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The supply chain spillovers of private equity buyouts

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Abstract

We study how private equity (PE) buyouts propagate through supply chains using unique firm-to-firm transactions data from Belgium. In normal times, suppliers of PE-backed firms outperform their peers by 5%–10% in employment and sales growth, primarily due to increased input demand from PE-backed customers rather than knowledge spillovers or other mechanisms. In economic downturns, however, this outperformance is attenuated and suppliers compress markups by around 8% as PE investors intensify bargaining pressure and reconfigure supply chains to extract cost savings. Beyond the direct effects on suppliers, we show that as PE-backed firms absorb supplier capacity, they crowd out competitors that rely on the same suppliers. Overall, our findings underscore that supply chains are central to how PE investors create and redistribute value.

JEL Classification: D22, D24, G32, G34
Keywords: Private equity, Supply chains, Spillover effects, Firm growth, Switching costs, Bargaining power

Non-technical summary

Private equity (PE) has become a major form of ownership in modern economies, yet its economic effects remain contested. On the one hand, proponents have argued that PE investors create value through operational improvements and stronger governance structures. On the other hand, critics have claimed that PE investors generate returns by redistributing surplus away from other stakeholders—for example, through aggressive cost cutting and contract renegotiation.

This debate has spurred a growing body of research on the economic implications of PE ownership, focusing on how buyouts affect the outcomes of acquired firms. Yet firms do not operate in isolation; they are embedded in complex production networks. Despite the central role of buyer–supplier linkages as channels of economic transmission, little is known about how PE buyouts propagate through production networks and affect the supply chain partners of PE-backed firms.

Theory offers opposing predictions. On the one hand, if PE buyouts enable targets to pursue new growth opportunities or improve operational efficiency, suppliers may benefit from increased demand for inputs or knowledge spillovers. On the other hand, PE firms' high leverage and short investment horizons may lead them to exert pressure on suppliers for cost reductions—e.g., by renegotiating contracts or switching suppliers.

Our paper empirically examines these predictions using unique production network data from Belgium—which record the buyer-supplier relationships of virtually all firms in the economy—linked to Belgian PE deals and firm balance sheet data for the period 2002–2021. Using this unique dataset, we employ a matched difference-in-differences framework to compare the economic trajectories of suppliers with versus without PE-backed customers, after versus before PE buyouts.

Our analysis yields three main findings. First, in normal times, suppliers of PE-backed firms significantly outperform comparable suppliers of non-PE-backed firms, with employment and sales growth higher by approximately 5% and 10%, respectively. These gains are primarily driven by increased input demand from faster-growing PE-backed customers, rather than by technology transfer or knowledge spillovers. Second, these effects are attenuated during economic downturns. In such periods, suppliers of PE-backed firms no longer outperform their peers and instead reduce markups by around 8% due to PE investors exerting greater pressure on suppliers and more actively reconfiguring supply chains to generate cost savings. Finally, beyond direct supplier effects, we document crowding-out effects on firms that share suppliers with PE-backed rivals. Specifically, we show that, as PE-backed firms absorb supplier capacity, competitors that rely on

the same suppliers experience significant performance declines.

Overall, our study provides new insights into the role of supply chains in PE investors' ability to create and redistribute value. Policymakers should consider not only the direct effects of PE buyouts but also their broader spillovers along production networks and the resulting implications for product market competition. These insights may inform antitrust evaluations of PE transactions, contribute to ongoing debates about the role of bank versus non-bank financing in fostering economic growth, and guide policy discussions on supply chain resilience.

I. Introduction

The private equity (PE) industry has grown tremendously over the past two decades, reaching more than $5 trillion in assets under management globally by 2024, corresponding to a four-fold increase since 2010 (Preqin 2024). This rapid growth has not been without criticism; politicians and labor unions increasingly raise concerns about the adverse impact of PE buyouts, prompting legislative responses such as the "Stop Wall Street Looting Act" recently proposed by several U.S. senators. Nevertheless, a growing body of research indicates that PE investors have a positive impact on their portfolio companies, for example by improving total factor productivity (Davis et al. 2014), managerial practices (Bloom et al. 2015), and innovation activities (Lerner et al. 2011).[1]

Despite ample empirical evidence on how PE investors affect their portfolio companies, we know little about how PE buyouts ripple through production networks and affect the supply chain partners of PE-backed firms. This gap is surprising, not only because of the growing policy and practitioner interest,[2] but also because theory yields ambiguous predictions about whether and how PE ownership would affect the suppliers of PE-backed firms.

On the one hand, assuming PE firms enable their portfolio companies to implement operational improvements and pursue new growth opportunities, suppliers may benefit from knowledge spillovers or increased demand for inputs (Holmström 1988). On the other hand, even if PE firms create value for their portfolio companies, they may do so at the expense of suppliers (Shleifer and Summers 1988). Specifically, PE firms may exert pressure on suppliers—e.g., by imposing price concessions or stricter payment terms—to achieve cost savings for their own portfolio companies.

The main obstacle to empirically examining these opposing predictions is the demanding data requirements. Although some commercial datasets provide supply chain data for a subset of large, publicly listed firms, over 90% of PE buyouts involve small, privately held companies. In this paper, we overcome these limitations by leveraging unique production network data from Belgium covering all firms—including small, private ones—to examine the role of supply chains in PE investors' ability to create and extract economic value.

Data Our analysis relies on three data sources from Belgium which, as we document below, is broadly representative of global PE activity. Our primary data source is administrative firm-to-firm sales data managed by the National Bank of Belgium, which enables us to

[1] While the impact of PE buyouts on firms in the private for-profit sector is generally positive, the evidence is more mixed for firms operating in regulated and subsidized industries, such as healthcare and education (e.g., Duggan et al. 2023; Eaton et al. 2020; Ewens et al. 2022; Gao et al. 2025; Gupta et al. 2024; Howell et al. 2026).

[2] For instance, a report from Jabian Consulting (2022) titled "Private Equity Ate My Customer" states that "B2B companies should be familiar with the ways that PE could disrupt their customer base and be prepared to reconsider their customer strategy." Similarly, a report from Alcott Global (2024) highlights that "Private equity firms, with their acute focus on value-creation, are increasingly turning their attention to the supply chain and value chain aspects of their portfolio companies."

construct the network of supply chain relationships for virtually all firms in Belgium. A key advantage of this dataset is that it covers *all* firms in the economy, as the majority of PE deals—both in Belgium and globally—involve small, private firms.[3] We then link these data to detailed firm balance sheet data, which provide information on sales, revenues, and costs of inputs (including capital, labor, and intermediates). Finally, we combine these data with PE deals involving Belgian targets obtained from Orbis M&A and S&P Global. This yields a final dataset that includes approximately 240 thousand firms and more than 200 PE deals over the period 2002–2021.

Using this dataset, we test for changes in supplier outcomes—such as sales, profitability, employment, and markups—after one of its customers is acquired by a PE firm. In this setting, a common identification challenge is that PE targets are not randomly selected, raising endogeneity concerns. A key advantage of our study, however, is that we do not focus on PE targets, but on the *suppliers* of those firms. Moreover, we do not find evidence that PE investors systematically target firms with an inherently different supplier base. Nevertheless, to mitigate remaining concerns, we follow prior research and carefully construct a control group of comparable firms for the suppliers of each PE-backed firm. Specifically, we match suppliers of PE-backed firms to comparable suppliers of non-PE-backed firms based on industry, firm size, leverage, and profitability measured in the year prior to the PE event (similar to the approach of, e.g., Boucly et al. 2011; Davis et al. 2014), ensuring that treated and control suppliers are comparable along key firm characteristics. Using this matched sample, we estimate a stacked difference-in-differences model that includes firm and year fixed effects. Our identification strategy therefore exploits within-firm variation over time, comparing suppliers exposed to PE targets with observably similar suppliers operating in the same industry and year.

Results Before addressing our main research question, we assess the validity of our empirical setup by analyzing the impact of PE buyouts on target firms in our data sample. Prior research has argued that private firm buyouts enable target firms to pursue new growth opportunities by providing managerial expertise and access to debt financing, among others (e.g., Kaplan and Strömberg 2009; Boucly et al. 2011; Cohn et al. 2022; Davis et al. 2025).[4] Consistent with this, we find that PE targets' financial leverage significantly increases after being acquired by a PE firm, and that they grow faster than their matched controls after the buyout. Moreover, consistent with prior studies, we find that target firms outperform their peers even more strongly during economic downturns, when PE investors' managerial expertise may be particularly valuable (Bernstein et al. 2019).

[3] Cohn et al. (2022) for example report that private firm buyouts have outnumbered public firm buyouts by more than thirty to one in the U.S. over the past decade.

[4] Other value-creation mechanisms, such as financial engineering, play a much more limited role in private firm buyouts than in public firm buyouts (also see Guo et al. 2011; Cohn et al. 2014; Jang and Mayer 2025).

We then turn to our main research question and analyze how PE buyouts affect the suppliers of target firms. We find that, in the years following a buyout, suppliers of PE-backed firms significantly outperform matched controls. For example, employment and sales growth are approximately 4% and 8% higher, respectively, than at comparable suppliers of non-PE-backed firms. However, this outperformance is highly state-contingent. In normal times, suppliers of PE-backed firms outperform their peers by about 5% in employment and 10% in sales. In downturns, they show no significant outperformance and reduce markups by roughly 8%. Thus, while *PE targets* outperform their peers in both normal times and downturns, their *suppliers* benefit only in normal times and are worse off in downturns.

Mechanisms To uncover the mechanisms behind these results, we exploit heterogeneity across PE buyouts, customer-supplier relationships, and industry structure. The positive effect observed in normal times is driven by increased demand for inputs. In particular, as PE-backed firms pursue new growth opportunities, their suppliers benefit from increased demand for inputs, leading them to grow faster. The most direct evidence consistent with this channel comes from customer-supplier level regressions that exploit within-supplier variation across PE-backed and non-PE-backed customers (enabling us to separate demand from supply effects). These regressions confirm that the sales increase observed for treated suppliers is driven by increased demand from PE-backed customers. In addition, further in line with a demand channel, the positive spillovers are larger when target firms had greater potential to grow post-buyout and when suppliers were more deeply embedded in targets' input chains, identified as those providing a larger input share or maintaining a longer pre-buyout relationship. Additional tests rule out alternative channels, such as changes in trade credit or knowledge spillovers, through which PE-backed firms might affect suppliers.

Next, we show that the attenuated effects observed during economic downturns can be attributed to PE investors becoming more involved in the strategic decision-making of their portfolio companies (Bernstein et al. 2019). Consistent with survey and anecdotal evidence,[5] we find that PE investors exert greater cost-cutting pressure on suppliers during periods of economic distress. Specifically, the attenuated effects during downturns are concentrated among suppliers for whom PE-backed firms face lower switching costs—such as those providing standardized inputs or operating in highly competitive industries (Giannetti et al. 2021)—and

[5]For instance, The New York Times (2012) reports that Blackstone used its purchasing power to reduce the price of overnight FedEx shipments for its portfolio companies, illustrating how PE firms pressure suppliers to achieve cost savings. Similarly, after Bain Capital and Blackstone acquired Michaels Stores, the largest arts and crafts retailer in North America, they implemented a fierce cost-cutting strategy that included renegotiating supplier contracts and streamlining the distribution network (VM 2024). Furthermore, survey results from Gompers et al. (2016) indicate that increased bargaining with suppliers to reduce costs is important in 16% of PE deals. Finally, industry reports further indicate that downturns both necessitate and facilitate PE investors' cost-cutting pressure on suppliers, highlighting that actions such as renegotiating prices and switching suppliers are particularly attractive during recessions due to their rapid implementation and immediate savings (Arthur D. Little 2008).

when PE firms have greater bargaining power—such as when PE firms are larger, more established (Arcot et al. 2015). For these suppliers, we also observe a significant reduction in markups during downturns, suggesting that PE firms enforce price concessions from suppliers. In addition to selectively pressuring suppliers, we show that PE-backed firms restructure their supply chains more actively compared to their peers during economic downturns. Together, these strategies appear to benefit PE firms' portfolio companies, as we find a significant reduction in those companies' cost for inputs during periods of economic distress.[6]

Beyond the first-order effects on suppliers, we document that PE buyouts create crowding-out effects for competitors that share common suppliers with PE-acquired firms. We find that treated suppliers are significantly more likely to terminate relationships with the rivals of their PE-backed customers. In principle, this pattern could reflect either capacity-constrained suppliers prioritizing faster-growing PE-backed customers or anti-competitive behavior by PE-backed customers. Consistent with a supply-side mechanism, we find that suppliers are more likely to sever ties with lower-performing customers, particularly when suppliers themselves are capacity-constrained. These relationship terminations have real effects for competitors, as we find that those more exposed to common suppliers experience substantial declines in firm performance following a PE buyout, highlighting a novel channel through which PE buyouts shape product market competition.

Robustness We perform several robustness checks aimed at mitigating potential endogeneity concerns, ruling out alternative mechanisms, and assessing measurement choices. Our identification strategy rests on the assumption that treated and control suppliers would have followed parallel trends absent the PE event.[7] We present several pieces of evidence supporting this assumption. First, dynamic difference-in-differences models show no pre-trends across all outcomes. Second, if PE-backed and non-PE-backed firms systematically sorted into different types of suppliers, one would expect to observe divergent outcomes even for (i) suppliers of firms tied to canceled PE deals or (ii) suppliers who ended their relationship with the PE-acquired firm right before the buyout.[8] We find no significant estimates in either of these placebo samples, suggesting that the post-buyout divergence identified in our baseline results reflects the impact of the buyout itself rather than pre-existing supplier differences. Finally,

[6] A complementary explanation is the high leverage typical of PE transactions, which makes downturns especially perilous for PE firms, creating strong incentives to cut costs and preserve liquidity (Jensen and Meckling 1976; Kaplan and Strömberg 2009; Franzoni et al. 2012). Consistent with the latter, we observe that the cost-cutting pressure exerted on suppliers is most pronounced for portfolio firms that face greater financial distress. However, as discussed below, leverage alone does not fully explain our results, underscoring the distinctive role played by PE management.

[7] Our identification strategy also relies on the stable unit treatment value assumption (SUTVA), which requires that control suppliers are unaffected by PE buyouts. Robustness tests discussed below indicate that our results are not biased by potential violations of this assumption.

[8] The latter falsification test is similar to the approach of Agrawal and Tambe (2016), who study the impact of PE buyouts on employees, and use employees who leave PE-backed firms prior to the buyout as a placebo group.

linear probability models predicting which firms are targeted by PE investors show that supply chain characteristics—such as supplier profitability or switching costs—do not systematically predict PE acquisition, further alleviating endogenous selection concerns.

We conduct two analyses to rule out alternative explanations. The first analysis focuses on alternative mechanisms for the main channel. One possibility is that our results reflect knowledge spillovers, whereby suppliers learn from the operational and technological improvements of their PE-backed customers (Isaksson et al. 2016; Amiti et al. 2024). We find no support for this channel; affected suppliers do not increase high-skilled hiring or R&D, and the effects are not stronger among suppliers to more technology-intensive targets. Another possibility is that our results reflect changes in trade credit (Billett et al. 2025). We find no evidence along this margin; neither the accounts payable of PE-backed firms nor the accounts receivable of their suppliers change following the buyout. Finally, PE investors' reputation and track record may generate a certification effect, whereby suppliers of PE-backed firms gain credibility or referrals (e.g., Dranove and Jin 2010). Consistent with this, we find that treated suppliers acquire new clients, disproportionately from within the PE-backed firm's network. Yet, this effect is quantitatively small relative to the direct increase in demand. The second analysis examines whether PE ownership generates effects beyond those attributable to leverage. To do so, we apply our empirical framework to two settings that increase leverage but lack PE-style governance; (i) high-leverage M&A transactions and (ii) first-time bank borrowing. We find no significant performance effects for suppliers in either setting, neither in normal times nor during downturns. This suggests that leverage alone is not sufficient; rather, it is leverage *combined with* active PE ownership that drives increased supplier demand in normal times and intensified cost-cutting pressures during downturns.

The results are robust to a range of measurement choices. In particular, they hold under a stricter matching procedure that conditions on both firm-specific and customer-base characteristics, when using an alternative measure of economic downturns or alternative measures of firm-level markups, when excluding buy-and-build deals, and when restricting the sample to PE deals executed in normal times (for which the subsequent economic downturns are plausibly more exogenous).

Related literature Our findings speak to several strands of research. Our first contribution is to the literature on the economic impact of PE ownership, which has generally found positive effects of buyouts on target firms' growth, managerial practices, innovation, and crisis resilience (Acharya et al. 2013; Boucly et al. 2011; Cohn et al. 2022; Davis et al. 2014; Kaplan 1989; Lichtenberg and Siegel 1990; Bernstein and Sheen 2016; Edgerton 2012; Bloom et al. 2015; Lerner et al. 2011; Bernstein et al. 2019; Gompers et al. 2022). In recent years,

this literature has expanded to examine how PE ownership affects employees, consumers, and rivals (for an overview, see Sorensen and Yasuda 2023).[9] Suppliers—a large and important stakeholder group—have received little attention. Our paper shows that PE-backed firms boost supplier growth in normal times through increased input demand, but exert cost-cutting pressure during downturns, highlighting the role of supply chains in how PE ownership creates, extracts, and redistributes value. Consistent with theories of asset specificity, property rights, and transaction costs (Klein et al. 1978; Williamson 1979; Grossman and Hart 1986; Hart and Moore 1988, 1990), our findings indicate that the balance between value sharing and value extraction in buyer–supplier relationships depends on switching costs and outside options.

One paper related to ours is Brown et al. (2009), who use stock price reactions to infer that leveraged buyouts (LBOs) strengthen customers' bargaining power over suppliers. Our analysis differs in four important ways. First, rather than studying public buyouts, we examine private firm buyouts, which fundamentally differ and account for the vast majority of PE deals globally.[10] Second, rather than using stock prices, we leverage production network and firm financial statement data to provide a richer analysis of supplier outcomes. Third, we show that the impact on suppliers varies with economic conditions. Fourth, beyond direct supplier effects, we also study indirect spillovers on rivals of PE-backed firms through common supplier networks.

Our second contribution is to the literature on how shocks propagate through production networks (Acemoglu et al. 2012).[11] The most closely related papers in this literature examine supply chain spillovers of ownership and market structure changes through horizontal and vertical mergers (Fee and Thomas 2004; Bhattacharyya and Nain 2011; Luco and Marshall 2020), multinational corporations (MNCs) (Alfaro-Urena et al. 2022), and superstar firms (Amiti et al. 2024). PE buyouts share elements with both of these settings; like mergers, they involve an ownership change, and like MNCs and superstar firms, they bring managerial capabilities and reputational capital. Yet, the impact of PE ownership differs from both.

[9] The empirical evidence on the effect on employees is mixed. In the U.S., Herkenhoff et al. (2025) find that workers of PE-acquired firms suffer sizable earnings losses and worse subsequent employment prospects. In Germany, Antoni et al. (2019) report reduced employment and lower wages post-buyout. Agrawal and Tambe (2016), using U.S. individual level data, show that PE buyouts boost IT investments, which enhances employees' human capital and wages. Other studies have found positive effects on workplace safety (Cohn et al. 2021) and negative effects on job satisfaction (Gornall et al. 2024; Lambert et al. 2021). Evidence on the impact of PE buyouts on consumers is also mixed and often focused on specific industries, such as retail (Chevalier 1995b; Fracassi et al. 2022), healthcare (Aghamolla et al. 2023; Gao et al. 2025; Gupta et al. 2024; Liu 2022), banking (Johnston-Ross et al. 2024), newspapers (Ewens et al. 2022), and education (Eaton et al. 2020), among many others. The effect on rivals seems positive. For instance, Bernstein et al. (2017) find that firms operating in industries with more PE investments grow more rapidly than other firms (also see Aldatmaz and Brown 2020; Chevalier 1995a).

[10] Many studies have emphasized the distinction between public versus private firm buyouts, including Davis et al. (2025), Cohn et al. (2022), and Strömberg (2008). For example, Davis et al. (2025) underscore that buyouts of private firms are associated with positive effects on target firms, whereas buyouts of public firms are associated with negative outcomes.

[11] Prior work has studied the supply chain spillovers of natural disasters, conflicts, credit shocks, bankruptcy, ESG incidents, and cyberattacks, among others (Hertzel et al. 2008; Jacobson and Von Schedvin 2015; Barrot and Sauvagnat 2016; Boehm et al. 2019; Giroud and Mueller 2019; Costello 2020; Giannetti et al. 2021; Carvalho et al. 2021; Pankratz and Schiller 2024; Bisetti et al. 2026; Crosignani et al. 2023; Ersahin et al. 2024; Miyauchi 2024; Korovkin et al. 2025).

Unlike the negative supplier wealth effects documented for mergers and the positive knowledge and reputation spillovers associated with MNCs and superstar firms, we demonstrate that the impact of PE buyouts on suppliers is state-contingent.

Finally, the crowding-out effects on competitors that we document connect to recent work on how shared supplier relationships shape product market competition (Chod et al. 2019; Giannetti et al. 2021; Freeman et al. 2024). This literature shows that firms strategically exploit common suppliers—e.g., through trade credit or exclusivity—to gain competitive advantages. We document a new mechanism; PE-backed firms indirectly weaken rivals by absorbing supplier capacity, leading competitors more exposed to common suppliers to experience substantial declines in sales, employment, and profitability following PE buyouts.

The remainder of the paper proceeds as follows. Section II describes the data. Section III outlines the empirical methodology. Section IV presents the main results and the economic mechanisms. Extensions and robustness tests are discussed in Section V. Section VI concludes.

II. Data

Our primary data source is the business-to-business (B2B) transactions database administered by the National Bank of Belgium. This dataset records the universe of B2B transactions among all VAT-liable firms in Belgium on an annual basis (for details, see Duprez et al. 2023), which enables us to identify firms' buyers and suppliers (the extensive margin) as well as the sales amount between each buyer-supplier pair (the intensive margin). Unlike commercial datasets, two key advantages of the Belgian B2B database are that it covers all firms—including small, private ones which are most likely to be acquired by PE firms—and that it covers the intensive margin of firm-to-firm trade which, as discussed below, is crucial for analyzing the mechanisms through which PE buyouts affect suppliers.[12] Our second data source is the annual accounts database from the National Bank of Belgium. This dataset contains detailed information from firms' balance sheets on sales, revenues, costs of inputs (such as capital, labor, and intermediates), as well as firms' 4-digit (NACE) industry code and zip code.[13]

Following Bernard et al. (2022) and Dhyne et al. (2022), we restrict the sample to private Belgian firms in the non-financial sector with positive sales and labor costs, and at least one full-time equivalent employee (to avoid potential issues with shell or management companies),

[12]By law, all Belgian firms are required to report client listings containing the firm identifier and annual sales value if the yearly sales value exceeds 250 euros. The Belgian tax authorities impose pecuniary sanctions for late or erroneous reporting, which ensures a very high quality of the data.

[13]Note that the unit of observation in these datasets are VAT-IDs, and one firm can potentially have multiple VAT-IDs. Following Dhyne et al. (2021), we aggregate VAT-IDs up to the firm level using ownership filings in the annual accounts and foreign ownership filings in the Balance of Payments survey. The Balance of Payments survey reports for each VAT-ID, the name of foreign parent firms that own at least 10% share, along with the associated ownership share. We group all VAT-IDs into firms if they are linked with more than or equal to 50% of ownership or if they share the same foreign parent firm that holds more than or equal to 50% of their shares.

tangible assets above 100 euros, and positive total assets for at least one year. We keep only the set of firms that are active in the production network. This results in a final data sample that yields 241,409 unique firms over the period 2002–2021. Descriptive statistics of the data sample are reported in Panel A of Table I.

We merge these data with data on PE transactions involving Belgian target firms obtained from Orbis M&A (formerly Zephyr), one of the most comprehensive databases on PE transactions in Europe. Following prior studies (Davis et al. 2014; Lambert et al. 2021), we restrict our focus to transactions for which the deal type is equal to "Private equity" or "Institutional buy-out" as well as all transactions for which the deal type is equal to "Acquisition" and the deal financing is equal to either "Leveraged buyout" or "Private equity", to ensure that we focus on later-stage buyout transactions and exclude venture capital investments, which differ in important ways. Further, we require for all transactions that the acquirer is an institutional investor, the initial stake in the firm is less than 50% and the final stake is larger than 75%. To mitigate potential concerns that Orbis M&A may not cover all PE deals, we further complement these data with PE deals obtained from S&P Global (Capital IQ). The Bureau van Dijk (BvD) identifier in Orbis M&A corresponds to the VAT number for Belgian firms, allowing us to directly link the PE transactions data to the B2B and firm financial statement data explained above. Ultimately, this results in 294 PE buyouts of Belgian firms between 2002 and 2021, of which we match around 75%—yielding 216 PE targets—to the VAT number of a firm in our data sample (after applying the filters mentioned above).[14] For each firm, we record the year of the first PE transaction that we observe as the buyout year.[15] Information on the number of deals and suppliers of PE-acquired firms is in Table II.

II.A. PE activity in Belgium

As discussed in detail in Internet Appendix O.B, the Belgian PE buyouts in our data sample are broadly comparable to PE buyouts in other countries. First, the cycle of Belgian private equity (PE) deals closely mirrors global PE activity, with similar expansions and contractions over time. Second, although Belgium records fewer deals in absolute terms than other major economies, its PE activity is broadly comparable once scaled by GDP. Third, the average deal size and the composition of deal types aligns with international benchmarks. Fourth, investor composition is representative, including both domestic and international sponsors. Finally,

[14]This match rate is comparable to the one obtained by Davis et al. (2014) with U.S. data and Boucly et al. (2011) with French data, for instance. The small number of unmatched PE deals primarily involves transactions in sectors excluded from the sample, such as health care and real estate companies.

[15]One might wonder whether PE firms consolidate supply chains by acquiring firms that are customers or suppliers of one another. In our sample, this does not appear to be a common strategy. We identify only a single instance in which a PE firm acquired both a company and one of its direct suppliers. Our results remain robust when excluding these two PE targets from the sample.

the sectoral distribution of deals is comparable to that of other countries and has shifted over time from manufacturing to information and communication services. The generalizability of our findings may partly depend on countries' institutional settings, but in general Belgium's rankings on several institutional quality measures in the World Bank's World Development Indicators are comparable to those of other developed economies.

III. Methodology

We analyze the impact effects of PE buyouts on suppliers of PE-acquired firms using a matched stacked difference-in-differences design.[16] A key identification advantage is that we focus on the suppliers of PE targets rather than the targets themselves. Since PE investors do not select firms with a systematically different supplier base—as we verify in Section V.D—our analysis is less exposed to endogeneity concerns than studies that focus on PE-acquired firms. To further mitigate remaining concerns, we leverage the granularity of our data to construct a control group of comparable firms (as in Davis et al. 2014; Boucly et al. 2011; Cohn et al. 2021).

To do so, for each PE event, we first identify the suppliers of each target firm and match those with a group of control firms. Following previous papers, we apply nearest neighbor propensity score matching (PSM) with replacement, where we require the potential matches to have similar size (total assets), leverage, and profitability (EBITDA) as the supply chain partners of the acquired firms in the year before the event. We also require potential matches to be in the same 4-digit NACE industry as the treated supply chain partners and to have data available at least in year $t-1$ and $t+1$. We retain the five closest control firms for each treated firm, forming cohorts of treated and control suppliers. The identification assumption is that firms in the same cohort would have followed parallel trends absent the PE event (an assumption we empirically verify below), in which case the control firms are an appropriate counterfactual for the treated firms. In robustness checks, discussed below, we show that our results are insensitive to the matching procedure or matching variables used.

We restrict the analysis to cohorts in which the PE-backed customer accounted for at least 5% of the treated supplier's total sales prior to the PE deal, ensuring the treatment effect is economically meaningful.[17] We track firms in each cohort for four years before to

[16] Recent studies in econometrics have shown that the use of standard two-way fixed effects models can generate biased estimates in settings with staggered timing of treatment assignment or treatment effect heterogeneity. Baker et al. (2022) review the alternative estimators proposed in the literature and find that a stacked difference-in-differences estimator allows to identify the true treatment effects. Gardner et al. (2024) further show that a stacked design is equivalent to estimating an average treatment effect in each cohort and then taking the average of the cohort-specific estimators, weighted by the relative sizes of the cohorts. Therefore, the stacked difference-in-differences estimator is similar to the idea proposed by Sun and Abraham (2021) and Callaway and Sant'Anna (2021) to estimate separate average treatment effects in different groups and then aggregate these estimators to form an overall estimate of the treatment effect.

[17] In Table A.II in the Internet Appendix, we present results without imposing a specific threshold, and instead include a series

five years after the event (consistent with the typical PE holding period of three to five years, see Kaplan and Strömberg 2009). We stack all cohorts to estimate average treatment effects across the full sample period using the following regression:

$$y_{i,t,c} = \beta \cdot Post\ PE_{i(j),t,c} + \lambda_{i,c} + \lambda_{t,c} + \epsilon_{i,t,c} \tag{1}$$

where i, j, t, and c correspond to supplier, customer, time, and cohort, respectively. $y_{i,t,c}$ represents various firm-level outcomes, including total sales, profitability, employment, and markups. The latter are computed following De Loecker and Warzynski (2012), by estimating industry-level revenue production functions using the Ackerberg et al. (2015) control function estimator (see Internet Appendix O.C for more details on the estimation procedure).[18] As explained by De Loecker and Warzynski (2012), the estimated markups essentially capture the wedge between a firm's output price and marginal cost, serving as a proxy for pricing power.

Our independent variable of interest is $Post\ PE_{i(j),t,c}$ which is an indicator variable equal to one in the years after customer j of supplier i in cohort c was acquired by a PE firm. $\lambda_{i,c}$ and $\lambda_{t,c}$ are firm-by-cohort and year-by-cohort fixed effects, respectively. The former ensures that we exploit within-firm variation and that our estimates are not affected by unobservable differences between the treated and control firms (as long as the unobservable differences are time-invariant within a cohort). The latter accounts for any time-specific unobserved heterogeneity. We cluster standard errors at the firm-cohort level.

In essence, our identification strategy compares within-firm dynamics of firms that supply inputs to PE-backed firms and control firms with similar observables in the same industry and year. The key identification assumption is that two suppliers with matching characteristics before a PE buyout would have had similar outcomes had the PE buyout never taken place. To confirm the validity of our matching approach, panels (a) and (b) of Figure I present balance diagnostics for suppliers of PE-backed firms and suppliers of non-PE-backed firms before and after applying our matching strategy explained above. We can observe that, after matching, the average size, leverage, and profitability are remarkably similar for treated and control groups. This can be derived from the fact that the standardized mean differences are generally between -25% and 25% after matching, indicating that the variables are well-balanced.[19]

of post-treatment indicators based on suppliers' pre-event sales share to the PE target. The results from this table support the validity of the 5% cutoff used in the main analysis as a threshold for economically meaningful treatment effects.

[18] As is common in the literature, we restrict our sample for estimating markups to firms in the manufacturing sector as firms in the services sector differ substantially in terms of their input-output conversion processes and their higher ratio of intangible assets, among others. We assume materials as variable inputs in the markup estimation procedure. As the data do not record the physical output of Belgian firms, we rely on revenue data in estimating firm level markups. A potential concern is that this may lead to mis-measurement in the output elasticity and, hence, markups. However, as De Ridder et al. (2026) show, even though markups based on revenue data for firms under oligopolistic competition may be biased in levels, they are well estimated in terms of dispersion.

[19] The standardized difference test is a scale-and-sample-size-free estimator proposed by Imbens and Wooldridge (2009), for

We also observe that, after matching, the treated and control firms are similar in terms of employment, tangible assets, age, and markups, among others, even though these variables are not used in our matching procedure. Importantly, as the event-study estimates in Section V.A show, treated and control suppliers follow parallel trends prior to the PE buyouts, supporting the identification assumption underlying our empirical framework.

IV. Results

IV.A. The effect of PE buyouts on target firms

We first validate our empirical setting by confirming that PE buyouts in our sample affect target firms in ways consistent with prior research. Private firm buyouts are argued to enable targets to pursue new growth opportunities through managerial expertise and improved access to debt financing (e.g., Boucly et al. 2011; Cohn et al. 2022). We test this by analyzing target firms' financial leverage and growth outcomes using the matched stacked difference-in-differences design.

Table III reports the results. Relative to control firms, the estimates in column (1) show that target firms' leverage significantly increases in the years after the transaction.[20] Furthermore, after a PE transaction, target firms also grow faster than control firms. Columns (2) and (3) imply that, relative to control firms, target firms' total sales and employment increase by around 22% and 16%, respectively, over the five years following the buyout.[21]

Table IV adds an interaction between the post-treatment indicator and a lagged economic downturn dummy (*Economic Downturn*), defined using the OECD recession indicators for Belgium.[22] Consistent with prior research, we find that PE-backed firms outperform their peers even more strongly during economic downturns, when the managerial expertise of PE investors may be particularly valuable (Wilson et al. 2012; Bernstein et al. 2019). Column (2) for instance, shows that total sales of PE-backed firms are 32% higher relative to matched controls during recessions, compared to 15% higher in normal times, suggesting increased operational involvement in portfolio companies during crisis periods.

which Imbens and Rubin (2015) proposed a heuristic threshold of 25% in absolute value for significant differences.

[20]The debt raised for a PE deal is typically borne by a holding company and therefore does not appear in the unconsolidated accounts reported to the tax authorities (Boucly et al. 2011). This positive effect on target firms' leverage indicates that the PE buyout allows firms to raise debt beyond what has been raised by the PE firm to finance the buyout.

[21]Table A.III in the Internet Appendix shows that these effects are more pronounced for targets with lower pre-buyout growth, supporting the notion that PE firms enable targets to capitalize on new growth opportunities (consistent with, e.g., Boucly et al. 2011; Davis et al. 2025).

[22]The OECD recession indicators for Belgium identify economic downturns for the years 2008, 2009, 2012, 2013, 2015, and 2020.

IV.B. The effect of PE buyouts on suppliers of target firms

We now turn to the findings of our main analysis, which studies the spillover effects of PE buyouts on *suppliers* of PE-backed firms. Table V presents the results from estimating Equation (1), with the natural logarithm of sales, employment, EBITDA, and markups as outcome variables across the different columns. PE buyouts have a positive impact on the suppliers of PE-backed firms. Columns (1)–(3) show that, relative to matched controls, suppliers of PE-backed firms increase sales, employment, and EBITDA by 8%, 4%, and 6%, respectively, in the years following the buyout. Column (4) further shows that there is no significant change in the markups of treated suppliers.

Table VI distinguishes between normal times and economic downturns, revealing that the impact of PE buyouts on suppliers is highly state-contingent. The estimates in columns (1)–(3) indicate that suppliers of PE-backed firms significantly outperform their peers in sales (10%), employment (5%), and EBITDA (8%) in normal times, but do not outperform during economic downturns. Column (4) further shows that, during downturns, treated suppliers reduce markups by approximately 8% relative to their matched controls. This contrasts with prior evidence that PE-backed firms themselves outperform even more strongly during periods of economic distress.

Importantly, dynamic difference-in-differences models support the parallel trends assumption underlying our estimates, and falsification tests confirm that our results are not simply driven by other, inherent differences between suppliers of PE-backed firms and suppliers of non-PE-backed firms. We postpone a detailed description of all robustness tests on the main results and mechanisms to Section V. We proceed by studying the mechanisms behind the main results, and then examine if PE buyouts generate crowding-out effects through common suppliers.

IV.C. Mechanism

We organize the analysis of mechanism around the central tension documented in our main results; while PE ownership benefits suppliers in normal times, these gains do not persist during periods of economic distress. We first show that suppliers' outperformance in normal times is driven primarily by increased demand for inputs from PE-backed customers (Subsection IV.C.1). We establish this using two complementary approaches; exploiting heterogeneity in target and supplier characteristics, and estimating customer-supplier level regressions that isolate demand from supply effects. We also find that suppliers benefit from a certification effect (Subsection IV.C.2), though this channel is quantitatively minor relative to the demand channel. Alternative mechanisms, including knowledge spillovers and trade credit, do not drive

the main results and are addressed in the robustness section. We then show that suppliers' outperformance disappears during downturns as PE investors exert greater cost-cutting pressure, particularly when switching costs are low or bargaining power is high (Subsection IV.C.3). By doing so, PE firms are able to achieve cost savings for their own portfolio companies during periods of economic distress. Finally, we examine why cost-cutting pressure intensifies during economic downturns and show that the explanation lies in PE management (Subsection IV.C.4).

IV.C.1. Increased input demand

As PE-backed firms grow and pursue new growth opportunities post-buyout, their suppliers benefit from increased input demand. Consistent with this mechanism, we first show that the effects are strongest when PE-backed customers have greater scope for post-buyout expansion. Panel A of Table VII shows more pronounced effects for suppliers of targets with lower pre-buyout growth, which are plausibly better positioned to expand and generate additional demand. Second, the effects are larger when PE-backed firms rely more heavily on a given supplier. In Panel B, we classify suppliers by whether they ex-ante accounted for a below- or above-average share of their customers' inputs and find that positive spillovers are strongest for those supplying a larger share. In Panel C, we distinguish suppliers by the duration of their ex-ante relationship with the PE-backed customer, as longer relationships typically indicate greater input dependence. Consistent with this, the effects are concentrated among suppliers with longer pre-existing relationships.

Next, we exploit the granularity of the Belgian firm-to-firm sales data to run customer-supplier level regressions, enabling us to separate demand from supply. For each treated supplier, we match its PE-backed customers to comparable non-PE-backed customers from the same supplier (similar to the approach of Benincasa et al. 2024). For each matched pair, we track customer-supplier relationships for four years prior to and five years following the event and estimate the following regression:

$$y_{i,j,t} = \beta \cdot Post\ PE_{j,t} + \lambda_{i,t} + \lambda_{i,j} + \epsilon_{i,j,t} \tag{2}$$

where $y_{i,j,t}$ corresponds to the purchases of customer j from supplier i in year t. The independent variable is a dummy variable equal to one in the years after customer j was acquired by a PE firm. As the regression is at the customer-supplier level, we are able to include supplier-by-year and customer-by-supplier fixed effects, represented by $\lambda_{i,t}$ and $\lambda_{i,j}$, respectively. The supplier-by-year fixed effects capture unobserved time-varying supplier-specific heterogeneity (such as changes in productivity) and enable us to isolate changes in

PE-backed firms' *demand* from potential supply effects. The customer-by-supplier fixed effects control for time-invariant supplier-customer characteristics (such as geographic proximity). The error term is double clustered at the customer and supplier level.

The results are reported in Table VIII. Across the different columns, we gradually saturate the regression with fixed effects to assess the stability of the coefficient estimates. The results consistently show a significantly positive coefficient, which supports the notion that PE suppliers benefit from increased input demand from PE-backed customers. Moreover, taking into account that the average sales share of treated suppliers to their PE-backed customers in the sample used for the estimations in Table VIII is around 30%, the estimates confirm that the firm level increase in suppliers' sales documented in Table V is primarily driven by purchases from PE-backed customers rather than other clients.[23]

IV.C.2. Certification effects

Beyond the increase in demand for inputs, we show that PE investors' reputational networks generate a certification effect. A company is often known by the customers it keeps (Simonin and Ruth 1998), and it is common to see firms being referred to by their famous customers.[24] PE investors typically have a reputation for excellence and an extensive network, which can benefit the suppliers of their portfolio companies by facilitating referrals, signaling quality, or reducing search costs for potential clients (Dranove and Jin 2010; Cai et al. 2024).

To test this, we first examine whether suppliers of PE-backed firms experience an increase in their customer base post-buyout. Indeed, column (1) of Table A.IV in the Internet Appendix shows that, following a PE buyout, suppliers of PE-backed firms gain on average 4 new customers. Based on the certification channel, we would expect that affected suppliers gain customers that are within the PE-backed firms' network. To this end, we use the production network data to distinguish between customers within and outside of a PE-backed firm's network that the treated and control suppliers of a given cohort sell to (similar to Amiti et al. 2024). The results are reported in columns (2) and (3), which show that, consistent with our prediction, affected suppliers increasingly deal with customers that are in the PE-backed firms' network. Finally, using import–export data, column (4) shows that treated suppliers significantly increase their exports to the country of origin of the target firms' PE investor relative to their matched peers, providing further support for a certification channel.[25,26]

[23]The coefficient estimate of 0.08 in column (1) of Table V is close to the coefficient estimate of 0.18 in column (3) of Table VIII multiplied by 0.30.

[24]For example, Foxconn, a Taiwanese electronics manufacturer with approximately 2.5 billion USD market capitalization, is often referred to as Apple supplier Foxconn. Similarly, Lamb Weston, one of the world's largest producers and processors of frozen french fries, is often introduced as a key supplier of McDonald's.

[25]In the last column, we restrict the sample to suppliers of PE-backed customers with a foreign PE investor, as the relationship between exports and domestic investors is not relevant in this context.

[26]The import–export data allow us to observe the countries from which a firm imports and to which it exports as well as the

Consequently, one may wonder about the quantitative importance of the increased demand channel versus the certification channel. We address this question in Internet Appendix O.D, where our analysis indicates that the increased demand channel is the primary driver of the positive effect observed for suppliers of PE-backed firms during normal times. Furthermore, as discussed in Section V.C below, we explore several alternative channels through which PE-backed firms might benefit their suppliers, such as knowledge spillovers, but we find no evidence supporting these mechanisms. The positive impact on suppliers during normal times is therefore largely "passive" in that it is driven by increased demand for inputs, rather than by knowledge transfer or operational improvements.

IV.C.3. Cost-cutting pressure during downturns

Our results show that suppliers of PE-backed firms outperform their peers in normal times, but not during economic downturns. This reversal is driven by increased cost-cutting pressure from PE investors—for example, through renegotiating contracts with existing suppliers or switching to lower-cost alternatives. Consistent with this mechanism, these effects are strongest when switching costs are low and when the PE investor's reputation enhances its bargaining position.

Panel A of Table IX differentiates between suppliers offering differentiated versus standardized inputs (following the classification by Giannetti et al. 2011). The negative interaction term observed during economic downturns is concentrated among suppliers that offer standardized inputs, which are generally easier to replace than differentiated ones (Cunat 2007). Moreover, these suppliers also significantly reduce their markups during downturns, consistent with PE firms enforcing price concessions to realize cost savings for their portfolio companies.[27]

Panel B shows the same pattern for suppliers operating in more competitive industries, identified as those with a below-average HHI. The negative interaction effect during economic downturns is driven by suppliers in more competitive industries, and suppliers in such industries reduce their markups during such periods. This is consistent with the idea that in industries with more competitors, PE-backed firms can exert more pressure as they have more outside options and can credibly threaten to switch.

Panel C shows that cost-cutting pressure is concentrated among suppliers of targets backed by more established or reputable PE firms. Following prior work (Kaplan and Schoar 2005; Arcot et al. 2015), we classify PE firms as more reputable if they are above the sample

corresponding trade values, but they do not identify the specific foreign firms involved in these transactions.

[27] For anecdotal evidence see the survey by Gompers et al. (2016) or media sources. For example, an article by The New York Times (2012) highlights how Blackstone used its purchasing power to reduce the price of overnight FedEx shipments for its portfolio companies, illustrating how PE firms pressure suppliers to achieve cost savings.

average in the number of past funds raised.[28] This suggests that reputation strengthens the PE investor's ability to extract concessions from suppliers, which is consistent with anecdotal evidence that large PE firms—so-called "PE giants"—use their scale and reputation to negotiate price reductions from suppliers during periods of economic distress (The New York Times 2012).

Next, we examine whether PE-backed firms actively reconfigure their supply chains rather than merely exerting pressure on existing suppliers. To do so, we extend the customer–supplier-level framework in Equation (2) to analyze the likelihood that PE-backed customers terminate supplier relationships, and how this varies with switching costs and PE firms' bargaining power. As noted earlier, the inclusion of supplier–year and supplier–customer fixed effects allows us to compare whether a PE-backed customer, relative to a non-PE-backed customer, is more likely to terminate its relationship with the same supplier in the same year. The results, reported in Table X,[29] show that PE-backed customers are less likely to terminate existing supplier relationships than non-PE-backed firms, as indicated by the negative post-treatment coefficient. However, this pattern reverses during periods of economic distress, as captured by the positive interaction with the downturn indicator. PE-backed customers are significantly more likely to terminate relationships during economic downturns, particularly when switching costs are low or when their strong reputation enhances their bargaining position, consistent with our earlier results. Overall, these findings suggest that PE-backed firms exert greater cost-cutting pressure and more actively reconfigure their supply chains during economic downturns.

Lastly, we shift our attention to the target firms, and provide evidence that PE firms more actively adjust their supplier networks during economic downturns, effectively allowing them to realize cost savings. Columns (1) and (2) of Table XI show that PE-backed firms significantly increase the number of suppliers they rely on, particularly during periods of economic distress, implying that they actively diversify their procurement sources. Furthermore, column (3) shows that, on average, targets' cost of inputs to total sales decreases post-buyout, while column (4) shows that this reduction is concentrated in periods of economic distress. The coefficient estimate in column (4) implies a reduction of 2 percentage points in the cost of inputs to total sales, which is not only statistically but also economically significant. Together, these findings support the interpretation that PE investors combine price concessions from existing suppliers with active supply chain reconfiguration to reduce input costs during periods of economic distress, leading suppliers to lose their outperformance and compress margins

[28] We use the number of previously closed funds rather than total assets under management as a proxy for PE firm size, as AUM data are unavailable for many PE firms. Unreported results show that our findings are robust to using PE firm age instead.

[29] The number of observations is slightly lower than in Table VIII because we cannot observe whether relationships are terminated after 2021, the final year of the sample.

during such periods.

IV.C.4. Why does cost-cutting pressure intensify during downturns?

A natural question is why PE investors exert greater cost-cutting pressure during economic downturns rather than continuously. We see two complementary explanations. First, the high leverage typical of PE transactions creates strong incentives to cut costs and preserve liquidity when economic conditions deteriorate in order to meet debt-servicing obligations (Jensen and Meckling 1976; Jensen 1989; Kaplan and Strömberg 2009; Franzoni et al. 2012). Consistent with this interpretation, Table XII shows that PE investors are significantly more likely to exert greater pressure on suppliers when their portfolio companies face greater financial distress, as reflected in higher leverage ratios or lower interest coverage ratios. Importantly, as discussed in greater detail below, this does not imply that our findings are driven solely by leverage. In fact, results in Section V.C show that similar patterns do not arise in highly leveraged M&A deals, suggesting that PE investors possess distinctive skills or incentives that enable them to pressure suppliers more effectively to reduce costs.

Second, PE investors tend to become more actively involved in the strategic decisions of their portfolio companies during crisis periods (see survey evidence in Bernstein et al. 2019; Gompers et al. 2022), making supply chain optimization a greater priority precisely when economic conditions deteriorate. Moreover, industry reports, such as "How to Manage Portfolio Companies When the Economy Is Down" by Arthur D. Little (2008), suggest that downturns both necessitate and facilitate cost-cutting; recessions tighten liquidity, making measures such as price renegotiation and supplier switching particularly attractive, while distressed suppliers may be more willing to accept less favorable terms to preserve key customer relationships.

IV.D. Crowding-out through common suppliers

Our finding that suppliers benefit from increased input demand from PE-backed customers—and consequently grow faster than their peers—suggests that suppliers can accommodate this additional demand. However, capacity constraints may lead suppliers to prioritize their (faster-growing) PE-backed customers. In such cases, negative externalities may arise for other clients reliant on the same suppliers, particularly for competitors of the PE-backed firms who tend to depend on similar inputs (Boehm and Sonntag 2023; Grossman et al. 2024).

To examine this, we adapt the customer-supplier level framework from Equation (2) and test whether affected suppliers are significantly more likely to terminate relationships with rivals of their PE-backed customers. For each affected supplier, we identify its non-PE-backed customers and match them to comparable customers of non-affected suppliers, allowing us to

compare whether affected suppliers are more likely to terminate a relationship with a given customer relative to non-affected suppliers of that same customer.

The results are presented in Table XIII. We find that affected suppliers are, on average, 6–10 percentage points more likely to terminate an existing relationship when the customer is a direct competitor of their PE-backed client (proxied based on whether the firms operate in the same 4-digit NACE industry). This result holds after including customer–year and customer–supplier fixed effects and is economically meaningful, corresponding to roughly 30% of the average probability of relationship termination in the sample.

In principle, this finding may be driven by two underlying mechanisms. It may result from capacity constraints faced by suppliers, particularly given that competitors often rely on the same inputs.[30] Alternatively, PE-backed customers might engage in anti-competitive behavior by limiting competitors' access to key suppliers (e.g., through exclusive dealing arrangements).

To distinguish between these channels, we conduct two additional analyses. First, if capacity constraints drive the effect, it should be stronger among constrained suppliers. As we do not observe capacity utilization directly, we follow prior work and proxy it using the ratio of turnover to tangible assets (e.g., Hendricks et al. 2009), classifying suppliers in the bottom quartile as capacity constrained. Column (1) of Table XIV shows that these suppliers are significantly more likely to drop rivals of PE-backed customers, supporting the capacity-constraint explanation. Second, we examine the characteristics of the customers that are more likely to be dropped. Columns (2)–(3) of Table XIV show that suppliers are more likely to sever ties with lower-performing firms, proxied by firms in the bottom quartile of profitability or the Altman Z-score.[31] These results challenge the notion of anti-competitive behavior by PE-backed firms, which would imply suppliers cutting ties with better-performing rivals who pose stronger competition. Instead, the evidence aligns with a supply-side mechanism where capacity-constrained suppliers prioritize faster-growing PE-backed customers over lower-performing firms.

We then quantify the economic implications for competitors by constructing a continuous measure of common supplier exposure, defined as the share of a competitor's inputs sourced from suppliers of PE-acquired rivals. Figure A.I in the Internet Appendix plots the distribution of this common supplier exposure measure, illustrating that there is substantial variation in the extent to which the competitors of a PE-acquired firm source inputs from the same versus

[30] The phenomenon of common suppliers serving same-industry rivals has become increasingly salient in recent years. For example, Freeman et al. (2024) document that the average number of rivals sharing at least one supplier rose from 0.53 in 1980 to 1.57 in 2017.

[31] Following prior research, we compute the Altman Z-score as follows:

$$\text{Altman Z-score} = 0.717 \times \frac{\text{Working Capital}_{it}}{\text{Total Assets}_{it}} + 0.847 \times \frac{\text{Retained Income}_{it}}{\text{Total Assets}_{it}} + 3.107 \times \frac{\text{EBIT}_{it}}{\text{Total Assets}_{it}} + 0.42 \times \frac{\text{Equity}_{it}}{\text{Debt}_{it}}.$$

different suppliers. Most firms have little exposure but some obtain nearly 40% of their inputs from common suppliers. Using this common supplier exposure measure, we assess how a PE buyout in a certain industry affects the competitors of the PE-acquired firm, depending on competitors' reliance on suppliers of the PE-acquired firm. Table XV shows that, following a PE buyout, competitors more exposed to common suppliers experience a substantial decline in economic activity relative to less exposed peers. A one standard deviation increase in common supplier exposure is associated with a 1% decline in employment, 2% in EBITDA, and 1% in markups, suggesting that PE buyouts have crowding-out effects through common supplier networks.

Together, these results offer new insights into how PE buyouts shape product market competition. By increasing demand for supplier inputs, PE-backed firms indirectly weaken rivals that rely on the same upstream partners. In other words, PE buyouts generate crowding-out effects on competitors sharing common suppliers.

V. Robustness and extensions

V.A. Parallel trends assumption

Our research design employs a matched difference-in-differences framework, comparing suppliers of PE-backed firms to observably similar suppliers of non-PE-backed firms. The key identification assumption is that, absent the PE buyout, treated and control suppliers would have followed similar economic trajectories. To assess the validity of this assumption, we estimate dynamic difference-in-differences event studies that test for pre-trends in supplier outcomes prior to the buyout. Specifically, we estimate the following regression model:

$$y_{i,t,c} = \sum_{\tau=-4,\tau\neq-1}^{\tau=+5} \beta_\tau \cdot (PE_{i(j),c} \times I_{\tau=t}) + \lambda_{i,c} + \lambda_{t,c} + \epsilon_{i,t,c} \tag{3}$$

where $I_{\tau=t}$ are leads and lags in event time, with $\tau = -1$ being the reference category.

Figures IIa–IId present the results and generally support the parallel trends assumption underlying our difference-in-differences framework. Across all four outcomes, the pre-treatment coefficients are close to zero and statistically insignificant, indicating that suppliers of PE targets were not on systematically different growth trajectories than their controls prior to the buyout.[32] The figures also show that the positive spillover effects dissipate roughly five years after the buyout, broadly coinciding with PE investors' typical exit horizon. This pattern is consistent with the view that supplier benefits are driven by active PE ownership rather

[32] Figures A.IIa–A.IId in the Internet Appendix present analogous estimates for target firms and likewise support the parallel trends assumption.

than pre-existing differences or other changes in target firms' operations, providing additional support for a treatment-effect interpretation of our results.

V.B. Falsification tests

One alternative explanation for our findings could be that PE-backed firms and non-PE-backed firms have relationships with inherently different types of suppliers. These differences may result from the ex-ante sorting process by which customers and suppliers match before a PE buyout. If the matching variables do not sufficiently capture such differences, this could explain the observed differences in supplier outcomes post-buyout. To rule out this alternative explanation, we conduct three falsification tests.

First, we repeat our baseline analysis, but focus on suppliers whose relationship with a PE-backed firm ended right before the PE buyout took place, and analyze their outcomes relative to matched control suppliers.[33] If PE-backed firms and non-PE-backed firms historically have relationships with suppliers that have different levels of growth potential—even in the absence of the PE event—then one might expect to see divergent outcomes for suppliers whose relationship with the PE-backed firm ended before the PE buyout. In contrast, Panels A and B of Table A.V in the Internet Appendix illustrate that there are no significant differences in our estimates for suppliers whose relationship with the PE-backed firm ended right before the PE event. In all columns of both panels, the treatment estimates for this sample are economically small and statistically insignificant. This suggests that our baseline results are not explained by systematic differences in the types of suppliers who have relationships with PE versus non-PE targets.

Second, we repeat our baseline analysis using canceled PE deals (e.g., Faccio and Hsu 2017). If selection rather than treatment from PE buyouts drive our results then one would expect to see divergent outcomes for suppliers of PE targets and non-PE targets, even if the PE deal was not executed in the end (e.g., because the deal was withdrawn).[34] Panels A and B of Table A.VI in the Internet Appendix show that this is not the case. Across all columns of both panels, we do not find statistically significant treatment effects for canceled deals, which further supports that our baseline findings are not driven by systematic differences in suppliers who have relationships with PE targets versus non-PE targets.

Finally, although the staggered timing of PE deals makes it unlikely that our results are driven by unrelated events, we conduct a falsification test using random placebo acquisition dates instead of the actual ones. Specifically, we assign random acquisition dates to PE

[33] This falsification test follows the approach of Agrawal and Tambe (2016), who study the impact of PE buyouts on workers, and use workers who leave PE-backed firms prior to the buyout as a placebo group.

[34] We manually reviewed the stated reasons for deal cancellations and found no instances in which supply chain considerations were cited as the motive for cancellation.

targets, re-run the matching procedure, and re-estimate the effects of these placebo PE deals on supplier outcomes. The results, reported in Table A.VII in the Internet Appendix, show no significant effects of placebo acquisitions on suppliers—neither during normal times nor during economic downturns.

Together, the null results across all three falsification tests confirm that the post-buyout divergence between treated and control suppliers is specific to actual PE events and is not simply spurious correlations.

V.C. Alternative channels

We examine several alternative channels through which PE-backed firms could influence their suppliers. We first assess the potential role of knowledge spillovers. Various studies have shown that the technological and operational advancements of one company can spill over to others within the same industry or across the supply chain (Grossman and Helpman 1991; Aghion and Jaravel 2015). This has been identified as a key channel through which multinational corporations and superstar firms generate benefits for their suppliers (Alfaro-Urena et al. 2022; Amiti et al. 2024). Given that previous research has also documented improvements in managerial practices and innovation activities within PE-backed firms (Lerner et al. 2011; Bloom et al. 2015), suppliers of these firms could potentially benefit by learning about innovative technologies or operational practices adopted by their PE-backed customers.

To explore this channel, we study whether the technological and operational investments of PE-backed firms and their suppliers change post-buyout, proxied by their R&D expenses and high-skilled employees.[35] The results for target firms are reported in Table A.VIII in the Internet Appendix. Consistent with previous studies, we find an increase in the share of high-skilled employees and R&D expenses of target firms, suggesting that PE investors enhance the technological and operational advancements of their portfolio companies. The increase in high-skilled employees is statistically significant, while the increase in R&D expenses is nearly significant ($p = 0.12$ in column (3)). In Table A.IX in the Internet Appendix, we focus on the suppliers of PE-backed firms, but we do not find any evidence that treated suppliers increase their share of high-skilled employees or R&D expenses, inconsistent with the idea that our findings are driven by knowledge spillovers from PE-backed firms to their suppliers.

Additionally, to further address the possibility that knowledge spillovers might be confined to a subset of suppliers, we focus on highly innovative sectors, identified as those with above-average patenting activity (using patent data for Belgian firms obtained from PATSTAT).[36]

[35] The number of observations in both analyses is smaller than in the baseline sample because information on the number of high-skilled employees is available only from 2008 onward.

[36] Unreported results confirm that our findings are consistent if we instead use the OECD's technology intensity classification, which is based on the average R&D intensity of manufacturing industries (Hatzichronoglou 1997; Isaksson et al. 2016).

Panels A, B, and C of Table A.X in the Internet Appendix present analyses restricted to three distinct subsamples; suppliers of target firms operating in highly innovative sectors, suppliers operating in highly innovative sectors, and buyer-supplier pairs within highly innovative sectors, respectively. Across all three subsamples, we find no evidence that suppliers increase their share of high-skilled employees or R&D expenses, which further rules out that our results can be explained by knowledge spillovers. Overall, these findings imply that the positive impact of PE-backed customers on their suppliers is driven by an increased demand mechanism, rather than alternative mechanisms such as technological transfer.

Second, we study the potential role of trade credit. Prior studies have highlighted the importance of trade credit in customer-supplier relationships and the transmission of shocks across supply chains (e.g., Cunat 2007; Garcia-Appendini and Montoriol-Garriga 2013; Costello 2020; Giannetti et al. 2021). For instance, if PE-backed firms have increased bargaining power relative to their suppliers, one might expect that they negotiate more favorable trade credit terms. Alternatively, PE-backed firms' increased access to external debt may reduce their demand for trade credit (Petersen and Rajan 1997; Billett et al. 2025).

To explore this, we analyze how trade credit usage and provision change for both PE-backed firms and their suppliers following a PE buyout. The results are presented in Table A.XI in the Internet Appendix. Panel A focuses on the accounts payable of PE-backed firms, while Panel B addresses the accounts receivable of the suppliers of PE-backed firms. The outcome variables in Panels A and B, respectively, include the amount of accounts payable to total purchase and accounts receivable to total sales in columns (1)–(2) and the average days payable and receivable are outstanding in columns (3)–(4).[37] We find no significant changes in the accounts payable of PE-backed firms or the accounts receivable of their suppliers, implying that changes in trade credit policies are unlikely to drive our results.

Finally, one could wonder whether our results are solely driven by changes in target firms' leverage, rather than by changes in ownership structure. For example, during normal times, increased leverage may allow firms to expand their activities and raise demand for inputs. During crisis periods, leverage may function as a commitment device, enabling firms to limit stakeholders' claims by credibly threatening to forgo investments that would otherwise benefit the stakeholders unless more favorable terms are negotiated (Bronars and Deere 1991; Perotti and Spier 1993). Suppliers, concerned that higher leverage could increase customers bankruptcy risk, may offer price concessions to mitigate the risk of customer default.[38]

To examine this, we compare the impact of PE buyouts with (i) high-leverage M&A

[37]The average days payable is proxied as 365 multiplied by the ratio of accounts payable over cost of goods sold. The average days receivables are outstanding is proxied as 365 multiplied by the ratio of accounts receivable over net sales.

[38]Prior research confirms that customer bankruptcies impose significant adverse effects on suppliers (e.g., Hertzel et al. 2008; Jacobson and Von Schedvin 2015; Carvalho et al. 2021).

transactions and (ii) first-time borrowers. Both events significantly increase firms' leverage, but neither involves the extensive governance changes that characterize PE buyouts. The results for high-leverage M&As, reported in Table A.XII in the Internet Appendix, show that suppliers of firms involved in high-leverage M&As do not perform significantly differently from their peers, neither during normal times nor during crisis periods. We also do not find that high-leverage M&As affect suppliers' markups, suggesting that our baseline results cannot be attributed solely to a change in leverage. Similarly, first-time borrowers, who gain substantial leverage without ownership changes, create only a small increase in sales for suppliers in normal times and do not exert cost-cutting pressure in downturns, as shown in Table A.XIII in the Internet Appendix.[39] Together, these findings indicate that leverage alone—whether from M&A or bank debt—does not replicate the effects of PE ownership (neither the demand-driven supplier growth in normal times nor the cost-cutting pressure during downturns). Rather, active management by PE firms appears to be the key differentiating factor.[40]

V.D. Selection

One could wonder whether, ex-ante, PE investors take into account firms' supply chain structure in their investment decisions. For example, PE funds might target firms that, on average, face lower switching costs vis-à-vis their suppliers (i.e., firms whose suppliers are easier to squeeze or replace during economic downturns). While this would not invalidate the empirical strategy used in our baseline analysis, we formally test this hypothesis by building on the approach of Cohn et al. (2022). Specifically, we estimate linear probability regression models to predict which firms are targeted by PE investors:

$$PE\ target_{f,t} = \beta \cdot X_{f,t} + \gamma \cdot Z_{f(i),t} + \lambda_f + \lambda_t + \epsilon_{f,t} \tag{4}$$

where the dependent variable is an indicator variable equal to one if firm f is acquired by a PE fund in year t, and zero otherwise. $X_{f,t}$ is a vector of firm characteristics (such as firm size, profitability, and leverage), while $Z_{f(i),t}$ is a vector of average supplier characteristics (such as the average size, leverage, and profitability of a firm's suppliers, or the share of a firm's suppliers operating in highly competitive industries). λ_f and λ_t represent firm and time fixed effects, respectively, and $\epsilon_{f,t}$ are robust standard errors clustered at the firm level.

The results are presented in Table A.XIV in the Internet Appendix. We report estimates based on two specifications; one with only the vector of firm controls and another with both firm and supplier controls. Additionally, we present separate regression results in which we

[39] To identify first-time borrowers, we use the credit register maintained by the National Bank of Belgium. Since the register contains data dating back to 2000, we define first-time borrowers as those with no record in the credit register prior to 2005. Accordingly, we restrict the sample period to 2005–2021 for the analysis.

[40] This broadly aligns with papers emphasizing the distinctive features of PE management, particularly in how PE investors handle leverage (e.g., Hotchkiss et al. 2021).

control for the average markups of the firm and its suppliers, which reduces the sample size.

On target firm characteristics, the results consistently show that PE investors target firms that are relatively larger, more profitable, more leveraged, with lower markups. These results accord with findings from Cohn et al. (2022), and could be interpreted as PE acquirers targeting firms with greater growth potential (also see Biesinger et al. 2023).[41] On supplier characteristics, none of the coefficients is consistently significant, suggesting that PE investors do not systematically account for firms' supply chain structure in their investment decisions. While this does not fully eliminate endogeneity concerns, the result—together with the absence of pre-trends and the null findings in the falsification tests—reduces the likelihood that our baseline estimates are driven by endogenous selection.

V.E. Data sample and measurement choices

We conduct four robustness checks with respect to our data sample and measurement choices. First, one could be concerned that PE deals executed during economic downturns may be systematically different from those executed in more stable periods. If that is the case, the differential effect documented in Table VI could be driven by these crisis deals, rather than by deals that were executed ex-ante and subsequently exposed to a downturn. To address this concern, Internet Appendix Table A.XV shows that our results remain robust when we restrict the sample to deals executed in normal times, for which subsequent downturns are arguably more exogenous.

Second, to ensure that our findings are not driven by the specific definition of economic downturns used in the main analysis, we apply an alternative approach. Specifically, we construct a sector-specific downturn indicator, which equals one in the year of and the year following a decline of more than 10% in sector-wide sales (based on 4-digit NACE codes). Overall, about 10% of all sector-year combinations are classified as sector-specific downturns, with roughly half occurring during global recessions and half outside such periods. The results using this alternative downturn measure, reported in Table A.XVI, remain robust.

Third, we show that our results remain robust when excluding buy-and-build private equity (PE) deals from the sample. In recent years, PE investors have increasingly relied on buy-and-build strategies, whereby a portfolio company expands through the acquisition and integration of additional firms (Bansraj and Smit 2025). To verify that our results are not driven by these types of deals, we exclude PE-backed firms that acquired at least one other firm within three years following the PE buyout (using data on mergers and acquisitions

[41] Using confidential textual data contained in pre-deal investment memos and value-creation plans, Biesinger et al. (2023) recently show that PE funds create value for their investors both by selecting firms that are more likely to outperform their peers over the next years and by helping their portfolio companies improve production processes through capital expenditures and acquisitions (not by financial engineering).

from Orbis M&A). Based on this procedure, we identify 28 PE targets that engaged in buy-and-build activity. 70% of these deals are concentrated in the second half of our sample period, consistent with the growing importance of this strategy in recent years. As reported in Table A.XVII in the Internet Appendix, our findings remain largely unchanged when potential buy-and-build deals are excluded from the analysis.

Fourth, we show that our results are robust to alternative measures of firm-level markups. In our baseline analysis, we follow the approach of De Loecker and Warzynski (2012). An advantage of this measure is that it is structurally grounded, in that it identifies markups by estimating production functions, exploiting the wedge between the output elasticity of a flexible input and its expenditure share in revenues. A limitation is that it is not well-suited for application to non-manufacturing firms. We therefore construct two alternative accounting-based measures; the ratio of total sales to input costs, and the ratio of total sales to input costs plus labor costs (Dhyne et al. 2022). These measures are available for all firms and are widely-used proxies for firms' average markups or profit shares. By constructing measures with and without labor costs, we aim to address potential mismeasurement of input costs in sectors where payroll is a central component of production (such as services). The results, reported in Table A.XVIII in the Internet Appendix, show that, on average, markups of suppliers to PE-backed firms remain unchanged, but compress significantly during economic downturns. Economically, the estimates are similar to the baseline when markups are measured as the ratio of total sales to input costs, and roughly half as large when measured as the ratio of total sales to input plus labor costs.

V.F. Matching strategy

To strengthen our identification strategy, we apply a stricter matching strategy that ensures treated and control suppliers are comparable not only in their own characteristics but also in the type of customers they serve before the buyout. Specifically, we augment the baseline matching covariates with the number of customers and the average size, leverage, and profitability of each supplier's customer base. In this setting, the identification assumption is that two suppliers with matching characteristics who have customers that, on average, have similar characteristics, before a PE buyout, would have had otherwise similar outcomes had the PE buyout never taken place. The results of this stricter matching strategy are presented in Panels A and B of Table A.XIX in the Internet Appendix. The number of observations slightly decreases, but our findings remain robust. On average, we continue to observe that suppliers of PE-backed firms exhibit faster growth than their matched controls, except during periods of economic distress.

V.G. SUTVA

Our difference-in-differences specification relies on the stable unit treatment value assumption (SUTVA), which requires that control suppliers are unaffected by PE buyouts. Because buyouts generate crowding-out effects on rivals of PE-backed customers—and these rivals may affect control suppliers—this assumption may be violated, potentially biasing estimates from Equation (1) (Berg et al. 2021). On the one hand, rivals may substitute toward other suppliers in the same industry as treated suppliers, in which case our baseline estimates represent a lower bound. On the other hand, rivals more reliant on the same suppliers as PE-acquired firms experience declines in economic activity; if this reduces demand for suppliers in our control group, our baseline estimates may represent an upper bound. To assess this, we re-estimate our baseline specification using a matched sample in which control suppliers do not supply more than 5% of their total sales to rivals of treated suppliers' PE-backed customers in any post-buyout year. The results are reported in Internet Appendix Table A.XX. The sample size remains largely unchanged, as there are still sufficient potential control firms for each treated firm that satisfy the exclusion restriction described above. The estimates are quantitatively similar to our baseline estimates.

VI. Conclusion

Despite the long-standing interest of academics and policymakers in the economic implications of PE ownership, we lack evidence on the production network spillovers of PE buyouts. This paper fills this gap by combining granular data on customer-supplier relationships and PE buyouts from Belgium—a representative country in terms of PE activity.

Using a difference-in-differences methodology, we show that, on average, suppliers of PE-backed firms perform significantly better than comparable suppliers of non-PE-backed firms. This positive effect is driven by increased demand for inputs from target firms that pursue new growth opportunities post-buyout. In contrast, during economic downturns, suppliers of PE-backed firms cease to outperform their peers and instead reduce their markups. This reversal is due to intensified PE involvement during crisis periods; PE investors exert greater bargaining pressure on suppliers and more actively reconfigure supply chains to achieve cost savings for their portfolio companies. Finally, we document that PE buyouts create crowding-out effects for competitors that share suppliers with PE-backed firms. Specifically, suppliers are more likely to terminate relationships with rivals of their PE-backed customers, particularly when they are ex-ante more capacity-constrained, which in turn harms competitors' performance.

Together, these results provide novel insights into the economic implications of PE own-

ership, highlighting that the effects extend well beyond the acquired firms. Suppliers of PE-backed firms benefit in good times but bear costs in downturns, while rivals are harmed by the reallocation of shared upstream relationships, underscoring that supply chains are a key channel through which PE ownership creates, extracts, and redistributes value.

Our findings have several implications. For one, our results may inform the antitrust evaluation of PE transactions. In many countries, including the U.S., listed firms are already required to publicly disclose their primary suppliers. Regulators could leverage this information to assess the degree of upstream overlap between PE targets and their competitors prior to approving a transaction. Moreover, by showing that bank debt does not generate meaningful upstream spillovers, whereas PE buyouts significantly benefit suppliers by increasing input demand, our findings may contribute to the debate on the roles of bank versus non-bank financing in stimulating economic growth.

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Tables

Table I. Summary statistics

	N	Mean	Median	SD	P10	P90
			Panel A: Full sample			
Firm age	1,486,528	18.069	15.000	12.997	4.000	35.000
Total assets (thousands)	1,486,528	3,796.400	709.258	12,990.786	145.094	6,038.657
Number of employees	1,486,528	19.370	4.100	226.915	1.000	26.800
Debt/TA	1,486,528	0.657	0.670	0.328	0.251	0.957
EBITDA/TA	1,486,528	0.175	0.147	0.124	0.047	0.334
Tangible assets/TA	1,486,528	0.301	0.239	0.250	0.024	0.687
Number of buyer relationships	1,486,528	75.058	22.000	144.991	2.000	194.000
Number of supplier relationships	1,486,528	68.860	49.000	66.955	17.000	141.000
ln(Markup)	304,616	0.917	0.729	0.785	0.230	1.739
Number of skilled employees	1,067,632	2.508	0.000	8.835	0.000	4.613
R&D expenses/ total assets	1,409,037	0.000	0.000	0.004	0.000	0.000
Accounts payable / purchases	1,284,367	0.884	0.885	0.096	0.772	0.992
Accounts receivable / sales	1,258,601	0.938	0.919	0.143	0.817	1.075
			Panel B: Matched sample used for the analysis of target firms			
Firm age	6,690	26.444	23.000	16.657	8.000	47.000
Total assets (thousands)	6,690	36,646.443	20,864.269	37,670.218	2,223.907	105,015.031
Number of employees	6,690	272.929	50.700	1,044.914	6.050	429.600
Debt/total asset	6,690	0.549	0.553	0.320	0.158	0.882
EBITDA / total assets	6,690	0.189	0.152	0.151	0.035	0.399
Tangible assets / total assets	6,690	0.157	0.089	0.178	0.005	0.411
Number of buyer relationships	6,690	233.345	76.000	309.875	5.000	922.000
Number of supplier relationships	6,690	205.135	190.000	123.933	43.000	391.000
ln(Markup)	3,409	0.633	0.328	0.791	0.113	1.480
Number of skilled employees	5,210	23.457	12.667	24.930	0.000	63.775
R&D expenses/ total assets	5,314	0.004	0.000	0.016	0.000	0.003
Accounts payable / purchases	6,339	0.941	0.949	0.073	0.849	1.016
Accounts receivable / sales	6,301	0.979	0.971	0.114	0.880	1.081
			Panel C: Matched sample used for the analysis of suppliers of target firms			
Firm age	42,194	21.641	19.000	14.624	6.000	40.000
Total assets (thousands)	42,194	10,568.426	1,341.123	22,760.062	124.803	30,535.453
Number of employees	42,194	61.827	7.100	453.615	1.400	100.500
Debt/total asset	42,194	0.578	0.553	0.421	0.113	0.951
EBITDA / total assets	42,194	0.197	0.157	0.151	0.044	0.410
Tangible assets / total assets	42,194	0.245	0.180	0.222	0.018	0.584
Number of buyer relationships	42,194	76.249	26.000	117.428	2.000	227.000
Number of supplier relationships	42,193	85.802	57.000	70.767	17.000	215.000
ln(Markup)	13,339	0.761	0.584	0.703	0.139	1.549
Number of skilled employees	28,493	6.792	0.000	15.627	0.000	24.600
R&D expenses/ total assets	41,513	0.001	0.000	0.005	0.000	0.000
Accounts payable / purchases	41,975	0.888	0.893	0.100	0.764	1.000
Accounts receivable / sales	38,819	0.940	0.924	0.123	0.824	1.068

This table reports the number of observations, mean, median, standard deviation, 10^{th} percentile, and 90^{th} percentile for the main variables of interest. Panel A contains statistics for the entire sample of firm-year observations. Panel B contains statistics for the matched sample of PE-backed and comparable non-PE-backed firms. Panel C contains statistics for the matched sample of suppliers of PE-backed firms and comparable suppliers of non-PE-backed firms. The sample period is from 2002 to 2021. Table A.I in the Internet Appendix provides more information about the variable definitions.

Table II. Number of treated firms

PE-backed firms	216
Suppliers of PE-backed firms	23,018
Suppliers of PE-backed firms with sales share $> 5\%$	4,972

This table reports the number of PE targets that could be linked to the firms included in the firm financial statement data from the National Bank of Belgium after applying the data filters explained in Section II. The table also reports the total number of suppliers that had a relationship with a PE-backed customer, as well as the number of suppliers that had a relationship with a PE-backed customer that made up at least 5% of the suppliers' total sales.

Table III. The effect of PE buyouts on target firms

	(1) ln(Debt)	(2) ln(Sales)	(3) ln(Employees)	(4) ln(EBITDA)
Post PE	0.498***	0.215***	0.164***	0.218***
	(0.060)	(0.060)	(0.033)	(0.065)
Observations	6662	6662	6662	6662
Adjusted R-squared	0.922	0.858	0.975	0.788
Firm × Cohort FE	Yes	Yes	Yes	Yes
Year × Cohort FE	Yes	Yes	Yes	Yes

This table reports difference-in-differences estimates of the effect of PE buyouts on target firms, comparing changes in outcomes at treated firms relative to matched control firms before and after the buyout. Across the different columns, the outcome variables are the natural logarithm of total debt, sales, employees, and EBITDA. All specifications include firm-by-cohort and year-by-cohort fixed effects. The sample of treated and control firms is constructed using a matching approach based on firm size, leverage, profitability, and industry, as explained in Section III. Table A.I in the Internet Appendix provides more information about the variable definitions. Standard errors are clustered at the firm-cohort level. *** denotes significance at the 1% level, ** at the 5% level, and * at the 10% level.

Table IV. The effect of PE buyouts on the economic resilience of target firms

	(1) ln(Debt)	(2) ln(Sales)	(3) ln(Employees)	(4) ln(EBITDA)
Post PE	0.469***	0.152**	0.119***	0.138*
	(0.059)	(0.068)	(0.036)	(0.073)
Post PE × Economic downturn	0.088	0.165*	0.123***	0.184*
	(0.079)	(0.099)	(0.041)	(0.100)
Observations	6662	6662	6662	6662
Adjusted R-squared	0.922	0.858	0.975	0.789
Firm × Cohort FE	Yes	Yes	Yes	Yes
Year × Cohort FE	Yes	Yes	Yes	Yes

This table reports difference-in-differences estimates of the effect of PE buyouts on the economic resilience of target firms, comparing changes in outcomes at treated firms relative to matched control firms before and after the buyout, allowing effects to vary during economic downturns. Across the different columns, the outcome variables are the natural logarithm of total debt, sales, employees, and EBITDA. All specifications include firm-by-cohort and year-by-cohort fixed effects. The sample of treated and control firms is constructed using a matching approach based on firm size, leverage, profitability, and industry, as explained in Section III. Table A.I in the Internet Appendix provides more information about the variable definitions. Standard errors are clustered at the firm-cohort level. *** denotes significance at the 1% level, ** at the 5% level, and * at the 10% level.

Table V. The effect of PE buyouts on suppliers of target firms

	(1) ln(Sales)	(2) ln(Employees)	(3) ln(EBITDA)	(4) ln(Markup)
Post PE	0.081***	0.038***	0.060**	-0.009
	(0.023)	(0.013)	(0.024)	(0.020)
Observations	40937	40937	40937	16911
Adjusted R-squared	0.933	0.972	0.901	0.731
Firm × Cohort FE	Yes	Yes	Yes	Yes
Year × Cohort FE	Yes	Yes	Yes	Yes

This table reports difference-in-differences estimates of the effect of PE buyouts on suppliers of PE targets, comparing changes in outcomes at treated relative to matched control suppliers before and after the buyout. Across the different columns, the outcome variables are the natural logarithm of total sales, employees, EBITDA, and markups. All specifications include firm-by-cohort and year-by-cohort fixed effects. The sample of treated and control firms is constructed using a matching approach based on firm size, leverage, profitability, and industry, as explained in Section III. Table A.I in the Internet Appendix provides more information about the variable definitions. Standard errors are clustered at the firm-cohort level. *** denotes significance at the 1% level, ** at the 5% level, and * at the 10% level.

Table VI. The effect of PE buyouts on the economic resilience of suppliers of target firms

	(1) ln(Sales)	(2) ln(Employees)	(3) ln(EBITDA)	(4) ln(Markup)
Post PE	0.099***	0.047***	0.082***	0.007
	(0.026)	(0.013)	(0.026)	(0.021)
Post PE × Economic downturn	-0.075**	-0.038**	-0.068*	-0.081*
	(0.035)	(0.018)	(0.040)	(0.046)
Observations	40937	40937	40937	16911
Adjusted R-squared	0.933	0.972	0.901	0.732
Firm × Cohort FE	Yes	Yes	Yes	Yes
Year × Cohort FE	Yes	Yes	Yes	Yes

This table reports difference-in-differences estimates of the effect of PE buyouts on suppliers of PE targets, comparing changes in outcomes at treated relative to matched control suppliers before and after the buyout, allowing effects to vary during economic downturns. Across the different columns, the outcome variables are the natural logarithm of total sales, employees, EBITDA, and markups. All specifications include firm-by-cohort and year-by-cohort fixed effects. The sample of treated and control firms is constructed using a matching approach based on firm size, leverage, profitability, and industry, as explained in Section III. Table A.I in the Internet Appendix provides more information about the variable definitions. Standard errors are clustered at the firm-cohort level. *** denotes significance at the 1% level, ** at the 5% level, and * at the 10% level.

Table VII. The effect of PE buyouts on suppliers of target firms: Increased demand for inputs channel

	(1) ln(Sales)	(2) ln(Employees)	(3) ln(EBITDA)	(4) ln(Markup)
Panel A				
Post PE	0.021	0.003	0.006	-0.011
	(0.034)	(0.020)	(0.037)	(0.026)
Post PE × Low pre-buyout target growth	0.103**	0.059**	0.093*	0.003
	(0.047)	(0.026)	(0.049)	(0.039)
Observations	40937	40937	40937	16911
Adjusted R-squared	0.933	0.972	0.901	0.731
Panel B				
Post PE	0.068*	0.013	0.016	0.005
	(0.038)	(0.018)	(0.033)	(0.032)
Post PE × High input dependence	0.024	0.045*	0.082*	-0.024
	(0.048)	(0.026)	(0.048)	(0.041)
Observations	40937	40937	40937	16911
Adjusted R-squared	0.933	0.972	0.901	0.731
Panel C				
Post PE	0.038	0.011	0.025	-0.013
	(0.029)	(0.017)	(0.033)	(0.025)
Post PE × Long relationship	0.103**	0.063**	0.085*	0.009
	(0.048)	(0.026)	(0.048)	(0.041)
Observations	40937	40937	40937	16911
Adjusted R-squared	0.933	0.972	0.901	0.731
Firm × Cohort FE	Yes	Yes	Yes	Yes
Year × Cohort FE	Yes	Yes	Yes	Yes

This table reports difference-in-differences estimates of the effect of PE buyouts on suppliers of PE targets, comparing changes in outcomes at suppliers of PE-backed firms relative to matched control suppliers before and after the buyout, allowing effects to vary across suppliers. The outcome variables are the natural logarithm of total sales in column (1), employees in column (2), EBITDA in column (3), and markups in column (4). All specifications include firm-by-cohort and year-by-cohort fixed effects. The sample of treated and control firms is constructed using a matching approach based on firm size, leverage, profitability, and industry, as explained in Section III. Table A.I in the Internet Appendix provides more information about the variable definitions. Standard errors are clustered at the firm-cohort level. *** denotes significance at the 1% level, ** at the 5% level, and * at the 10% level.

Table VIII. The effect of PE buyouts on target firms' demand for inputs from suppliers: Increased demand for inputs channel

	(1) ln(Purchases)	(2) ln(Purchases)	(3) ln(Purchases)
Post PE	0.149***	0.153**	0.182***
	(0.057)	(0.067)	(0.067)
Observations	10285	9538	9499
Adjusted R-squared	0.776	0.775	0.822
Supplier FE	Yes	No	No
Customer FE	Yes	Yes	No
Year FE	Yes	No	No
Supplier × Year FE	No	Yes	Yes
Supplier × Customer FE	No	No	Yes

This table reports difference-in-differences estimates of the effect of PE buyouts on target firms' demand for supplier inputs, comparing changes in purchases from treated PE-backed customers relative to matched non-PE-backed customers before and after the buyout. Across the different columns, the outcome variable is the natural logarithm of total purchases from customer j at supplier i in year t. Across the different columns, the regressions are saturated with supplier fixed effects, customer fixed effects, year fixed effects, supplier-by-year fixed effects, and supplier-by-customer fixed effects, as indicated at the bottom of the table. For each treated supplier, the sample contains PE-backed customers and non-PE-backed customers, which are selected using a matching approach based on firm size, leverage, profitability, and industry, as explained in Section IV.C. Table A.I in the Internet Appendix provides more information about the variable definitions. Standard errors are double clustered at the customer and supplier level. *** denotes significance at the 1% level, ** at the 5% level, and * at the 10% level.

Table IX. The effect of PE buyouts on the economic resilience of suppliers of target firms: Cost-cutting pressure channel

	(1) ln(Sales)	(2) ln(Employees)	(3) ln(EBITDA)	(4) ln(Markup)
Panel A				
Post PE	0.035	-0.004	0.039	0.005
	(0.051)	(0.027)	(0.050)	(0.066)
Post PE × Economic downturn	0.010	-0.003	0.008	0.020
	(0.027)	(0.013)	(0.031)	(0.034)
Post PE × Standardized goods	-0.012	0.004	0.058	-0.044
	(0.050)	(0.026)	(0.050)	(0.041)
Post PE × Economic downturn × Standardized goods	-0.097*	-0.050*	-0.137**	-0.121*
	(0.055)	(0.030)	(0.063)	(0.068)
Observations	40937	40937	40937	16911
Adjusted R-squared	0.933	0.972	0.901	0.732
Panel B				
Post PE	0.077**	0.041**	0.060*	-0.000
	(0.034)	(0.017)	(0.035)	(0.021)
Post PE × Economic downturn	0.000	0.004	0.001	0.018
	(0.025)	(0.013)	(0.031)	(0.022)
Post PE × High competition	0.035	-0.004	0.039	0.005
	(0.051)	(0.027)	(0.050)	(0.066)
Post PE × Economic downturn × High competition	-0.095*	-0.021	-0.116*	-0.244*
	(0.056)	(0.028)	(0.063)	(0.133)
Observations	40937	40937	40937	16911
Adjusted R-squared	0.933	0.972	0.901	0.732
Panel C				
Post PE	0.082*	0.055**	0.042	-0.013
	(0.044)	(0.022)	(0.044)	(0.038)
Post PE × Economic downturn	0.030	-0.010	-0.003	0.021
	(0.033)	(0.015)	(0.038)	(0.036)
Post PE × Large PE firm	0.017	-0.013	0.063	0.020
	(0.054)	(0.027)	(0.054)	(0.045)
Post PE × Economic downturn × Large PE firm	-0.121**	-0.041	-0.114*	-0.113*
	(0.057)	(0.029)	(0.063)	(0.067)
Observations	40937	40937	40937	16911
Adjusted R-squared	0.933	0.972	0.901	0.731
Firm × Cohort FE	Yes	Yes	Yes	Yes
Year × Cohort FE	Yes	Yes	Yes	Yes

This table reports difference-in-differences estimates of the effect of PE buyouts on suppliers of PE targets, comparing changes in outcomes at treated relative to matched control suppliers before and after the buyout, allowing effects to vary during economic downturns and across suppliers. The outcome variables are the natural logarithm of total sales in column (1), employees in column (2), EBITDA in column (3), and markups in column (4). All specifications include firm-by-cohort and year-by-cohort fixed effects. The sample of treated and control firms is constructed using a matching approach based on firm size, leverage, profitability, and industry, as explained in Section III. Table A.I in the Internet Appendix provides more information about the variable definitions. Standard errors are clustered at the firm-cohort level. *** denotes significance at the 1% level, ** at the 5% level, and * at the 10% level.

Table X. The effect of PE buyouts on customer-supplier relationship terminations: Cost-cutting pressure channel

	(1) Relationship terminated	(2) Relationship terminated	(3) Relationship terminated
Panel A			
Post PE	-0.027*	-0.094***	-0.094***
	(0.016)	(0.019)	(0.019)
Post PE × Economic downturn	-0.031	-0.029	-0.024
	(0.022)	(0.031)	(0.032)
Post PE × Standardized goods	-0.043*	-0.036	-0.020
	(0.025)	(0.042)	(0.047)
Post PE × Economic downturn × Standardized goods	0.118**	0.135*	0.130*
	(0.047)	(0.069)	(0.075)
Observations	10109	9373	9334
Adjusted R-squared	0.211	0.350	0.324
Panel B			
Post PE	-0.021	-0.106***	-0.097***
	(0.016)	(0.023)	(0.024)
Post PE × Economic downturn	-0.041	-0.054	-0.048
	(0.027)	(0.036)	(0.037)
Post PE × High competition	-0.023	0.022	0.003
	(0.024)	(0.032)	(0.034)
Post PE × Economic downturn × High competition	0.063*	0.134***	0.126**
	(0.033)	(0.052)	(0.050)
Observations	10109	9373	9334
Adjusted R-squared	0.211	0.350	0.324
Panel C			
Post PE	-0.036**	-0.083***	-0.082***
	(0.017)	(0.020)	(0.020)
Post PE × Economic downturn	-0.026	-0.040	-0.032
	(0.023)	(0.031)	(0.032)
Post PE × Large PE firm	0.025	-0.069**	-0.066*
	(0.027)	(0.034)	(0.035)
Post PE × Economic downturn × Large PE firm	0.027	0.123**	0.112*
	(0.043)	(0.060)	(0.061)
Observations	10109	9373	9334
Adjusted R-squared	0.211	0.350	0.324
Supplier FE	Yes	No	No
Customer FE	Yes	Yes	No
Year FE	Yes	No	No
Supplier × Year FE	No	Yes	Yes
Supplier × Customer FE	No	No	Yes

This table reports difference-in-differences estimates of the effect of PE buyouts on the probability that customer-supplier relationships are terminated, comparing changes for relationships involving treated PE-backed customers relative to matched non-PE-backed customers before and after the buyout, allowing effects to vary during economic downturns and across suppliers. Across the different columns, the outcome variable is a dummy variable equal to one if the relationship between supplier i and customer j is terminated in year $t+1$. Across the different columns, the regressions are saturated with supplier fixed effects, customer fixed effects, year fixed effects, supplier-by-year fixed effects, and supplier-by-customer fixed effects, as indicated at the bottom of the table. For each treated supplier, the sample contains PE-backed customers and non-PE-backed customers, which are selected using a matching approach based on firm size, leverage, profitability, and industry, as explained in Section IV.C. Table A.I in the Internet Appendix provides more information about the variable definitions. Standard errors are double clustered at the customer and supplier level. *** denotes significance at the 1% level, ** at the 5% level, and * at the 10% level.

Table XI. The effect of PE buyouts on target firms' number of suppliers and cost of inputs

	ln(Number of suppliers)		Cost of inputs/Sales	
	(1)	(2)	(3)	(4)
Post PE	0.064***	0.048***	-0.015***	-0.009
	(0.013)	(0.017)	(0.005)	(0.006)
Post PE × Economic downturn		0.044*		-0.017*
		(0.027)		(0.010)
Observations	6662	6662	6106	6106
Adjusted R-squared	0.958	0.958	0.701	0.701
Firm × Cohort FE	Yes	Yes	Yes	Yes
Year × Cohort FE	Yes	Yes	Yes	Yes

This table reports difference-in-differences estimates of the effect of PE buyouts on target firms, comparing changes in outcomes at treated firms relative to matched control firms before and after the buyout, allowing effects to vary during economic downturns. The outcome variables are the natural logarithm of the firm's total number of suppliers in columns (1) and (2), and the ratio of the cost of inputs over total sales in columns (3) and (4). All specifications include firm-by-cohort and year-by-cohort fixed effects. The sample of treated and control firms is constructed using a matching approach based on firm size, leverage, profitability, and industry, as explained in Section III. Table A.I in the Internet Appendix provides more information about the variable definitions. Standard errors are clustered at the firm-cohort level. *** denotes significance at the 1% level, ** at the 5% level, and * at the 10% level.

Table XII. The effect of PE buyouts on the economic resilience of suppliers of target firms: The role of target firms' leverage

Indebtedness measure	PE-backed firms with low ICR		PE-backed firms with high leverage	
	ln(Markup)	ln(Markup)	ln(Markup)	ln(Markup)
	(1)	(2)	(3)	(4)
Post PE	-0.040	-0.048	-0.038	-0.047*
	(0.042)	(0.044)	(0.026)	(0.026)
Post PE × Indebtedness	0.042	0.071	0.063	0.094**
	(0.047)	(0.050)	(0.040)	(0.043)
Post PE × Economic downturn		0.022		0.026
		(0.043)		(0.030)
Post PE × Economic downturn × Indebtedness		-0.138**		-0.113*
		(0.071)		(0.068)
Observations	16911	16911	16911	16911
Adjusted R-squared	0.731	0.732	0.731	0.731
Firm×Cohort FE	Yes	Yes	Yes	Yes
Year×Cohort FE	Yes	Yes	Yes	Yes

This table reports difference-in-differences estimates of the effect of PE buyouts on suppliers of PE targets, comparing changes in outcomes at treated relative to matched control suppliers before and after the buyout, allowing effects to vary during economic downturns and across PE targets. Across the different columns, the outcome variable is the natural logarithm of the supplier's markups. All specifications include firm-by-cohort and year-by-cohort fixed effects. The sample of treated and control firms is constructed using a matching approach based on firm size, leverage, profitability, and industry, as explained in Section III. Table A.I in the Internet Appendix provides more information about the variable definitions. Standard errors are clustered at the firm-cohort level. *** denotes significance at the 1% level, ** at the 5% level, and * at the 10% level.

Table XIII. The effect of PE buyouts on customer-supplier relationship terminations with rivals of PE-backed customers

	(1) Relationship terminated	(2) Relationship terminated	(3) Relationship terminated
Post PE	0.016*	-0.002	0.002
	(0.009)	(0.025)	(0.035)
Post PE × Competitor	-0.011	0.064*	0.105***
	(0.018)	(0.038)	(0.037)
Observations	77417	76059	48673
Adjusted R-squared	0.164	0.181	0.240
Supplier FE	Yes	No	No
Customer FE	Yes	Yes	No
Year FE	Yes	No	No
Customer × Year FE	No	Yes	Yes
Supplier × Customer FE	No	No	Yes

This table reports difference-in-differences estimates of the effect of PE buyouts on the probability that treated suppliers terminate customer-supplier relationships with competitors of their PE-backed customers, comparing changes for suppliers of PE-backed customers relative to matched suppliers of non-PE-backed customers before and after the buyout. Across the different columns, the outcome variable is a dummy variable equal to one if the relationship between supplier i and customer j is terminated in year $t+1$. Across the different columns, the regressions are saturated with supplier fixed effects, customer fixed effects, year fixed effects, customer-by-year fixed effects, and supplier-by-customer fixed effects, as indicated at the bottom of the table. For each treated supplier, the sample contains suppliers of PE-backed customers and suppliers of non-PE-backed customers, which are selected using a matching approach based on firm size, leverage, profitability, and industry, as explained in Section IV.C. Table A.I in the Internet Appendix provides more information about the variable definitions. Standard errors are clustered at the supplier level. *** denotes significance at the 1% level, ** at the 5% level, and * at the 10% level.

Table XIV. The effect of PE buyouts on customer-supplier relationship terminations with rivals of PE-backed customers: Capacity constraints channel

	(1) Relationship terminated	(2) Relationship terminated	(3) Relationship terminated
Post PE	-0.003	-0.001	0.003
	(0.035)	(0.052)	(0.057)
Post PE × Competitor	0.102***	-0.031	-0.027
	(0.038)	(0.053)	(0.054)
Post PE × Competitor × Low capacity slack supplier	0.081*		
	(0.048)		
Post PE × Competitor × Low EBITDA customer		0.141**	
		(0.063)	
Post PE × Competitor × Low Altman Z-score customer			0.163***
			(0.050)
Observations	48673	48673	48673
Adjusted R-squared	0.240	0.239	0.239
Customer × Year FE	Yes	Yes	Yes
Supplier × Customer FE	Yes	Yes	Yes

This table reports difference-in-differences estimates of the effect of PE buyouts on the probability that treated suppliers terminate customer-supplier relationships with competitors of their PE-backed customers, comparing changes for suppliers of PE-backed customers relative to matched suppliers of non-PE-backed customers before and after the buyout, allowing effects to vary across suppliers and customers. Across the different columns, the outcome variable is a dummy variable equal to one if the relationship between supplier i and customer j is terminated in year $t+1$. All specifications include customer-by-year and supplier-by-customer fixed effects. For each treated supplier, the sample contains suppliers of PE-backed customers and suppliers of non-PE-backed customers, which are selected using a matching approach based on firm size, leverage, profitability, and industry, as explained in Section IV.C. Table A.I in the Internet Appendix provides more information about the variable definitions. Standard errors are clustered at the supplier level. *** denotes significance at the 1% level, ** at the 5% level, and * at the 10% level.

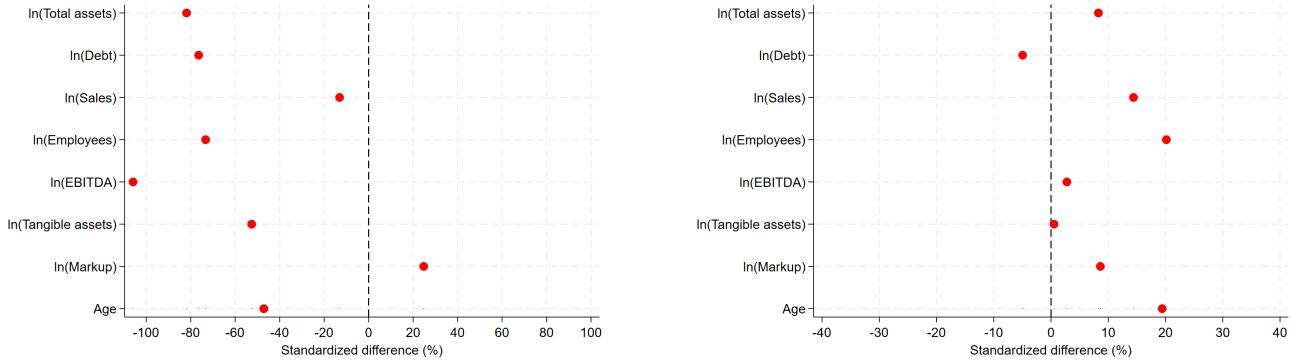
Table XV. The effect of PE buyouts on rivals of PE-backed firms reliant on common suppliers

	(1) ln(Sales)	(2) ln(Employees)	(3) ln(EBITDA)	(4) ln(Markup)
Post PE	-0.000	-0.011***	-0.026***	0.010
	(0.006)	(0.003)	(0.006)	(0.006)
Post PE× Common supplier exposure	-0.081	-0.080*	-0.195***	-0.120**
	(0.075)	(0.048)	(0.072)	(0.052)
Observations	262698	262698	262698	81604
Adjusted R-squared	0.913	0.940	0.823	0.682
Firm×Cohort FE	Yes	Yes	Yes	Yes
Year×Cohort FE	Yes	Yes	Yes	Yes

This table reports difference-in-differences estimates of the effect of PE buyouts on competitors of PE-backed firms through common suppliers, comparing changes in outcomes for exposed competitors relative to less exposed competitors before and after the buyout. The sample is restricted to firms operating in (4-digit NACE) industries with at least one PE buyout over the sample period. Across the different columns, the outcome variables are the natural logarithm of total sales, employees, EBITDA, and markups. All specifications include firm-by-cohort and year-by-cohort fixed effects. Table A.I in the Internet Appendix provides more information about the variable definitions. Standard errors are clustered at the industry level. *** denotes significance at the 1% level, ** at the 5% level, and * at the 10% level.

Figures

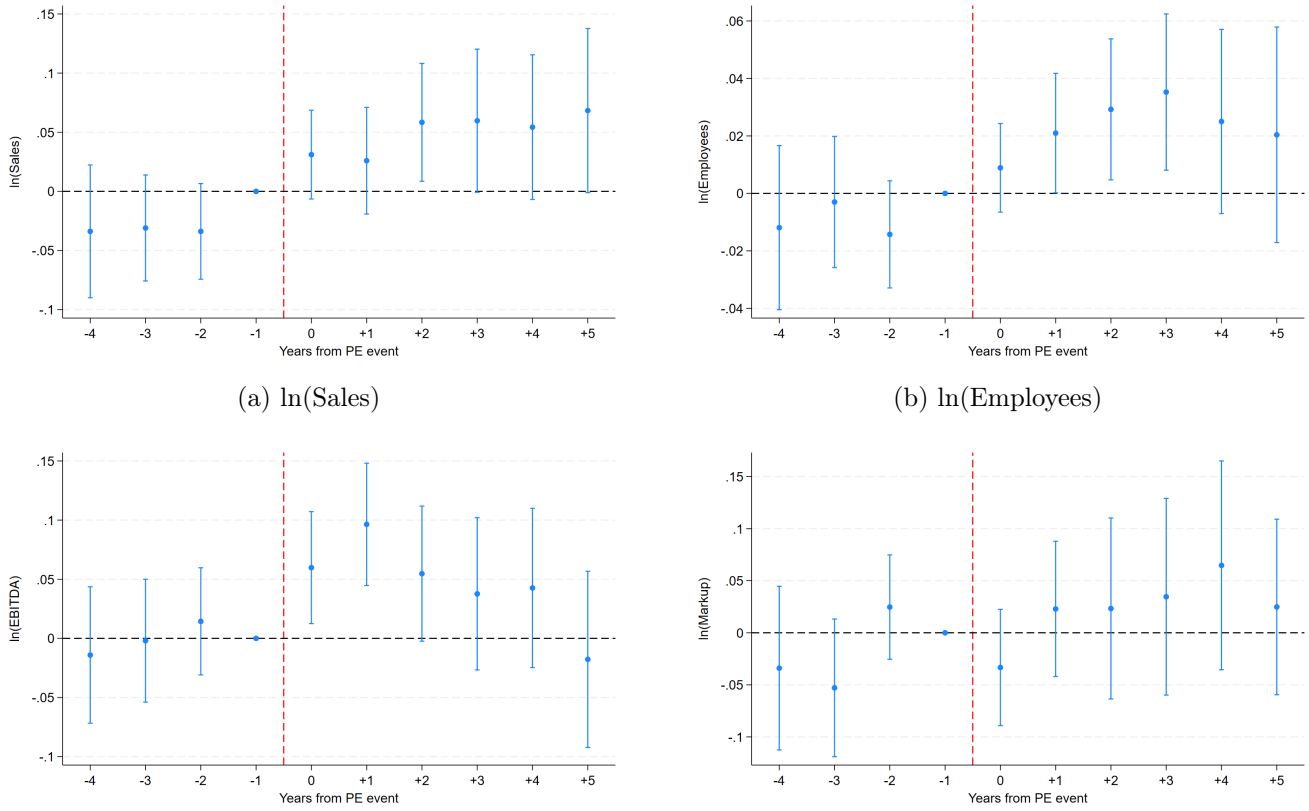
Figure I. Balance tests: suppliers of target firms



(a) Unmatched (b) Matched

This figure presents the normalized mean differences for the sample of treated suppliers and control suppliers, before and after applying the matching strategy explained in Section III. The standardized difference test is a scale-and-sample-size-free estimator proposed by Imbens and Wooldridge (2009), for which Imbens and Rubin (2015) proposed a heuristic threshold of 25% in absolute value for significant differences.

Figure II. Dynamic difference-in-differences estimates for the effect of PE buyouts on suppliers of target firms



(a) ln(Sales)

(b) ln(Employees)

(c) ln(EBITDA)

(d) ln(Markup)

This figure presents the dynamic difference-in-differences estimates of the effect of PE buyouts on suppliers of PE targets, comparing changes in outcomes at treated relative to matched control suppliers before and after the buyout. The y-axis corresponds to the coefficient estimates of β from Equation (3). The x-axis corresponds to years relative to the year in which the target firm was acquired. The dependent variables are the natural logarithm of sales, employees, EBITDA, and markups. The sample of treated and control firms is constructed using a matching approach based on firm size, leverage, profitability, and industry, as explained in Section III. Table A.I in the Internet Appendix provides more information about the variable definitions. The vertical bars represent confidence intervals at the 95% level.

INTERNET APPENDIX:

The Supply Chain Spillovers of Private Equity Buyouts

Cédric Huylebroek and Olivier De Jonghe

Internet Appendix A

Table A.I. Variable definitions

Variable	Description
Age	The number of years since the firm was founded.
Total assets (thousands)	The total assets of the firm, measured in thousands of euros.
Sales	The total sales of the firm, measured in thousands of euros.
Number of employees	The total number of employees in the firm.
Debt/TA	The ratio of total debt to total assets.
EBITDA/TA	The ratio of earnings before interest, taxes, depreciation, and amortization (EBITDA) to total assets.
Tangible assets/TA	The ratio of tangible assets to total assets.
R&D expenses/TA	The ratio of research and development expenses to total assets.
ln(Markup)	The natural logarithm of firm-level markups, estimated following the procedure from De Loecker and Warzynski (2012).
Number of skilled employees	The total number of employees with a higher education degree.
Accounts payable/purchases	The ratio of accounts payable over total purchases.
Accounts receivable/sales	The ratio of accounts receivable over total sales.
Average days payable	365 multiplied by the ratio of accounts payable over cost of goods sold.
Average days receivable	365 multiplied by the ratio of accounts receivable by net sales.
Number of suppliers	The total number of suppliers that the firm has a relationship with.
Number of customers	The total number of customers that the firm has a relationship with.

This table provides the variable definitions of our main variables of interest.

Table A.II. The effect of PE buyouts on suppliers of target firms: Treatment intensity heterogeneity

	(1) ln(Sales)	(2) ln(Employees)	(3) ln(EBITDA)	(4) ln(Markup)
Post PE × Sales share ∈ (0%, 5%]	0.032	0.019	0.051	0.008
	(0.034)	(0.017)	(0.033)	(0.029)
Post PE × Sales share ∈ (5%, 10%]	0.140***	0.060***	0.072**	-0.027
	(0.031)	(0.019)	(0.034)	(0.028)
Post PE× Sales share ∈ (10%, 100%]	0.129***	0.071***	0.079***	-0.008
	(0.008)	(0.005)	(0.008)	(0.009)
Observations	361640	361640	361640	121178
Adjusted R-squared	0.926	0.972	0.898	0.752
Firm × Cohort FE	Yes	Yes	Yes	Yes
Year × Cohort FE	Yes	Yes	Yes	Yes

This table reports difference-in-differences estimates of the effect of PE buyouts on suppliers of PE targets, comparing changes in outcomes at treated relative to matched control suppliers before and after the buyout, allowing for heterogeneity in pre-buyout sales shares. Across the different columns, the outcome variables are the natural logarithm of total sales, employees, EBITDA, and markups. All specifications include firm-by-cohort and year-by-cohort fixed effects. The sample of treated and control firms is constructed using a granular matching approach based on firm size, leverage, profitability, and industry, as explained in Section III. Table A.I in the Internet Appendix provides more information about the variable definitions. Standard errors are clustered at the firm-cohort level. *** denotes significance at the 1% level, ** at the 5% level, and * at the 10% level.

Table A.III. The effect of PE buyouts on target firms:
Pre-buyout target growth heterogeneity

	(1) ln(Debt)	(2) ln(Sales)	(3) ln(Employees)	(4) ln(EBITDA)
Post PE	0.345***	0.113	0.077*	0.100
	(0.090)	(0.090)	(0.042)	(0.084)
Post PE × Low pre-buyout target growth	0.308***	0.205*	0.174***	0.235**
	(0.118)	(0.119)	(0.066)	(0.117)
Observations	6662	6662	6662	6662
Adjusted R-squared	0.922	0.858	0.975	0.801
Firm × Cohort FE	Yes	Yes	Yes	Yes
Year × Cohort FE	Yes	Yes	Yes	Yes

This table reports difference-in-differences estimates of the effect of PE buyouts on target firms, comparing changes in outcomes at treated firms relative to matched control firms before and after the buyout, allowing for heterogeneity in pre-buyout target growth. Across the different columns, the outcome variables are the natural logarithm of total debt, sales, employees, and EBITDA. All specifications include firm-by-cohort and year-by-cohort fixed effects. The sample of treated and control firms is constructed using a granular matching approach based on firm size, leverage, profitability, and industry, as explained in Section III. Table A.I in the Internet Appendix provides more information about the variable definitions. Standard errors are clustered at the firm-cohort level. *** denotes significance at the 1% level, ** at the 5% level, and * at the 10% level.

Table A.IV. The effect of PE buyouts on suppliers of target firms: Certification channel

	ln(Number of customers) (1)	ln(Number of within-network customers) (2)	ln(Number of outside-network customers) (3)	ln(Exports to PE investor country) (4)
Post PE	0.047***	0.050***	-0.004	0.062**
	(0.017)	(0.011)	(0.016)	(0.031)
Observations	40937	40937	40937	22396
Adjusted R-squared	0.933	0.836	0.921	0.909
Firm × Cohort FE	Yes	Yes	Yes	Yes
Year × Cohort FE	Yes	Yes	Yes	Yes

This table reports difference-in-differences estimates of the effect of PE buyouts on suppliers of PE targets, comparing changes in outcomes at treated relative to matched control suppliers before and after the buyout. Across the different columns, the outcome variables are the natural logarithm of the total number of customers, the number of customers within the PE-backed firms' network, the number of customers outside of the PE-backed firms' network, and the value of exports to the country of origin of the target firms' PE investor. All specifications include firm-by-cohort and year-by-cohort fixed effects. The sample of treated and control firms is constructed using a matching approach based on firm size, leverage, profitability, and industry, as explained in Section III. Table A.I in the Internet Appendix provides more information about the variable definitions. Standard errors are clustered at the firm-cohort level. *** denotes significance at the 1% level, ** at the 5% level, and * at the 10% level.

Table A.V. The effect of PE buyouts on suppliers of target firms:
Falsification test based on supplier-customer relationships that ended one year pre-buyout

	(1) ln(Sales)	(2) ln(Employees)	(3) ln(EBITDA)	(4) ln(Markup)
Panel A:				
Post $PE_{placebo}$	-0.041	-0.001	-0.023	-0.001
	(0.032)	(0.016)	(0.032)	(0.034)
Observations	29653	29653	29653	9713
Adjusted R-squared	0.940	0.969	0.891	0.784
Panel B:				
Post $PE_{placebo}$	-0.040	-0.011	-0.023	0.034
	(0.040)	(0.018)	(0.038)	(0.037)
Post $PE_{placebo}$ × Economic Downturn	0.008	0.038	0.019	-0.055
	(0.053)	(0.027)	(0.057)	(0.059)
Observations	29653	29653	29653	9713
Adjusted R-squared	0.940	0.969	0.891	0.784
Firm × Cohort FE	Yes	Yes	Yes	Yes
Year × Cohort FE	Yes	Yes	Yes	Yes

This table reports difference-in-differences estimates of the effect of PE buyouts on the suppliers of target firms, using as a placebo sample of suppliers whose relationships with the target firm ended before the buyout, comparing changes in outcomes of placebo treated suppliers relative to matched control suppliers before and after the buyout, allowing effects to vary during economic downturns. Across the different columns, the outcome variables are the natural logarithm of total sales, employees, EBITDA, and markups. All specifications include firm-by-cohort and year-by-cohort fixed effects. The sample of treated and control firms is constructed using a granular matching approach based on firm size, leverage, profitability, and industry, as explained in Section III. Table A.I in the Internet Appendix provides more information about the variable definitions. Standard errors are clustered at the firm-cohort level. *** denotes significance at the 1% level, ** at the 5% level, and * at the 10% level.

Table A.VI. The effect of PE buyouts on suppliers of target firms: Falsification test based on canceled deals

	(1) ln(Sales)	(2) ln(Employees)	(3) ln(EBITDA)	(4) ln(Markup)
Panel A:				
Post $PE_{canceled}$	0.037	0.001	0.010	-0.018
	(0.037)	(0.025)	(0.041)	(0.049)
Observations	13098	13098	13098	5405
Adjusted R-squared	0.949	0.972	0.913	0.689
Panel B:				
Post $PE_{canceled}$	0.028	-0.000	-0.014	-0.007
	(0.047)	(0.028)	(0.050)	(0.057)
Post $PE_{canceled}$ × Economic Downturn	0.022	0.003	0.049	-0.027
	(0.050)	(0.030)	(0.065)	(0.073)
Observations	13098	13098	13098	5405
Adjusted R-squared	0.949	0.972	0.913	0.689
Firm × Cohort FE	Yes	Yes	Yes	Yes
Year × Cohort FE	Yes	Yes	Yes	Yes

This table reports difference-in-differences estimates of the effect of PE buyouts on the suppliers of target firms, using as a placebo sample suppliers linked to canceled PE deals, and comparing changes in outcomes for placebo-treated suppliers relative to matched control suppliers before and after the buyout, allowing effects to vary during economic downturns. All specifications include firm-by-cohort and year-by-cohort fixed effects. The sample of treated and control firms is constructed using a granular matching approach based on firm size, leverage, profitability, and industry, as explained in Section III. Table A.I in the Internet Appendix provides more information about the variable definitions. Standard errors are clustered at the firm-cohort level. *** denotes significance at the 1% level, ** at the 5% level, and * at the 10% level.

Table A.VII. The effect of PE buyouts on suppliers of target firms: Placebo test

	(1) ln(Sales)	(2) ln(Employees)	(3) ln(EBITDA)	(4) ln(Markup)
Panel A:				
Post $PE_{placebo}$	-0.022	0.038	0.000	-0.001
	(0.050)	(0.025)	(0.045)	(0.025)
Observations	10722	10722	10722	4610
Adjusted R-squared	0.922	0.972	0.911	0.779
Panel B:				
Post $PE_{placebo}$	0.003	0.035	0.023	-0.003
	(0.052)	(0.026)	(0.045)	(0.027)
Post $PE_{placebo}$ × Economic Downturn	-0.100	0.062	-0.095	-0.027
	(0.091)	(0.044)	(0.103)	(0.114)
Observations	10722	10722	10722	4610
Adjusted R-squared	0.922	0.972	0.911	0.779
Firm × Cohort FE	Yes	Yes	Yes	Yes
Year × Cohort FE	Yes	Yes	Yes	Yes

This table reports difference-in-differences estimates of the effect of PE buyouts on the suppliers of target firms, using as a placebo sample of suppliers linked to PE deals with randomized instead of actual deal years, comparing changes in outcomes of placebo treated suppliers relative to matched control suppliers before and after the buyout, allowing effects to vary during economic downturns. Across the different columns, the outcome variables are the natural logarithm of total sales, employees, EBITDA, and markups. All specifications include firm-by-cohort and year-by-cohort fixed effects. The sample of treated and control firms is constructed using a granular matching approach based on firm size, leverage, profitability, and industry, as explained in Section III. Table A.I in the Internet Appendix provides more information about the variable definitions. Standard errors are clustered at the firm-cohort level. *** denotes significance at the 1% level, ** at the 5% level, and * at the 10% level.

Table A.VIII. The effect of PE buyouts on target firms:
Alternative channel: Knowledge spillovers

	ln(Skilled labor)		ln(R&D expenses)	
	(1)	(2)	(3)	(4)
Post PE	0.254***	0.234**	0.577	0.368
	(0.090)	(0.109)	(0.390)	(0.364)
Post PE × Economic downturn		0.035		0.639
		(0.136)		(0.576)
Observations	5163	5163	5204	5204
Adjusted R-squared	0.830	0.829	0.743	0.743
Firm×Cohort FE	Yes	Yes	Yes	Yes
Year×Cohort FE	Yes	Yes	Yes	Yes

This table reports difference-in-differences estimates of the effect of PE buyouts on target firms, comparing changes in outcomes at treated firms relative to matched control firms before and after the buyout, allowing effects to vary during economic downturns. The outcome variables are the natural logarithm of highly skilled employees in columns (1) and (2), and the natural logarithm of R&D expenses in columns (3) and (4). All specifications include firm-by-cohort and year-by-cohort fixed effects. The sample of treated and control firms is constructed using a granular matching approach based on firm size, leverage, profitability, and industry, as explained in Section III. Table A.I in the Internet Appendix provides more information about the variable definitions. Standard errors are clustered at the firm-cohort level. *** denotes significance at the 1% level, ** at the 5% level, and * at the 10% level.

Table A.IX. The effect of PE buyouts on suppliers of target firms: Alternative channel: Knowledge spillovers

	ln(Skilled labor)		ln(R&D expenses)	
	(1)	(2)	(3)	(4)
Post PE	-0.007	-0.013	0.005	-0.005
	(0.027)	(0.033)	(0.050)	(0.049)
Post PE × Economic downturn		0.003		0.034
		(0.032)		(0.069)
Observations	27013	27013	40171	40171
Adjusted R-squared	0.865	0.865	0.729	0.729
Firm×Cohort FE	Yes	Yes	Yes	Yes
Year×Cohort FE	Yes	Yes	Yes	Yes

This table reports difference-in-differences estimates of the effect of PE buyouts on the suppliers of target firms, comparing changes in outcomes of treated suppliers relative to matched control suppliers before and after the buyout, allowing effects to vary during economic downturns. The outcome variables are the natural logarithm of highly skilled employees in columns (1) and (2), and the natural logarithm of R&D expenses in columns (3) and (4). All specifications include firm-by-cohort and year-by-cohort fixed effects. The sample of treated and control firms is constructed using a granular matching approach based on firm size, leverage, profitability, and industry, as explained in Section III. Table A.I in the Internet Appendix provides more information about the variable definitions. Standard errors are clustered at the firm-cohort level. *** denotes significance at the 1% level, ** at the 5% level, and * at the 10% level.

Table A.X. The effect of PE buyouts on suppliers of target firms: Alternative channel: Knowledge spillovers in specific sectors

	ln(Skilled labor)		ln(R&D expenses)	
	(1)	(2)	(3)	(4)
Panel A:	Innovative sectors (targets)			
Post PE	-0.036	-0.039	-0.019	-0.035
	(0.036)	(0.046)	(0.056)	(0.051)
Post PE × Economic downturn		0.003		0.064
		(0.048)		(0.095)
Observations	15023	15023	25800	25800
Adjusted R-squared	0.856	0.856	0.728	0.728
Panel B:	Innovative sectors (suppliers)			
Post PE	0.004	-0.011	-0.074	-0.074
	(0.055)	(0.066)	(0.093)	(0.084)
Post PE × Economic downturn		0.028		-0.026
		(0.063)		(0.137)
Observations	10091	10091	16063	16063
Adjusted R-squared	0.839	0.839	0.761	0.761
Panel C:	Innovative sectors (targets & suppliers)			
Post PE	-0.067	-0.060	-0.053	-0.073
	(0.066)	(0.082)	(0.106)	(0.089)
Post PE × Economic downturn		-0.015		0.111
		(0.086)		(0.189)
Observations	6465	6465	11590	11590
Adjusted R-squared	0.832	0.832	0.767	0.767
Firm×Cohort FE	Yes	Yes	Yes	Yes
Year×Cohort FE	Yes	Yes	Yes	Yes

This table reports difference-in-differences estimates of the effect of PE buyouts on the suppliers of target firms, comparing changes in outcomes of treated suppliers relative to matched control suppliers before and after the buyout, allowing effects to vary during economic downturns and across sectors. The outcome variables are the natural logarithm of highly skilled employees in columns (1) and (2), and the natural logarithm of R&D expenses in columns (3) and (4). All specifications include firm-by-cohort and year-by-cohort fixed effects. The sample of treated and control firms is constructed using a granular matching approach based on firm size, leverage, profitability, and industry, as explained in Section III. Table A.I in the Internet Appendix provides more information about the variable definitions. Standard errors are clustered at the firm-cohort level. *** denotes significance at the 1% level, ** at the 5% level, and * at the 10% level.

Table A.XI. The effect of PE buyouts on suppliers of target firms: Alternative channel: Trade credit

	(1)	(2)	(3)	(4)
Panel A: PE-backed firms	Accounts payable		Days payable outstanding	
Post PE	0.003	-0.001	3.425	2.395
	(0.003)	(0.004)	(3.149)	(3.225)
Post PE × Economic downturn		0.009		1.578
		(0.006)		(4.399)
Observations	6296	6296	5247	5247
Adjusted R-squared	0.767	0.767	0.699	0.699
Panel B: Suppliers of PE-backed firms	Accounts receivable		Days sales outstanding	
Post PE	-0.002	-0.000	2.384	3.586
	(0.002)	(0.003)	(2.333)	(2.440)
Post PE × Economic downturn		-0.006		-5.970
		(0.004)		(4.123)
Observations	37405	37405	22329	22329
Adjusted R-squared	0.690	0.690	0.677	0.677
Firm × Cohort FE	Yes	Yes	Yes	Yes
Year × Cohort FE	Yes	Yes	Yes	Yes

This table reports difference-in-differences estimates of the effect of PE buyouts on the suppliers of target firms, comparing changes in outcomes of treated suppliers relative to matched control suppliers before and after the buyout, allowing effects to vary during economic downturns. The outcome variables in columns (1) and (2) are the ratio of accounts payable to total purchases in Panel A, and the ratio of accounts receivable to total sales for the sample in Panel B. The outcome variables in columns (3) and (4) are the average days payables are outstanding in Panel A, and the average days receivables are outstanding in Panel B. All specifications include firm-by-cohort and year-by-cohort fixed effects. The sample of treated and control firms is constructed using a granular matching approach based on firm size, leverage, profitability, and industry, as explained in Section III. Table A.I in the Internet Appendix provides more information about the variable definitions. Standard errors are clustered at the firm-cohort level. *** denotes significance at the 1% level, ** at the 5% level, and * at the 10% level.

Table A.XII. The effect of high-leverage M&As on suppliers

	(1) ln(Sales)	(2) ln(Employees)	(3) ln(EBITDA)	(4) ln(Markup)
Panel A:				
Post High-leverage M&A	0.037	0.001	0.010	-0.018
	(0.037)	(0.025)	(0.041)	(0.049)
Observations	13098	13098	13098	5405
Adjusted R-squared	0.949	0.972	0.913	0.689
Panel B:				
Post High-leverage M&A	0.028	-0.000	-0.014	-0.007
	(0.047)	(0.028)	(0.050)	(0.057)
Post High-leverage M&A × Economic Downturn	0.022	0.003	0.049	-0.027
	(0.050)	(0.030)	(0.065)	(0.073)
Observations	13098	13098	13098	5405
Adjusted R-squared	0.949	0.972	0.913	0.689
Firm × Cohort FE	Yes	Yes	Yes	Yes
Year × Cohort FE	Yes	Yes	Yes	Yes

This table reports difference-in-differences estimates of the effect of high-leverage M&As on the suppliers of acquired firms, comparing changes in outcomes of treated suppliers relative to matched control suppliers before and after the high-leverage M&A transaction, allowing effects to vary during economic downturns. Across the different columns, the outcome variables are the natural logarithm of total sales, employees, EBITDA, and markups. All specifications include firm-by-cohort and year-by-cohort fixed effects. The sample of treated and control firms is constructed using a granular matching approach based on firm size, leverage, profitability, and industry, as explained in Section III. Table A.I in the Internet Appendix provides more information about the variable definitions. Standard errors are clustered at the firm-cohort level. *** denotes significance at the 1% level, ** at the 5% level, and * at the 10% level.

Table A.XIII. The effect of first-time bank borrowers on suppliers

	(1) ln(Sales)	(2) ln(Employees)	(3) ln(EBITDA)	(4) ln(Markup)
Panel A:				
Post first-time borrower	-0.013	0.025	0.044	-0.029
	(0.049)	(0.021)	(0.037)	(0.040)
Observations	28519	28519	28519	6751
Adjusted R-squared	0.917	0.974	0.903	0.790
Panel B:				
Post first-time borrower	-0.005	0.027	0.037	-0.005
	(0.051)	(0.021)	(0.039)	(0.042)
Post first-time borrower × Economic Downturn	-0.026	-0.016	0.037	-0.055
	(0.082)	(0.031)	(0.068)	(0.072)
Observations	28519	28519	28519	6751
Adjusted R-squared	0.917	0.974	0.903	0.791
Firm × Cohort FE	Yes	Yes	Yes	Yes
Year × Cohort FE	Yes	Yes	Yes	Yes

This table reports difference-in-differences estimates of the effect of first-time bank credit on suppliers of first-time bank borrowers, comparing changes in outcomes for treated suppliers relative to matched control suppliers before and after the bank loan, allowing effects to vary during economic downturns. Across the different columns, the outcome variables are the natural logarithm of total sales, employees, EBITDA, and markups. All specifications include firm-by-cohort and year-by-cohort fixed effects. The sample of treated and control firms is constructed using a granular matching approach based on firm size, leverage, profitability, and industry, as explained in Section III. Table A.I in the Internet Appendix provides more information about the variable definitions. Standard errors are clustered at the firm-cohort level. *** denotes significance at the 1% level, ** at the 5% level, and * at the 10% level.

Table A.XIV. The determinants of PE buyouts

	(1) PE target	(2) PE target	(3) PE target	(4) PE target
ln(Total assets)	0.0003***	0.0003***	0.0006***	0.0007***
	(0.0000)	(0.0000)	(0.0001)	(0.0002)
ln(Employees)	0.0001**	0.0001**	-0.0000	0.0000
	(0.0000)	(0.0000)	(0.0001)	(0.0001)
Debt/TA	0.0002***	0.0002***	0.0004*	0.0004*
	(0.0001)	(0.0001)	(0.0002)	(0.0002)
Accounts receivable	0.0001	0.0001	-0.0001	0.0002
	(0.0001)	(0.0001)	(0.0004)	(0.0005)
EBITDA/TA	0.0006***	0.0006***	0.0017***	0.0018***
	(0.0001)	(0.0001)	(0.0005)	(0.0006)
ln(Markup)			-0.0002**	-0.0002**
			(0.0001)	(0.0001)
$\ln(\text{Total assets})_{\text{average supplier}}$		0.0000		0.0001
		(0.0000)		(0.0002)
$\ln(\text{Employees})_{\text{average supplier}}$		-0.0000		-0.0000
		(0.0000)		(0.0002)
$\text{Debt/TA}_{\text{average supplier}}$		0.0001		0.0010
		(0.0001)		(0.0006)
$\text{Accounts payable}_{\text{average supplier}}$		0.0009		-0.0013
		(0.0006)		(0.0018)
$\text{EBITDA/TA}_{\text{average supplier}}$		0.0001		0.0010
		(0.0002)		(0.0008)
Number of suppliers		-0.0000		-0.0004**
		(0.0000)		(0.0001)
Share of suppliers offering standardized inputs		0.0001		0.0001
		(0.0001)		(0.0003)
Share of suppliers in low competition sectors		-0.0001		-0.0000
		(0.0001)		(0.0003)
$\ln(\text{Markup})_{\text{average supplier}}$				0.0001
				(0.0001)
Observations	1392361	1392361	293239	284258
Adjusted R-squared	0.06	0.06	0.08	0.08
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

This table reports the determinants of PE buyouts using a linear probability model. Across the different columns, the outcome variable is a dummy variable equal to one if firm f is a PE target in year t, and zero otherwise. All specifications include firm and year fixed effects. Table A.I in the Internet Appendix provides more information about the variable definitions. Standard errors are clustered at the firm level. *** denotes significance at the 1% level, ** at the 5% level, and * at the 10% level.

Table A.XV. The effect of PE buyouts on suppliers of target firms: Excluding PE deals completed during economic downturns

	(1) ln(Sales)	(2) ln(Employees)	(3) ln(EBITDA)	(4) ln(Markup)
Panel A:				
Post PE	0.090***	0.033**	0.066**	-0.018
	(0.027)	(0.015)	(0.028)	(0.022)
Observations	34051	34051	34051	14901
Adjusted R-squared	0.930	0.970	0.896	0.751
Panel B:				
Post PE	0.108***	0.042***	0.090***	0.012
	(0.029)	(0.015)	(0.029)	(0.022)
Post PE × Economic downturn	-0.081*	-0.043*	-0.108**	-0.129**
	(0.041)	(0.024)	(0.049)	(0.053)
Observations	34051	34051	34051	14901
Adjusted R-squared	0.930	0.970	0.896	0.751
Firm × Cohort FE	Yes	Yes	Yes	Yes
Year × Cohort FE	Yes	Yes	Yes	Yes

This table reports difference-in-differences estimates of the effect of PE buyouts on suppliers of PE targets, comparing changes in outcomes at treated relative to matched control suppliers before and after the buyout, allowing effects to vary during economic downturns, excluding PE deals completed during economic downturns. Across the different columns, the outcome variables are the natural logarithm of total sales, employees, EBITDA, and markups. All specifications include firm-by-cohort and year-by-cohort fixed effects. The sample of treated and control firms is constructed using a granular matching approach based on firm size, leverage, profitability, and industry as explained in Section III. Table A.I in the Internet Appendix provides more information about the variable definitions. Standard errors are clustered at the firm-cohort level. *** denotes significance at the 1% level, ** at the 5% level, and * at the 10% level.

Table A.XVI. The effect of PE buyouts on suppliers of target firms: Alternative economic downturn indicator

	(1) ln(Sales)	(2) ln(Employees)	(3) ln(EBITDA)	(4) ln(Markup)
Post PE	0.091***	0.042***	0.072***	-0.012
	(0.024)	(0.013)	(0.024)	(0.021)
Post PE × Economic downturn	-0.133**	-0.059**	-0.152**	0.029
	(0.062)	(0.029)	(0.068)	(0.064)
Observations	40937	40937	40937	16911
Adjusted R-squared	0.933	0.972	0.901	0.731
Firm × Cohort FE	Yes	Yes	Yes	Yes
Year × Cohort FE	Yes	Yes	Yes	Yes

This table reports difference-in-differences estimates of the effect of PE buyouts on suppliers of PE targets, comparing changes in outcomes at treated relative to matched control suppliers before and after the buyout, allowing effects to vary during economic downturns, using a sector-specific economic downturn indicator. Across the different columns, the outcome variables are the natural logarithm of total sales, employees, EBITDA, and markups. All specifications include firm-by-cohort and year-by-cohort fixed effects. The sample of treated and control firms is constructed using a granular matching approach based on firm size, leverage, profitability, and industry as explained in Section III. Table A.I in the Internet Appendix provides more information about the variable definitions. Standard errors are clustered at the firm-cohort level. *** denotes significance at the 1% level, ** at the 5% level, and * at the 10% level.

Table A.XVII. The effect of PE buyouts on suppliers of target firms: Excluding buy-and-build PE deals

	(1) ln(Sales)	(2) ln(Employees)	(3) ln(EBITDA)	(4) ln(Markup)
Panel A:				
Post PE	0.067***	0.032**	0.052**	-0.019
	(0.026)	(0.014)	(0.026)	(0.021)
Observations	38226	38226	38226	15564
Adjusted R-squared	0.931	0.970	0.898	0.735
Panel B:				
Post PE	0.089***	0.048***	0.068**	0.005
	(0.028)	(0.015)	(0.028)	(0.022)
Post PE × Economic downturn	-0.080**	-0.058***	-0.061	-0.099**
	(0.038)	(0.020)	(0.043)	(0.050)
Observations	38226	38226	38226	15564
Adjusted R-squared	0.931	0.970	0.898	0.735
Firm × Cohort FE	Yes	Yes	Yes	Yes
Year × Cohort FE	Yes	Yes	Yes	Yes

This table reports difference-in-differences estimates of the effect of PE buyouts on suppliers of PE targets, comparing changes in outcomes at treated relative to matched control suppliers before and after the buyout, allowing effects to vary during economic downturns, excluding buy-and-build PE deals. Across the different columns, the outcome variables are the natural logarithm of total sales, employees, EBITDA, and markups. All specifications include firm-by-cohort and year-by-cohort fixed effects. The sample of treated and control firms is constructed using a granular matching approach based on firm size, leverage, profitability, and industry as explained in Section III. Table A.I in the Internet Appendix provides more information about the variable definitions. Standard errors are clustered at the firm-cohort level. *** denotes significance at the 1% level, ** at the 5% level, and * at the 10% level.

Table A.XVIII. The effect of PE buyouts on suppliers of target firms: Accounting-based markup measures

Markup measure:	$\frac{Sales}{Input\ cost}$		$\frac{Sales}{Input\ cost+labor\ cost}$	
	(1)	(2)	(3)	(4)
Post PE	-0.001	-0.000	-0.000	0.000
	(0.002)	(0.002)	(0.001)	(0.001)
Post PE × Economic downturn		-0.004*		-0.002*
		(0.002)		(0.001)
Observations	40937	40937	40937	40937
Adjusted R-squared	0.880	0.880	0.873	0.873
Firm×Cohort FE	Yes	Yes	Yes	Yes
Year×Cohort FE	Yes	Yes	Yes	Yes

This table reports difference-in-differences estimates of the effect of PE buyouts on suppliers of PE targets, comparing changes in outcomes at treated relative to matched control suppliers before and after the buyout, allowing effects to vary during economic downturns. Across the different columns, the outcome variables are two accounting-based markup measures, one defined as sales over the cost of inputs and another defined as sales over the cost of inputs plus labor. All specifications include firm-by-cohort and year-by-cohort fixed effects. The sample of treated and control firms is constructed using a granular matching approach based on firm size, leverage, profitability, and industry as explained in Section III. Table A.I in the Internet Appendix provides more information about the variable definitions. Standard errors are clustered at the firm-cohort level. *** denotes significance at the 1% level, ** at the 5% level, and * at the 10% level.

Table A.XIX. The effect of PE buyouts on suppliers of target firms: Stricter matching procedure

	(1) ln(Sales)	(2) ln(Employees)	(3) ln(EBITDA)	(4) ln(Markup)
Panel A:				
Post PE	0.033**	0.013*	0.039**	0.009
	(0.016)	(0.008)	(0.017)	(0.016)
Observations	39489	39489	39489	15560
Adjusted R-squared	0.936	0.968	0.899	0.734
Panel B:				
Post PE	0.061***	0.020**	0.050**	0.010
	(0.019)	(0.009)	(0.020)	(0.017)
Post PE × Economic downturn	-0.100***	-0.027*	-0.049**	-0.021*
	(0.034)	(0.016)	(0.020)	(0.010)
Observations	39489	39489	39489	15560
Adjusted R-squared	0.936	0.968	0.899	0.734
Firm × Cohort FE	Yes	Yes	Yes	Yes
Year × Cohort FE	Yes	Yes	Yes	Yes

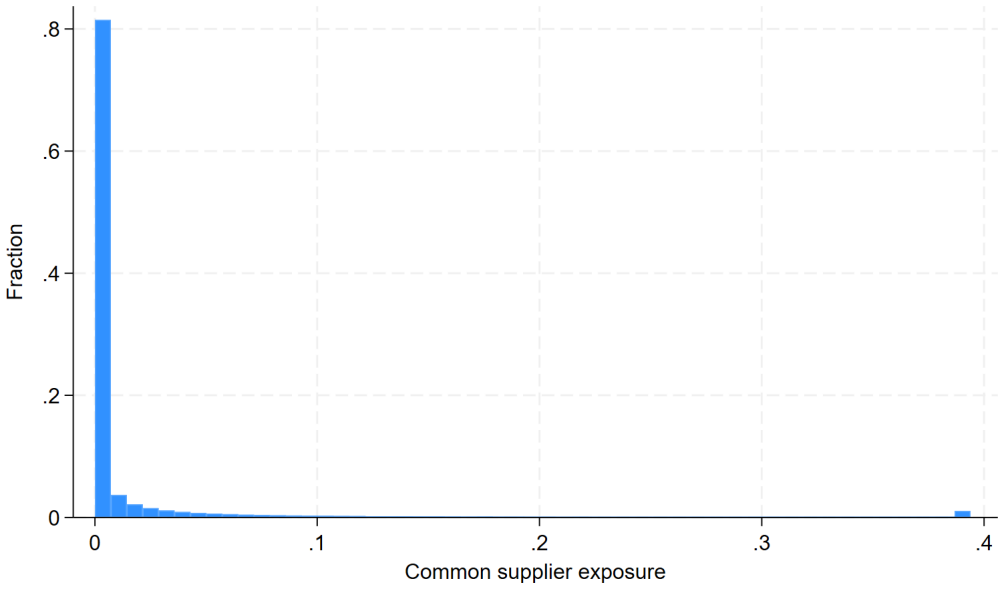
This table reports difference-in-differences estimates of the effect of PE buyouts on suppliers of PE targets, comparing changes in outcomes at treated relative to matched control suppliers before and after the buyout, allowing effects to vary during economic downturns, using a stricter matching strategy. Across the different columns, the outcome variables are the natural logarithm of total sales, employees, EBITDA, and markups. All specifications include firm-by-cohort and year-by-cohort fixed effects. The sample of treated and control firms is constructed using a granular matching approach based on firm size, leverage, profitability, industry, number of customers and average customer base characteristics as explained in Section V.F. Table A.I in the Internet Appendix provides more information about the variable definitions. Standard errors are clustered at the firm-cohort level. *** denotes significance at the 1% level, ** at the 5% level, and * at the 10% level.

Table A.XX. The effect of PE buyouts on suppliers of target firms:
Excluding control suppliers with large sales shares to competitors of PE-backed firms

	(1) ln(Sales)	(2) ln(Employees)	(3) ln(EBITDA)	(4) ln(Markup)
Panel A:				
Post PE	0.073***	0.036***	0.056***	-0.007
	(0.021)	(0.013)	(0.020)	(0.022)
Observations	40934	40934	40934	15620
Adjusted R-squared	0.946	0.973	0.901	0.739
Panel B:				
Post PE	0.092***	0.044***	0.031	0.008
	(0.023)	(0.014)	(0.027)	(0.024)
Post PE × Economic downturn	-0.076**	-0.031*	-0.031	-0.072*
	(0.031)	(0.018)	(0.039)	(0.043)
Observations	40934	40934	40934	15620
Adjusted R-squared	0.946	0.973	0.901	0.739
Firm × Cohort FE	Yes	Yes	Yes	Yes
Year × Cohort FE	Yes	Yes	Yes	Yes

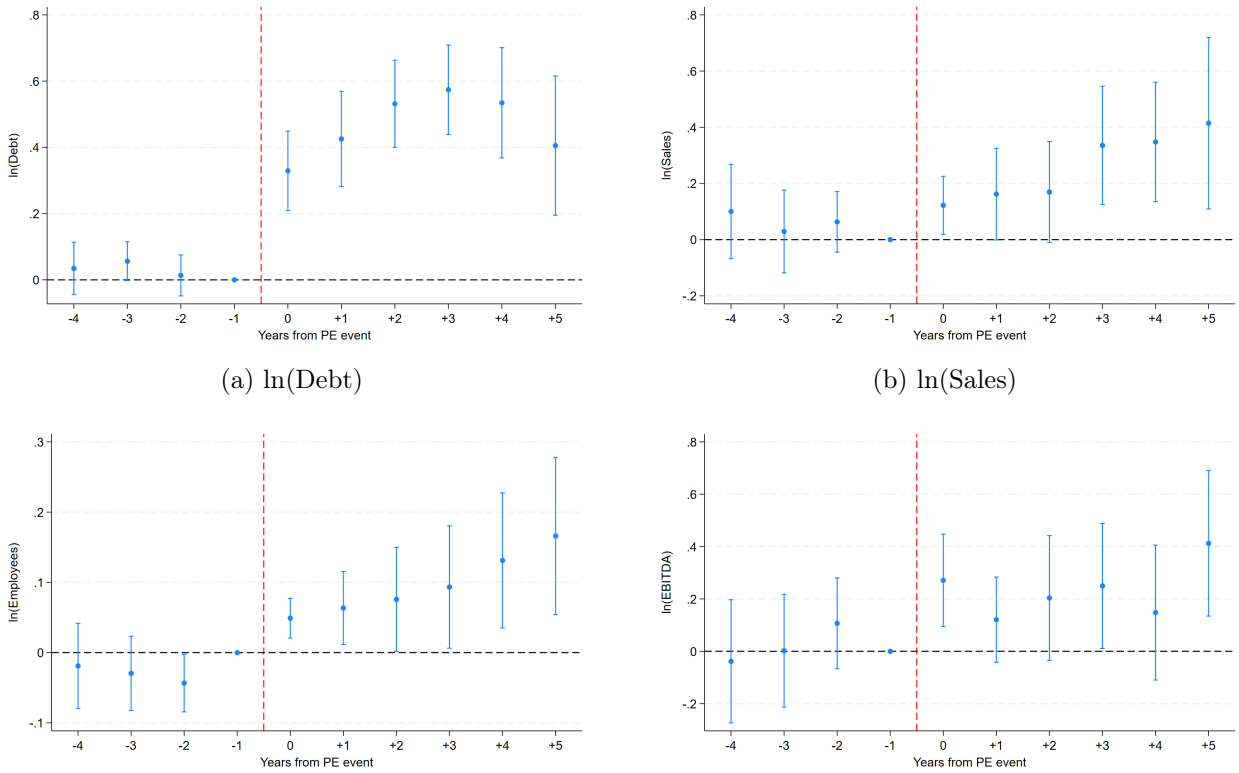
This table reports difference-in-differences estimates of the effect of PE buyouts on suppliers of PE targets, comparing changes in outcomes at treated relative to matched control suppliers before and after the buyout, allowing effects to vary during economic downturns. Across the different columns, the outcome variables are the natural logarithm of total sales, employees, EBITDA, and markups. All specifications include firm-by-cohort and year-by-cohort fixed effects. The sample of treated and control firms is constructed using a granular matching approach based on firm size, leverage, profitability, and industry as explained in Section III. Before matching, firms that supply more than 5% of their total sales to rivals of treated suppliers' PE-backed customers in any post-buyout year are excluded from the potential control sample, as explained in Section V.G. Table A.I in the Internet Appendix provides more information about the variable definitions. Standard errors are clustered at the firm-cohort level. *** denotes significance at the 1% level, ** at the 5% level, and * at the 10% level.

Figure A.I. Distribution of competitors' exposure to common suppliers of PE-backed firms



This figure shows the distribution of competitors' exposure to common suppliers of PE-backed firms. The sample is restricted to firms operating in 4-digit NACE industries that experienced at least one PE buyout during the sample period.

Figure A.II. Dynamic difference-in-differences estimates for the effect of PE buyouts on target firms



(a) ln(Debt)

(b) ln(Sales)

(c) ln(Employees)

(d) ln(EBITDA)

This figure presents the dynamic difference-in-differences estimates of the effect of PE buyouts on target firms, comparing changes in outcomes at treated relative to matched control firms before and after the buyout. The y-axis corresponds to the coefficient estimates of β from Equation (3). The x-axis corresponds to years relative to the year in which the target firm was acquired. The dependent variables are the natural logarithm of debt, sales, employees, and EBITDA. The sample of treated and control firms is constructed using a matching approach based on firm size, leverage, profitability, and industry, as explained in Section III. Table A.I in the Internet Appendix provides more information about the variable definitions. The vertical bars represent confidence intervals at the 95% level.

Internet Appendix B

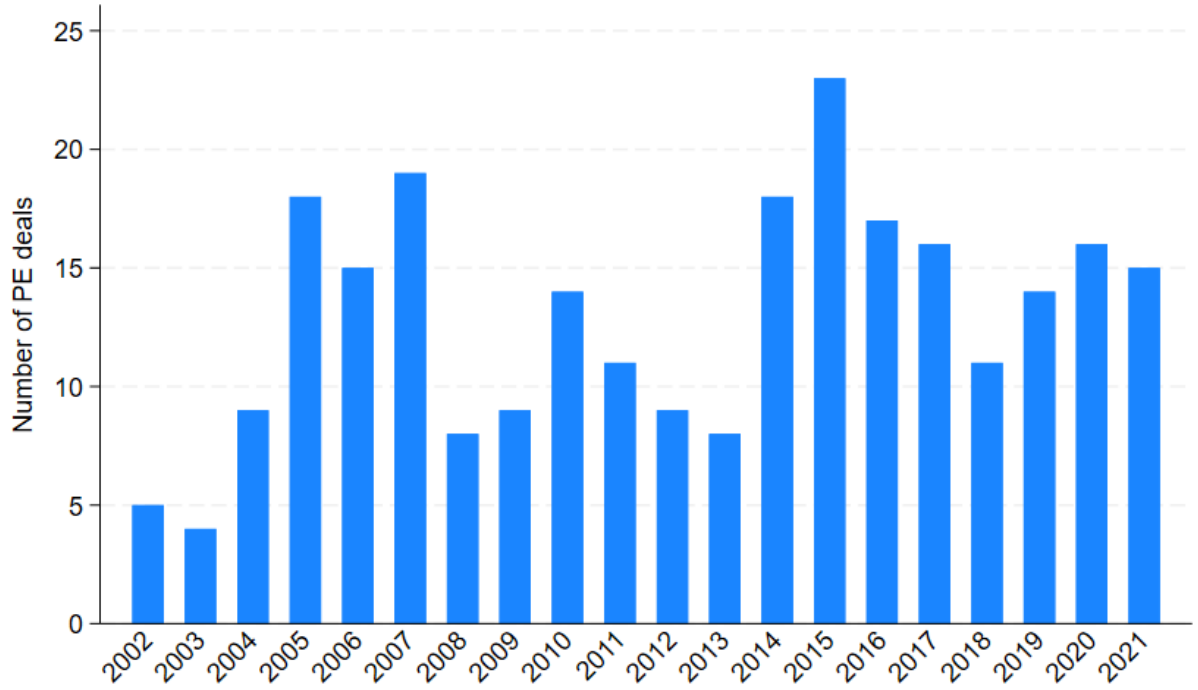
In general, the Belgian PE buyouts in our data sample are relatively comparable to PE buyouts in the rest of the world. First, Figure B.I shows the number of PE buyouts per year in our sample. Overall, the number of buyouts gradually increases from 2002 until 2007 when it peaks, followed by a severe drop in the aftermath of the global financial crisis. The number of deals then slowly recovers, followed by a decrease in the years 2011-2013, after which it strongly increases in the years 2014-2015. In 2018, there is a small dip, after which the number of deals increases again until the end of our sample period. These patterns are similar to the evolution recorded by Aldatmaz and Brown (2020) for their global sample of PE deals. In terms of the total number of deals, our sample obviously includes fewer deals than the sample of French deals from Boucly et al. (2011) and U.S. deals from Davis et al. (2014), but this is primarily due to the fact that these economies are several times larger than the Belgian economy. Adjusting for differences in the size of the economy, Belgian PE activity is broadly comparable to that of France or the U.S., with average PE capital-to-GDP ratios of 0.049% in Belgium, 0.053% in France, and 0.172% in the U.S. (see Aldatmaz and Brown 2020).

Second, the types of sellers involved in our sample of Belgian transactions do not differ much from the typical transactions in the rest of the world. Three points are worth highlighting. First, only 4% of the deals in our sample are public-to-private transactions, a number close to the 7% found in the sample of global PE deals documented by Strömberg (2008). In Belgium, as in the world, about 55% of PE transactions are pure private-to-private transactions. Second, divisional buyouts comprise 23% in our sample, compared to 26% in Strömberg's sample. Finally, secondary buyouts (i.e., transactions involving a financial vendor) comprise 19% in our sample compared to 13% in Strömberg's sample. Third, average deal size is also very similar to international data. Looking at enterprise value, Strömberg (2008) documents that the mean deal size is $389 million in the U.S. and $280 million in the U.K. over the period 2001-2007, while Boucly et al. (2011) report a mean deal size of $395 million in France over the period 1994–2004. These figures are comparable to the mean deal size of $280 million in our sample of Belgian deals. The PE firms in our sample are also representative of the universe of PE firms around the world. Among the 147 sponsors backing the deals in our sample, there are both very large sponsors (such as CVC Capital Partners, The Carlyle Group, and Goldman Sachs Capital Partners) as well as small ones (such as Bencis Capital Partners). Table B.I in the Internet Appendix reports the distribution of PE deals by investor country. The majority (50%) of PE firms in our sample are Belgian firms which are, on average, small (with $1.1 billion of assets under management). U.S., U.K., and Dutch funds are common (10%, 10% and 16%, respectively, of the deals in our sample) and, on average,

larger (with $4.5 billion of assets under management). Overall, domestic funds are prevalent but an important fraction of deals are backed by larger U.S. or U.K. based funds. Finally, Table B.II in the Internet Appendix shows the sectoral distribution of PE deals. Around 35% involved firms in manufacturing, primarily in the earlier years of the sample, while more recent deals increasingly target the information and communication services sector, which accounts for just under 15% of all PE deals. These patterns are broadly consistent with those reported by Davis et al. (2025), for instance.

One difference compared to U.S. buyouts is that the target firms in our sample are slightly older than the typical U.S. targets, but this accords with the idea that PE buyouts involve more mature firms in continental Europe than in the U.S. or the U.K. (Boucly et al. 2011). For instance, in the sample of Davis et al. (2014), about 50% of targets are more than 10 years old and 25% are less than 5 years old. In our sample, 78% of targets are more than 10 years old, and only 6% are younger than 5 years old. Nevertheless, it should be stressed that the treated firms in our sample do not systematically differ from their matched control firms on the age dimension (even though age was not a criterion in the matching procedure), which mitigates potential concerns that our results would be driven by the effect of firm age on firm performance, for instance.

Figure B.I. Number of PE deals per year



This histogram presents the number of PE deals per year in Belgium over the period 2002-2021.

Table B.I. Distribution of PE deals by investor country

Acquirer country	Number of PE deals	Percentage of total deals (%)
Belgium	91	42.13
Netherlands	32	14.81
United States	20	9.26
United Kingdom	15	6.94
Luxembourg	7	3.24
Germany	6	2.78
France	5	2.31
Other	40	18.52
Total	216	100.00

This table reports the distribution of PE deals by investor country.

Table B.II. Distribution of PE deals by sector

Sector	Number of PE deals	% of total deals
Accommodation and food services	2	0.9
Administrative and support services	10	4.6
Agriculture, forestry, and fishing	1	0.5
Construction	8	3.7
Electricity, gas, steam	1	0.5
Information and communication services	26	12.0
Manufacturing	68	31.5
Mining and Quarrying	1	0.5
Professional, scientific and technical services	27	12.5
Transportation and storage	3	1.4
Water supply	2	0.9
Wholesale and retail trade	49	22.7
Other	18	8.3
Total	216	100.0

This table reports the distribution of PE deals by sector.

Internet Appendix C

C Markup estimation

Estimating markups requires the revenue share of a variable input and the output elasticity of that input. The former can be computed as expenditure on input X divided by total firm revenue. The latter must be recovered from an estimated production function. Following De Loecker and Warzynski and the subsequent production-approach literature, we estimate output elasticities using a control-function approach under the assumption that productivity is Hicks-neutral.

C.1 Production function

For the translog production function with capital (k_{it}), labor (l_{it}), and materials (m_{it}), the logged production function excluding Hicks-neutral productivity is:[a]

$$f_{it} = \beta_k k_{it} + \beta_l l_{it} + \beta_m m_{it} + \beta_{kk} k_{it}^2 + \beta_{ll} l_{it}^2 + \beta_{mm} m_{it}^2 + \beta_{kl} k_{it} l_{it} + \beta_{km} k_{it} m_{it} + \beta_{lm} l_{it} m_{it}. \quad (5)$$

The production-function coefficients are assumed to be constant over time. Because the production function is translog, however, the output elasticity of each input varies with the level of all inputs and can therefore differ across firms and over time.

C.2 Control-function estimation

We follow the Ackerberg et al. (2015) estimator. The two key assumptions are that productivity is (i) Hicks-neutral and (ii) evolves according to a first-order Markov process.

The control-function approach allows measured log revenue to include an additive measurement-error term ϵ_{it}. Thus, conditional on log productivity ω_{it}, observed log revenue y_{it} satisfies:

$$y_{it} = f(k_{it}, l_{it}, m_{it}) + \omega_{it} + \epsilon_{it}. \quad (6)$$

Let materials be the flexible input chosen after the firm observes current productivity. Then materials can be written as a function of the state variables and productivity,

$$m_{it} = g(k_{it}, l_{it}, \omega_{it}),$$

which can be inverted for productivity:

$$\omega_{it} = g^{-1}(k_{it}, l_{it}, m_{it}).$$

[a] For notation purposes, all lower case variables are in logged form.

Substituting the inverted control function into (6) yields

$$y_{it} = f(k_{it}, l_{it}, m_{it}) + g^{-1}(k_{it}, l_{it}, m_{it}) + \epsilon_{it} = h(k_{it}, l_{it}, m_{it}) + \epsilon_{it}, \tag{7}$$

where the nonparametric function $h(\cdot)$ collects the production function and the control function.

Because both productivity and the production function depend on the inputs, they are not separately identified in the first stage. Rather, the first stage flexibly absorbs the composite term $h(k_{it}, l_{it}, m_{it})$, and the residual is the measurement-error term ϵ_{it} under the maintained assumptions.

The second key assumption of the ACF approach is that productivity follows a first-order Markov process:

$$\omega_{it} = k(\omega_{it-1}) + \nu_{it}, \tag{8}$$

where $k(\cdot)$ is a flexible function and ν_{it} is the productivity innovation.

Given the production-function coefficients β, productivity can be written as

$$\omega_{it}(\beta) = y_{it} - \epsilon_{it} - f_{it}(\beta).$$

The implied productivity innovation is therefore

$$\nu_{it}(\beta) = \omega_{it}(\beta) - k(\omega_{it-1}(\beta)). \tag{9}$$

Because the productivity innovation is orthogonal to variables chosen before period t, the production-function coefficients are identified from moment conditions of the form

$$E[\nu_{it}(\beta) z_{it-1}] = 0,$$

where z_{it-1} is a vector of predetermined instruments.

We use lagged capital, lagged labor, and their lagged quadratic and interaction terms as instruments in the translog function. Further, following the production-approach literature, we correct the revenue denominator in the input expenditure share for the measurement error estimated in the first stage. For materials M, the estimated markup is:

$$\hat{\mu}_{it} = \frac{\hat{\theta}^M_{it}}{\tilde{s}^M_{it}} = \frac{\hat{\theta}^M_{it}}{s^M_{it} \exp(\epsilon_{it})}, \tag{10}$$

where $\hat{\theta}^M_{it}$ is the estimated output elasticity of materials implied by the translog production function, and s^M_{it} is the observed expenditure share of materials in measured revenue.

Internet Appendix D

Our main results show that, on average, PE-backed firms have a positive impact on the performance of their suppliers. This effect operates through two main channels. On the one hand, suppliers benefit from increased demand for inputs as PE-backed firms pursue new growth opportunities and expand their activities following the buyout. On the other hand, PE-backed firms appear to have a certification effect, helping their suppliers to gain new customers from within the PE-backed firms' network.

To assess the relative economic importance of these two mechanisms, we augment our baseline regression model by including a variable (*Post-buyout within-PE-network customers*) that measures the number of new customers a treated supplier gains within the PE-backed firms' network post-buyout. This variable isolates the effect of new customer acquisition on suppliers' post-buyout performance, while the post-treatment indicator would capture the impact of increased demand from PE-backed firms.

Table D.I below presents the results. Across the different columns, the post-treatment indicator remains statistically significant and positive, with coefficient estimates of a magnitude comparable to those in our baseline results. In contrast, the estimated effect of new within-PE-network customers is statistically insignificant across all columns. These findings suggest that the improvement in affected suppliers' performance cannot be attributed to the certification effect; instead, it is more likely driven by increased demand from the PE-backed firm.

Moreover, as mentioned earlier, our results from Table VIII provide further support that the direct increase in demand from PE-backed customers is the primary driver of the positive impact on suppliers' performance. This table reports the changes in sales of treated suppliers to PE-backed customers versus (comparable) non-PE-backed customers, before versus after the buyout event. The results confirm that treated suppliers experience a significant increase in purchases from PE-backed customers relative to other (comparable) customers. Furthermore, the coefficient estimates suggest that the firm-level increase in suppliers' sales is predominantly driven by purchases from PE-backed customers rather than other clients. Specifically, multiplying the estimated coefficient of 0.18 in column (3) of Table VIII by the average sales share of treated suppliers to their PE-backed customers in the sample (approximately 30%) yields a value close to the estimated firm-level sales increase (with coefficient estimate of 0.081) in column (1) of Table V.

Table D.I. The effect of PE buyouts on suppliers of target firms: Disentangling the direct demand and certification channel

	(1) ln(Sales)	(2) ln(Employees)	(3) ln(EBITDA)	(4) ln(Markup)
Post PE	0.081***	0.038***	0.060**	-0.009
	(0.023)	(0.013)	(0.024)	(0.020)
Post-buyout within-PE-network customers	0.003	0.004	0.001	-0.000
	(0.004)	(0.003)	(0.003)	(0.001)
Observations	40937	40937	40937	16911
Adjusted R-squared	0.933	0.972	0.901	0.731
Controls	No	No	No	No
Firm×Cohort FE	Yes	Yes	Yes	Yes
Year×Cohort FE	Yes	Yes	Yes	Yes

This table reports difference-in-differences estimates of the effect of PE buyouts on suppliers of PE targets, comparing changes in outcomes at treated relative to matched control suppliers before and after the buyout, allowing for heterogeneity in the within-network clients that suppliers gain from their PE-backed customers. Across the different columns, the outcome variables are the natural logarithm of total sales, employees, EBITDA, and markups. All specifications include firm-by-cohort and year-by-cohort fixed effects. The sample of treated and control firms is constructed using a granular matching approach based on firm size, leverage, profitability, and industry, as explained in Section III. Table A.I in the Internet Appendix provides more information about the variable definitions. Standard errors are clustered at the firm-cohort level. *** denotes significance at the 1% level, ** at the 5% level, and * at the 10% level.

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