected by other industry shocks -defined as 3-year events	3,914
		% of obs. affected by other industry shocks -defined as 3-year events	12.3%

Table 2

This table displays the distribution of observations in our sample for treated firms (firms that eventually become stand-alone) and control firms (firms that remain as 2-firm business groups). Treated firms are split into two groups according to whether a shock occurred in their companion firm industry –regardless of the timing of the shock. The upper part of the table display the distribution of observations by years; the middle part displays observations by country; and the lower part by 1-digit SIC codes.

		Treated Firms		**Control firms**
		With Shocks in Other Industry	Without Shocks in Other Industry	All
By Year	2009	595	1,319	2,274
	2010	907	2,811	3,764
	2011	1,003	2,767	3,827
	2012	597	3,221	3,822
	2013	405	2,480	2,075
	Total	3,507	12,598	15,762
By Country	AT	307	1,096	1,179
	DE	1,670	6,388	8,738
	DK	66	228	362
	ES	357	718	1,167
	FI	0	28	20
	FR	29	83	64
	GR	4	31	18
	IE	56	100	0
	IT	340	1,451	1,962
	NO	266	1,521	1,464
	PT	10	90	284
	UK	402	864	504
	Total	3,507	12,598	15,762
By 1-digit SIC	0	56	172	183
	1	591	1,794	2,445
	2	132	629	766
	3	236	1,151	1,363
	4	221	724	876
	5	763	3,116	3,675
	6	730	2,219	2,938
	7	509	1,762	2,231
	8	266	1,030	1,281
	9	3	1	4
	Total	3,507	12,598	15,762

Table 3

This table presents summary statistics for our sample. Panel A describes observations for firms that eventually become stand-alone (treated) and Panel B describes observations for firms that always remain as part of a 2-firm business group (control firms). Firm-level characteristics include firms' financial and ownership. Leverage is book value of debt over assets. Assets is measured in millions of Euros. Assets growth is the difference between firms' logarithm of assets and its lag. OROA is EBIT divided by assets. Tangibility is PP&E over Assets. Stake is the controller's ownership stake in the firm. Tobin's Q is computed as the mean market to book ratio of all publicly traded firms' in Europe under the same 3-digit sic-code. Stand-alone takes a value of 1 for observations of firms that operate in a year as stand-alone, and 0 if they are part of a 2-firm business group. Own Shock takes a value of 1 if a firm operates in a year and industry were we identify an industrial shock, and for the next two years, and zero otherwise. Other Shock takes a value of 1 if the companion firm in a firms' business group operates in a year and industry were we identify an exogenous shock, and for the next two years, and zero otherwise. Relative Tangibility is the average tangibility of the companion firm across sample years divided by the average tangibility of the focal firm across sample years. Sales correlation represents the correlation coefficient between a firm's industry and the companion firm's industry sales. We use U.S. sales data to compute this measure. Relative Tobin's Q is the ratio of the companion firm's industry Tobin's Q and the focal firm's industry Tobin's Q. Domestic credit represents the ratio of domestic credit to private institutions divided by a country's GDP, for firms operating in those countries. Industry Financial Dependence and Equity Financial Dependence are Rajan and Zingales (1998) measures of external financial dependence, computed at the 3-digit sic-code using US data. Industy Debt Dependence derives from Rajan and Zingales as well: For each firm in the U.S. we subtract the measure of equity dependence to overall financial dependence, and then aggregate at the industry level.

Panel A: Eventually Stand-alone firms

	Variable	Mean	Median	SD	N
Firm characteristics	Leverage	0.59	0.64	0.30	13,939
	Assets (million)	1.51	0.68	1.86	16,105
	Asset Growth	0.02	0.00	0.24	12,671
	OROA	0.05	0.04	0.14	6,437
	Tangibility	0.28	0.16	0.29	15,211
	Tobin's Q (industry)	2.97	2.71	1.48	16,105
	Stand-alone	0.57	1.00	0.49	16,105
	Stake	95.30	100	14.38	16,105
Shocks	Own Shock	0.23	0.00	0.42	16,105
	Other Shock	0.22	0.00	0.41	16,105
Relative-to-pair characteristics	Relative Tangibility (other/own)	6.65	0.15	82.2	15,537
	Sales Correlation	0.54	0.74	0.46	16,105
	Relative Tobin's Q (other/own)	1.39	1.03	1.85	16,105
	Relative OROA (other/own)	1.10	1.04	0.53	6,654
External financing characteristics	Domestic Credit/GDP	1.29	1.13	0.38	14,318
	Industry Fin. Dep. (R&Z)	0.85	1.00	0.39	16,105
	Industry Equity Dep. (R&Z)	0.31	0.07	0.54	16,105
	Industry Debt Dep. (R&Z)	0.15	0.35	0.86	16,105

Panel B: Always Business Group firms

	Variable	Mean	Median	SD	N
Firm characteristics	Leverage	0.59	0.65	0.30	13,680
	Assets	1.51	0.68	1.87	15,762
	Asset Growth	0.02	0.00	0.23	12,358
	OROA	0.05	0.04	0.15	6,722
	Tangibility	0.27	0.15	0.30	14,782
	Tobin's Q (industry)	2.93	2.67	1.45	15,762
	Stand-alone	0	0	0	15,762
	Stake	98.62	100	6.74	15,762
Shocks	Own Shock	0.26	0.00	0.44	15,762
	Other Shock	0.23	0.00	0.42	15,762
Relative-to-pair characteristics	Relative Tangibility (other/own)	6.83	0.30	53.4	15,033
	Sales Correlation	0.53	0.73	0.46	15,762
	Relative Tobin's Q (other/own)	1.39	1.02	1.89	15,762
	Relative OROA (other/own)	1.14	1.07	0.52	7,155
External financing characteristics	Domestic Credit/GDP	1.26	1.13	0.35	14,298
	Industry Fin. Dep. (R&Z)	0.84	1.00	0.39	15,762
	Industry Equity Dep. (R&Z)	0.30	0.08	0.55	15,762
	Industry Debt Dep. (R&Z)	0.15	0.31	0.86	15,762

Table 4

Columns I-III present results for the first stage regressions of the leverage equation. All columns present the coefficients of Other Shock (the instrument) and Own Shock (control variable). The columns differ in the number of observations and controls included. The specifications in cols. I and II do not have any additional controls besides firm and year fixed effects. The results from col I. differ from those in col II, in that in the later we restrict the sample to that available when including the additional controls used in col. III. Controls used in col III include the firms' industry Tobin's Q, lagged Tangibility and lagged logarithm of assets. Columns IV to VI replicate columns I-III, but replacing the instrument by placebo shocks —random (0,1) shocks following a uniform distribution, with the same mean as Other Shock. Standard errors are adjusted by heteroscedasticity and clusters at the (3-digit) sic-by-country level. Significant at the *10%, **5%, ***1%.

First Stage - Leverage Regressions Sample

Variable	Stand-alone	Stand-alone	Stand-alone	Stand-alone	Stand-alone	Stand-alone
Other Shock	0.0889***	0.1120***	0.1118***			
	(0.0122)	(0.0162)	(0.0162)			
Own Shock	0.0902***	0.1284***	0.1285***	0.0959***	0.1370***	0.1370***
	(0.0213)	(0.0203)	(0.0203)	(0.0217)	(0.0202)	(0.0203)
Placebo Shock -Other Ind.				0.0001	-0.0029	-0.0029
				(0.0049)	(0.0063)	(0.0063)
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Additional Controls	No	No	Yes	No	No	Yes
Sample restr. to controls data	No	Yes	Yes	No	Yes	Yes
Craig-Donald Wald F	83.18	83.46	83.17	0.00	0.23	0.23
R-squared (whithin)	0.3905	0.317	0.317	0.388	0.312	0.312
N	27210	19150	19150	27210	19150	19150

Table 5

Panel A presents the second stage (IV) estimates of stand-alone, instrumented with Other Shock. Cols I-III show results using leverage as dependent variable, while columns IV-VI display results using asset growth as dependent variable. All specifications include as control the variable Own Shock. Specifications differ in the additional controls included and whether the data is restricted to that available for the additional controls — even if they are not included. Panel B present the OLS estimates for the same dependent variables, treating stand-alone as an exogenous variable. Standard errors are adjusted by heteroscedasticity and clusters at the (3-digit) sic-by-country level. Significant at the *10%, **5%, ***1%.

Panel A- Second Stage IV Estimations

Variable	Leverage	Leverage	Leverage	Asset Growth	Asset Growth	Asset Growth
Stand-alone	-0.1034**	-0.0866**	-0.0884**	-0.2239**	-0.2136***	-0.1562**
	(0.0490)	(0.0418)	(0.0420)	(0.0957)	(0.0811)	(0.0729)
Own Shock	Yes	Yes	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Additional Controls	No	No	Yes	No	No	Yes
Sample restr. to controls data	No	Yes	Yes	No	Yes	Yes
N	27210	19150	19150	24904	22385	22385

Panel B- OLS Estimations

Variable	Leverage	Leverage	Leverage	Asset Growth	Asset Growth	Asset Growth
Stand-alone	0.0029	0.0023	0.0022	-0.0017	0.0006	0.0024
	(0.0023)	(0.0030)	(0.0030)	(0.0060)	(0.0069)	(0.0054)
Own Shock	Yes	Yes	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Additional Controls	No	No	Yes	No	No	Yes
Sample restr. to controls data	No	Yes	Yes	No	Yes	Yes
N	27432	19485	19485	24904	22441	22441

Table 6

Panels A and B present second stage (IV) estimates of stand-alone, instrumented with Other Shock for different sample splits. In Panel A we split the sample using above and below the median Relative Tangibility. In Panel B we split the sample using above and below the median Sales Correlation. In the bottom of each estimation we present the coefficient of the instrument, Other Shock, in the first stage regression. Standard errors are adjusted by heteroscedasticity and clusters at the (3-digit) sic-by-country level. Significant at the *10%, **5%, ***1%.

Panel A -Sample Split using Relative Tangibility (other/own)

	Low Tangibility Other Firm		High Tangibility Other Firm	
Variable	Leverage	Asset Growth	Leverage	Asset Growth
Stand-alone	0.0036	-0.0461	-0.2627**	-0.4045**
	(0.0496)	(0.0774)	(0.1077)	(0.2027)
Own Shock	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed effects	Yes	Yes	Yes	Yes
Additional Controls	Yes	Yes	Yes	Yes
N	9865	11301	9283	11082
First-Stage				
Other Shock	0.1418***	0.1385***	0.0715***	0.0627***
	(0.0250)	(0.0222)	(0.0190)	(0.0174)

Panel B -Sample Split using Sales Correlation

	Low Sales Correlation		High Sales Correlation	
Variable	Leverage	Asset Growth	Leverage	Asset Growth
Stand-alone	-0.0470	-0.1458*	-0.1570*	-0.2454
	(0.0422)	(0.0763)	(0.0939)	(0.1782)
Own Shock	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed effects	Yes	Yes	Yes	Yes
Additional Controls	Yes	Yes	Yes	Yes
N	9764	11407	9386	10978
First-Stage				
Other Shock	0.1278***	0.1276***	0.1024***	0.0882***
	(0.0240)	(0.0220)	(0.0219)	(0.0197)

Table 7

Panels A-D present second stage (IV) estimates of stand-alone, instrumented with Other Shock for different sample splits. In Panel A we split the sample using above and below the median Industry Debt Dependence. In Panel B we split the sample using above and below the median Industry Equity Dependence. In Panel C we split the sample using above and below the median Industry External Financial Dependence. All the external dependence measures are derived from Rajan and Zingales (1998) and are computed using industrial U.S. data. In Panel D we split the sample using above and below the median Domestic Credit to GDP from the country where the firm operates. In the bottom of each estimation we present the coefficient of the instrument, Other Shock, in the first stage regression. Standard errors are adjusted by heteroscedasticity and clusters at the (3-digit) sic-by-country level. Significant at the *10%, **5%, ***1%.

Panel A -Sample Split using Industry Debt Dep. (R&Z)

	Low Debt Dependence		High Debt Dependence	
Variable	Leverage	Asset Growth	Leverage	Asset Growth
Stand-alone	-0.0460	-0.1128	-0.1261**	-0.1882**
	(0.0556)	(0.1107)	(0.0639)	(0.0957)
Own Shock	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed effects	Yes	Yes	Yes	Yes
Additional Controls	Yes	Yes	Yes	Yes
N	9468	10889	9682	11496
First-Stage				
Other Shock	0.1015***	0.0912***	0.1223***	0.1174***
	(0.0222)	(0.0188)	(0.0228)	(0.0223)

Panel B -Sample Split using Industry Equity Dep. (R&Z)

	Low Equity Dependence		High Equity Dependence	
Variable	Leverage	Asset Growth	Leverage	Asset Growth
Stand-alone	-0.1222*	-0.1971*	-0.0571	-0.1121
	(0.0666)	(0.1122)	(0.0541)	(0.0950)
Own Shock	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed effects	Yes	Yes	Yes	Yes
Additional Controls	Yes	Yes	Yes	Yes
N	9778	11515	9372	10870
First-Stage				
Other Shock	0.1102***	0.1046***	0.1126***	0.1039***
	(0.0227)	(0.0218)	(0.0228)	(0.0199)

Panel C -Sample Split using Industry Fin. Dep. (R&Z)

	Low Financial Dependence		High Financial Dependence	
Variable	Leverage	Asset Growth	Leverage	Asset Growth
Stand-alone	-0.0505	-0.2102*	-0.1266*	-0.1155
	(0.0528)	(0.1088)	(0.0699)	(0.1018)
Own Shock	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed effects	Yes	Yes	Yes	Yes
Additional Controls	Yes	Yes	Yes	Yes
N	9192	10786	9958	11599
First-Stage				
Other Shock	0.1107***	0.0956***	0.1105***	0.1105***
	(0.0198)	(0.0191)	(0.0251)	(0.0224)

Panel D -Sample Split using Domestic Credit/GDP

	Low Domestic Credit/GDP		High Domestic Credit/GDP	
Variable	Leverage	Asset Growth	Leverage	Asset Growth
Stand-alone	-0.1354**	-0.2230**	-0.0436	-0.0752
	(0.0594)	(0.0953)	(0.0685)	(0.1250)
Own Shock	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed effects	Yes	Yes	Yes	Yes
Additional Controls	Yes	Yes	Yes	Yes
N	10115	11778	9035	10607
First-Stage				
Other Shock	0.0975***	0.0982***	0.1203***	0.0993***
	(0.0199)	(0.0171)	(0.0244)	(0.0245)

Table 8

Panels A and B present second stage (IV) estimates of stand-alone, instrumented with Other Shock for different sample splits, according to firms relative-to-peers initial characteristics. In Panel A we split the sample using above and below the median relative-to-peers OROA. In Panel B we split the sample using above and below the median relative-to-peers Leverage. To construct firms' relative to peers measures we keep firms' initial observation in the sample and run OROA and Leverage regressions against two-digit sic code dummies. From the regressions we obtain the standardized residuals and we use these to order firms in terms of their relative-to-peers Leverage and OROA. In the bottom of each estimation we present the coefficient of the instrument, Other Shock, in the first stage regression. Standard errors are adjusted by heteroscedasticity and clusters at the (3-digit) sic-by-country level. Significant at the *10%, **5%, ***1%.

Panel A

Sample Split using Relative to Industry Peers Initial Performance (OROA)

	Low Initial OROA		High Initial OROA	
Variable	Leverage	Asset Growth	Leverage	Asset Growth
Stand-alone	-0.1873**	-0.1079	0.0972*	0.0895
	(0.0947)	(0.1210)	(0.0528)	(0.1098)
Own Shock	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed effects	Yes	Yes	Yes	Yes
Additional Controls	Yes	Yes	Yes	Yes
N	3911	4708	4385	4719
First-Stage				
Other Shock	0.1436***	0.1417***	0.1884***	0.1802***
	(0.0372)	(0.0318)	(0.0311)	(0.0303)

Panel B

Sample Split using Relative to Industry Peers Initial Leverage

	Low Initial Leverage		High Initial Leverage	
Variable	Leverage	Asset Growth	Leverage	Asset Growth
Stand-alone	0.0098	-0.1169	-0.1996**	-0.1079
	(0.0551)	(0.0773)	(0.0822)	(0.1305)
Own Shock	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed effects	Yes	Yes	Yes	Yes
Additional Controls	Yes	Yes	Yes	Yes
N	9192	9345	9134	9989
First-Stage				
Other Shock	0.1217***	0.1229***	0.0978***	0.0996***
	(0.0208)	(0.0205)	(0.0229)	(0.0206)

Table 9

Panels A and B present second stage (IV) estimates of stand-alone, instrumented with Other Shock for different sample splits. In Panel A we split the sample using above and below the median Relative Tobins' Q. In Panel B we split the sample using above and below the median Relative OROA. In the bottom of each estimation we present the coefficient of the instrument, Other Shock, in the first stage regression. Standard errors are adjusted by heteroscedasticity and clusters at the (3-digit) sic-by-country level. Significant at the *10%, **5%, ***1%.

Panel A

Sample Split using Relative Tobin's Q (other/own)

	Low Relative Tobin's Q		High Relative Tobin's Q	
Variable	Leverage	Asset Growth	Leverage	Asset Growth
Stand-alone	-0.0231	-0.1014	-0.1105**	-0.1050
	(0.0634)	(0.1128)	(0.0521)	(0.0773)
Own Shock	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed effects	Yes	Yes	Yes	Yes
Additional Controls	Yes	Yes	Yes	Yes
N	9032	10457	9127	10805
First-Stage				
Other Shock	0.1007***	0.0929***	0.1488***	0.1398***
	(0.0206)	(0.0193)	(0.0233)	(0.0213)

Panel B

Sample Split using Relative OROA (other/own)

	Low Relative Other OROA		High Relative Other OROA	
Variable	Leverage	Asset Growth	Leverage	Asset Growth
Stand-alone	0.0054	0.1213	-0.0474	-0.2484
	(0.0593)	(0.1091)	(0.0938)	(0.1576)
Own Shock	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed effects	Yes	Yes	Yes	Yes
Additional Controls	Yes	Yes	Yes	Yes
N	4518	4937	4246	5071
First-Stage				
Other Shock	0.1729***	0.1643***	0.1231***	0.1155***
	(0.0312)	(0.0294)	(0.0385)	(0.0332)

Table 10

This table examines firms' OROA. Col. I presents the second stage (IV) estimates of stand-alone, instrumented with Other Shock. Col. II presents the OLS estimate. In the bottom of col. I we present the coefficient of the instrument, Other Shock, in the first stage regression. Standard errors are adjusted by heteroscedasticity and clusters at the (3-digit) sic-by-country level. Significant at the *10%, **5%, ***1%.

	Performance	
	Second Stage IV	OLS
Variable	OROA	OROA
Stand-alone	-0.0153	0.0065
	(0.0471)	(0.0046)
Own Shock	Yes	Yes
Firm Fixed Effects	Yes	Yes
Year Fixed effects	Yes	Yes
Additional Controls	Yes	Yes
N	9472	9595
First-Stage		
Other Shock	0.1519***	
	(0.0246)	