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The effects of losing business group affiliation

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Abstract

We propose a novel identification strategy for estimating the effects of business group affiliation. We study two-firm business groups, some of which split up during the sample period, leaving some firms as stand-alones. We instrument for stand-alone status using shocks to the industry of the *other* group firm. We find that firms that become stand-alone reduce leverage and investment. Consistent with collateral cross-pledging, the effects are more pronounced when the *other* firm had high tangibility. Consistent with capital misallocation in groups, the reduction in leverage is stronger in firms that had low (high) profitability (leverage) relative to industry peers.

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Business groups –sets of firms with a common controlling shareholder— are frequent in both developed and developing countries. They account for more than 20% of stock market capitalization in Western Europe and, in many Asian countries, their assets represent up to 70% of GDP (Morck, Wolfenzon, and Yeung, 2005). A firm's affiliation to a business group is an endogenous process for which different authors have suggested different rationales. It can, for example, be argued that profitable firms use their abundant cash flow to form business groups around them (Almeida and Wolfenzon, 2006a) and that weak firms need the support of business groups to survive (Gopalan, Nanda, and Seru, 2007). Precisely because business group affiliation is not randomly distributed in the economy, it is hard to estimate its causal effects (Khanna and Yafeh, 2007). In this paper, we propose a novel identification strategy for estimating the effects of business group affiliation on capital structure and investment.

Our strategy is based on two building blocks. First, we study the effects of loss of affiliation to a business group. A firm in a business group is inherently different from a stand-alone firm so one cannot rely on purely cross-sectional comparisons of group firms and stand-alone firms. 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. More precisely, the first building block of our strategy is a differences-in-differences approach where we compare changes in firms that transit from business groups to stand-alone status with those in firms that remain in a business group.

Studying firms that change status is, however, only a partial solution to the endogeneity problem because those that become stand-alone are far from a random sample. One could argue that firms leave a business group precisely when affiliation destroys value. A similar problem occurs when studying conglomerate spin-offs (Gertner, Powers, and Scharfstein, 2002). For this reason, our identification strategy needs a second crucial ingredient: the ability to find firms that exogenously become stand-alone.

We study groups composed of only two firms and instrument for a firm's stand-alone status using negative shocks to the industry of which the *other* firm in the group is part. Such shocks can force the sale of this other firm, leaving the firm being studied as a stand-alone, not by choice but arguably by chance. In our setting not all firms that become stand-alone receive a shock to the industry of the other firm in the group, and not all industry shocks to one firm produce a group split. These shocks only allow us to isolate the fraction of transitions to standalone status that is exogenous. So, the second building block of our strategy is an instrumental variables (IV) approach on top of the previous differences-in-differences.

We exploit industrial shocks that can be cleanly identified such as commodity shocks (e.g., in milk, aluminum and wood pulp) and regulatory shocks (e.g., the tobacco industry). For the purposes of our strategy, the two firms in the group should operate in unrelated industries in order to guard against contagion of the shock to the firm being studied (Whited, 2001) since this would violate the exclusion restriction in the IV setting. By studying groups with firms in unrelated industries, we are testing the effects of both conglomeration (being associated with other firms under a common ownership structure) and diversification. This joint hypothesis has been the focus of interest in the literature since business groups are typically diversified across several industries (see Khanna and Yafeh, 2007).

Our data come from the universe of European private firms in *Amadeus* for the years 2009-2013. A particular advantage of *Amadeus* is that it provides information on ownership structures, and hence we can identify pairs of firms with a common controlling shareholder. We find that becoming stand-alone has a strong negative effect on leverage and asset growth in the IV regressions that use shocks to the other firm as an instrument for stand-alone status, with leverage falling by close to 10% and asset growth by close to 20%. These results indicate that capital structure and investment are different in groups and stand-alone firms.

One explanation for our results is that business groups relax credit constraints. In line with credit constraints, we find that the results are stronger among firms in debt-dependent industries (on the lines of the financial dependence discussed by Rajan and Zingales, 1998) and in countries with less developed banking systems as measured by the ratio of domestic credit to GDP. In terms of specific mechanisms, our evidence suggests the existence of collateral cross-pledging between firms in the same group. The assets of one firm can be used as collateral for the debt of the other, increasing financing as compared to a situation in which both firms are stand-alone. Consistent with this, we find that reductions in leverage and asset growth are more pronounced when the other firm contributed more tangible assets to the group. Our results are not consistent with other types of cross-pledging and, in particular, we find no evidence of cash-flow transfers between firms.

The above tests indicate that firms in business groups have access to more funds, resulting in higher leverage and investment. However, more funds can be harmful if business groups use capital inefficiently. For example, groups can give some firms more capital than is justified by investment opportunities (e.g., pet projects). Consistent with this misallocation of capital, we find that firms which reduce leverage most as they become stand-alone are precisely those that appear to have been previously over-investing with subsidized financing. We find that firms that had initial profitability below that of their industry peers and initial leverage above that of their industry peers see their leverage ratios fall by more when becoming stand-alone. In addition, firms that had industry Tobin's Q below the other firm in the group experience a stronger fall in leverage. Overall, we do not find that becoming stand-alone has a negative effect on accounting measures of performance. This suggests that the misallocation of capital is not severe enough to overturn the benefits of business group affiliation.

Our paper is related to the literature that showcases the costs and benefits of business group affiliation. Business groups can harm firms by engaging in unproductive activities or through the outright expropriation of minority shareholders (i.e., "tunneling" as in Johnson, La Porta, López-de-

Silanes, and Shleifer, 2000; Bertrand, Mehta, and Mullainathan, 2002). They can, however, also help firms by relaxing financial constraints (e.g., Almeida, Park, Subrahmanyam, and Wolfenzon, 2011; Bena and Ortiz-Molina, 2013; Gopalan, Nanda, and Seru, 2014). In particular, Buchuk, Larrain, Muñoz, and Urzúa (2014) study the use of intra-group loans in Chile to finance the investment of capital-intensive firms. Similarly, Almeida, Kim, and Kim (2015) show that Korean groups were able to sustain investment in high-growth firms during the Asian crisis through cross-equity investments. Both of these papers study the endogenous selection of firms into providers and receivers of capital within a group. Macroeconomic shocks, such as the Asian crisis or the 2008-9 crisis studied by Lins, Volpin, and Wagner (2013), provide a good source of identification of short-run differences between group and non-group firms. Common to these analyses is that they implicitly assume that affiliation to a business group is fixed or very sticky in the short run. Our setup provides a more frontal attack at the estimation of business group effects on capital structure and investment by looking at exogenous variation in affiliation. Also, by studying heterogeneity in these effects we can shed light on particular mechanisms not studied before, such as collateral cross-pledgeability.

We contribute to the literature by estimating causal effects of business group affiliation using a novel identification strategy based on small and diversified groups. Most of the literature has focused on large groups of numerous listed firms, rather than our small groups of two private firms. Many of the mechanisms studied in the literature, such as financial constraints, are still present and even more relevant in small groups. Some costs, such as tunneling, are not relevant in our sample given the high levels of ownership concentration. As in many other settings, the trade-off between identification and external validity is present in the sense that, in order to improve identification, we have to use a particular setup. Other contemporaneous work follows a similar path, although the authors study different trade-offs and mechanisms. For example, Pérez-González (2015) estimates a related causal effect for holding companies, with an identification strategy based on the deregulation of U.S. power utilities in the 1930s. Shi (2015) studies the effects of business group affiliation using changes in the ownership chain two or more levels away from the firm of interest. Our findings, combined with causal estimates from other settings, can advance in achieving external validity above and beyond the specific features of each setup.

Our paper also contributes to the literature on internal capital markets, which has studied the allocation of capital mostly (albeit not exclusively) in U.S. conglomerates (Stein, 2003). The theoretical underpinnings are similar to those for the costs and benefits of conglomerates. Rent-seeking behavior by divisional managers and power struggles can distort allocation of capital between divisions (Matvos and Seru, 2014; Ozbas and Scharfstein, 2010; Rajan, Servaes, and Zingales, 2000; Scharfstein and Stein, 2000). On the other hand, conglomerates may have financial advantages and be better at picking winner projects (Giroud and Mueller, 2015; Khanna and Tice, 2001; Shin and Stulz, 1998; Stein, 1997). Although similar trade-offs apply to the allocation of capital both within conglomerates and within business groups, there is one important difference that we exploit to our advantage. Conglomerates are composed of one firm with multiple divisions, but one balance sheet. Although the degree of industrial diversification can be the same in conglomerates as in business groups, business groups are composed of independent corporations, each with its own balance sheet and ownership structure. This implies that the effects of affiliation on capital structure can be measured directly in business groups.

Section 1 of this paper describes the empirical design of the study and the data used. Section 2 presents the main results regarding leverage and asset growth while Section 3 explores the mechanisms behind our main results, with an emphasis on collateral cross-pledging. In Section 4, the assumptions behind the IV design, particularly the exclusion restriction, and alternative hypotheses for our findings are discussed and, in Section 5, we present our conclusions.

1. Empirical Design

1.1. Identification strategy

We start with a sample of firms associated in pairs or small business groups. In this sample, we study the effects of loss of affiliation to the business group, i.e., the "treatment" consists of becoming a stand-alone firm. "Control" firms are those firms that remain in a group. The fundamental economic problem we face is that the treatment does not occur randomly. In general, firms become stand-alone by choice. Hence, estimating the treatment effect of becoming a stand-alone is challenging.

We can think of the following IV system:

$$First\ Stage: Standalone_{it} = \theta\ OtherShock_{it} + \pi\ OwnShock_{it} + \rho' X_{it-1} + \mu_i + \tau_t + \vartheta_{it} \quad (1)$$

$$Second\ Stage: Y_{it} = \beta\ Standalone_{it} + \gamma\ OwnShock_{it} + \delta' X_{it-1} + \mu_i + \tau_t + \varepsilon_{it}. \quad (2)$$

Equation (2) is the main regression of interest. We regress firm outcome Y_{it} on the dummy $Standalone_{it}$ that takes a value of 1 if firm *i* is stand-alone in year *t*, and 0 otherwise. We control for shocks to the same industry ($OwnShock_{it}$) and lagged firm characteristics in 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, it could be argued that firms in particular industries are more prone to form groups or that managerial quality or preferences explain the industrial diversification of business groups. As long as these characteristics are time-invariant, they will be absorbed by the firm fixed effect. We also control for time fixed effects τ_t.

Regression (2) is, in essence, a differences-in-differences regression. The $Standalone_{it}$ dummy allows for a before-and-after comparison in firms that become stand-alone. Firms that remain in a group (i.e., control firms) are also included in regression (2) and allow us to estimate dynamic effects (captured by X_{it-1}) and macro effects (captured by τ_t) more precisely. The null hypotheses is that $\beta = 0$ since, in a frictionless world, ownership status should not affect 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 becoming stand-alone. For example, the life cycle of a firm can simultaneously affect productivity and stand-alone status. In terms of equation (2), unobserved productivity contained in ε_{it} can affect the outcome Y_{it}, but also the decision to become stand-alone, so the standard exogeneity assumption of OLS does not hold. In short, in order to talk about causality, we need an instrument for $Standalone_{it}$.

The instrument we propose consists of industrial shocks to the *other* firm in the group. The idea behind our instrument is depicted in stylized form in Figure 1. Think of a group with two firms, Firm 1 and Firm 2, controlled by owner A, of which Firm 1 receives a severe negative shock and is shortly sold to another owner (owner C) or disappears. Alternatively (not shown in the figure as a possibility), Firm 2 is sold to a different owner who does not own other firms.[1] In either case, the shock to Firm 1 results in Firm 2 becoming stand-alone. The key element in our identification strategy is that the shock allows us to identify exogenous changes in the status of Firm 2. As a control, we use Firm 3 in a different business group under a different owner (owner B), selected through a one-to-one matching procedure with Firm 2 that is explained below. This Firm 3 remains in a business group throughout the sample period and the other firm in its group (Firm 4) may or may not receive shocks.

It is becoming stand-alone, not a shock to the other firm in the group, that is the treatment of interest in our setup. Since not all firms become stand-alone because the other firm in the group receives a shock and not all shocks produce group splits, the instrument does not have to explain all potential transitions to stand-alone status, but only isolate those that are arguably exogenous. Firms

[1] 85% of the firms that become stand-alone in our sample stay with the original owner.

that remain in a group are useful precisely to obtain better estimates of the role of shocks in producing transitions to stand-alone status.

Our empirical strategy is a combination of a differences-in-differences design and instrumental variables. Throughout the paper, we use the control-treated dichotomy that is standard in differences-in-differences setups. In our case, however, the actual "treated" firm picked up by the IV system is a firm that is induced to become stand-alone when the other firm in the group is hit by a shock and the actual "control" firm, or counterfactual in the regression, is a composite rather than a single type of firm that we can point to in the data. For simplicity, we keep the standard control-treated dichotomy, although the above caveat should be borne in mind when interpreting the results.

Equation (1) is our first-stage regression where $OtherShock_{it}$ is the instrument for the stand-alone status of each firm. We estimate (1) with OLS, hence the first stage is a linear probability model since $Standalone_{it}$ is a dichotomous variable. Like any instrument, $OtherShock_{it}$ has to obey the exclusion restriction and has to be relevant.

We note two things in terms of the exclusion restriction. First, the instrument is at the industry level, not 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. 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 shock to the construction sector in the sample period is likely to be endogenous to the financial crisis). Second, the two firms in our business groups are in unrelated industries, as measured by coefficients of the input-output matrix as explained below. If the industries were related, it could be argued that spillovers other than through the ownership structure may explain 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 larger groups, the link between a shock to one firm and the stand-alone status of the other firm is more tenuous or potentially endogenous.[2] For instance, in a group of four firms in four unrelated industries, a shock to one of them would leave three unaffected firms for potential use in our regression setup. However, if one firm is sold and leaves the group, it is less plausible to argue that it was exogenously forced into stand-alone status since the group could have sold any of the three unaffected firms.

1.2. Industrial Shocks

In order to identify the shocks in $OwnShock_{it}$ and $OtherShock_{it}$, we proceed in a reverse engineering fashion by first identifying candidate shocks based on the stock returns of listed firms. We compute six-month rolling windows of returns for four-digit SIC industrial portfolios in each European stock market during the past decade. Within this universe, we select the returns in the lowest 5% of the distribution during the years that are relevant for our business group data. In this sample, we first check that poor returns are sufficiently widespread in the industry and not driven by idiosyncratic shocks to a few firms. Then we check, by hand, in the press or analyst reports, the type of shock that likely caused the negative return and eliminate those cases where we are unable to pin down the source of the shock precisely. In about a fifth of the cases, we could identify the source of the shock, ranging from commodity-related shocks (e.g., metals, grains and livestock) to regulatory decisions (e.g., safety laws and tobacco-related laws). Overall, we identify 359 country-industry-year shocks, out of which 322 are commodity shocks and 37 are regulatory shocks. Only ten are country-

[2] For example, the effect of shocks on becoming stand-alone is weaker in a sample of three-firm business groups than in our main sample of two-firm business groups. The sample of three-firm business groups that we compiled is much smaller than our main sample. Only 889 firms initially belong to a three-firm business group of industrially-unrelated firms.

industry specific shocks and the rest affect an entire industry in Europe. Panel A of Table A.1 in the Appendix summarizes our selection process.

Figure 2 shows two examples of the shocks in our sample. The first is related to safety regulation in the games and toys industry in June 2009, with a trough of about -25% in six-month returns observed a couple of months afterwards. The second corresponds to the poor returns (almost -20%) of the prepared meats industry in late 2011 and early 2012, which go hand-in-hand with a 42% decline in the price of lamb in 12 months. Although the returns we show are for listed firms, the nature of the shocks suggests that they also affected private firms. More examples of industrial shocks in our sample can be found in the Appendix.

We are conservative in the identification of shocks in the sense that our final sample only contains shocks that we could identify with confidence. As a consequence, we focus on relatively large and long-lasting shocks that produce deep and widespread changes in the respective industry. In particular, we show in the Appendix (Table A.2) that our shocks are related to industry-wide M&A activity. From *Zephyr*, we take data on M&A transactions at the four-digit SIC code in Europe in 2009-2013 and show that the number of deals in an industry-year is positively correlated with our shocks. In line with previous literature, economic shocks such as more expensive inputs or tighter regulation are correlated with merger waves and industry-wide consolidation (Harford, 2005; Mitchell and Mulherin, 1996).

1.3. Firm-level Data

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 was obtained from *Amadeus*' DVDs at a yearly frequency. This data includes the names of the controlling shareholders and their ownership stakes as well as information about whether the controlling shareholder is a family, an individual, a publicly listed firm or other type of corporation.

We collect data for 16 Western European countries in 2009-2013. In many countries, coverage for small firms prior to 2009 is sparse. After 2009, and since firms in Europe have strict reporting requirements, *Amadeus*' coverage is almost equivalent to the universe of firms. There are, for instance, 8.6 million firms in 2011. We focus on business groups controlled by either families or individuals, which represent 1.3 million firms. The minimum ownership stake we require to consider a family or individual as controlling shareholder is 50%. All firms are privately held.

A crucial step for our identification strategy is to focus on business groups of *only* two firms incorporated in the same country. One example is the group controlled by Sergio Traversa in Vicenza, Italy. In 2010, he held controlling stakes in a company producing cushions and fabrics for garden furniture (Lollo Due SRL) and in a vineyard (Ongaresca Societa' Agricola SRL).

Firms are considered to become stand-alone in the year when there is no other firm in the sample with the same controlling shareholder. This can happen because of the sale of either firm to a different owner or if the other firm in the group goes bankrupt.[3] Having identified firms belonging to two-firm business groups, we eliminate groups whose firms are in well-integrated industries. Following Fan and Goyal (2006), we construct a measure of vertical integration based on the U.S. input-output matrix. Using this matrix we compute the fraction of input (output) that an industry acquires (sells) from (to) other industries. Then, for each business group, we compute the average of what the industry of each firm sells to the industry of the other firm and, conversely, what it buys

[3] Bankruptcy is infrequent in our sample (less than 5% of cases). Our results are robust to excluding these cases.

from the industry of the other firm. The group is eliminated if this average is larger than 1%. Similarly, we exclude groups where both firms are in the same three-digit SIC code.[4] Overall, a little under 1% of firms in *Amadeus* (e.g., 81,275 firms in 2011) meet the criterion of belonging at some point to a business group with two firms in unrelated industries.

To further ensure the data's quality and homogeneity, we apply several restrictions. First, we restrict the sample to those firms that, during their first year in the sample, are part of a two-firm business group (i.e., we drop firms that were not originally in a two-firm business group and become a business group during the sample period). Second, we eliminate firms with less than four years of data and with annual asset growth below -90% or above 200%. Third, we ensure that the firms that remain in a two-firm business group and firms that become stand-alone are as similar as possible. To do this, we perform a one-to-one propensity score matching, based on the two-digit SIC code and size (book assets) in the initial year in the sample.[5] Figure 3 shows the distribution of size in both sets of firms. As expected from the matching procedure, both distributions are basically overlapping.

Our final sample consists of 3,843 firms that transit to stand-alone status during our sample period, representing 16,105 annual observations, and 3,843 firms that remain in a two-firm business group, representing 15,762 annual observations. Although perhaps not small in size, this sample represents only a tiny fraction of the original *Amadeus* universe. Satisfying our strict selection criteria implies discarding millions of firms, including all observations from four countries (Sweden, the Netherlands, Belgium and Switzerland).

[4] Our results are also robust to excluding firms in the same two-digit SIC code, which represent less than 5% of the observations.

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

The Appendix shows the industrial shocks that we are able to match to the business group data (Table A.1, Panel B).[6] We consider a match to exist if there is a firm in our business group data for the country-industry-year of the shock, and up to two years later. We consider the years after the shock because divestiture decisions can take time to materialize. We are not able to match all of the shocks because not all affected industries are represented among business groups. Out of the initial 359 shocks, we have firms in industries affected by 121 shocks, and, in the case of 119 shocks, firms in other industries paired with firms directly affected by the shock.

One example in the sample is provided by a group in Germany. In 2009, two firms - Bernhard Upmann Verpackungsmaschinen, a manufacturer of calibration, weighing and packaging machines, and Backenecker Liegenschafts-verwaltungs, a real estate agency – were 100% owned by the same individual, Johannes Backenecker. In November 2009, the European Commission issued a directive on maximum possible errors in measuring instruments, triggering poor returns in this industry. In 2010, the manufacturing firm was sold to three partners, each of whom took a 33.33% stake. As from 2011, the real estate agency, therefore, operated as a stand-alone business under Johannes Backenecker, who kept a 100% stake.

Table 1 describes the composition of our sample by year, country and industry. It is relatively well balanced. Some differences can be expected because of country size while others are related to regulatory standards for reporting data on private firms. Germany accounts for the largest share of observations, followed by Italy, Norway, Austria, Spain and the UK. There is no cluster of observations in any particular industry. SIC 5 (wholesale and retail trade) accounts for the largest share of observations but is still less than 25% of the sample. SIC 6 (financials) includes real estate

[6] Given the SIC-code availability in the business group database, we aggregate shocks from the four-digit up to the three-digit SIC-code level in the merged sample. We initially look at shocks at the four-digit level so we have a better understanding of the shocks (their nature, source, duration, etc.).

agencies and small service firms (e.g., consulting and accounting services), but does not include banks or large financial intermediaries.

Figure 4 shows the distribution of observations according to the industry of both firms in the business group. We split observations into those pairs with shocks to the industry of the other firm and those with no such shocks. The purpose of the figure is to show that there is no cluster of observations, with or without shocks, in any particular industry 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 2 provides summary statistics for the main variables in our analysis. Panel A shows firms that eventually become stand-alone (treated firms) while Panel B shows firms that remain in a group 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. Average firm characteristics such as size (book assets in millions of euros), leverage, tangibility, and others are remarkably similar 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 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 controlling shareholder's stake is generally above 95% so minority shareholders are almost non-existent in this sample.[7] Averages for the dummy variables representing shocks correspond to the frequency of being hit by a shock. For both treated and control firms, between one-fifth and one-quarter of observations are hit by shocks. This

[7] There are minority shareholders in only 18% of the firms in our sample, and the controlling stake is still on average 85% in those firms with minority stakes. Our results change little if we exclude firms with minority shareholders from the sample.

underlines that not all firms become stand-alone as a result of shocks, and not all shocks induce firms to become stand-alone.

We also compute ratios of characteristics of the other firm in the group to those of the firm being studied (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), relative OROA and relative equity are computed analogously. Sales correlation is computed as the correlation coefficient between sales growth of the industries of the two firms in U.S. data. Pair characteristics are on average very similar across treated and control firms. This implies that the business groups which split do not differ, on average, from the business groups that stay together. For example, groups that split do not on average show more disparity between their firms in terms of tangibility, Tobin's Q or OROA or have a higher sales correlation than other groups.

Together with firm characteristics, Table 2 shows industrial and country characteristics. Rajan and Zingales (1998) measure the external dependence of an industry as the average fraction of investment that is not financed by internal cash flow (everything measured for U.S. industries) and equity dependence as the average fraction of investment financed with equity. We compute debt dependence as the difference between external dependence and equity dependence. Again, we do not find differences between treated and control firms in terms of industry or country characteristics (e.g., domestic credit over GDP).

Finally, we report summary statistics for the creation and destruction of banking relationships. Since *Amadeus* reports the names of the banks with which each firm has a relationship each year, we know whether the firm starts a relationship with a bank (creation) or finishes a relationship (destruction). As expected in a sample of small firms, the average and median numbers of banking relationships are just one. The average of creation and destruction is 0.1 per year. Unfortunately, we do not know the amount of credit or the corresponding interest rate.

2. Main Results

2.1. Effects on capital structure and asset growth

Table 3 shows the results for our first stage (with the sample corresponding to the subsequent second-stage leverage regressions). The dummy variable for stand-alone status is the dependent variable. The explanatory shocks correspond to indicator variables that take a value of 1 in the year of a given shock and the following two years, and 0 otherwise. We show the coefficient for shocks in the industry of the *other* firm in the group (the instrument) and for shocks in the same industry (not an instrument). Lagged variables (X_{it-1}) include log assets of the firm, tangibility and the Tobin's Q of the industry. The estimated coefficient for $OtherShock_{it}$ ranges from 0.0889 to 0.1120, implying that shocks to the other industry have a sizeable impact on the likelihood of becoming a stand-alone. The coefficient for own industry shock is only slightly higher (between 0.0902 and 0.1285). We do not claim that these two shocks explain all transitions to stand-alone status. The R-squared is less than 40% in all specifications, meaning that there is a substantial unexplained variation in stand-alone status in the data. We are interested in the fraction of variation in stand-alone status that is exogenous, which we identify with the portion explained by $OtherShock_{it}$. The large F-statistics confirm that the instrument is strong, making it unlikely that there is an "IV blow-up" problem in the second stage as suggested by Jiang (2017).

Table 4, Panel A shows the results for the second stage of the IV system. The effect of becoming stand-alone on leverage ranges from -0.0866 to -0.1034, implying that the reduction in leverage is approximately one-third of the standard deviation of leverage in the sample (Table 2). The different specifications in Table 4 vary according to whether control variables (X_{it-1}) are used or not and whether the sample is restricted for availability of these controls. Including control variables reduces the sample size from 27,000 to approximately 19,000 observations, but the coefficient on $Standalone_{it}$ remains significant and of similar magnitude throughout.[8] Irrespective of the specification and sample, the effect is significant at least at the 5% level. The effect on asset growth ranges from -0.1562 and -0.2239 and is always significant at the 5% level at the least. The effect of becoming stand-alone is stronger on growth than on leverage since it represents about three-quarters of the standard deviation of asset growth in the sample (Table 2).

The OLS regressions (Panel B in Table 4) show no impact of becoming stand-alone on either leverage or asset growth. OLS estimates are likely to be smaller (in magnitude) than IV estimates because many firms become stand-alone intentionally when they have little to lose or when it is more convenient to do so. In other words, many firms self-select into stand-alone status, biasing the results against a negative effect on leverage and growth. The IV procedure allows us to isolate those firms that receive the treatment unintentionally and estimate the causal effect of business group affiliation, rather than the selection of firms into business groups.

Figure 5 illustrates our main results in an event-study fashion. We define year zero as the year when a firm becomes stand-alone and plot firm outcomes before and after the 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 those that do not. Both types of treated firms transit to stand-alone status in the sample, but the ones that become stand-alone after a shock to the other firm arguably do it for exogenous reasons. Since not all firms have data for the entire window, and not all firms transit to stand-alone in the same year, we first compute differences between two consecutive years for each firm. We then take the average of these differences across all firms in each event year. We repeat this for the control firm paired with each treated firm at the beginning of the sample. Finally, we add back

[8] Our results are also robust to including country-year fixed effects as seen in Table A.3 of the Appendix.

the average initial level of each variable to each subsample of firms: i) firms that become stand-alone and do not face shocks to the other firm, ii) firms that become stand-alone and face shocks to the other firm, iii) control firms with shocks to the other firm, and iv) control firms without shocks to the other firm.

Panel A shows our results for leverage. The negative trend is consistent with the overall reduction in leverage seen after the financial crisis. More importantly, firms that become stand-alone and face shocks to the other firm in the group experience a strong reduction in leverage after becoming stand-alone. A similar reduction is not seen in firms that become stand-alone without shocks to the other firm. In fact, these other treated firms behave very much like control firms. Panel B shows the results for asset growth. They start in year -1 since we have fewer observations for growth rates in year -2 (which would imply having data for year -3). Again, the downward trend is clear in years 2 and 3 only for treated firms that face a shock to the other firm in the group. The main conclusion from the figure is that the reduction in leverage and asset growth seen in firms that become stand-alone is related to shocks to the other firm in the group, which is consistent with our second-stage results in the IV regressions. In other words, Figure 5 illustrates the reduced form of our IV results.

2.2. Decomposing the effect on leverage

In Table 5 we study the effects on debt and equity separately in order to better understand the reduction of leverage when firms become stand-alone. We run regressions with (log) levels and with dummy indicators for large issuance or retirement of debt and equity. The regressions in columns 1 and 4 of Table 5 show that debt falls strongly but equity does not react significantly to stand-alone status. We find a significant reduction in the frequency of debt issuance (coef. -0.3284, t-stat 2.05, in column 2), but no effect on debt retirement (column 3). There are insignificant effects on equity issuance and retirement (columns 5 and 6). These results suggest that the reduction in leverage is a consequence of the inability of firms to continue borrowing once they become stand-alone.

The results in Table 5 reduce the scope of potential explanations. For instance, they help to rule out a mechanical reduction in leverage caused by the controlling shareholder using the proceeds of the sale of the other firm to increase equity in this firm. Moreover, a mechanical reduction in leverage of this type does not predict a fall in asset growth. Why, indeed, would a company grow less if the controlling shareholder has just increased its investment?[9]

Table 5 suggests that the impact of stand-alone status has its origin in credit markets, rather than equity markets. The next section, therefore, focuses on linking the results to credit constraints.

3. Mechanisms

We first study cases where credit constraints are more likely to be binding. We then try to pin down the precise mechanism through which business groups alleviate credit constraints. Next, we explore firm characteristics that are related to inefficient investment and capital misallocation. We draw on the literature on internal capital markets (see Stein, 2003, for a survey), which, although focusing on conglomerates, can guide the discussion about capital allocation in groups. Finally, we study the average effect of becoming stand-alone on firm performance.

3.1. Credit Constraints

[9] See also the results regarding cash holdings in Table 14, which belie a mechanical reduction in leverage if the alleged increase in equity is saved as cash.

As suggested by the decomposition of leverage in Section 2.2, the inability of stand-alone firms to borrow explains their reduction in leverage. One implication of credit constraints is that the effect should be particularly strong in industries that are naturally more debt-reliant. Consistent with this, Table 6, Panel A shows that our results do indeed come mostly from firms in debt-dependent industries. In the leverage regressions, the coefficient on $Standalone_{it}$ in the high debt-dependence sample is almost three times that for the low debt-dependence sample (-0.1261 vs. -0.0460). Similarly, in the asset growth regressions, the coefficient on $Standalone_{it}$ is stronger and more statistically significant in the high debt-dependence sample (-0.1882 vs. -0.1128). Since the first stage is similarly strong in both subsamples, the lack of an effect in the low-dependence sample is not due to a selection problem, i.e., not only firms in high-dependence industries are affected by the instrument. We also find that low equity-dependence industries, rather than high equity-dependence industries, suffer the most. This result serves as a sort of falsification test to relate our results to credit constraints, rather than some generic form of external dependence (results split by both external dependence and equity dependence are reported in the Appendix, Table A.4).

Results should also be stronger in less developed credit markets where constraints are more binding. In Panel B of Table 6, we split the countries in our sample by the size of their credit market 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 is larger for both leverage (-0.1354 vs. -0.0436) and asset growth (-0.2230 vs. -0.0752). Fauver, Houston, and Naranjo (2003) argue that industrial diversification is more valuable in less developed financial markets. In line with this, we find that the loss of business group affiliation and, hence, the loss of diversification in the form of a partner in an unrelated industry is more costly in less developed credit markets.[10]

[10] In the Appendix (Table A.5), we also show that our results are stronger in countries with relatively low investor protection as measured by the anti-self-dealing index of Djankov, La Porta, Lopez-de-Silanes, and Shleifer (2008). Hence, our results are related to the development of financial markets as measured by the depth of debt markets and the protection of minority shareholders.

In Table 7, we look at whether banking relationships are affected by becoming stand-alone. In IV regressions, we find that becoming stand-alone has a strong positive effect on both the creation and destruction of banking relationships (no effect in OLS). 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, the evidence of a strong reshuffling of banking relationships is suggestive of more binding credit constraints for firms that become stand-alone.

3.2 Cross-pledging

Following these tests of generic credit constraints, we focus on the precise mechanisms through which business groups can relax credit constraints. Their financing advantage can reflect cross-pledging between firms. A firm faces less binding credit constraints if leveraging on the assets or cash flows of the other firm in the group. From the contractual point of view, this can be achieved through collateral cross-pledging, using the assets of one firm as collateral for the debt of the other or transferring cash flow between firms through intra-group loans or accounts payable (Ghatak and Kali, 2001; Hart and Moore, 1998).

The channel we propose is related to collateral cross-pledging, which disappears as a possibility when the firm is standalone, but not when the firm remains in a group. We proxy for collateral using tangible assets (e.g., real estate). In Panel A of Table 8, we use the relative tangibility of the two firms in the group to divide the sample according to its potential for collateral cross-pledging. A firm that becomes stand-alone should suffer more in terms of debt capacity and, consequently, growth when the other firm added relatively more tangible assets to the group. In a sense, it loses the financial "subsidy" it was receiving as 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 lose a high-tangibility partner.

Following Hann, Ogneva, and Ozbas (2013), we test for the effects of cash-flow cross-pledging using the correlation of cash flows across firms. As the correlation decreases, total cash flow in the group should be more stable and financing capacity should increase (see Tirole, 2006, chapter 4, for a formal model; and Khanna and Yafeh, 2005). A firm that becomes stand-alone should be more affected if the partner it loses is a low-correlation partner. In Panel B of Table 8, we divide the sample according to the sales correlation of the industries of the firms in the group. We do not find evidence of cash-flow cross-pledging. In fact, the decrease in leverage is marginally stronger in the high-correlation sample. In Section 4.1.1 on the exclusion restriction, we show more evidence against the existence of cash-flow risk-sharing in our setup.

3.3 Misallocation in Business Groups

Relaxing credit constraints is good if firms underinvest as compared to their first best but can be bad if they invest in poor projects that credit markets would otherwise not fund. The prediction for our setup is that inefficient firms which are subsidized in groups should be hurt the most when they become stand-alone.

In Table 9, we explore several proxies for being inefficiently subsidized. In Panel A, we divide firms using their performance (OROA) relative to industry peers at the beginning of the sample. We find that the reduction in leverage occurs mostly in firms with below-peers initial performance (coefficient -0.1873, t-stat 1.98). In fact, firms with high performance relative to their industry peers see their leverage ratios increase after becoming stand-alone (coefficient 0.0972, t-stat 1.84). This suggests that high-performing firms can increase leverage once free from the burden of less profitable partners and, consequently, not all firms benefit from belonging to a group. Differences in asset growth point in the same direction, but the coefficients are not statistically significant in either sample. In Panel B, we divide firms according to their initial leverage relative to industry peers and find a larger reduction in leverage among high-leverage firms. One possible interpretation is that high-leverage firms received more credit from banks precisely because they were part of a group. We do not find significant results for asset growth in these subsamples.

In Table 9, Panel C, we divide firms according to their characteristics relative to the other firm in the group. This is a measure of relative standing within the group, not within the industry as in previous panels. Following Rajan, Servaes, and Zingales (2000), we use the divergence in Tobin's Q between the industries of both firms in the group to divide the sample. These authors find that the degree of subsidization is stronger when the divergence in Tobin's Q is bigger. Our results point in the same direction since the reduction in leverage after becoming stand-alone is larger in those firms where the Tobin's Q of the other firm in the group is higher (-0.1105 vs. -0.0231). Firms with a low Q relative to their partners are stronger candidates for receiving subsidized funding when belonging to a group and naturally suffer the most when becoming stand-alone. We do not find a similarly strong effect on asset growth. When the sample is divided according to the relative OROA of both firms in the group (Panel D), 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, due partly to the smaller sample size.

3.4 Average Effect on Performance

The theoretical literature does not give a strong prior regarding the *average* effect of becoming stand-alone on performance. This is because of the interplay of the costs and benefits of business groups. By becoming stand-alone, a firm may lose access to financing but may also gain focus and cut value-destroying investments.

In private firms, we can only rely on profitability as a measure of performance (Bennedsen, Nielsen, Perez-Gonzalez, and Wolfenzon, 2007). Table 10, Panel A shows that becoming stand-alone does not have a statistically significant effect on OROA. The first stage is as strong as in our main results for leverage and asset growth so this lack of significance cannot be attributed to weak instruments. It is true that the OROA sample is smaller than our main sample since OROA is missing in many cases but, given the theoretical ambiguity, it is 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).

The overall insignificant effect on performance can also serve as a reality check in the style suggested by Jiang (2017). A strong effect on performance would raise the question of why firms stay affiliated to groups if so much value can be created by splitting. The negligible average effect on performance suggests an equilibrium where there are both affiliated and unaffiliated firms.

4. Additional Results

4.1 Examination of IV assumptions

Our empirical design combines differences-in-differences with instrumental variables. Figure 5 shows that the parallel trends assumption of differences-in-differences holds for treated and control firms, prior to treated firms becoming stand-alone. We now explore the assumptions behind the instrumental variables design in more detail.

4.1.1 Exclusion restriction

The exclusion restriction means that $OtherShock_{it}$ does not belong to the second stage equation on its own merit, or in other words, that the entire effect of $OtherShock_{it}$ is channeled through the stand-alone status of the firm. Ultimately, the exclusion restriction cannot be tested since the regression of firm outcomes on $OtherShock_{it}$ is also the reduced-form version of our results. However, we do find suggestive evidence that $OtherShock_{it}$ does not have a direct effect on firms' outcomes, at least in our sample.

A theoretical threat to the exclusion restriction would be the presence of active cash-flow risk-sharing between group firms, which was already studied using the sales correlations shown in Table 8. In this case, cash flows are transferred from one firm to another, for example through intra-group loans, in order to smooth transitory shocks. Consistent with this type of risk-sharing, Lamont (1997) shows that non-oil segments of big oil conglomerates reduce investment in response to negative oil shocks. 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 further examine this hypothesis in several ways. First, we examine the behavior only of *control* firms. If $OtherShock_{it}$ has a direct effect on firms' outcomes, other than through changes in business group affiliation, effects should also be seen in firms that remain in a group. 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 where there was not a shock to the other firm. Instead, Figure 5 shows that leverage is very similar across control firms regardless of whether there was a shock to the other firm in the group. The difference is not statistically significant. By the same token, differences in asset growth for years t=1 and t=3 are not significant between the two types of control firms. In year t=2, firms where there was a shock to the other firm have higher growth, rather than the lower growth predicted by the cash-flow risk-sharing hypothesis. Albeit transitory, this effect should, if anything, bias the results against our main finding. Our conclusion from Figure 5 is that shocks to the other firm do not have a relevant effect on firms that remain in a group, which supports the exclusion restriction.

Intra-group loans are often hard to identify in the data, sometimes because regulatory disclosure requirements are lax. However, they are typically included in the borrowing firm's balance sheet in items such as "other debt" or "other accounts payable" and in that of the lending firm in "other assets" or "other accounts receivable". The literature uses these entries as proxies for intra-group loans (Jiang, Lee, and Yue, 2010). If there is active borrowing and lending between group firms in response to shocks, "other debt" should increase in response to own shocks as the firm borrows from the partner firm and "other assets" should increase in response to shocks in the other firm as it lends to the partner firm. This possibility is examined in Table 11 (Panel A), again within the sample of control firms. We use the ratios of "other current debt", "other non-current debt" and "other current assets" to total assets as dependent variables and find no association between these types of debt or assets and our shocks, which again supports the exclusion restriction.

As a second test, we look at what we call the "early sample", which includes treated firms before they become stand-alone and their matched control firms for the same years. The entire sample period for each firm cannot be used because, in that case, the test would reflect the reduced-form version of the IV results. We focus exclusively on years t-1 and earlier (where year t is the year the firm becomes stand-alone). This early sample allows us to test the idea that firms react to shocks by borrowing and lending among themselves or by selling assets to each other, before becoming stand-alone. If this is the case, shocks propagate through several mechanisms of which becoming stand-alone is only the most drastic.

Table 11 (Panel B) shows the results of regressions with the early sample using as explanatory variables $OtherShock_{it}$ and the interaction of $OtherShock_{it}$ and a dummy variable for the firms that eventually become stand-alone. We find that $OtherShock_{it}$ has no explanatory power in any of the regressions (for "other current debt," "other non-current debt" and "other current assets") and the interaction with the treated dummy is also insignificant. This suggests that firms do not borrow from their partners in anticipation of becoming stand-alone.

There are at least three possible explanations for the absence of a direct impact of shocks other than through stand-alone status. First, the shocks we study are relatively large profitability shocks, representing long-lasting changes in an industry's economic outlook. It is not obvious that firms would find it optimal to smooth these shocks or 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 have an impact at a much higher frequency, or to smaller shocks relative to firm size could be different. Second, as argued by Whited (2001), the effect of other-firm cash flow that has been documented in earlier studies is often contaminated with measurement error and by the relatedness of firms. At least in our sample we exclude, by construction, firms in related industries so it is perhaps not too surprising that we do not find a direct impact of shocks. Finally, the previous literature on cash-flow risk-sharing deals mostly with conglomerates of fully-owned divisions (e.g., Lamont, 1997). It is contractually harder to transfer cash flows between independent corporations in business groups, which are subject to the limits often imposed by regulation, covenants, and bank monitoring.

Besides cash-flow risk-sharing there are demand-side mechanisms that could violate the exclusion restriction. In particular, demand spillovers can make the shocks to the other firm 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. The shock to construction could, therefore, enter the second stage regression of the food producer, regardless of its stand-alone status. Although demand spillovers are hard to measure because they involve cross-industry effects, we argue that they should be stronger in industries with a high income elasticity. 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 divide the sample based on whether firms produce durable goods or not, which we use as a proxy for high income elasticity.[11] If our results are explained by demand spillovers, they should be concentrated in industries with high elasticity such as car manufacturing. As seen in Table 12 Panel A, the effect of becoming stand-alone is not stronger among firms in high elasticity industries, which speaks against the idea of demand spillovers. This can also be expected because the housing crisis, arguably the largest source of income effects, accounts for only a small subset of our shocks.[12]

A second possibility is that demand in a geographical area is affected by industrial shocks, which could be particularly relevant for firms in services. Similarly, banks in an area can be affected by the poor performance of a given industry, consequently affecting other industries in the area by restricting the supply of funds. We address this by forming two samples depending on whether both firms in the business group are located in the same city or not. As seen in Table 12, Panel B, coefficients in groups with both firms in the same city are similar to those in groups with firms in different cities.

Overall, our results suggest that the exclusion restriction is not violated by cash-flow risk sharing between firms or by demand spillovers.

[11] High elasticity industries include construction (SIC 15-17), manufacturing of durable goods (SIC 24-25, 32-38), wholesale trade in durable goods (SIC 50), retail trade in durable goods (SIC 52, 55, and 57) and real estate (SIC 65).
[12] The housing crisis –decline in construction and real estate— that affected Europe during this period propagated to seven four-digit SIC codes, representing 21 SIC-year shocks out of the 359 shocks in Table A.1.

4.1.2 Balance

One important concern with the implementation of IV strategies is the "as if" random assignment between treated and control groups (Atanasov and Black, 2016). In line with covariate common support, the treated and the matched control firms appear to be very similar in several dimensions before treatment (Table 2, Figures 3 and 5, and the more formal analysis in Table A.6, Panel A, of the Appendix).

A subtler concern is that, while our sample may display covariate balance for the treatment assignment in the second stage regression, it may not be balanced regarding the assignment of the instrument in the first stage. In other words, it is possible that firms affected by the instrument are different from those not affected by it. We examine this possibility by comparing firms' characteristics prior to receiving the "other shock" (and, for the treated firms, also prior to becoming stand-alone). The main variables are not on average significantly different across these firms (Table A.6, Panel B). The only marginally significant difference (at the 6% level) is that firms with partners that receive shocks have fewer assets. A common remedy for an imbalance of this type is to trim the sample to have common support. In fact, if we drop the very small firms (assets of less than 100,000 euros), we obtain covariate balance in all variables including assets (Table A.6, Panel C). For the sample of firms with assets above 100,000 euros, we repeat the IV regressions and find estimates very similar to the ones obtained with main sample (Table A.7). This suggests that the small differences in asset size according to the instrument are inconsequential for our findings.

Regarding this concern for balance, we also explore an alternative matching procedure. We first select firms that have a partner in the group with a shock. We then match those firms to other firms with partners that do not receive shocks. In simple, we match on the instrument instead of the treatment. The resulting sample is 40% larger than with our preferred method but has fewer firms moving to stand-alone status and fewer "compliers" (firms that are induced to become stand-alone because of shocks to their partner). In unreported results, we find that the $OtherShock_{it}$ is still significant at the 1% level in the first stage, but the first stage is in general weaker: the magnitude of the effect and the R-squared are smaller than with our main method. In the second stage, the coefficients are very similar, implying that matching based on the instrument or treatment is inconsequential for our main finding. However, when we match based on the instrument the statistical significance is lower, so our method, because of the higher fraction of compliers, has more power. Our method also gives a more balanced sample in terms of covariates.

A final way to assess the "as if" random assignment is to run an "intent-to-treat" differences-in-differences (DiD). The intent-to-treat DiD gives the average effect of the shock for all firms, while the IV gives the effect for compliers alone. Results for the intent-to-treat DiD are in line with what would be expected (Table A.8). The DiD coefficient for the other shock variable is approximately a tenth of the IV coefficient for stand-alone, which makes sense since the probability of treatment given these shocks is about 10% as seen in the first stage (Table 3). The DiD results are significant at least at the 5% level. Importantly, these results are robust to the inclusion of additional covariates, which is indicative of quasi-randomly assigned instruments (Atanasov and Black, 2017).

4.1.3 The magnitude of IV coefficients

The magnitude of IV coefficients, in particular in relation to OLS coefficients, is a potential warning sign of violations of IV assumptions. Jiang (2017) shows that most studies in her survey report IV estimates that are larger than OLS estimates, even if there is no *a priori* reason to expect a bias in that direction. Our results in Table 4 fall into the category of larger-IV-than-OLS estimates. However, as explained above, we expect endogeneity to dampen the effects of stand-alone status in

OLS since firms that leave a group are endogenously more likely to gain little from business group affiliation.

Even if there are good reasons to expect the IV estimates to be larger than the OLS estimates, another concern is that the former may not be representative of the entire population. This is because they only reveal a local average treatment effect (LATE), not the true population average treatment effect (ATE). In our case, the IV estimates represent the effect of becoming stand-alone on compliers. Non-compliers, i.e., firms that remain in groups despite the industrial shocks to their partner, fail to respond to the instrument potentially because the benefits of group affiliation are too large. Since compliers are likely to be firms with smaller gains from business group affiliation, we expect the LATE to be smaller than the true ATE. Hence, it is unlikely that the significant changes in leverage and asset growth we find are the result of our IV results overestimating the true population effects.

4.1.4 External Validity

Most of the literature on business groups studies large groups with multiple listed firms (e.g., Khanna and Yafeh, 2007). The groups that we study, instead, are small groups with private firms. Focusing on small groups facilitates identification of causal effects. However, precisely because our setup is not typical of previous work, it raises the problem of external validity.

Small groups can shed light on the behavior of more complex groups since the underlying trade-offs are analogous in both cases. The agency problem between an owner and creditors, which produces credit constraints, is still present in our sample. The groups in our sample are also industrially diversified like the majority of large groups (Khanna and Yafeh, 2007).

Our sample has three main differences with respect to other samples studied in the literature. First, some of the agency problems of listed firms are not present in our sample of private firms. For example, we cannot study tunneling, which is a concern in groups with many listed firms and low ownership concentration. The very high controlling stakes of our sample (above 95%) mean there is little incentive to expropriate minority shareholders. However, although ownership is not dispersed, there is still the possibility of capital misallocation because of owner preferences (i.e., pet projects).

Second, the large groups studied in other papers have the additional complication of general equilibrium effects. Business groups often represent non-trivial parts of the economy and, with their market power, influence the price of goods and capital (Almeida and Wolfenzon, 2006b; Morck, Wolfenzon and Yeung, 2005). The absence of general equilibrium effects means that our setting of small groups provides cleaner estimates of the effects of business group affiliation.

Third, because our sample period (2009-13) coincides with that of recovery from the 2008-9 financial crisis, it may not be fully representative of distressed conditions. However it does include episodes of negative GDP growth experienced by some European countries due to the 2011-12 banking crisis (e.g., Spain, Ireland, Greece and Portugal).

4.2 Alternative Hypotheses

Finally, we explore several alternative hypotheses where groups matter, but not because of their ability to raise more funds by cross-pledging collateral.

A first alternative hypothesis relates to the loss of owner's wealth produced by our shocks due to the decrease in the equity value of the affected firm. Wealthier owners can raise more on financial markets than their less wealthy equivalents, even if the firm being studied is not directly affected by the shock (Tirole, 2006). Hence, the wealth channel could explain the capital structure effects we find.

One problem with the wealth interpretation is that the effects we find are specific to firms that become stand-alone. Similar firms that remain in a business group also face shocks to the other firm in the group and their owners also suffer wealth losses, but they do not experience the same reductions in leverage and asset growth as firms that become stand-alone (Figure 5).

The lack of an effect of owner's wealth on firms that remain in a group is perfectly consistent with a collateral channel. When a firm becomes stand-alone, the assets of the other firm are no longer pledgeable whereas a firm that remains in a group can still use the other firm's assets as collateral even if its equity value has decreased. The value of collateral (e.g., real estate) is, moreover, smoother than equity values and is not necessarily affected by equity shocks.[13] For example, a shock to the price of milk, although affecting a dairy farm's cash flows, is not immediately translated into a shock to the value of its land. If collateral is not affected, then the debt capacity of the firm (under the collateral cross-pledging hypothesis) stays relatively constant despite the shock to the owner's wealth. Moreover, loans are often over-collateralized and fluctuations in the value of collateral, even if they occur, do not typically lead to debt renegotiation.

In Table 13, we attempt to distinguish between the effects of collateral and wealth. We cannot directly compute the loss of owner's wealth since we cannot observe the market value of equity in private firms. However, we suggest that the size of the wealth loss is related to the ratio of the book value of the other firm's equity to the sum of the book value of both firms' equity. Since tangible assets and equity values are related by accounting identities, this ratio is correlated to the ratio of tangible assets used in Table 8 as a proxy for the size of the collateral loss. Therefore, we use the difference between the equity ratio and the tangibility ratio to distinguish between the two

[13] We also find empirical evidence that our shocks are not correlated with collateral value. From *Eurostat*, we collect data on house prices at the country level as a proxy for real estate values. We find an insignificant correlation of yearly growth rates in house prices and the frequency of shocks by country. Countries with worse housing market crises during this period (e.g., Ireland, Italy or Spain) do not show a higher frequency of our shocks.

mechanisms. A positive difference implies that the wealth loss is stronger than the collateral loss while a negative difference implies the opposite. As seen in Table 13, the effect on leverage is concentrated in firms with a strong collateral loss, rather than in firms whose owners suffered a strong equity loss. Overall, the evidence supports the collateral interpretation and not the wealth interpretation.

A second alternative hypothesis concerns the controlling shareholder's risk tolerance. After a negative shock, owners whose wealth has decreased may decide to reduce risk by cutting leverage and asset growth. We analyze this possibility in two ways in Table 14. First, in Panel A we run regressions with the ratio of cash holdings to assets as the dependent variable. The IV coefficient for stand-alone firms is negative (-0.0643) and significant at the 10% level. Therefore, if anything, there is evidence of reduced cash holdings as opposed to the cash hoarding predicted by the risk tolerance hypothesis. A second approach is to divide our sample into subsamples with different levels of industrial volatility. If the reduction in leverage and asset growth is explained by lower risk tolerance, then our results should be stronger among firms in more volatile industries. Instead, as shown in Table 14, Panel B, they are concentrated in less volatile industries.

Finally, taxes can be a powerful incentive to form business groups. In particular, the losses of one firm can reduce the tax burden of the controlling shareholder by helping to offset the other firm's earnings. This could explain the fall in asset growth after firms become stand-alone because the incentives to invest are dampened. Taxes should be more relevant for pairs of firms with low cash-flow correlations because they are particularly well placed to take advantage of the tax shield provided by each other's losses. However, when sorting firms according to cash-flow correlations (Table 8), we do not find strong differences. The point estimate of the effect of becoming stand-alone on asset growth is even stronger in the high correlation sample, in contrast to what the tax hypothesis would suggest.

5. Conclusions

We estimate the causal effect of business group affiliation using a sample of groups composed of only two firms in 2009-2013. Some of these groups split up during this time, leaving firms as stand-alones. We instrument for 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 since the industrial shock to the other firm could otherwise affect the firm of interest and violate the exclusion restriction in an instrumental variables setting.

We find that firms that become stand-alone reduce leverage and asset growth. This 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 collateral cross-pledging, and when the firm operates in a debt-dependent industry or a country with low domestic credit. In line with misallocation in business groups, the firms more affected by becoming stand-alone are those with a poor previous performance and high leverage relative to their industry peers, and those with a low industry Tobin's Q relative to the other firm in the group. Our results underscore the important consequences of affiliation to a business group for financing and investment policies.

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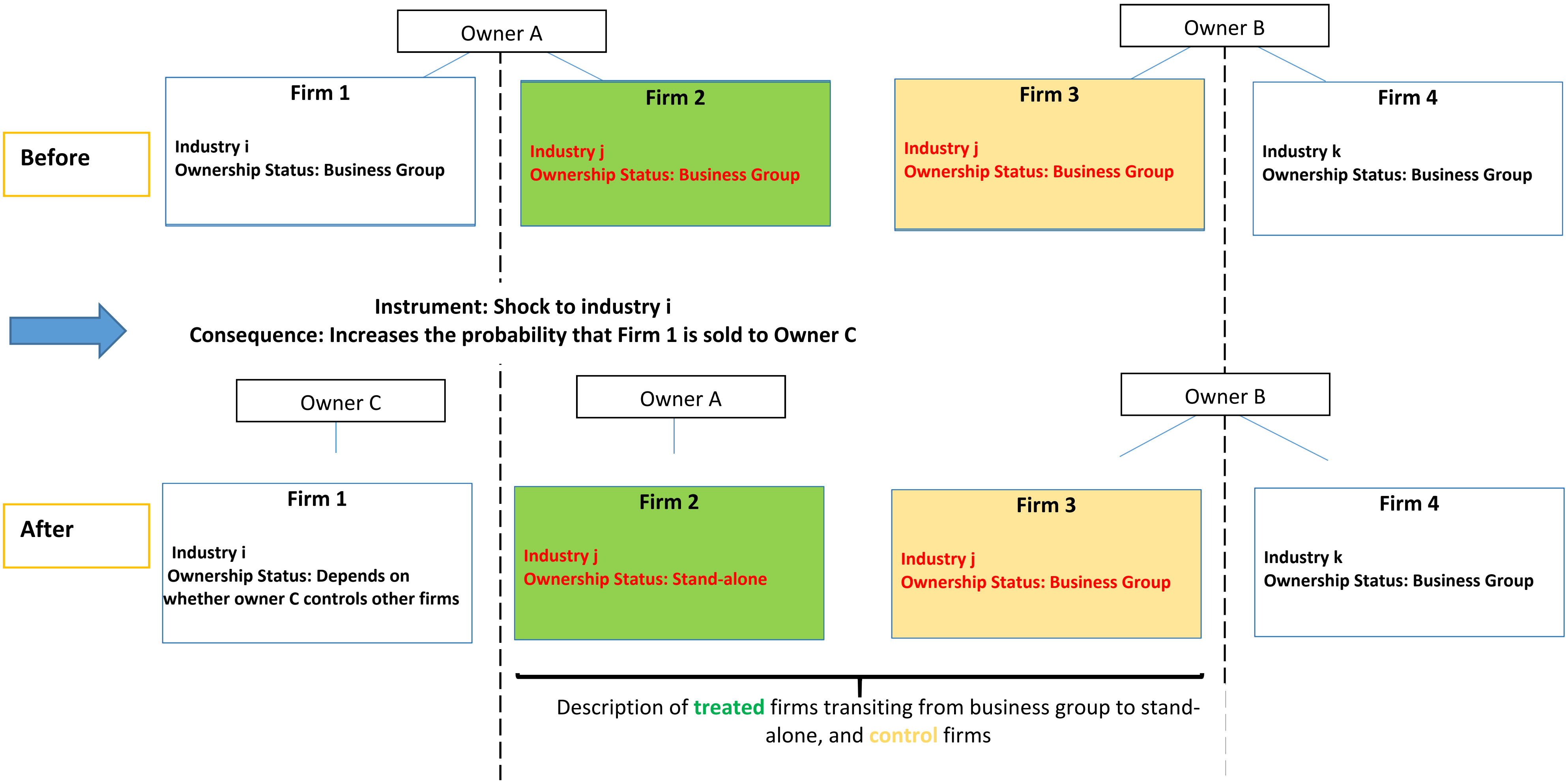
Figure 1: Empirical design

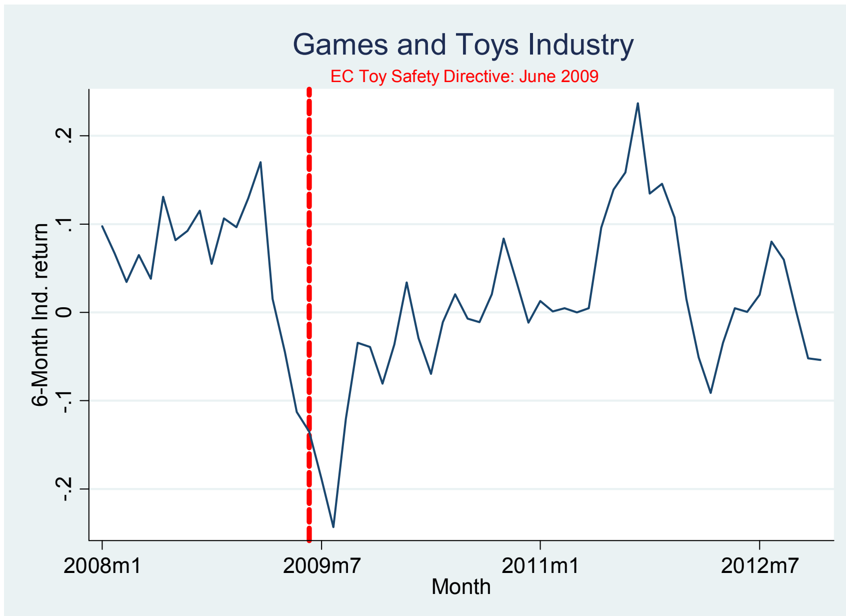


Note: There may, or may not be shocks to industry i (seen in the figure) and to industry k (not seen in the figure). These are "other shocks" in equation (1), and they represent the instrument in the first stage regression: $StandAlone_{it} = \theta\ OtherShock_{it} + \pi\ OwnShock_{it} + \rho' X_{it-1} + \mu_i + \tau_t + \vartheta_{it}$. Shocks to industry k help improve the first stage estimation. There may, or may not be shocks to industry j (not seen in the figure), for treated firms (firm 2 in the figure) and control firms (firm 3 in the figure). These shocks are "own shocks" which have a direct effect on the second stage regression— equation (2): $Y_{it} = \beta\ StandAlone_{it} + \gamma\ OwnShock_{it} + \delta' X_{it-1} + \mu_i + \tau_t + \varepsilon_{it}$ — and as a consequence they are also included in the first stage regression (equation 1)

Figure 2: Examples of Industrial Shocks

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 firms' returns in the industry. 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

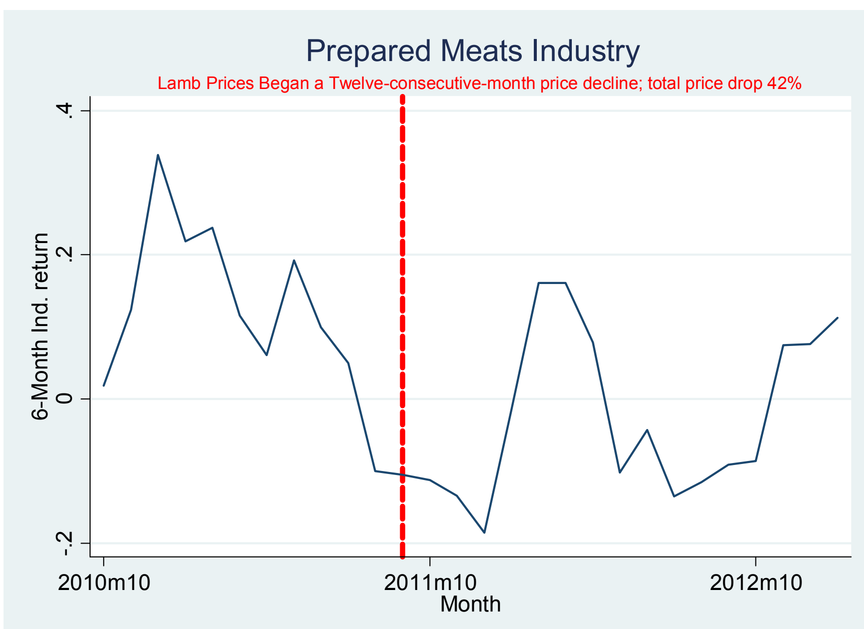


Figure 3: Size densities of treated and control firms

This figure shows the kernel distribution of assets for two set of firms: Firms that belonged to a two-firm business group during the whole sample period (control firms); and firms that belonged to a two-firm business group at the beginning of the sample, but then become stand-alone (treated firms).

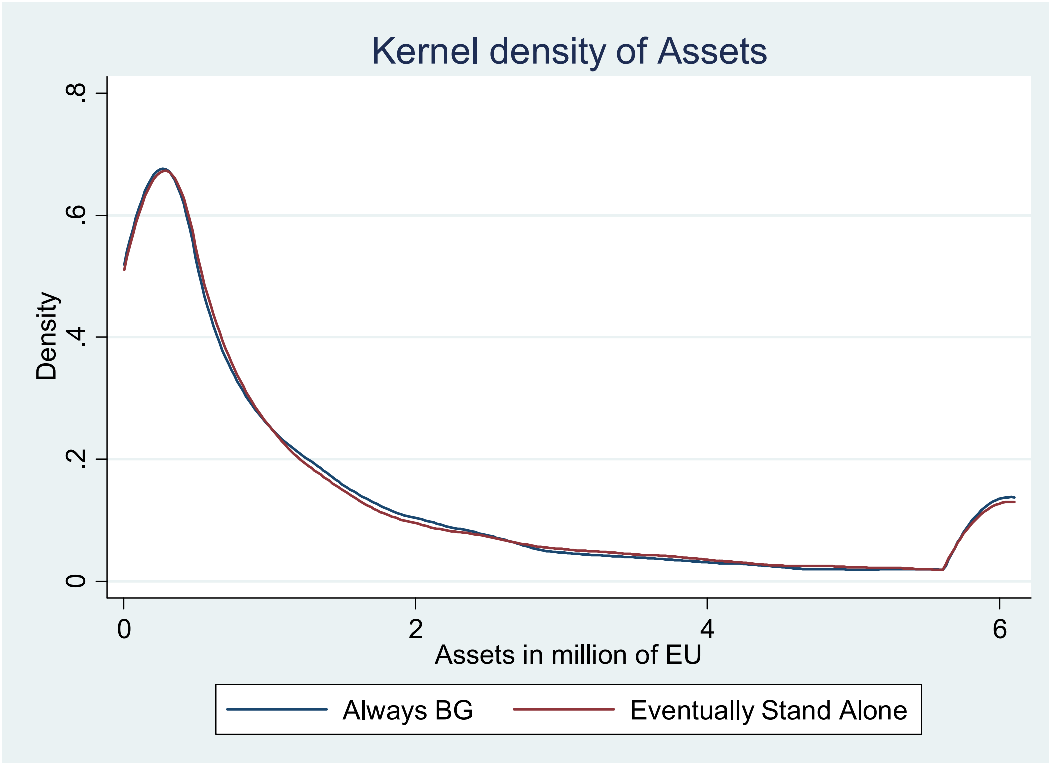


Figure 4: Pairwise distribution of firms' industries

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

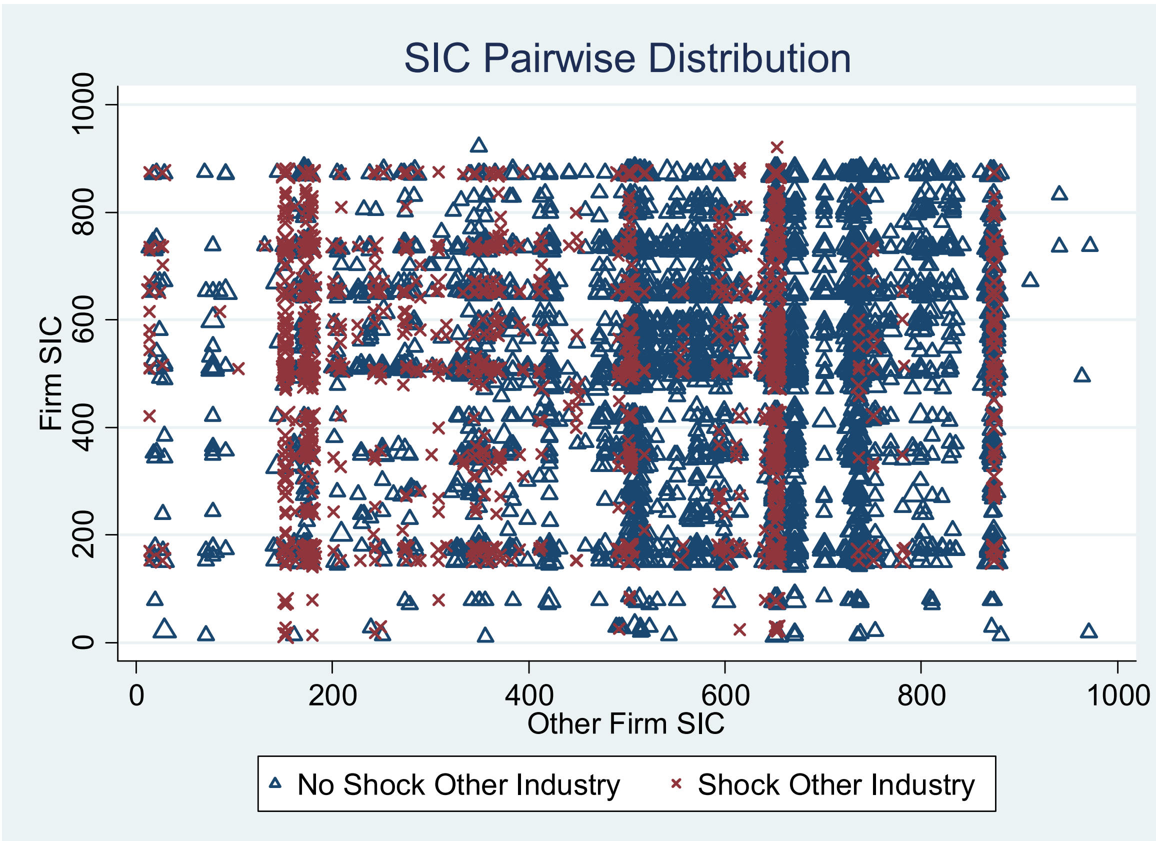
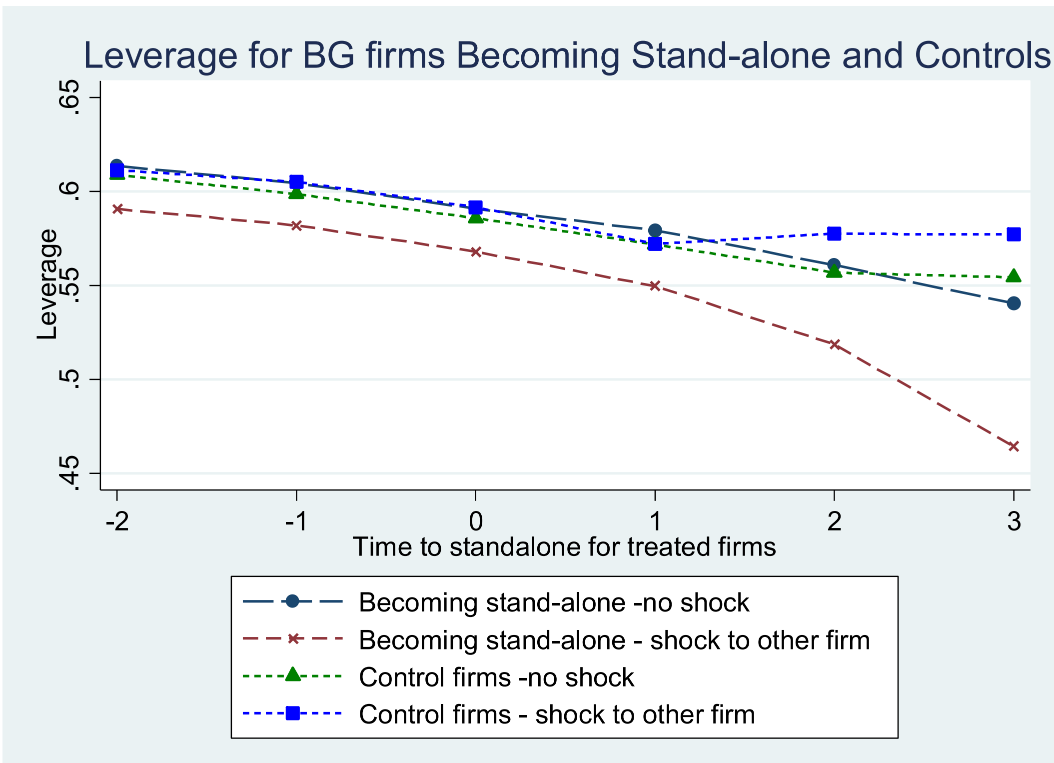


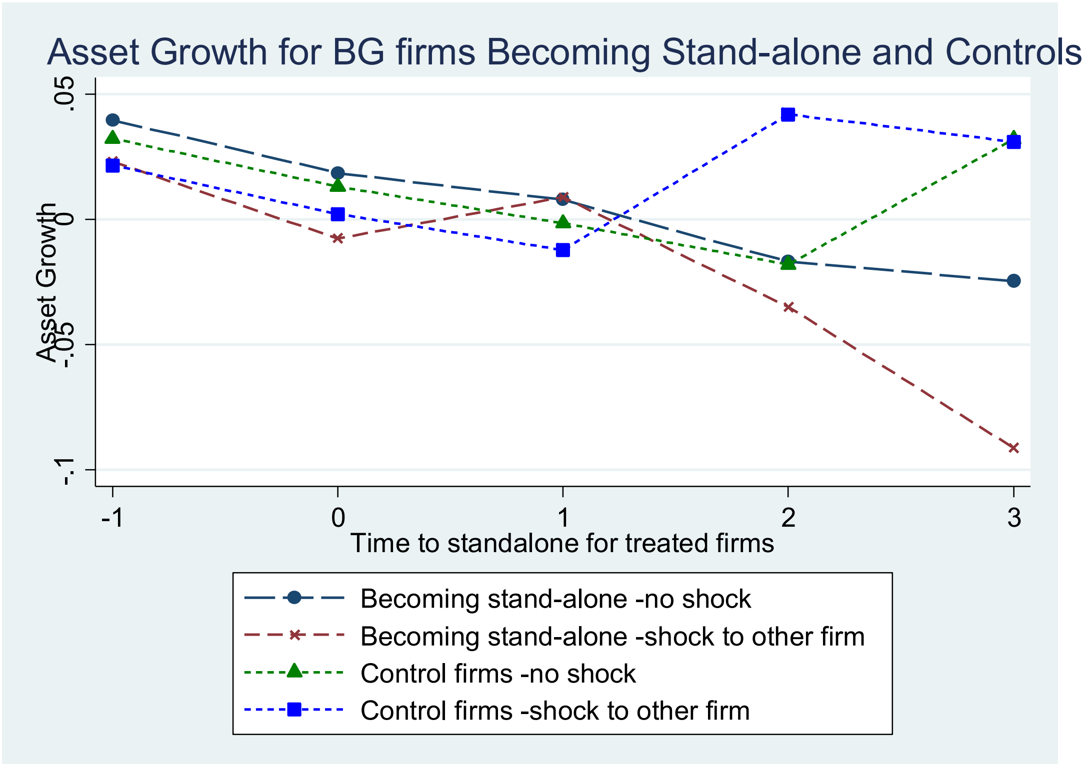
Figure 5: Event study

This figure shows the evolution of leverage (Panel A) and Asset Growth (Panel B) for firms the sample. For each treated firm we define event years relative to stand-alone status (t=0 is the year in which treated firms become stand-alone). 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. We differentiate between treated firms that face shocks to the other firm before they split (before t=0) and treated firms that do not. Similarly, we differentiate between control firms that face shocks to the other firm (before time 0) and control firms that do not. To emulate within firm differences, we compute the average change in firm leverage (asset growth) between event years for each group, 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
Asset Growth
-1
0
1
2
3
Time to standalone 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: Sample description

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 two-firm business groups). The upper part of the table displays the distribution of observations by years; the middle part displays observations by country; and the lower part by two-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 2: Summary statistics

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 two-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. 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 three-digit SIC code. Sales/Assets is the ratio of annual sales divided by assets. OROA is EBIT divided by assets. 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 two-firm business group. 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. Stake is the controller's ownership stake in the firm. 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 firm's business group operates in a year and industry were we identify an industrial 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. Relative OROA is the OROA of the companion firm divided by the OROA of the focal firm. Wealth-collateral exposure is $E_{other}/(E_{own}+E_{other})- T_{other}/(T_{own}+T_{other})$, where E stands for the dollar amount of book equity and T for the dollar amount of tangible assets (PP&E). 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 three-digit sic-code using US data. Industry 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 discontinues a relation with a 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
	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
	Sales/Assets	1.84	1.42	1.71	9,495
	OROA	0.05	0.04	0.14	6,437
	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 retirement	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
	Wealth-collateral exposure	-0.02	0.00	0.30	13,533
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 (million)	1.51	0.68	1.87	15,762
	Asset Growth	0.02	0.00	0.23	12,358
	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
	Sales/Assets	1.83	1.38	1.74	9,345
	OROA	0.05	0.04	0.15	6,722
	Stand-alone	0	0	0	15,762
	Debt (million)	1.00	0.41	1.37	13,680
	Debt issue	0.30	0.00	0.46	9,932
	Debt retirement	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
	Stake	98.62	100	6.74	15,762
Shocks	Own Shock	0.26	0.00	0.44	15,762
	Other Shock	0.23	0.00	0.42	15,762
Relative-to-pair characteristics	Relative Tangibility (other/own)	6.83	0.30	53.4	15,033
	Sales Correlation	0.53	0.73	0.46	15,762
	Relative Tobin's Q (other/own)	1.39	1.02	1.89	15,762
	Relative OROA (other/own)	1.14	1.07	0.52	7,155
	Wealth-collateral exposure	-0.03	-0.01	0.30	13,704
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 3: First stage regressions

Columns 1-3 present results for the first stage regressions of the leverage (second stage) 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. 1 and 3 do not have any additional controls besides firm and year fixed effects. The results from col 1. differ from those in col. 2, in that in the later we restrict the sample to that available when including the additional controls used in col. 3. Controls used in col. 3 include the firms' industry Tobin's Q, lagged Tangibility and lagged logarithm of assets. Standard errors are adjusted by heteroscedasticity and clusters at the (three-digit) sic-by-country level. Significant at the *10%, **5%, ***1%.

First Stage - Leverage Regressions Sample

Variable	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***
	(0.0213)	(0.0203)	(0.0203)
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	83.18	83.46	83.17
R-squared (whithin)	0.3905	0.317	0.317
N	27210	19150	19150

Table 4: Second stage regressions

Panel A presents the second stage (IV) estimates of stand-alone, instrumented with Other Shock. Cols 1-3 show results using leverage as dependent variable, while columns 4-6 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 (three-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 5: Decomposing the effect on leverage

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 4. 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 (three-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 6: Mechanisms – credit constraints

Panels A-B present second stage (IV) estimates of stand-alone 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 (three-digit) sic-by-country level. Significant at the *10%, **5%, ***1%.

Panel A -Sample Split by 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 by 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 7: Creation and destruction of banking relationships

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 (three-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 8: Mechanisms – cross pledging

Panels A-B present second stage (IV) estimates of stand-alone 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 (three-digit) sic-by-country level. Significant at the *10%, **5%, ***1%.

Panel A -Sample Split by 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 by 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 9: Capital misallocation

Panels A-D present second stage (IV) estimates of stand-alone, instrumented with Other Shock for different sample splits. Panels A and B split the sample 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 Panels C and D we split the sample according to firms' relative standing within their group. In Panel C we split the sample using above and below the median Relative Tobins' Q. In Panel D 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 (three-digit) sic-by-country level. Significant at the *10%, **5%, ***1%.

Panel A

Sample Split by 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 by 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)

Panel C
Sample Split by Relative Tobin's Q (other/own)

Variable	Low Relative Other Tobin's Q		High Relative Other Tobin's Q	
	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 D
Sample Split by Relative OROA (other/own)

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

Table 10: Performance effects of becoming standalone

This table examines firms' performance (OROA in Panel A, and Sales over assets in Panel B). Col. 1 presents the second stage (IV) estimates of stand-alone, instrumented with Other Shock. Col. 2 presents the OLS estimate. In the bottom of col. 1 we present the coefficient of the instrument, Other Shock, in the first stage regression. Standard errors are adjusted by heteroscedasticity and clusters at the (three-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 11: Testing for possible violations of the exclusion restriction

In this table we present regressions studying the direct effect of shocks on three dependent variables: Other current debt over assets, other non-current debt over assets, and other current liabilities over assets. Panel A considers only the control sample. Panel B considers both the treated and control samples, but includes periods from before the treated firms become standalone (i.e., the "early" sample). Standard errors are adjusted by heteroscedasticity and clusters at the (three-digit) sic-by-country level. Significant at the *10%, **5%, ***1%.

Panel A: Control firms

Variable	(Other current debt)/Assets	(Other non-current debt)/Assets	(Other current assets)/Assets
Other Shock	0.0174	-0.0040	-0.0092
	(0.0383)	(0.0048)	(0.0096)
Own Shock	-0.0545	-0.0053	0.0007
	(0.0485)	(0.0049)	(0.0067)
Firm Fixed Effects	Yes	Yes	Yes
Year Fixed effects	Yes	Yes	Yes
Additional Controls	Yes	Yes	Yes
R-squared (whithin)	0.0011	0.0013	0.0046
N	7262	8819	8964

Panel B: Early sample

Variable	(Other current debt)/Assets	(Other non-current debt)/Assets	(Other current assets)/Assets
Other Shock	-0.0041	0.0017	0.0126
	(0.0156)	(0.0064)	(0.0113)
Other Shock x Treated	-0.0333	0.0061	-0.0131
	(0.0266)	(0.0104)	(0.0171)
Own Shock	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes
Year Fixed effects	Yes	Yes	Yes
Additional Controls	No	No	No
R-squared (whithin)	0.0020	0.0019	0.0006
N	9240	11623	11968

Table 12: Testing for demand and geographic spillovers

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 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). In Panel B we split the sample according to whether firms in a group operate in the same city or in a different city. Standard errors are adjusted by heteroscedasticity and clusters at the (three-digit) sic-by-country level. Significant at the *10%, **5%, ***1%.

Panel A: Sample Split by Income 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)

Panel B: Sample Split by Group Firms in Same vs Other City

	Same City		Other City	
Variable	Leverage	Asset Growth	Leverage	Asset Growth
Stand-alone	-0.1211**	-0.1980**	-0.0822	-0.2931*
	(0.0557)	(0.0975)	(0.0877)	(0.1776)
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	11176	13162	4266	5045
First-Stage				
Other Shock	0.1103***	0.0976***	0.0928***	0.0946***
	(0.0180)	(0.0168)	(0.0331)	(0.0292)

Table 13: Wealth and Collateral

This table presents second stage (IV) estimates of stand-alone, instrumented with Other Shock for sample splits based on the relative potential loss of owners' wealth and collateral due to other shock. We construct the variable Wealth-collateral exposure as $E_{other}/(E_{own}+E_{other})$- $T_{other}/(T_{own}+T_{other})$, where E stands for the dollar amount of book equity and T for the dollar amount of tangible assets (PP&E). We split the sample for firms with above and below the median of this variable. Firms with Wealth-collateral exposure above the median are more "equity exposed" to the other shock and firms with Wealth-collateral exposure below the median are more "tangibility exposed" to the other shock. Standard errors are adjusted by heteroscedasticity and clusters at the (three-digit) sic-by-country level. Significant at the *10%, **5%, ***1%.

	Sample Split by Wealth-collateral exposure			
	High wealth exposure		High collateral exposure	
Variable	Leverage	Asset Growth	Leverage	Asset Growth
Stand-alone	-0.0562	-0.1409	-0.1377**	-0.1367
	(0.0775)	(0.1141)	(0.0547)	(0.1004)
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	9430	10020	9266	9950
First-Stage				
Other Shock	0.1068***	0.1081***	0.1027***	0.0982***
	(0.0256)	(0.0238)	(0.0180)	(0.0172)

Table 14: Testing for a risk-tolerance explanation

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 (three-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 by 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)

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 TiO2, 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: Sample Selection of Industrial Shocks

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 four-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 three-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 three-year events. We define shocks as three-year events, starting the year of the shock and up to two 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 three-year events	7,746
# of commodity shocks	322	% of obs. affected by own industry shocks -defined as three-year events	24.3%
# of regulatory shocks	37	# of obs. affected by other industry shocks -defined as three-year events	7,141
		% of obs. affected by other industry shocks -defined as three-year events	22.4%

Table A.2: M&A Activity and Shocks

This table presents results relating mergers and acquisition activity (from Zephyr) with our shocks. The unit of observation is a four-digit SIC code-year. We use all the shock data (Table A.1 –Panel A), not just the ones matched with our main sample. Columns 1 and 3 use the logarithm of the number of deals as dependent variable, while columns 2 and 4 use a dummy variable that takes a value of 1 if there was an M&A deal in that industry-year and zero otherwise. Columns 3 and 4 differ from columns 1 and 2 in that in the specifications we exploit only within-industry variation by including industry fixed effects. Standard errors are adjusted by heteroscedasticity and clusters at the (four-digit) sic level. Significant at the *10%, **5%, ***1%.

Variable	Log(deals)	Dummy Transaction	Log(deals)	Dummy Transaction
Shock	0.4729***	0.1442***	0.0899**	0.0594***
	(0.0928)	(0.0258)	(0.0353)	(0.0210)
Industry Fixed Effects	No	No	Yes	Yes
Year Fixed effects	Yes	Yes	Yes	Yes
R-squared (whithin)	0.0127	0.0095	0.0141	0.0065
N	6310	6310	6310	6310

Table A.3: Controlling for country-by-year fixed effects

This table present our main results (Table 4), but now controlling for country-by-year fixed effects, rather than year fixed effects. Standard errors are adjusted by heteroscedasticity and clusters at the (three-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	-0.1462**	-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

Table A.4: Other measures of external financial dependence

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. 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 (three-digit) sic-by-country level. Significant at the *10%, **5%, ***1%.

Panel A -Sample Split by Industry Equity Dep. (R&Z)

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

Panel B -Sample Split by 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)

Table A.5: Investor protection

This table presents second stage (IV) estimates of stand-alone, instrumented with Other Shock for sample splits based on the anti-self-dealing index (Djankov et al 2008) which is a measure of investor protection at the country level. Standard errors are adjusted by heteroscedasticity and clusters at the (three-digit) sic-by-country level. Significant at the *10%, **5%, ***1%.

	Sample Split by anti-self-dealing index			
	Low investor protection		High investor protection	
Variable	Leverage	Asset Growth	Leverage	Asset Growth
Stand-alone	-0.1435**	-0.2709**	-0.0229	-0.0019
	(0.0604)	(0.1122)	(0.0675)	(0.1002)
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	11533	13588	7617	8797
First-Stage				
Other Shock	0.0904***	0.0827***	0.1470***	0.1383***
	(0.0181)	(0.0165)	(0.0274)	(0.0263)

Table A.6: Balance

This table compares the main variable means for firms that become standalone vs firms that remain as business groups throughout the sample (Panel A), and for firms that were affected by the instrument (Other Shock) vs firms that were not (Panels B and C). The mean comparisons in Panel A are for the year prior to the split for treated firms and their matched controls. The mean comparisons in Panels B and C are based on observations prior to the split (for both treated firms and their matched controls), and prior to receiving the Other Shock, in case firms were to receive that shock. The data used Panel C differs from that in Panel A in that we exclude firms with assets of less than 100,000 euros. Significant at the *10%, **5%, ***1%.

Panel A - Mean differences for second stage:
Firms that become stand alone and control firms, year prior to the split

Variable	(1) Always BG	(2) Becoming Standalone	(2)-(1) Difference	p-value
Leverage	0.60	0.60	0.00	0.90
Assets (million)	1.51	1.53	0.02	0.68
Asset Growth	0.03	0.04	0.01	0.32
Tangibility	0.27	0.27	0.00	0.78
Cash	0.16	0.16	0.00	0.42
Tobin's Q (industry)	2.88	2.88	0.00	0.98
Sales/Assets	1.90	1.84	-0.06	0.24

Panel B - Mean differences for first stage:
Firms that eventually receive "Other Shock" and firms that do not, before both the split and shock

Variable	(1) No Other Shock	(2) Other Shock	(2)-(1) Difference	p-value
Leverage	0.61	0.60	0.00	0.89
Assets (million)	1.53	1.23	-0.31	0.06*
Asset Growth	0.04	0.03	0.00	0.88
Tangibility	0.27	0.27	0.00	0.98
Cash	0.16	0.17	0.01	0.56
Tobin's Q (industry)	2.89	2.88	-0.01	0.95
Sales/Assets	1.88	1.86	-0.02	0.92

Panel C - Mean differences for first stage (Assets >100,000 Euros):
Firms that eventually receive "Other Shock" and firms that do not, before both the split and shock

Variable	(1) No Other Shock	(2) Other Shock	(2)-(1) Difference	p-value
Leverage	0.63	0.64	0.01	0.83
Assets (million)	1.77	1.61	-0.16	0.40
Asset Growth	0.05	0.09	0.04	0.23
Tangibility	0.28	0.27	-0.01	0.72
Cash	0.15	0.13	-0.01	0.49
Tobin's Q (industry)	2.86	2.84	-0.02	0.88
Sales/Assets	1.81	1.68	-0.13	0.56

Table A.7: Excluding Small Firms

This table present the results from our main estimations (Table 4 panel A), but excluding firms with assets of less than 100,000 euros. Standard errors are adjusted by heteroscedasticity and clusters at the (three-digit) sic-by-country level. Significant at the *10%, **5%, ***1%.

Second Stage IV Estimations (Assets>100,000 Euros)

Variable	Leverage	Leverage	Leverage	Asset Growth	Asset Growth	Asset Growth
Stand-alone	-0.1243***	-0.1017***	-0.1006***	-0.2188***	-0.1189**	-0.1822***
	(0.0417)	(0.0362)	(0.0362)	(0.0795)	(0.0568)	(0.0676)
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	23664	17041	17041	21058	19347	19347
First-Stage						
Other Shock	0.1000***	0.1282***	0.1283***	0.1039***	0.1201***	0.1201***
	(0.0136)	(0.0170)	(0.0170)	(0.0152)	(0.0159)	(0.0159)

Table A.8: Intention to Treat

This table presents results from an Intention to Treat (ITT) DiD estimation: We estimate the effect of the instrument (Other Shock) directly on firms' leverage and asset growth. 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. Standard errors are adjusted by heteroscedasticity and clusters at the (three-digit) sic-by-country level. Significant at the *10%, **5%, ***1%.

OLS ITT Estimations

Variable	Leverage	Leverage	Leverage	Asset Growth	Asset Growth	Asset Growth
Other Shock	-0.0092***	-0.0099**	-0.0097**	-0.0196**	-0.0163**	-0.0223***
	(0.0032)	(0.0045)	(0.0045)	(0.0079)	(0.0072)	(0.0078)
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