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

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Abstract

We propose a novel identification strategy to estimate the effects of business group affiliation. We study two-firm business groups, some of which split up during our sample period and consequently leave firms as standalone. We instrument for standalone status using shocks to the industry of the *other* firm in the group. We find that firms that become standalone reduce leverage and investment. Consistent with groups lifting credit constraints, the effects are more pronounced in debt-dependent industries and when the *other* firm had high tangibility. We also find evidence of capital misallocation in groups, but an insignificant overall effect on performance.

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Business groups –sets of firms under a common controlling shareholder—are prevalent around the world. Groups control more than 20% of the stock market capitalization in Western Europe, and their assets represent up to 70% of GDP in many Asian countries (Morck, Wolfenzon, and Yeung, 2005). The literature has identified bright and dark sides to business groups (Khanna and Yafeh, 2007). The bright side lies in the 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 (Almeida and Wolfenzon, 2006a). The dark side refers to inefficiencies that go from the misallocation of capital across firms, where some group firms receive more funds than what their investment opportunities deserve (Stein, 2003), to political influence and corruption. 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 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 a group is not random according to Almeida and Wolfenzon (2006a). 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). However, exogenous variation is crucial to get good estimates of the effects of business group affiliation.

We propose a novel identification strategy to estimate the effects 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 a second crucial ingredient. In a nutshell, we focus on firms that are "left alone" rather than 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 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 in unrelated industries. In groups with more than two firms, the link between shocks to other firms and becoming standalone is more tenuous. 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.) and regulatory shocks (e.g., tobacco industry).

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 capital structure and investment are indeed different in groups than in stand-alone firms. Unlike in a Modigliani-Miller type of setup where ownership does not matter, we find that ownership has real effects: business group affiliation matters, and the identity of the other firms with the same controlling shareholder also matters.

One explanation for why business group matter is that business groups lift credit constraints. Through cross-pledging a business group that puts both firms under a common roof can increase financing with respect to the case where both firms remain standalone (Tirole, 2006). 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 contributed with more tangible assets to the group. 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. Finally, we find that the transition towards standalone is associated with substantial reshuffling of firm-bank relationships. The destruction of banking relationships is detrimental for the amount of credit that small firms, like the firms in our sample, can get (Petersen and Rajan, 1994).

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 business groups use capital inefficiently. For example, groups can give some firms a preferential treatment by which they get more capital than what their investment opportunities call for (e.g., pet projects). Consistent with this misallocation of capital 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 when they were part of a group. In particular, we find that firms that had initial profitability below their industry peers and initial leverage above their industry peers see their leverage ratios fall by more. Also, firms that had lower industry 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 capital misallocation, it is perhaps not surprising that we do not find an overall effect of becoming standalone on profitability.

Our paper is related to the literature that estimates the costs and benefits of business group affiliation. Business groups can benefit firms by lifting financial constraints (e.g., Gopalan, Nanda and Seru, 2007), but groups can also harm firms by engaging in unproductive activities or in the outright expropriation of minority shareholders (i.e., "tunneling" as in Johnson, La Porta, López-de-Silanes, and Shleifer, 2000, or Bertrand, Mehta, and Mullainathan, 2002).[1] Given the endogeneity of group structures it has been hard to provide a clear answer regarding the overall effect of business group affiliation on a firm's financing and investment decisions. Our main contribution is that we are, to the best of our knowledge, the first to estimate the causal effect of business group affiliation.[2] We also explore the variation of this causal effect along firm and industry characteristics, which allows us to test the particular mechanisms through which business group affiliation matters.

Most of this literature deals with large groups. The groups that we study, instead, are small groups, probably quite different from the complex structures seen in Korea, Sweden, or other countries. Small groups help us to understand the behavior of more complex groups since the underlying tradeoffs are analogous in both cases. In this sense small groups can be considered as textbook examples of business groups. More importantly, our results represent arguably cleaner estimates of the causal effect of business group affiliation, which is much harder to get with large groups. 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). For example, the split of large groups can have implications for market power and goods prices in particular industries.

Focusing on small groups helps us with the identification of causal effects, but this comes at a price. The price is that some of the dark elements of large groups are not present in small groups. In small groups we cannot study tunneling, which is a concern in large groups. The stakes of controlling shareholders are very high in our sample (above 95%), and

[1] Regarding the financial advantage of business groups see also Buchuk, Larrain, Muñoz, and Urzua (2014) on intra-group loans; 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. Evidence in favor of tunneling in business groups is provided by Jiang, Lee, and Yue (2010) who 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).

[2] In contemporaneous and independent work, Pérez-González (2015) tries to estimate a similar causal effect for holding companies, with an identification strategy based on the deregulation of U.S. electric utilities in the 1930s.

therefore there is little incentive to expropriate minority shareholders. Also, there is no scope for "socialism", as in Scharfstein and Stein (2000), where the source of capital misallocation is a double-layer agency problem with CEOs and divisional managers. There is still room for misallocation in our setup, but the root of the problem is different, namely the preferences of the controlling shareholder (e.g., pet projects). Also, the effects of agglomeration in lifting financial constraints are present both in small and large groups. Overall, it is important to keep in mind these caveats when extrapolating our results to the costs and benefits of large groups.

Our paper also contributes to the literature on internal capital markets, which studies the allocation of capital within firms, mostly (although not exclusively) in U.S. conglomerates. There are similar theoretical underpinnings for the bright and dark sides of conglomerates. On the one hand, conglomerates may be better at picking winners (Giroud and Mueller, 2015; Khanna and Tice, 2001; Shin and Stulz, 1998; Stein, 1997). 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 divisional managers and power struggles can distort the allocation of capital between divisions (Matvos and Seru, 2014; Ozbas and Scharfstein, 2010; Rajan, Servaes, and Zingales, 2000; Scharfstein and Stein, 2000). Although similar tradeoffs 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 mechanisms 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 business groups, are ubiquitous in the corporate world. For instance, the literature studies customer-supplier networks (Acemoglu, Akcigit, and Kerr, 2015; Acemoglu, Carvalho, Ozdaglar, and Tahbaz-Salehi, 2012; Barrot and Sauvagnat, 2015), banking networks (Khwaja and Mian, 2008; Peek and Rosengren, 2000; Schnabl, 2012), 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 only share owners with the affected firms. Another theme in this literature is to estimate the value of belonging to a network (Karlan, Mobius, Rosenblat, and Szeidl, 2009). We study the most simple of networks with just two agents (firms in our application), so our results can be interpreted as an estimate of the value of a single link in a network.

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 shows additional tests and deals with alternative hypotheses for our results. Section 5 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 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 (who does not own more firms). In either case, the shock 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:

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

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 managerial skills 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. 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 shock is exogenous to the business group itself, but we do not argue that the shock is exogenous to market forces in general. (For example, the housing bust in this period is likely to be endogenous to the financial crisis). Second, we take business groups with two firms in unrelated industries as measured by coefficients of the 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 with 4 firms in 4 unrelated industries. After one of them is hit by a shock, we are left with 3 unaffected firms to potentially use as part of our identification strategy. However, the only chance we would have to actually use one of those 3 firms if that one of them is sold and leaves the group. Unfortunately, it gets harder to sell the idea that this firm is "left alone" or forced into standalone status from outside since the group had the choice to sell among 3 firms.

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. As a product of our methodology we identify relatively large and long-lasting shocks instead of small or transitory liquidity shocks.

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. 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 caused the negative returns. We only use those cases where we can pin down the source of the shock in a precise way. 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 and early 2012. The returns of almost -20% go together with a 42% price decline in the price of lamb in 12 months. Although the returns that we show represent listed firms, the nature of the shocks suggests that they also affected private firms in these industries. The appendix describes more examples of the shocks in our sample.

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 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 for small firms prior to 2009 is not good enough in many of the countries in our sample. After 2009, and 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 had 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.[3]

[3] Bankruptcy only happens rarely in our sample (less than 5% of cases).

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 composed by firms in unrelated industries. Overall, a little less than 1% of 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 the two-digit SIC code classification and size (book assets) in their initial year in the sample.[4] 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,

[4] Matching on additional criteria (on top of industry and assets) reduces our sample size too much.

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.[5] 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 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 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. 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.

[5] 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.

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 such 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 shows 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 10,000 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. Asset growth is our main proxy for investment since CAPEX is typically unreported in our sample (more than 70% of the observations are zero). Following Leary and Roberts (2005), we define debt (or equity) issue or retirement as dummy variables for firm-year observations where the change in debt (or equity) is at least 5% of lagged book assets. 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 (at the industry level) 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. 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.

Table 3 also shows 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.

Finally, we report summary statistics for the creation and destruction of banking relationships of the firms in our sample. *Amadeus* reports the names of the banks with which each firm does business each year. From this we know whether the firm starts a new relationship with a bank (creation) or ends a relationship with a bank (destruction). The average and median number of banking relationships is just one, which can be expected in a sample of small firms. The average of creation and destruction is 0.1 per year. Unfortunately, we do not know the amount of credit or the interest rate on loans.

2. Main Results

2.1. Effects on capital structure and growth

Table 4 shows the results from our first stage (for the subsequent second stage 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 following two years, and zero otherwise. We include an indicator variable for shocks in the industry of the *other* firm in the group (the instrument) and an 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 on 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 4 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.[6] 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 goes between -0.1562 and -0.2239, 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 3).

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

[6] Our results are also robust to including country-year fixed effects as seen in Table A.1 in the appendix.

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 implies 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 face shocks to the other firm and treated firms that do not. Since not all firms have data for the entire window and not all firms transit to standalone in the same year, 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 do not face shocks to the other firm, firms that become standalone and face shocks to the other firm, and control firms with and without shocks to the other firm. Finally, we add back the average initial 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 fewer 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 treated firms affected by a shock to the other firm in their group are those that experience a stronger reduction in leverage and growth.

Second, we explore 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 tested. However, we can show suggestive evidence in support of the idea that $OtherShock_{it}$ does not have a direct effect on firms' outcomes for our sample of small business groups.

Figure 5 shows 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 leverage is very similar when there is a shock to the other firm in the group and 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 even have higher growth, instead of lower growth. Albeit transitory, this effect, if anything, should bias the results against our main findings.

Our conclusion 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, at least 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 smoothing shocks through intra-group lending and equity flows, are not (see more on this in Section 4.3). 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 more easily smoothed.

2.2. Decomposing the effect on leverage

In Table 6 we study the effects on debt and equity separately in order to better understand the reduction in leverage when firms become standalone. We run regressions with (log) levels and with dummy indicators for the frequency of large issuance or retirement of debt and equity, as previously described in Table 3. The regressions in columns 1 and 4 of Table 6 show that debt falls strongly in terms of levels, while equity does not react significantly to standalone status. In columns 2 and 3 we find a significant reduction in the frequency of debt issuance (coef. -0.3284, t-stat 2.05), although no incidence on debt retirement. There are no significant effects on equity issuance nor retirement. These results suggest that the reduction in leverage is a consequence of the inability of firms to continue borrowing as they become standalone.

Table 6 helps us to reduce the scope of potential explanations. For instance, it helps to rule out a mechanical decrease of leverage that could happen if controlling shareholders use the proceeds of the sale of the other firm to increase equity in this firm. Also, a mechanical decrease in leverage of this type does not predict a fall in asset growth as we find in the data. In fact, why grow less in the firm where the controlling shareholder has just increased her investment?[7]

This evidence suggests that the impact of standalone status is coming from credit markets instead of equity markets. Group affiliation is arguably of great importance when small firms face financial intermediaries, in particular banks, and ask them for credit. In the next section we explore in more detail scenarios where credit constraints are more likely to be relevant.

[7] See also the results regarding cash holdings in Table 13, which speak against a mechanical fall in leverage in the case where the alleged increase in equity is saved as cash.

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 mechanism we can be more confident about the interpretation of the main result as due to credit constraints. Second, we explore firm characteristics that are typically related to inefficient investment, in order to get a sense of the capital misallocation in business groups. The overall effect on firm performance depends on the interplay of lifting credit constraints and the potential misallocation. We borrow from the literature on internal capital markets (see Stein, 2003, for a survey), which although focused on conglomerates, can guide the discussion for capital allocation in groups as well.

3.1. Credit Constraints

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. Business groups can have this financing advantage due to cross-pledging between the different firms in the group. 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 (Ghatak and Kali, 2001).

In the tables that follow we split our data into high and low according to the median of each characteristic. In Table 7 Panel A we split firms according to the potential for cross-pledging of assets using 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 being part of a group with a high-tangibility partner. 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 subsamples 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., it is not the case that only firms with high-tangibility partners are affected by our instrument.

In Panel B of Table 7 we split the sample according to the sales correlation of the industries of both firms in the group, which proxies for cash-flow cross-pledging. 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 (sometimes called co-insurance or risk-sharing; see Hann, Ogneva, and Ozbas, 2013; Khanna and Yafeh, 2005).

Cross-pledging relaxes credit constraints, which should be particularly important in industries that naturally rely more on debt. In fact, we find in Table 8 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). We also find that it is not the high equity dependence industries that suffer the most, but the low equity dependence industries. This last result serves as a sort of placebo test to attach our results to credit constraints and not to some generic external dependence (results split by both external dependence and equity dependence are reported in the appendix, Tables A.2 and A.3).

Cross-pledging should also be particularly important in less developed credit markets, where collateral is crucial to obtain debt financing. In Panel B of Table 8 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).

In Table 9 we study whether the extensive margin of banking relationships is affected by becoming standalone. In OLS regressions we find that becoming standalone does not affect the destruction nor the creation of banking relationships. However, we find a strong positive effect on both creation and destruction in the IV regressions. Long-lasting banking relationships relax credit constraints since soft information, in particular about small firms like those in our sample, is hard to convey to new lenders (Petersen and Rajan, 1994). Hence, our evidence of a strong reshuffling of banking relationships is suggestive of credit constraints becoming more binding for firms that become standalone. Unfortunately, we do not know the amount of credit nor the interest rate of loans so we cannot study the intensive margin of these relationships.

3.2. Misallocation in Business Groups

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 invest in poor projects that credit markets would otherwise not fund. Firms in groups can be subsidized when compared to market allocations. The prediction for our setup would be that inefficient firms that are subsidized in groups should be hurt the most as they become standalone.

In Table 10 we explore several candidates for being inefficiently subsidized if there is capital misallocation. 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 (coefficient -0.1873, t-stat 1.98). In fact, firms with high performance relative to their industry see their leverage ratios increase after becoming standalone (coefficient 0.0972, t-stat 1.84). This suggests that being partnered with another firm reduces the amount of credit that high-performing firms can use, and once free from that burden they can increase leverage. This also suggests that not all firms benefit from belonging to a group, and that some of them are better off as standalone. 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 those firms received more credit from financial institutions precisely because they were part of a group. We do not find significant results for asset growth in these subsamples.

In Table 11 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 within 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 subsidization is stronger when the divergence in Tobin's q is greater. Our results point in the same direction, 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 (-0.1105 vs. -0.0231). Firms with low q relative to their partners are stronger candidates for receiving subsidized funding when belonging to a group, hence it is natural that they suffer the most when the group splits up. We do not find a similarly strong effect in terms of asset growth. When splitting the sample according to the relative OROA of both firms in the group (Panel B) we find that the results are stronger in firms that had a high OROA partner, which is again consistent with misallocation in groups. However, the results in this sample are not statistically significant, partly due to the smaller sample size.

4. Additional Results

4.1. Average Effect on Performance

The prior theoretical literature does not give a strong prior regarding the average effect on performance of becoming standalone. This is because of the interplay of the bright and dark sides of business groups. 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 12 Panel A 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. We find similar results for sales over assets as an alternative measure of performance (Panel B).

4.2. Alternative Hypotheses

We explore four alternative hypotheses to credit constraints. In these hypotheses groups matter, but not for their ability to raise more funds from financial markets. First, taxes can be a powerful incentive to form business groups. In particular, the losses of the firm can offset the gains of the other firm and help reduce the tax burden of the controlling shareholder. This could explain the fall in asset growth after firms become standalone because the incentives for growth are dampened. Taxes should be more relevant for pairs of firms with low cash flow correlations, because they take particular advantage of the tax shield provided by each other's losses. However, we do not find strong differences when we sort firms according to cash-flow correlations in Table 7. The point estimate of the effect of standalone on asset growth is even stronger in the high correlation sample, in contrast to what this hypothesis suggests.

A second alternative hypothesis concerns the risk tolerance of controlling shareholders. After negative shocks, when risk becomes more salient, controlling shareholders can decide to reduce their risk exposure by cutting leverage and asset growth. We approach this idea in two ways in Table 13. First, we run regressions with cash holdings as dependent variable. The IV coefficient for standalone is negative (-0.0643) and significant at the 10% level. Therefore, if anything, there is evidence of reduced cash holdings in contrast to cash hoarding as predicted by the risk hypothesis. A second approach is to split our regressions for leverage and asset growth in different samples according to the level of industrial volatility. If the fall in leverage and asset growth is explained by a reduction in risk tolerance, then our results should be stronger among firms in more volatile industries. Instead, in Table 13 Panel B we find that the results are concentrated in less volatile industries. Overall, our results do not seem to be explained by a simple risk channel.

A third hypothesis is that the identity of the controlling shareholder matters, rather than business group affiliation itself. Table 14 explores this idea by splitting treated firms into those that stay under the same controlling shareholder ("stayers") and those that change controlling shareholder ("leavers"). The latter case occurs when the shock to the other firm leads to a sale of the unaffected firm. Following Figure 1, this means that firm 2, rather than firm 1, is sold (to a different investor who does not own another firm so it does not become a group). Control firms for each type of treated firm come from the initial one-to-one matching. In Table 14 we find very similar results for leverage and asset growth in both subsamples, which suggests that it is not the identity of the controlling shareholder but the fact that the firm is now standalone what matters.[8]

A final alternative hypothesis is the presence of a lemon's problem in the firms that become standalone. Under this hypothesis, owners with superior information would only let go firms that they know have poor future prospects, hence the low subsequent growth that we find. However, the comparison between stayers and leavers in Table 14 suggests that the decision of owners to sell or to keep (and therefore to sell the other firm) is not essential for our results. Thus, the lemon's problem does not seem to play an important role in our findings probably because of the extensive due diligence process by buyers. Furthermore, as seen in the first stage for each subsample separately, our instrument for standalone is strong regardless of the status of stayer or leaver (this is also the case in all of the subsample analyses we perform). Thus, selection concerns in general are unlikely to explain our results. The main takeaway from Table 14 is that, even if there are forces that affect the price or the identity of the firm being sold, they do not seem to drive the reduction in leverage and asset growth we observe for stand-alone firms.

[8] The results for OROA are also similar across subsamples, and still not statistically significant.

4.3. About the Exclusion Restriction

Under the exclusion restriction, $OtherShock_{it}$ does not have an effect on firm outcomes other than through changes in business group affiliation. In other words, $OtherShock_{it}$ is not included in equation (1). Although we cannot test this directly, in this section we explore ancillary predictions of mechanisms that would violate the exclusion restriction.

Our sample, by construction, rules out linkages in production between the two firms in the group. However, there can be demand spillovers that make the shocks to the other industry matter for the firm under study. For example, think of a group with one firm in construction and another firm in the food industry. Although production in the two firms is not integrated, a negative shock to construction (e.g., a housing bust) can also affect the food industry through the wealth effect of housing on consumption. Therefore, the shock to construction could enter the second stage regression of the food firm regardless of the standalone status of that firm. Demand spillovers are hard to measure because they involve cross-industry effects. However, we argue that they should be stronger in industries with high income elasticity. Basically, goods with high income elasticity are more affected by wealth effects. For example, if the second firm in the group produces a durable good (e.g., cars) rather than food, then it should be more affected by the construction shock.

In order to test this idea we split the sample according to an estimate of the income demand elasticity of each industry.[9] If our results are explained by demand spillovers, then they should be concentrated in industries with high elasticity such as cars. As seen in Table 15, the effect of becoming standalone is not stronger among firms in high elasticity industries, which speaks against the idea of demand spillovers. This can also be expected because the

[9] High elasticity industries include construction (SIC 15-17), manufacturing of durable goods (SIC 24-25, 32-38), wholesale trade of durable goods (SIC 50), retail trade of durable goods (SIC 52, 55, and 57), and Real Estate (SIC 65).

housing crisis represents only a small subset of our shocks, and housing is arguably the largest responsible for wealth effects.[10]

Another threat to the exclusion restriction would be the presence of active cash-flow risk sharing between group firms. In this case, current earnings are transferred from one firm to another to smooth transitory shocks. Consistent with this type of risk sharing between firms, Lamont (1997) shows that non-oil segments of big oil conglomerates reduce investment as a response to negative shocks to the price of oil. In terms of our regression setup, shocks to the oil industry (the "other" industry in Lamont's example) would enter the second stage regression of non-oil firms even if they remain affiliated to the conglomerate or group.

We argued before in Figure 5 that this type of risk-sharing does not seem to be present among control firms, which lends support for the exclusion restriction. The lack of clear variation in our results with respect to cash-flow correlations in Table 7 also hints in this direction, because cash-flow risk-sharing should be particularly relevant among firms with low correlations. The final piece of suggestive evidence is shown in Table 16 where we present regressions with equity stakes as the dependent variable. The idea here is that, if risk-sharing is active, some of the smoothing of shocks to the other industry will be translated into smaller equity stakes as the controlling shareholder sells parts of his stake to raise funds and contribute to the other firm. In Table 16 we do not find any correlation of shocks to the other industry and equity stakes, which again lends support to the exclusion restriction.

There can be at least two explanations for the absence of risk-sharing of these shocks. First, the shocks that we study are large, relatively long-lasting shocks. Therefore, it is not obvious that firms would find it optimal to smooth these shocks, nor that they would have the capacity to do so only with the help of the other (small) firm in the group. The response to transitory liquidity shocks, which are smaller and have an impact at much higher frequencies, could be different. Second, and as argued by Whited (2001), the effect of other-firm cash flow that is documented in other studies is often contaminated with measurement

[10] 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.

error and by the relatedness of firms. At least in our sample we rule out, by construction, the presence of firms in related industries.

Overall, the ancillary tests that we show speak in favor of the exclusion restriction. The exclusion restriction is not patently violated by demand spillovers or active cash-flow risk-sharing. Our interpretation of the results is that business group affiliation matters as it allow firms to obtain more credit from financial institutions.

5. Conclusions

To the best of our knowledge, we are the first to estimate the causal effect of business group affiliation. We use a sample of groups composed of only two firms in the period 2009-2013. Some of these groups split up during this time, leaving firms as stand-alone. 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 cleaner 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 an instrumental variables setting.

We find that firms that become 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 misallocation in business groups, firms more affected by becoming standalone are firms with previous poor performance and high leverage relative to their industry peers, and firms with low industry 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 business groups, so it is not surprising that the on-average effect on performance is close to zero.

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Figure 1

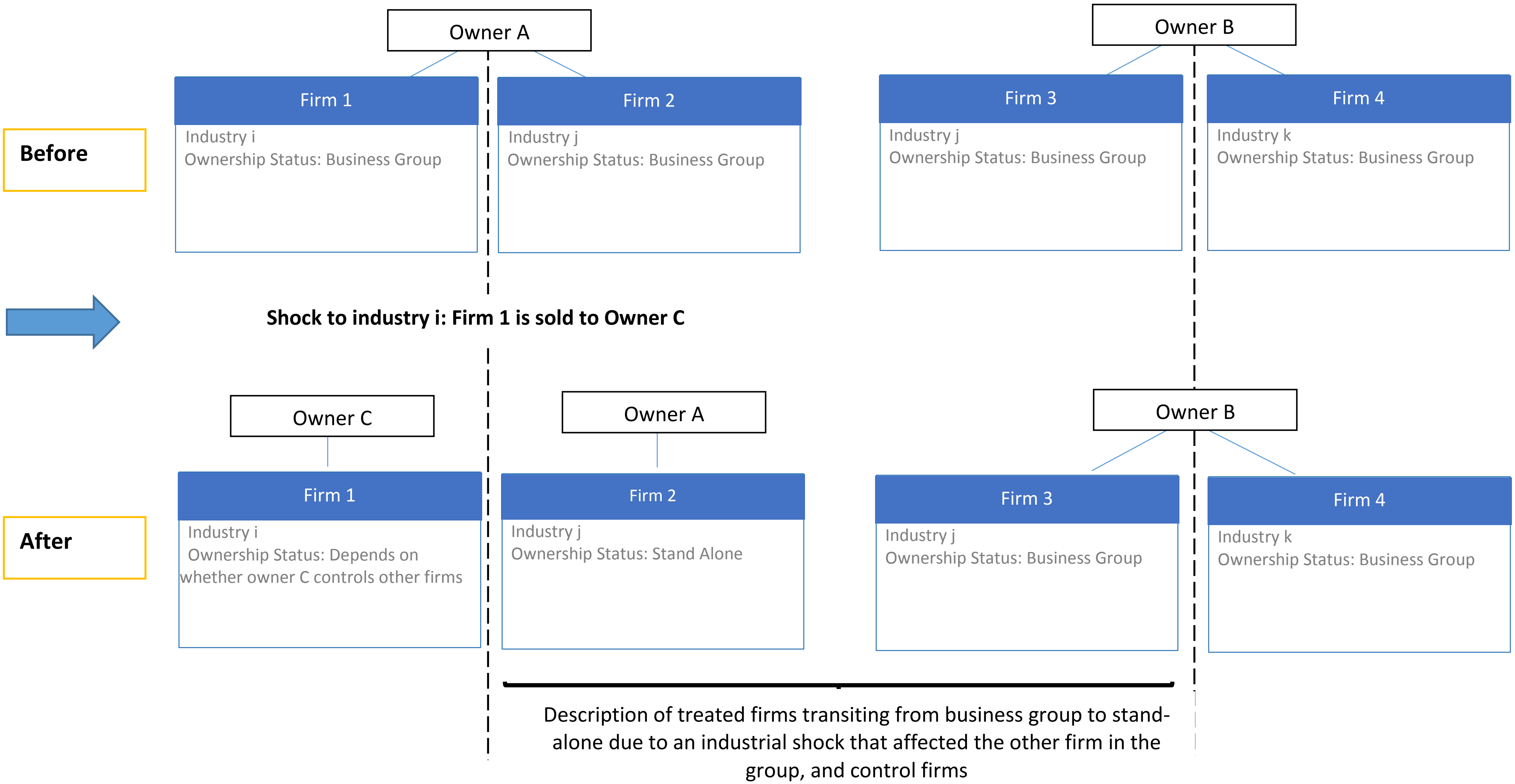
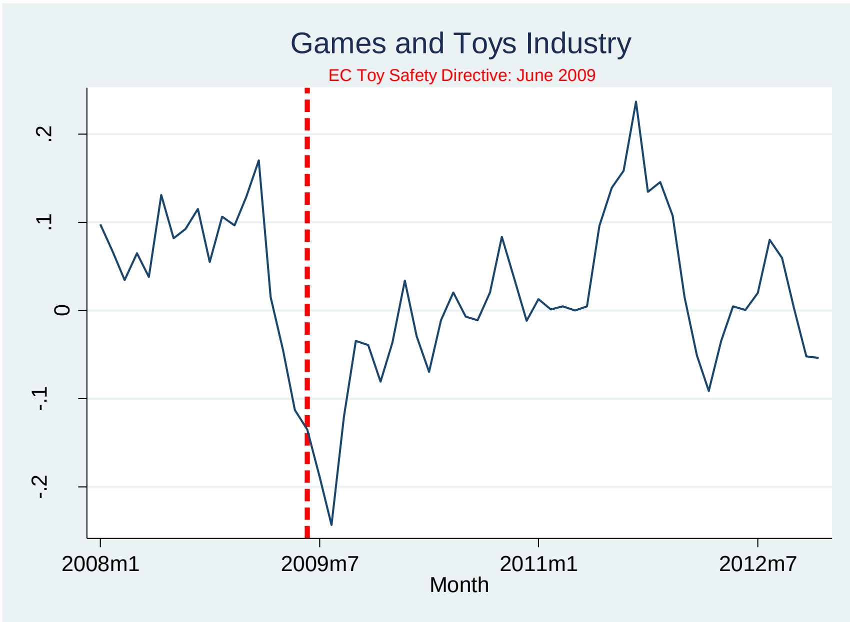

Owner A
Owner B
Before
Firm 1
Industry i
Ownership Status: Business Group
Firm 2
Industry j
Ownership Status: Business Group
Firm 3
Industry j
Ownership Status: Business Group
Firm 4
Industry k
Ownership Status: Business Group
Shock to industry i: Firm 1 is sold to Owner C
Owner C
Owner A
Owner B
After
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
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

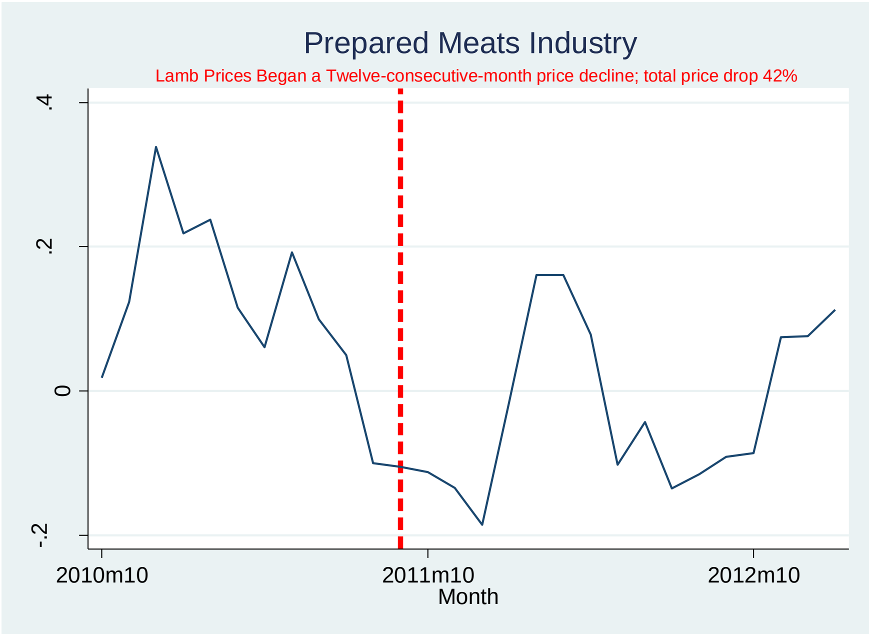


Figure 3

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 are left alone when the other firm in the group was sold, or disappeared.

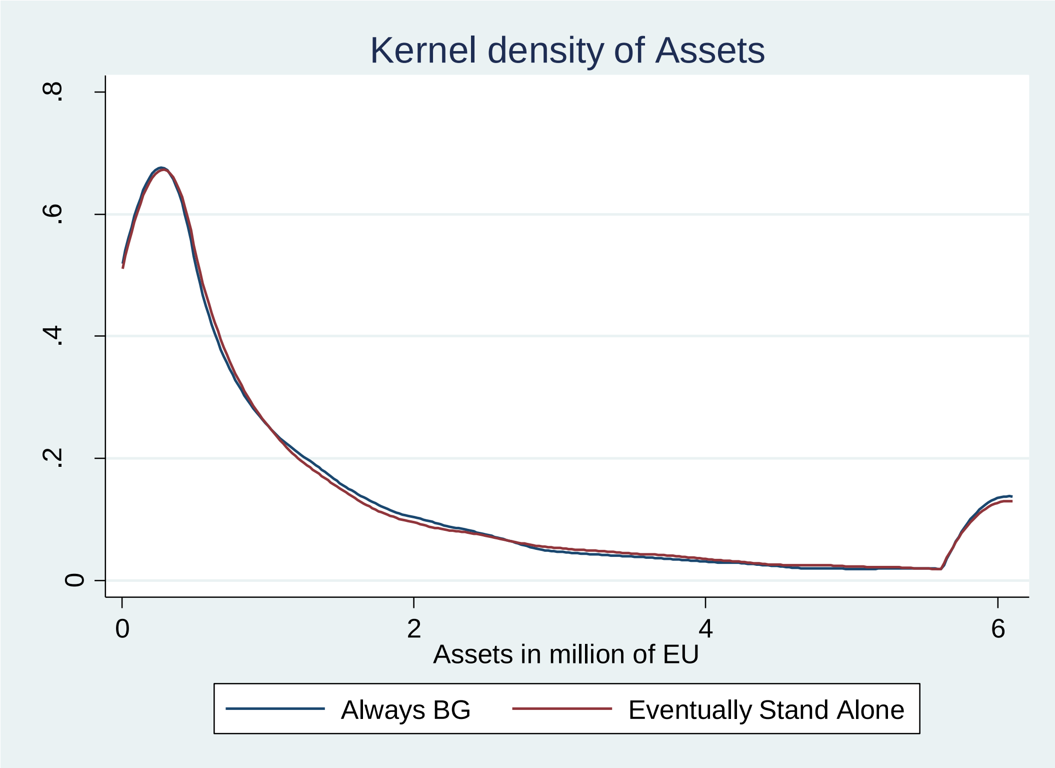


Figure 4

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.

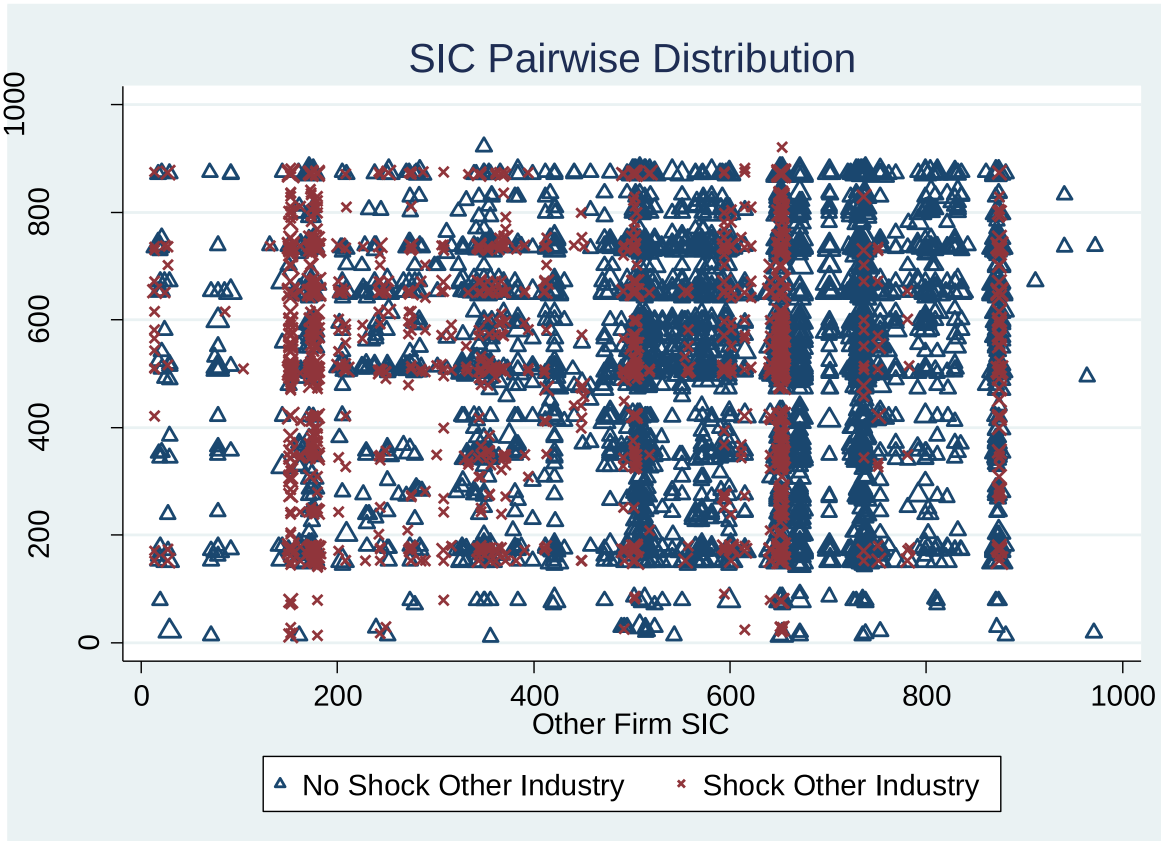
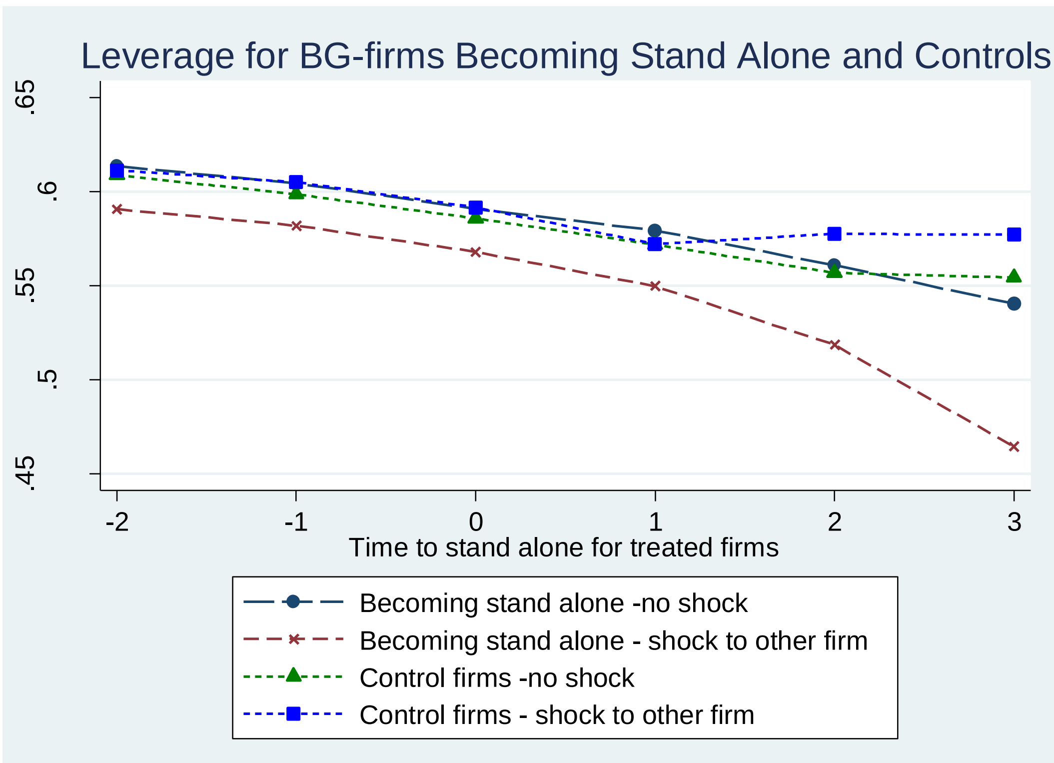


Figure 5

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

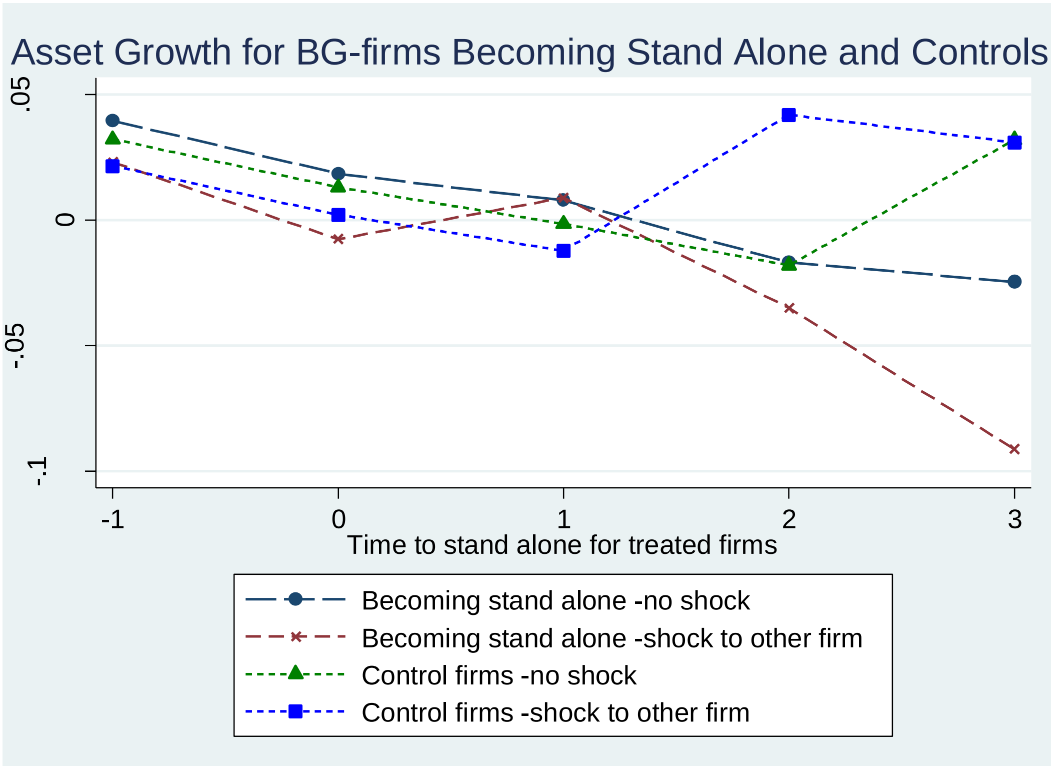

Asset Growth for BG-firms Becoming Stand Alone and Controls
.05
0
-.05
-.1
-1
0
1
2
3
Time to stand alone for treated firms
Becoming stand alone -no shock
Becoming stand alone -shock to other firm
Control firms -no shock
Control firms -shock to other firm

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 we 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	7,141
		% of obs. affected by other industry shocks -defined as 3-year events	22.4%

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). 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**
By Year	2009	1,914	2,274
	2010	3,718	3,764
	2011	3,770	3,827
	2012	3,818	3,822
	2013	2,885	2,075
	Total	16,105	15,762
By Country	Austria	1,403	1,179
	Denmark	294	362
	Finland	28	20
	France	112	64
	Germany	8,058	8,738
	Greece	35	18
	Ireland	156	0
	Italy	1,791	1,962
	Norway	1,787	1,464
	Portugal	100	284
	Spain	1,075	1,167
	United Kingdom	1,266	504
	Total	16,105	15,762
By 1-digit SIC	0	228	183
	1	2,385	2,445
	2	761	766
	3	1,387	1,363
	4	945	876
	5	3,879	3,675
	6	2,949	2,938
	7	2,271	2,231
	8	1,296	1,281
	9	4	4
	Total	16,105	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' financials 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. Cash is cash holdings over assets. 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. Stake is the controller's ownership stake in the firm. Debt and Equity are measured in millions of Euros. Debt and equity issue (retirement) take a value of 1 if their yearly change relative to lagged assets is greater than 5% (less than -5%), and 0 otherwise. 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 PP&E of the companion firm across sample years divided by the average PP&E 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. *Creating New Banking Relations* takes a value of 1 if a firm in a year establish a relation with a new bank, and 0 otherwise. *Destroying Banking Relations* takes a value of 1 if a firm in a year establish a relation with a new bank, and 0 otherwise.

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
	Cash	0.16	0.08	0.21	15,098
	Tobin's Q (industry)	2.97	2.71	1.48	16,105
	Stand-alone	0.57	1.00	0.49	16,105
	Debt (million)	0.97	0.40	1.32	13,939
	Debt issue	0.30	0.00	0.46	10,097
	Debt retirment	0.30	0.00	0.46	10,097
	Equity (million)	0.61	0.19	1.02	13,939
	Equity issue	0.29	0.00	0.46	10,097
	Equity retirment	0.15	0.00	0.36	10,097
	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
	Creating New Bank Rel.	0.11	0.00	0.32	16,105
	Destroying Existing Bank Rel.	0.10	0.00	0.29	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
	Cash	0.16	0.08	0.20	14,734
	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
	Debt (million)	1.00	0.41	1.37	13,680
	Debt issue	0.30	0.00	0.46	9,932
	Debt retirment	0.30	0.00	0.46	9,932
	Equity (million)	0.58	0.18	0.98	13,680
	Equity issue	0.28	0.00	0.45	9,932
	Equity retirment	0.14	0.00	0.35	9,932
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
	Creating New Bank Rel.	0.10	0.00	0.31	15,762
	Destroying Existing Bank Rel.	0.09	0.00	0.29	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

Panel A presents the second stage (IV) estimates of stand-alone, instrumented with Other Shock. Different measures of debt and equity are used as dependent variables. All specifications include as control the variable Own Shock, and the additional controls used in Table 5. 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	log(debt)	Debt issue	Debt ret.	log(equity)	Equity issue	Equity ret.
Stand-alone	-0.4492**	-0.3284**	0.1157	0.1037	-0.1054	-0.0851
	(0.1754)	(0.1599)	(0.1562)	(0.1831)	(0.1635)	(0.1248)
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	Yes	Yes	Yes	Yes	Yes	Yes
N	19150	18469	18469	19150	18469	18469

Panel B- OLS Estimations

Variable	log(debt)	Debt issue	Debt ret.	log(equity)	Equity issue	Equity ret.
Stand-alone	0.0067	0.0037	-0.0012	-0.0123	-0.0007	-0.0058
	(0.0137)	(0.0125)	(0.0132)	(0.0135)	(0.0135)	(0.0098)
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	Yes	Yes	Yes	Yes	Yes	Yes
N	19485	18858	18858	19485	18858	18858

Table 7

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 8

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 Industry Debt Dependence, derived from Rajan and Zingales (1998) and computed using industrial U.S. data. In Panel B 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 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 9

This table presents the second stage (IV) and OLS estimates of stand-alone, on banking relation variables. The dummy *Creating New Relation* takes a value of 1 if a firm in a year starts a banking relation with a new bank, and 0 otherwise. The dummy *Destroying Existing Relation* takes a value of 1 if a firm in a year stops dealing with a bank with which it had an existing relation, and 0 otherwise. In the bottom of the first two columns we present the coefficient of the instrument, Other Shock, in the first stage regressions. Standard errors are adjusted by heteroscedasticity and clusters at the (3-digit) sic-by-country level. Significant at the *10%, **5%, ***1%.

Banking Relations

	Second Stage IV		**OLS**	
Variable	Creating New Relation	Destroying Existing Relation	Creating New Relation	Destroying Existing Relation
Stand-alone	0.3134***	0.3324***	0.0113	0.0052
	(0.1195)	(0.1182)	(0.0101)	(0.0091)
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	22385	22385	22441	22441
First-Stage				
Other Shock	0.1044***	0.1044***		
	(0.0148)	(0.0148)		

Table 10

Panels A and B present second stage (IV) estimates of stand-alone, instrumented with Other Shock for different sample splits, according to relative-to-peers firms' 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 using two-digit sic code dummies as explanatory variables. 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 11

Panels A and B present second stage (IV) estimates of stand-alone, instrumented with Other Shock for different sample splits according to firms' relative standing within their group. 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 12

This table examines firms' performance (OROA in Panel A, and Sales over assets in Panel B). 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%.

Panel A: Performance (OROA)

	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)	

Panel B: Performance (Sales over assets)

	Second Stage IV	**OLS**
Variable	Sales/Assets	Sales/Assets
Stand-alone	0.2342	0.0276
	(0.2895)	(0.0199)
Own Shock	Yes	Yes
Firm Fixed Effects	Yes	Yes
Year Fixed effects	Yes	Yes
Additional Controls	Yes	Yes
N	13349	13744
First-Stage		
Other Shock	0.1271***	
	(0.0210)	

Table 13

Panel A examines firms' cash holdings over assets. 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. Panel B present second stage (IV) estimates of stand-alone, instrumented with Other Shock, splitting the sample according to the median industry volatility of firms. Industry volatility is computed using the standard deviation of industry sales coming from U.S. Compustat firms. Standard errors are adjusted by heteroscedasticity and clusters at the (3-digit) sic-by-country level. Significant at the *10%, **5%, ***1%.

Panel A -Cash

	Second Stage IV	OLS
Variable	Cash	Cash
Stand-alone	-0.0643*	0.0013
	(0.0369)	(0.0028)
Own Shock	Yes	Yes
Firm Fixed Effects	Yes	Yes
Year Fixed effects	Yes	Yes
Additional Controls	Yes	Yes
N	21113	21299
First-Stage		
Other Shock	0.1095***	
	(0.0155)	

Panel B -Sample Split using Industry Volatility

	Low Industry Volatility		High Industry Volatility	
Variable	Leverage	Asset Growth	Leverage	Asset Growth
Stand-alone	-0.1236*	-0.1947**	-0.0457	-0.1083
	(0.0640)	(0.0979)	(0.0524)	(0.1138)
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	11069	12904	8081	9481
First-Stage				
Other Shock	0.1042***	0.0985***	0.1212***	0.1115***
	(0.0225)	(0.0209)	(0.0217)	(0.0196)

Table 14

This table present second stage (IV) estimates of stand-alone, instrumented with Other Shock. We split the sample according to whether treated firms stay under the same owner (stayers) or are sold to a different owner (leavers). For both stayers and leavers, control firms are the control pairs from the matching procedure. Standard errors are adjusted by heteroscedasticity and clusters at the (3-digit) sic-by-country level. Significant at the *10%, **5%, ***1%.

	Split based on Stayers vs Leavers			
	Stayers vs Controls		Leavers vs Controls	
Variable	Leverage	Asset Growth	Leverage	Asset Growth
Stand-alone	-0.1677**	-0.2874**	-0.1124*	-0.2850*
	(0.0791)	(0.1225)	(0.0683)	(0.1482)
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	14038	16494	14581	16959
First-Stage				
Other Shock	0.0732***	0.0722***	0.0743***	0.0664***
	(0.0165)	(0.0146)	(0.0136)	(0.0135)

Table 15

This table present second stage (IV) estimates of stand-alone, instrumented with Other Shock splitting the sample according to whether firms operate in high elasticity or low elasticity industries. High elasticity industries include construction (SIC 15-17), manufacturing of durable goods (SIC 24-25, 32-38), wholesale trade of durable goods (SIC 50), retail trade of durable goods (SIC 52, 55, and 57), and Real Estate (SIC 65). Standard errors are adjusted by heteroscedasticity and clusters at the (3-digit) sic-by-country level. Significant at the *10%, **5%, ***1%.

	Sample Split using Demand Elasticity			
	Low Elasticity: Non Durables		High Elasticity: Durables	
Variable	Leverage	Asset Growth	Leverage	Asset Growth
Stand-alone	-0.0949*	-0.1583	-0.0780	-0.1572
	(0.0537)	(0.1038)	(0.0725)	(0.1017)
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	12402	14701	6748	7684
First-Stage				
Other Shock	0.1091***	0.0905***	0.1100***	0.1274***
	(0.0174)	(0.0165)	(0.0328)	(0.0300)

Table 16

This table replicates the first three columns of Table 4 (first stage regressions of the leverage equation), but using *Stake* as instrumental variable, instead of *Stand-alone*. The (unreported) second stage show no effect of stake on leverage. Standard errors are adjusted by heteroscedasticity and clusters at the (3-digit) sic-by-country level. Significant at the *10%, **5%, ***1%.

First stage for Stake -using Leverage Regression Sample

Variable	Stake	Stake	Stake
Other Shock	0.0039	0.0016	0.0016
	(0.0039)	(0.0050)	(0.0050)
Own Shock	0.0069	0.0083	0.0085
	(0.0045)	(0.0055)	(0.0055)
Firm Fixed Effects	Yes	Yes	Yes
Year Fixed effects	Yes	Yes	Yes
Additional Controls	No	No	Yes
Sample restr. to controls data	No	Yes	Yes
Craig-Donald Wald F	1.46	0.16	0.16
R-squared (whithin)	0.006	0.005	0.005
N	27210	19150	19150

Appendix: Other examples of shocks in our sample

SIC 100, Agricultural Production. Year 2011. Large increase in agricultural raw material (inputs) prices. Reference: http://www.indexmundi.com/commodities/?commodity=agricultural-raw-materials-price-index&months=240

SIC 206, Sugar and Confectionery Products. Year 2009. Large increases in input prices. Cocoa increased by 75% and sugar by 100%. References:
http://www.indexmundi.com/commodities/?commodity=cocoa-beans&months=300
http://www.indexmundi.com/commodities/?commodity=sugar&months=300

SIC 242, Sawmills and Planing Mills. Years 2009-2010. The European Commission proposed a regulation with the aim of reinforcing the voluntary measures in the FLEGT Action Plan. The Regulation was formally adopted at the end of 2010.

SIC 271, Newspapers: Publishing, or Publishing and Printing. Years 2009-2011. Rise in costs of ink materials like TiO_2, nitrocellulose and other resins like acrylics increased between 50% and 60% during 2011 and 2012. Reference: http://www.inkworldmagazine.com/the-european-ink-report

SIC 308, Miscellaneous Plastic Products. Years 2010-2011. Large increase in plastic (input) prices. Reference: http://www.indexmundi.com/commodities/?commodity=rubber&months=301

SIC 382, Measuring and Controlling Instruments. Year 2009. Directive 2009/23/EC of the European Parliament and of the Council of 23 April 2009 (OJ 2009 / L 122 p.6). Codification replacing Council Directive 90/384/EEC of 20 June 1990 on the harmonization of the laws of the Member States relating to non-automatic weighing instruments (NAWI). This lead to increased production costs. Reference:
http://eur-lex.europa.eu/LexUriServ/LexUriServ.do?uri=OJ:L:2009:294:0007:0009:EN:PDF

SIC 391, Jewelry, Silverware, and Plated Ware. Year 2011. Gold price increased by 30% and silver by 53%. References: http://themoscownews.com/business/20121224/191055616.htm
http://www.indexmundi.com/commodities/?commodity=gold&months=311

Table A.1

This table present our main results (Table 5), but now controlling for country-by-year fixed effects, rather than year fixed effects.

Panel A- Second Stage IV Estimations

Variable	Leverage	Leverage	Leverage	Asset Growth	Asset Growth	Asset Growth
Stand-alone	-0.1462**	-0.1317**	-0.1341**	-0.3712**	-0.3421***	-0.2244**
	(0.0695)	(0.0619)	(0.0624)	(0.1578)	(0.1289)	(0.1116)
Own Shock	Yes	Yes	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Country-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	27394	19242	19242	25029	22504	22504

Panel B- OLS Estimations

Variable	Leverage	Leverage	Leverage	Asset Growth	Asset Growth	Asset Growth
Stand-alone	0.0030	0.0030	0.0029	-0.0025	-0.0009	0.0030
	(0.0024)	(0.0029)	(0.0030)	(0.0062)	(0.0072)	(0.0055)
Own Shock	Yes	Yes	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Country-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	27619	19583	19583	25029	22561	22561

Tables A.2 and A.3

Panels A-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 Industry Equity Dependence. In Panel B we split the sample using above and below the median Industry External Financial Dependence. External dependence measures are derived from Rajan and Zingales (1998) and are computed using industrial U.S. data. Standard errors are adjusted by heteroscedasticity and clusters at the (3-digit) sic-by-country level. Significant at the *10%, **5%, ***1%.

Table A.2 -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)

Table A.3 -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)