BUILT FOR BAD TIMES?

PRIVATE EQUITY AND OPERATIONAL PERFORMANCE ACROSS ECONOMIC DOWNTURNS IN SWEDEN

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Built for Bad Times? Private Equity and Operational Performance Across Economic Downturns in Sweden

Abstract:

This paper studies how Swedish firms acquired through leveraged buyouts between 2000 and 2020 perform operationally and financially compared with similar non-PE-backed firms. We find that firms tend to grow faster after an LBO; employee count, sales, capital employed, and capital expenditure all rise over the three years following the deal. This growth advantage appears to persist during macroeconomic downturns; capital expenditure nearly doubles during the Global Financial Crisis, remains flat through the European Debt Crisis, and holds steady during COVID-19 even as profitability weakens. These findings are largely consistent with the credit-constraint channel proposed by Boucly, Sraer, and Thesmar (2011); PE ownership matters most precisely when external credit dries up.

Keywords:

Private Equity, Leveraged Buyouts, Credit Constraints, Financial Crises, Operational Growth

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1 Introduction

This study examines the impact of private equity sponsorship on firms targeted in leveraged buyouts between 2000 and 2020 by addressing two primary questions: Do Swedish private equity-backed firms experience faster growth than comparable non-private equity-backed firms following the buyout? If so, does this growth advantage increase, decrease, or remain unchanged during periods of macroeconomic contraction? The sample period encompasses three distinct economic downturns: the Great Financial Crisis, the European Debt Crisis, and the COVID-19 recession. Sweden provides a relevant context for this analysis, as private equity-backed firms represent a significant portion of Swedish GDP (Swedish Private Equity & Venture Capital Association, 2024), and four of Europe's largest funds, EQT, Adelis Equity Partners, Summa Equity, and Altor Equity Partners, were founded in Sweden (Private Equity International, 2025).

Using the Serrano database, which provides financial statement data for Swedish firms, we apply the difference - in - differences design of Boucly, Sraer, and Thesmar (2011). We match each PE-backed firm with non PE-backed firms that share similar employee counts, profitability levels, and operations in the same industry. We then analyze operating results for the three years preceding the LBO transaction. To study the impact of PE-ownership during times of economic downturns, we adopt the methodology of Lins, Servaes, and Tamayo (2017). While their study investigates whether firms with higher corporate social responsibility (CSR) ratings earn superior returns during crises as trust in corporations declines, this analysis replaces the CSR measure with an LBO interaction term.

We test two hypotheses: *First, Swedish PE-backed firms exhibit superior operational growth and profitability after a buyout compared to similar non-PE-backed firms. Second, PE-backed firms exhibit greater operational resilience during economic downturns. This is consistent with the notion that PE ownership alleviates credit constraints, giving portfolio companies the financial flexibility to maintain investment when external conditions deteriorate.*

We document two main findings. First, Swedish LBO targets tend to outperform matched control firms across all operating dimensions we measure. Averaged over the three years after the deal, employment, sales, capital employed, and capital expenditure are higher for LBO targets relative to comparable controls. The return on assets declines, possibly because the asset base of a PE-backed firm grows faster than its earnings. This would suggest that sponsors are mainly focused on funding growth. Second, the growth advantage appears to persist during the crisis, when the macroeconomy contracts. By defining crisis years as those in which Swedish real GDP growth falls more than one standard deviation below trend, the gap between targets and controls on employment, sales, capital employed, and capital expenditure remains large and significant. Moreover, the gap itself is not statistically different between crisis and non-crisis years, indicating that PE-backed growth holds up in downturns as well as in regular times.

However, observing the crises individually reveals their different characters. During the Great Financial Crisis, when Swedish credit tightened (Sveriges Riksbank, 2009), the coefficient on CAPEX increased significantly, indicating that PE-backed firms invest counter-cyclically. During the European Debt Crisis, which affected Swedish credit markets to a lesser extent (Sveriges Riksbank, 2013), CAPEX remained unchanged from regular times. During COVID-19, LBO targets investments held up, but profitability fell sharply. This pattern aligns with the credit-constraint channel proposed by Boucly et al. (2011). If PE ownership creates value primarily by relaxing credit constraints, the effects on PE-backed firms' operational growth relative to controls should be the largest when external credit tightens.

Our evidence contributes to a literature that has substantially revised the view of how private equity creates value. Early work on U.S. public-to-private deals in the 1980s attributed PE returns to cost reduction and budgetary discipline. Kaplan (1989) showed that PE-backed firms improved profitability by cutting investment and selling assets, and Jensen (1993) argued that high leverage served as a discipline device.

Over time, the view shifted; Davis, Haltiwanger, Jarmin, Lerner, and Miranda (2014) demonstrate that leveraged buyouts (LBOs) restructure, rather than dismantle, target firms, resulting in modest net employment effects. Bergström, Grubb and Jonsson (2007) attribute operational improvements to enhanced governance rather than financial engineering in their study of Swedish LBOs ranging from 1993 to 2005. Boucly et al. (2011) provide a closely related analysis, examining 839 French LBOs between 1994 and 2004. Their findings indicate that LBO targets experience increases in employment, sales, and capital employed by 12–18% relative to matched controls within three years post-transaction; capital expenditure rises by 24% compared to controls. This growth is attributed to the alleviation of credit constraints.

Private equity sponsors are found to strengthen governance and monitoring, appoint experienced managers who enhance creditor confidence, commit capital over longer horizons than dispersed investors, and benefit from tax policies that favor earnings retention over dividend distribution. Growth effects are most pronounced in private-to-private transactions, where firms are more likely to face credit constraints prior to the LBO. Cohn, Hotchkiss, and Towery (2022) document a similar pattern in 288 U.S. buyouts from 1995 to 2009, with post-deal sales growth supported by sponsor equity injections. Bernstein, Lerner, Sorensen, and Strömberg (2017) provide causal evidence by showing that industries receiving greater private equity investment grew approximately 15% faster in production, value added, and employment.

We extend the credit-constraint growth channel to a recent period (2000–2020) in Sweden, and by testing its implications during crises. This provides a meaningful extension, since contractions in credit supply during a crisis result in real losses in investment and employment for firms denied credit (Chodorow-Reich, 2014). The marginal dollar of investment is therefore likely to have the greatest real effects when credit is scarce, since tight lending standards can prevent creditworthy firms from financing productive investment opportunities (Farboodi & Kondor, 2023). If PE sponsors relax credit constraints for their portfolio firms (Boucly et al., 2011) and continue to fund investment when credit-constrained competitors retrench (Bernstein, Lerner, and Mezzanotti, 2019), they would create economic value beyond the private returns they earn for investors. That value would be concentrated in downturns, periods largely unexplored by the literature.

The existing literature on PE ownership during crisis primarily focuses on financial distress rather than operating performance. Hotchkiss, Smith, and Strömberg (2021) analyze 2,151 highly leveraged firms from 1997 to 2010 and find that PE-backed firms do not exhibit higher default risk compared to similar non-PE-backed firms. In cases of distress, PE-backed firms restructure more rapidly, more frequently out of court, and with higher survival rates, as sponsors provide additional equity prior to default. However, their analysis is limited to exploring financial distress and does not address operating performance. Uddin and Chowdhury (2021) demonstrate that crises reduce deal values and delay PE exits, but neither examines operating performance. Bernstein et al. (2019) investigate UK PE-backed firms during the 2008 financial crisis and find that these firms reduced investment less than matched peers, raised more equity and debt, gained market share, and were more likely to renegotiate loans. These effects were concentrated among firms that were financially constrained prior to the crisis, supporting the hypothesis that the credit-constraint channel becomes more pronounced when external credit is limited. Nevertheless, evidence beyond a single country and crisis episode remains limited, and existing research does not address heterogeneity across shocks. The aim of our study is to address this gap.

The remainder of the paper is organized as follows. Section 2 describes the data and sample construction. Section 3 presents the empirical methodology. Section 4 reports and discusses the main results. Section 5 presents robustness tests. Section 6 reports limitations and suggestions for future research. Section 7 concludes.

2 Data

2.1 Deal data

To construct our treatment group, we identify companies acquired by a private equity sponsor through a leveraged buyout (LBO) following a period of ownership by a non-private equity entity. We source deal data from three databases: Capital IQ, Securities Data Company (SDC) Platinum, and Bolagsverket.

From Capital IQ, we retrieve deals that fulfill five criteria: announced between January 1, 2000, and December 31, 2020; classified as "closed" or "effective"; targeting a Swedish company; reported as a leveraged buyout; and attributed to a PE/VC investment firm. Capital IQ returns 641 transactions meeting these criteria. From SDC Platinum, we aim to identify transactions not captured by Capital IQ by applying analogous criteria: deals announced over the same period; status “unconditional” or “completed”; targeting a Swedish company; classified as a leveraged buyout; and with a financial buyer as the acquirer. SDC Platinum returns 660 transactions. The January 1, 2000, start date reflects a constraint imposed by our methodology: we require a three-year pre-deal estimation window, and Serrano’s coverage begins in fiscal year 1997. The December 31, 2020, cutoff ensures the corresponding three-year post-deal performance window remains fully within Serrano's coverage, which extends through 2023.

We then clean the samples separately in four steps. First, we verify that each acquirer is a private equity firm. Capital IQ's search returns both private equity and venture capital firms, and we exclude the latter because they typically acquire minority stakes (Kaplan & Strömberg, 2009). For SDC Platinum, the financial buyer filter also captures asset management firms and infrastructure funds, which we likewise exclude. As a result, 126 firms were excluded from our Capital IQ sample, and 230 firms from our SDC Platinum sample. Second, we remove all targets with prior private equity or venture capital ownership. In total, 143 firms were excluded from the Capital IQ sample, and 104 from the SDC Platinum sample. Third, we manually collect organizational numbers for all LBO targets using Bolagsverket. Where the organizational number cannot be identified with confidence, we exclude the firm to preserve data accuracy. This resulted in 149 firms being excluded from the Capital IQ sample, and 148 from the SDC Platinum sample. Fourth, we remove any holding companies. In total, 11 firms were excluded from the Capital IQ sample, and 9 were excluded from the SDC Platinum sample. After cleaning, we retain 212 transactions from Capital IQ and 169 from SDC Platinum. Merging the two samples and removing duplicates yields a sample size of 315 transactions.

A limitation we face is that financial statements from the Serrano database are not consolidated at the entity level. Swedish firms often sit within corporate group structures, meaning that the legal entity targeted in a leveraged buyout may be a parent to one or more subsidiaries. Should the acquiring sponsor restructure the target's corporate group following the buyout, the target's reported employment, sales, and assets will rise mechanically, regardless of any actual operational improvement. This could occur, for example, if subsidiaries are merged into the parent entity. However, given the limitations of available group structure data for our sample, we cannot fully exclude the possibility that some of our observed post-buyout growth reflects consolidation rather than real economic expansion.

2.2 Firm-level data

We obtain firm-level financial and operational data from the Serrano database (Weidenman, n.d.). The Serrano database draws on Bolagsverket's financial statements and covers fiscal years 1997 through 2023. The database is used to identify profitability and operational growth measures at the firm level. Specifically, we gather the following variables: number of employees, sales, total fixed assets, trade receivables, total inventories, trade payables, tangible fixed assets, depreciation and amortization, earnings before interest and taxes (EBIT), current liabilities to credit institutions, non-current liabilities to credit institutions, bonds payable[1], and industry code.

When gathering financial and operational data for the 315 transactions, 29 firms are excluded because their organizational number could not be matched in the Serrano database. This results in 286 retained transactions. We then apply three additional exclusion criteria. First, 28 firms report fewer than 1 employee. These entities are generally non-operating, such as holding companies. The exclusion of these entities increases the study's robustness by identifying cases that may have been overlooked during manual cleaning of the sample. Second, 4 firms lack an SNI industry code. Third, 4 firms are missing ROA. Because several firms fail multiple criteria simultaneously, a combined 26 firms are excluded, leaving a sample of 257 observations.

We further restrict the sample to targets with complete firm-year observations throughout the full seven-year window. This requirement excludes 14 additional firms. Missing firm-year observations are most commonly attributable to bankruptcy or merger activity under a different organizational name. Firm-years are retained even if some individual variables within them are missing. After applying these filters, we are left with 243 deals. Beyond these adjustments, we make no further modifications to the sample as the Swedish House of Finance applies its own corrections to the database. These include adjustments for broken accounting periods, gaps and omissions in financial statements, and rules for determining whether a firm is active.

[1] Referred to "obligationslån" in Swedish.

2.3 Constructing the control group

To examine how LBO targets perform relative to comparable non-acquired firms, we match each treated firm with up to five controls from the Serrano database. We impose three criteria, all measured at t = −1, the fiscal year immediately preceding the buyout: (1) it belongs to the same industry as the treated firm, determined by the two-digit SNI code; (2) its employee count falls within ±20% of the treated firm's; and (3) its ROA falls within ±50% of the treated firm's. Any potential control with missing data on industry code, employee count, or ROA at t = −1 is excluded. Likewise, control firms lacking coverage over the full seven-year window are excluded.

Return on assets (ROA) is defined as EBITDA[2] divided by capital employed. Capital employed is the sum of fixed assets and working capital, with working capital equal to trade receivables plus inventories minus trade payables. We use EBITDA rather than net income in the numerator because it captures operational profitability independent of capital structure, following Boucly et al. (2011). A logarithmic transformation of employment is applied, so that a given percentage difference in firm size receives equal weight regardless of its absolute level. Furthermore, we express each control's ROA relative to the treated firm's ROA, preventing either variable from dominating the matching due to differences in scale.

Our matching procedure differs from that of Boucly et al. (2011) in two respects. First, we use optimal matching Hansen (2004) rather than the nearest-neighbor approach. The nearest-neighbor methodology assigns controls sequentially yielding a locally optimal solution: each match is best given the choices already made, but the overall set of pairs need not minimize total distance. Optimal matching instead minimizes the sum of distances across all matched pairs, yielding a globally optimal solution. Each treated firm receives at most five controls, without replacement, and treated firms with the fewest eligible controls are matched first to ensure they are not left without partners. The two methods identify controls for the same number of firms, but they share only 58% of those controls. Second, we tighten the matching bandwidth. Where (Boucly et al., 2011) require employment to fall within ±50% of the treated firm's, we use ±20%. A ±50% bandwidth produces statistically significant differences in log(employees) between treated and control groups, indicating insufficient common support (Appendix Table 1). Appendix Tables 2–4 report covariate balance under alternative specifications using Euclidean nearest-neighbor matching for comparison.

Figure 1 shows the distribution of matched firms, and Table 1 reports covariate balance between treated and control firms. Our baseline specification (±20% on employees and ±50% on ROA, compared to target levels) identifies controls for 212 LBO targets, yielding 917 controls in total, and an average of 4.33 per target. Increasing the matching bandwidth to ±50% on both variables identifies 1,083 controls for 233 LBO targets, averaging 4.65 controls per target. Tightening the ROA bandwidth to ±20% excludes 17 additional targets compared to our baseline specification and lowers the average to 3.74 controls per target. Both alternative matching bandwidths deteriorate the covariate balance[3].

Table 1
Covariate balance between LBO targets and matched controls at $t = -1$ (the year preceding the leveraged buyout), under the baseline bandwidth specification (±20% on employee count, ±50% on ROA). The difference column reports LBO targets less matched controls, for log(employees) and ROA. The t-statistic tests the null hypothesis of equal means; p-values above 0.10 indicates that there is no significant difference between LBO targets and matched controls.

	LBO Target (mean)	Control (mean)	Difference	t-stat	p-value
log(Employees)	3.633	3.472	0.162	1.616	0.107
ROA	0.606	0.554	0.052	0.672	0.502

[2] Serrano reports depreciation and amortization as a negative value. We therefore construct EBITDA as EBIT minus the reported DA figure, which is algebraically equivalent to the standard definition of EBIT plus depreciation and amortization.
[3] The joint constraint forces the algorithm to bypass the best size-matched controls in favor of those satisfying both calipers.

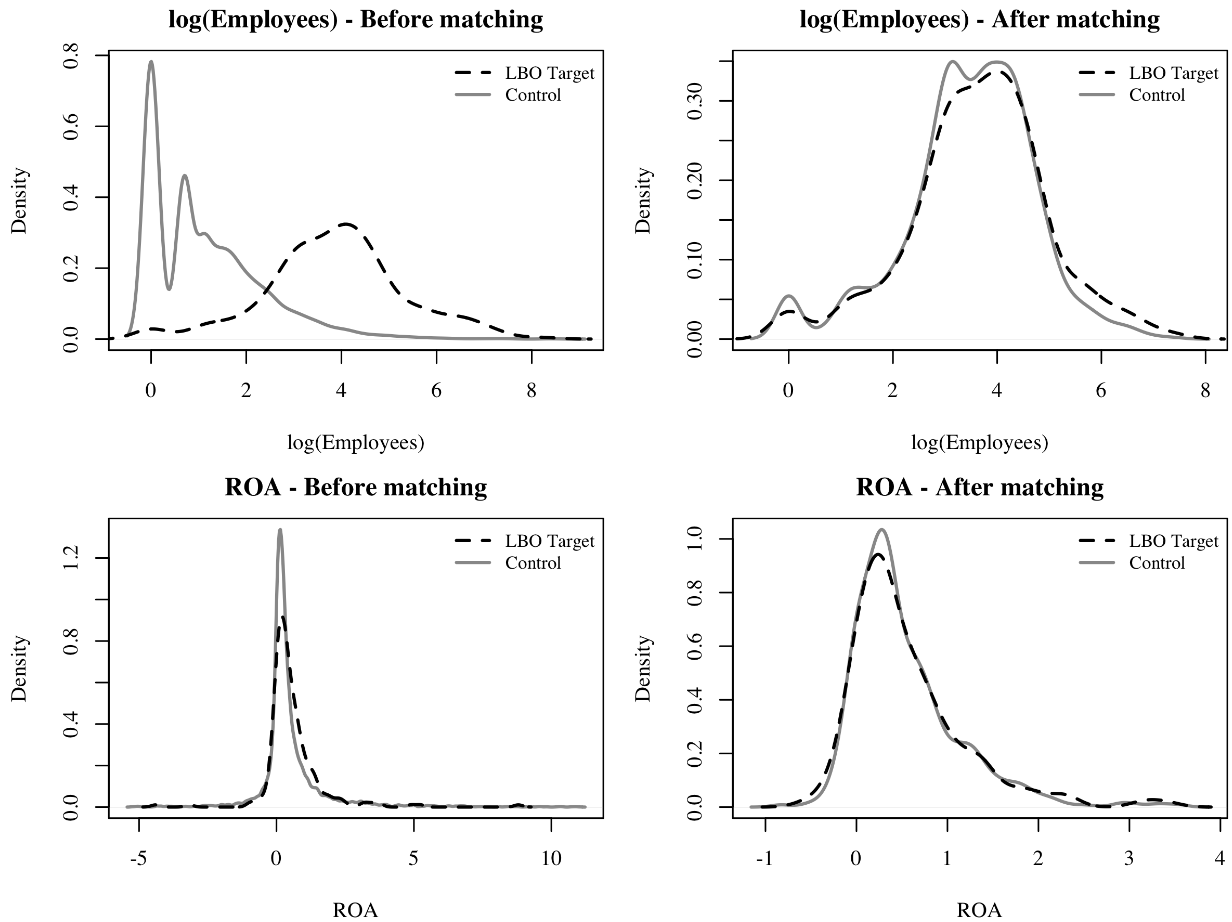

Figure 1. Distribution of log(Employees) and ROA for LBO targets (dashed line) and matched controls (solid line), before and after matching on size and profitability. The bandwidth applied to control firms are ±20% of the target's employee count, and ±50% of the target's ROA. The distributions before matching differ substantially; the distributions after matching overlap closely, confirming common support for the matched DiD estimator.

2.4 Summary statistics

The sample comprises 212 Swedish deals, representing approximately one-quarter of Boucly et al.'s (2011) 839 French deals. Figure 2 displays the yearly distribution, which is concentrated in more recent years, a pattern also observed by Boucly et al. (2011). However, it remains unclear whether this trend reflects improved data coverage for leveraged buyout transactions or an actual increase in the number of LBOs. The Swedish and the French samples also differ in industry composition. Boucly et al. (2011) note that their sample spans a wide range of sectors, while our sample is more concentrated in services. Three sectors together account for 41% of our sample: wholesale trade (19.8%), computer programming and consultancy (10.8%), and head offices and management consultancy (10.4%). Other large sectors include specialized construction (6.6%) and retail trade (4.7%). This concentration in trade and professional services is consistent with the broader structure of the Swedish economy, where services account for the largest share of value added (World Bank, 2025), and with the typical LBO target profile of asset-light, scalable businesses with recurring, predictable revenues (Weidenman, n.d.). A full industry breakdown is presented in Appendix Table 5.

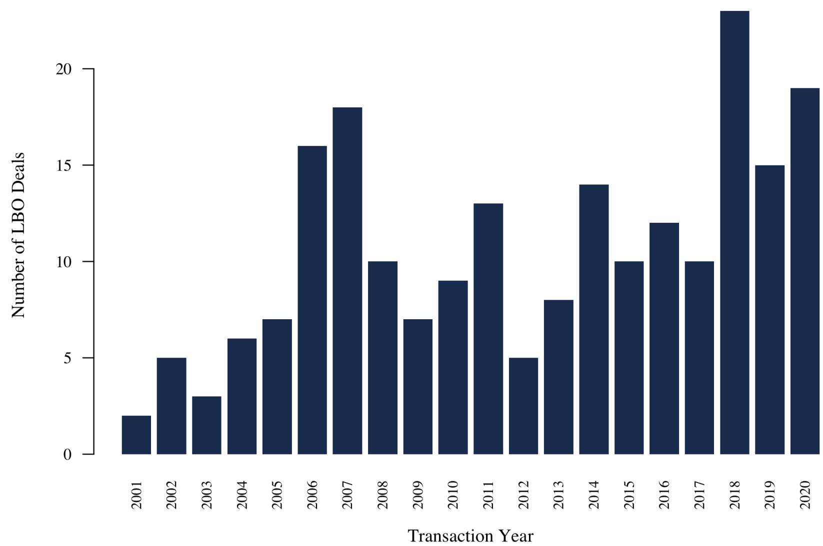

Figure 2. Number of LBOs per year in the sample of 212 deals. LBO's from 2000 are excluded due to insufficient accounting data.

Table 2 reports descriptive statistics for our 212 Swedish LBO targets and their matched control firms, averaged over three years preceding the transaction. The median target has sales of 103 MSEK and 36 employees. The corresponding means are roughly twice as large, indicating a right-skewed distribution driven by a few large firms. Boucly et al. (2011) report a larger firm size in their French sample; they observe a median of 64 employees and 13.9 M€ in sales (approximately 150 MSEK in 2026 prices). The pre-deal profitability in our sample is high: the median ROA is 0.35, and the mean is 0.50. Control firms are almost equally profitable, with a median ROA of 0.32, suggesting that this profitability level is a characteristic of Swedish small- and medium-sized firms rather than specific to LBO targets. Targets enter the buyout with virtually no financial debt. The median leverage is 0.01, and the interquartile range is 0.00 to 0.23, indicating that at least half the sample carries close to zero financial debt at the time of the transaction. This contrasts with Boucly et al. (2011), who report a median pre-buyout leverage of 0.20. Our leverage measure is constructed from unconsolidated firm-level accounts in Serrano, based on Swedish tax filings and excluding both owner loans and any debt held at the group level. To the extent that Swedish SMEs[4] rely on owner financing, our measure may understate pre-buyout indebtedness for some targets. Targets grow in the years leading up to the deal, with median annual sales growth of 18%, employment growth of 9%, and growth in capital employed of 15%. The corresponding figures in Boucly et al. (2011) are roughly half: 8%, 3%, and 8%, respectively. Control firms grow more slowly than targets in both samples. In ours, controls display a median growth of 10% in sales, 5% in employment, and 9% in capital employed.

One limitation of this study is the lack of consistent deal size data for the LBO transactions in our sample. Deal size would have provided a useful basis for comparison, particularly to examine whether post-buyout performance varies with transaction size, but the data do not support its inclusion.

[4] We follow the European Commission's definition: firms with fewer than 250 employees and either turnover below €50 million or a balance sheet total below €43 million (European Commission, n.d.).

Table 2
Sample of LBO targets and their matched control firms, over the period 2000–2020. Firm variables are measured as the mean over the 3 years prior to the transaction ($t = -3$ to $t = -1$). Panel A shows the distribution of outcome variables for LBO targets, and Panel B shows the median for every group of control firms. Capital Employed is computed by adding fixed assets and operating working capital. CE growth is its yearly growth rate. ROA is EBITDA divided by capital employed. Leverage is financial debt divided by Capital Employed.

	Median	Mean	SD	Q1	Q3	*N*
Panel A: LBO targets						
Sales (MSEK)	103.43	215.13	333.21	39.84	224.62	212
Employment	36.17	80.51	159.44	17.83	73.08	212
Capital employed (MSEK)	34.59	206.40	1013.12	9.67	92.52	212
ROA	0.35	0.50	0.82	0.10	0.74	212
Leverage	0.01	0.14	0.20	0.00	0.23	212
Sales growth	0.18	0.29	0.51	0.04	0.40	196
Employment growth	0.09	0.12	0.31	0.00	0.23	194
CE growth	0.15	0.22	0.45	-0.05	0.41	197
Panel B: Control firms						
Sales (MSEK)	71.50	207.73	661.35	26.57	172.94	212
Employment	37.50	83.16	164.10	18.58	77.21	212
Capital employed (MSEK)	16.69	207.22	1167.20	5.74	62.12	212
ROA	0.32	0.45	0.60	0.16	0.66	212
Leverage	0.00	0.05	0.09	0.00	0.06	212
Sales growth	0.10	0.12	0.30	0.03	0.18	208
Employment growth	0.05	0.08	0.25	0.00	0.12	209
CE growth	0.09	0.10	0.24	-0.01	0.21	207

3 Empirical Methodology

3.1 Baseline difference-in-differences design

To estimate the impact of private equity ownership on financial and operational performance, we employ a difference-in -differences regression. We examine seven outcome variables constructed from the Serrano database: return on assets (ROA), earnings before interest, taxes, depreciation, and amortization (EBITDA), number of employees, sales, capital employed, leverage, and capital expenditure (CAPEX). Capital employed (FA+WC) is the sum of fixed assets and working capital, where working capital equals trade receivables plus inventories minus trade payables. ROA is defined as in the matching procedure described in Section 2.3. Leverage is defined as financial debt divided by capital employed, where financial debt is the sum of current liabilities to credit institutions, non-current liabilities to credit institutions, and bonds payable[5]. The rest of the variables are self-explanatory. The following regression is performed for all seven outcome variables:

$$Y_{jt} = \alpha_j + \delta_t + \beta_0 \cdot POST_{jt} + \beta_1 \cdot (POST_{jt} \times LBO_j) + \varepsilon_{jt} \tag{1}$$

[5] Acquisition debt raised to finance the buyout is often held at a separate entity above the operating company (Andersson et al., 2025) and therefore, may not appear on the target's balance sheet in Serrano. Accordingly, we observe changes in operating-company leverage around the transaction, but not the deal-level acquisition debt used to finance the buyout.

Y_{jt} denotes the outcome variable for firm j in year t, where outcomes include ROA, EBITDA, employment, sales, capital employed, leverage, and CAPEX. We estimate the regression separately for each outcome. The term α_j represents firm fixed effects, absorbing all time-invariant firm characteristics. Since industry is time-invariant at the firm level, it is already captured by the firm fixed effects and not included separately. δ_t represents year fixed effects and absorbs common macroeconomic shocks affecting all firms in a given year. β_0 is the coefficient on $POST_{jt}$, capturing the average post-period change in the outcome variable for control firms. $POST_{jt}$ is a dummy variable equal to 1 in the post-deal years, t = 1 to t = 3, and 0 otherwise. For LBO targets, t=1 is the year subsequent the buyout. For matched control firms, the same timing is applied, defined by the deal year of the target to which they are matched. β_1 is the coefficient on $(POST_{jt} \times LBO_j)$, capturing the change in profitability or growth for LBO targets relative to control firms in the post-deal period. This is the parameter of interest and tests our first hypothesis. $(POST_{jt} \times LBO_j)$ is the interaction term, where LBO_j is a dummy variable equal to 1 for LBO targets and 0 for controls. The term ε_{jt} is the error term.

Boucly et al. (2011) cluster standard errors at the firm × POST level. We take a more conservative approach by clustering at the firm level, which allows serial correlation within each firm's panel rather than imposing independence between the pre-deal and post-deal periods. Appendix Table 6 reports the $(POST_{jt} \times LBO_j)$ coefficient and standard errors under three specifications: firm, firm × POST, and matched-pair clustering. Coefficient estimates are identical across specifications; only standard errors differ. Firm and matched-pair clustering produce nearly identical standard errors for every outcome. Clustering on Firm × POST yields standard errors approximately 25 to 30 percent smaller, which provides further motivation for our more conservative firm-level clustering in the main specification. Every coefficient that is statistically significant under firm clustering retains significance under both alternatives, though the leverage coefficient is significant only at the 10 percent level under pair clustering. Our conclusions are robust to the choice of clustering.

3.2 Endogeneity and identifying assumptions

Even with improved matching on pre-deal characteristics, the design cannot fully resolve the selection problem inherent in LBO research. Private equity sponsors actively choose their targets and may systematically seek out firms with stronger underlying growth potential; post-transaction differences between treated and control firms could reflect target selection rather than the influence of private equity ownership (Kaplan & Strömberg, 2009; Osborne et al., 2012). Boucly et al. (2011) acknowledge this limitation and interpret some of their estimates as descriptive associations rather than causal effects. Below, we follow the same approach, and document which of our estimates we view as causally interpretable and which we present descriptively.

The central identifying assumption of difference-in-differences design is that, absent the buyout, treated and control firms would follow parallel trajectories. Prior LBO studies typically assess parallel trends visually. Davis et al. (2014) match on pre-deal growth and observe pre-period trajectories. Boucly et al. (2011) plot cumulative excess changes relative to matched controls and interpret the flatness of the pre-period bars as supporting parallel trends. We supplement visual inspection with formal tests.

First, we estimate a dynamic difference-in-differences specification in which the LBO indicator is interacted with a set of year-relative-to-deal dummies, with t= -1 as the omitted reference category. Each coefficient measures the treated-control gap at event time k, relative to the corresponding gap at t = -1. Flat pre-period coefficients close to zero support parallel trends. Upward-trending pre-period coefficients indicate that targets are diverging from controls before the buyout. Figure 3 plots coefficients from our dynamic specification. The seven panels reveal meaningful heterogeneity across outcomes. Employment, CAPEX, and leverage display pre-period coefficients close to zero. For sales and capital employed, the pre-period coefficients trend upward toward zero, indicating that targets were already outgrowing controls before the deal. ROA and EBITDA show the most concerning pre-trends; the ROA coefficient at t = -3 is approximately -0.15, indicating that targets were substantially below controls three years before the deal and converged by t = -1.

The second is a F-test of the joint null hypothesis that the pre-period event-study coefficients at t = -3 and t = -2 are equal to zero. Failure to reject this null supports parallel-trends hypothesis; rejection indicates that treated and control firms were on statistically distinguishable trajectories before the buyout. Results are reported in Table 3. The F-test confirms parallel trends only for leverage ($p = 0.874$) and CAPEX ($p = 0.604$). For all other outcomes, we reject the null hypothesis at the 10% significance level or higher, including employment ($p = 0.004$) and ROA ($p = 0.020$). Although the employment pre-trend is small in absolute magnitude, the pre-period coefficients are estimated precisely enough that the formal test rejects equality with zero.

Since the F-test treats parallel-trend violations as a binary outcome, we complement it with the sensitivity analysis of Rambachan and Roth (2023). Their sensitivity analysis quantifies the robustness of each estimate to violations of varying magnitudes, parameterized by $\bar{M}$, the maximum allowed post-treatment deviation relative to the largest pre-treatment deviation. Setting $\bar{M} = 0$ imposes strict parallel trends. $\bar{M} = 0.5$ allows post-treatment violations up to half the size of the largest pre-treatment deviation. Robust confidence intervals are reported in Table 4. Employment and ROA retain significant effects at $\bar{M} = 0.5$, indicating that their post-deal effects are large enough to survive moderate pre-trend violations. Sales, capital employed, CAPEX, and EBITDA lose significance at $\bar{M} = 0.5$. Leverage is never statistically distinguishable from zero.

These tests establish the interpretive hierarchy we carry into the results. Employment passes the Rambachan-Roth analysis at $\bar{M} = 0.5$ with a visually clean pre-period pattern and is our most defensible causal estimate. ROA also passes the Rambachan-Roth at $\bar{M} = 0.5$, but displays a visible pre-trend in Figure 3 that warrants caution; we treat it as causally interpretable under the assumption that post-treatment violations do not exceed half the size of the pre-treatment deviation, while acknowledging that this assumption may not hold. CAPEX and leverage pass the F-test but not the Rambachan-Roth test; we treat it as suggestive but not conclusively causal. Sales, capital employed, and EBITDA fail both diagnostics and are reported as descriptive associations of the post-deal divergence rather than causal effects.

A limitation applies to all our estimates; no matching procedure can fully account for selection bias due to unobservables (Cohn et al., 2022). PE sponsors may have private information about their targets, management quality, growth prospects, and competitive positioning that does not appear in our data. If sponsors systematically acquire firms with these hidden advantages, part of what we attribute to PE ownership reflects pre-existing firm quality rather than a causal effect of the buyout. Resolving this would require a firm-level instrument for PE deal selection, but none exists in our setting.

Appendix Table 7 reports a specification, following Boucly et al. (2011), that includes an interaction between POST and pre-deal sales growth to absorb the portion of post-deal divergence attributable to pre-existing growth differences. The POST × LBO coefficients remain close to the baseline estimates, indicating that the post-buyout differences we attribute to the LBO are not driven by pre-existing sales-growth trajectories.

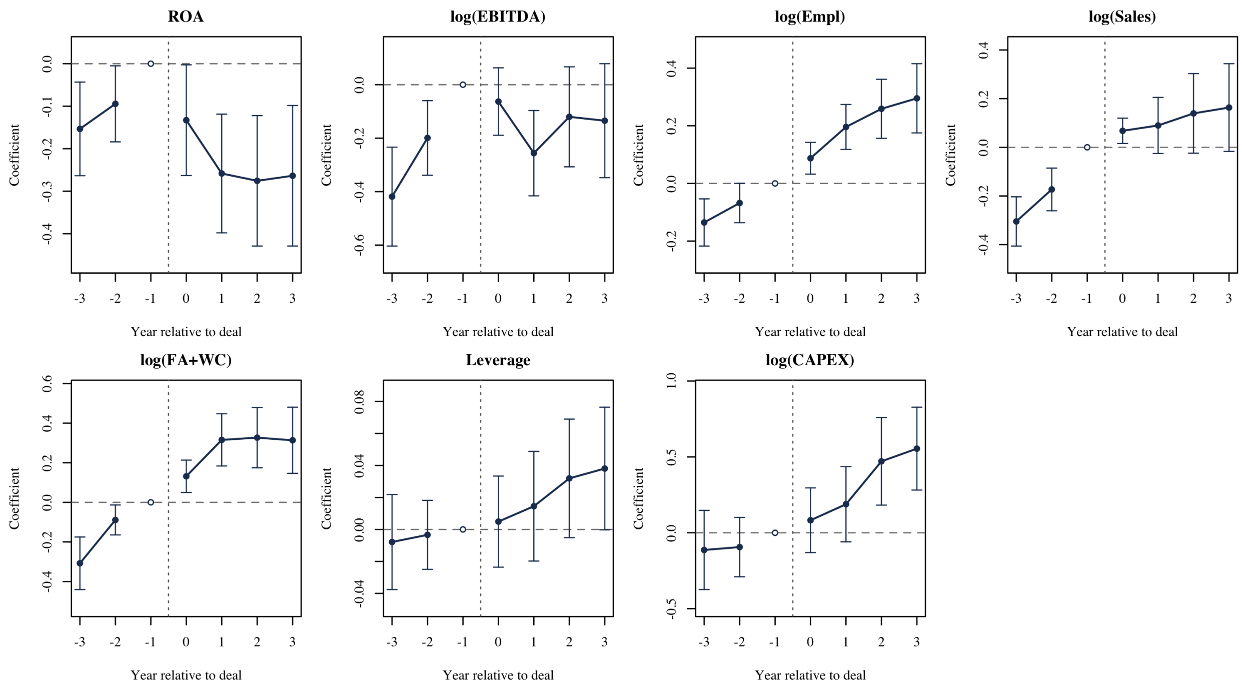

Figure 3. Event-study coefficients on POST $\times$ LBO interacted with event-time dummies, for each of the seven outcome variables. Each panel plots the estimated coefficient on each event-time dummy from a regression that replaces the single POST $\times$ LBO term with a full set of event-time interactions, with $t = -1$ omitted as the reference period. Vertical bars denote 95% confidence intervals based on standard errors clustered at the firm level. The dashed vertical line at $t = 0$ marks the deal year. Pre-period coefficients ($t = -3, -2$) test the parallel-trends assumption: coefficients close to zero with confidence intervals that include zero support the assumption. Post-period coefficients ($t = 0, +1, +2, +3$) trace the dynamic treatment effect.

Table 3

F-test of the joint null hypothesis that the pre-period event-study coefficients at $t = -3$ and $t = -2$ are equal to zero. The test uses the clustered variance-covariance matrix from the event-study regression with standard errors clustered at the firm level. Failure to reject ($p > 0.10$) supports the parallel-trends assumption; rejection indicates that targets and controls were on statistically distinguishable trajectories before the buyout.

Outcome	F-statistic	df	p-value
ROA	3.927	2	0.020
log(EBITDA)	9.858	2	0.000
log(Empl)	5.566	2	0.004
log(Sales)	17.587	2	0.000
log(FA+WC)	10.997	2	0.000
Leverage	0.135	2	0.874
log(CAPEX)	0.504	2	0.604

Table 4
Sensitivity of the post-treatment effect to violations of parallel trends, following Rambachan and Roth (2023). Each cell reports the robust 95% confidence interval for the average post-treatment effect ($t = 0$ to $t = +3$) under the relative-magnitude restriction Δ^{RM}, which bounds the maximum post-treatment deviation from parallel trends as a fraction $\bar{M}$ of the largest pre-treatment deviation. $\bar{M} = 0$ imposes strict parallel trends; $\bar{M} = 0.5$ allows post-treatment violations up to half the largest pre-treatment deviation; $\bar{M} = 1$ allows them to be as large as the largest pre-treatment deviation. Standard errors are clustered at the firm level. An interval that excludes zero indicates that the estimated effect survives the corresponding degree of pre-trend relaxation.

	Robust 95% CI under Δ^{RM} restriction		
Outcome	$\bar{M} = 0$	$\bar{M} = 0.5$	$\bar{M} = 1$
ROA	[-0.345, -0.115]	[-0.506, -0.005]	[-0.716, 0.175]
log(EBITDA)	[-0.275, -0.015]	[-0.596, 0.325]	[-0.996, 0.726]
log(Empl)	[0.135, 0.285]	[0.025, 0.385]	[-0.125, 0.526]
log(Sales)	[0.015, 0.215]	[-0.235, 0.455]	[-0.526, 0.746]
log(FA+WC)	[0.155, 0.385]	[-0.135, 0.696]	[-0.485, 1.056]
Leverage	[-0.005, 0.045]	[-0.015, 0.055]	[-0.045, 0.085]
log(CAPEX)	[0.125, 0.526]	[-0.085, 0.676]	[-0.395, 0.966]

3.3 Crisis years in the Swedish context

To investigate the second hypothesis, years of economic downturn are identified using a quantitative criterion based on Swedish real GDP growth. Specifically, we classify a year as being in a macroeconomic contraction if real GDP growth falls by one standard deviation, or more, below the average growth rate for the period 1997–2023. GDP data at an annual frequency is obtained from Statistikmyndigheten SCB (Statistics Sweden, 2025). Four years of economic downturn are identified in our sample: 2008, 2009, 2012, and 2020.

Each downturn corresponds to a known macroeconomic shock; we therefore refer to these periods as crises. The economic contraction in 2008 and 2009 coincided with the Great Financial Crisis, following the collapse of Lehman Brothers in September 2008 (Chodorow-Reich, 2014). The 2012 downturn coincides with the European Debt Crisis. Sharply rising bond yields in Greece, Ireland, Portugal, Italy, and Spain forced EU and IMF bailouts, as well as fiscal retrenchment measures across the Euro area (Lane, 2012). Although Sweden retained its own currency, the resulting slowdown in its main trading partners was transmitted to the Swedish economy through reduced external demand (OECD, 2012). The economic downturn identified in 2020 is related to the COVID-19 pandemic, which reached Sweden in spring 2020 (Ludvigsson, 2023). The pandemic differs in nature from typical recessions in that it originated as a public health shock that forced the temporary shutdown of contact-intensive sectors, generating a sharp but sectorally uneven contraction (Del Rio-Chanona et al., 2020). Unlike conventional recessions driven primarily by demand, the initial impulse came from supply-side restrictions, which, in turn, induced significant demand-side effects (Guerrieri et al., 2022).

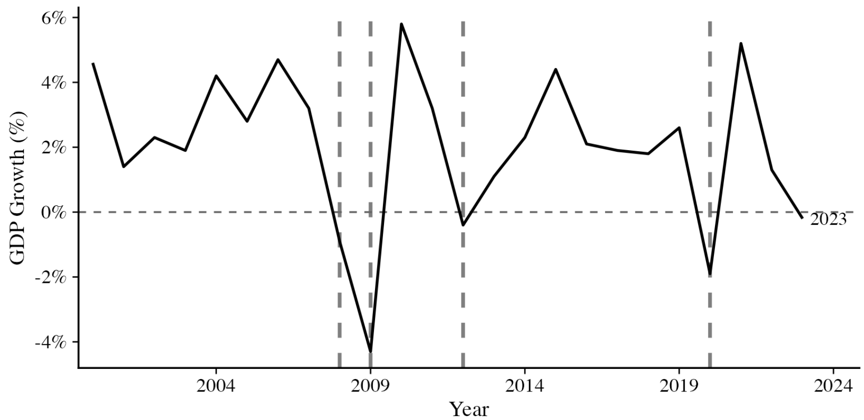

Figure 4. Swedish real GDP growth, 2001–2023. The solid line plots the annual growth rate of Swedish real GDP, measured as the percentage change in volume terms. The dashed horizontal line marks zero growth. The dashed vertical lines indicate years classified as economic downturns under the crisis definition: years in which real GDP growth falls more than one standard deviation below the average growth rate over the period 1997–2023. GDP data are obtained from Statistikmyndigheten SCB.

3.4 Difference-in-difference during crisis estimates

We extend the baseline regression by adding an interaction term for crisis periods, following the methodology of Lins et al. (2017). This approach is appropriate for our analysis for two main reasons. First, economic downturns are exogenous events, which allows the triple interaction (POST x LBO × CRISIS) to isolate the performance of LBO targets compared to control firms during a crisis. Second, we limit the sample to transactions completed at least one year prior to the onset of the downturn. This restriction ensures that private equity ownership is a pre-existing characteristic rather than a reaction to deteriorating economic conditions. Therefore, the interaction term captures the effect of existing PE ownership at the onset of a crisis. Lins et al.'s (2017) specification includes a post-crisis interaction term covering four years of post-crisis data. We omit this from our analysis because our post-crisis data extend at most two years, which is insufficient for a reliable estimate of the coefficient. The resulting specification is as follows:

$$Y_{jt} = \alpha_j + \delta_t + \beta_0 POST_{jt} + \beta_1(POST_{jt} \times LBO_j) + \beta_2(POST_{jt} \times LBO_j \times CRISES_t) + \beta_3(POST_{jt} \times CRISES_t) + \varepsilon_{jt} \quad (2)$$

$CRISES_t = 1$ for years $t \in \{2008, 2009, 2012, 2020\}$ and 0 otherwise.

In addition to Y_{jt}, α_j,δ_t and ε_{jt} for equation (1) we add $CRISES_t$, a dummy variable equal to 1 in periods classified as economic downturns and 0 otherwise. β_1 is the coefficient on $(POST_{jt} \times LBO_j)$ and captures the average post-deal change in outcome variables for LBO targets relative to matched controls in normal, non-crisis years. β_2 is the coefficient on $(POST_{jt} \times LBO_j \times CRISES_t)$ and captures the change in performance for LBO targets relative to matched controls during economic downturns, above the normal post-deal effect. β_3 is the coefficent on $(POST_{jt} \times CRISES_t)$ and captures the average change in the outcome for control firms during downturns relative to non-crisis periods, providing a check on whether the crisis affects the control group differentially.

To examine whether the LBO effect varies across these distinct shocks, we re-estimate equation (2) separately for each crisis, redefining $CRISES_t$ as follows:

Great Financial Crisis $t \in \{2008, 2009\}$ and 0 otherwise.

European Debt Crisis $t \in \{2012\}$ and 0 otherwise.

COVID-19 $t \in \{2020\}$ and 0 otherwise.

4 Empirical results

4.1 Baseline difference-in-differences estimates

Table 5 reports the baseline difference-in-differences estimates for equation (1). In the POST $\times$ LBO period, targets exceed matched controls by 0.291 log points in employment, 0.251 log points in sales, 0.393 log points in capital employed, and 0.438 log points in capital expenditure, all at the 1% significance level. Leverage rises by 2.8 percentage points of capital employed, and ROA falls by 14.5 percentage points of capital employed, both at the 5% significance level. The point estimate for log(EBITDA) is small and statistically indistinguishable from zero (0.032 log points), suggesting that the ROA decline reflects high asset growth rather than a decline in operating profit.

Matched control firms contract over the same observation window. POST is negative and significant at the 1% level for every outcome where targets expand: falling by 10 percentage points on ROA, -0.201 log points on EBITDA, -0.091 log points on employment, -0.138 log points on sales, and -0.1 log points on capital employed. The total post-deal change for targets, therefore, exceeds the POST $\times$ LBO coefficient; targets grow in absolute terms while the matched peers decline. This pattern does not extend to leverage or capital expenditure, where POST is statistically insignificant, and the effect of treatment reflects movement in targets alone.

Table 5

Sample of LBO targets paired with their matched control firms under equation (1). Every regression incorporates firm and year fixed effects. POST is an indicator variable taking the value 1 in years after the LBO, and 0 in years before it. LBO is an indicator variable taking the value 1 when the observation corresponds to an LBO target and 0 when it corresponds to a control firm. log(Empl) denotes the natural logarithm of the number of employees; log(FA+WC) denotes the natural logarithm of fixed assets combined with working capital (i.e., capital employed); log(CAPEX) denotes the natural logarithm of capital expenditures. Leverage is defined as financial debt scaled by capital employed. Standard errors, clustered at the firm level, appear in parentheses.

	ROA	log(EBITDA)	log(Empl)	log(Sales)	log(FA+WC)	Leverage	log(CAPEX)
POST $\times$ LBO	-0.145**	0.032	0.291***	0.251***	0.393***	0.028**	0.438***
	(0.061)	(0.079)	(0.049)	(0.070)	(0.076)	(0.014)	(0.109)
POST	-0.100***	-0.201***	-0.091***	-0.138***	-0.100***	0.000	-0.087
	(0.033)	(0.042)	(0.015)	(0.023)	(0.026)	(0.005)	(0.070)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	7,858	6,517	7,651	7,679	7,724	7,819	6,234
Number of deals	212	212	212	212	212	212	212
Overall Adj. R^2	0.39	0.80	0.92	0.89	0.91	0.62	0.72

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Our results indicate that PE sponsors pursue an expansionary strategy, characterized by aggressive growth in employment, sales, capital employed, and capital expenditure.

The most distinctive finding, relative to the existing literature, is our coefficient estimate on profitability. Boucly et al. (2011) document a 4.4 percentage-point increase in POST x LBO on its ROA variable; we find a 14.5 percentage-point decline. There are two plausible explanations behind the decrease in ROA, and distinguishing between them is important for interpreting our paper. ROA is defined as EBITDA divided by capital employed. Hence, a decrease in the ratio can reflect lower profitability, an increase in the growth rate of the asset base, or both. Lower profitability would suggest that PE-backed firms are becoming less profitable in absolute terms, a sign of value destruction. An increase in the growth rate of the asset base would suggest a stable operating profit alongside aggressive investments, resulting in a lower ROA without any underlying deterioration in performance. Our results indicate the latter. The EBITDA coefficient is small, 0.032 log points, and statistically indistinguishable from zero; this is consistent with the operating profit remaining unchanged in absolute terms. Capital employed, by contrast, expands substantially by 0.393 log points. A flat numerator and a sharply increasing denominator mechanically produce a falling ratio, even if the firm is creating rather than destroying value. We caution that the EBITDA coefficient fails the parallel-trends diagnostics in Section 3.2. Hence, we cannot fully rule out a change in operating profitability. However, the point estimate is small and the direction is positive, neither of which is consistent with deteriorating profitability.

Whether the expansion eventually pays off is a separate question, and one that equation (1) cannot answer. PE firms typically have holding periods exceeding three years (Kaplan & Strömberg, 2009), so investments made early in our observation window may not have realized their full returns by t = +3. A reasonable critique of our results is therefore that we capture the full cost of expansion, but not the resulting benefit. The 14.5 percentage point decline in ROA should thus be observed with this in mind.

Across all operating dimensions, our LBO targets exceed those in Boucly et al.'s (2011) French sample; the CAPEX coefficient is roughly twice as large. An explanation for this could be differences in firm size; the median PE-backed firm in our sample has 36 employees, compared with 64 in theirs. Smaller firms generate a larger percentage growth for any given absolute expansion, and the credit constraints whose relaxation Boucly et al. (2011) identify as a value-creation channel are more applicable to smaller firms in the first place. Industry composition reinforces this. Our sample is more concentrated in services, where growth manifests primarily through employment and sales rather than through physical capital. This is consistent with our growth observations for employment and sales.

Operating-company leverage rises by 2.8 percentage points, broadly in line with the 2.6 percentage points reported by Boucly et al. (2011). Given the greater growth in capital expenditure in our sample, post-buyout investment is likely financed through channels other than the operating company's own debt, such as retained earnings or sponsor equity injections. In line with the prediction of Boucly et al.'s (2011) credit-constraint channel, sponsors enable investment that the operating company alone could not have funded.

One limitation of our study is the way the Serrano database reports CAPEX. Capital expenditure is reported as an aggregate, and we cannot distinguish organic investments from acquisition-driven growth. A distinction that would matter; if most of our CAPEX increase reflects acquisitions of separate legal entities rather than organic investment, the credit-constraint channel does not apply, and the value-creation explanation shifts toward consolidation. Boucly et al. (2011) rule this possibility out using plant-level data on establishment creation and destruction. We are unable to conduct an equivalent test with our data. The expansion in employment is, however, consistent with organic growth, since employment at the target level would not necessarily increase in response to acquisitions of separate legal entities.

4.2 Differences-in-differences during crisis

Table 6 expands upon the baseline regression by incorporating a triple interaction term, POST $\times$ LBO $\times$ CRISES. This term helps us understand how LBO targets behave during economic downturns. The total crisis-year effect, as measured by $(\hat{\beta}_1 + \hat{\beta}_2)$, is positive and significant at the 1% level for employment (0.236 log points), sales (0.257 log points), capital employed (0.389 log points), and capital expenditure (0.610 log points). For employment, sales, and capital employed, the total crisis-year effect is similar in magnitude to the non-crisis-year coefficient (0.312, 0.248, and 0.425 log points, respectively). The POST $\times$ CRISES $\times$ LBO interaction term $(\hat{\beta}_2)$ is individually insignificant for every outcome.

The crisis interaction is largest for capital expenditure, with an increase of 0.25 log points, compared to non-crisis periods. However, this estimate is not statistically significant. Only 117 of the 212 deals in our sample have post-deal observations in at least one crisis year, which limits the statistical power of the triple interaction. The total crisis effect on CAPEX, of 0.61 log points, is at the 1% significance level. However, this primarily reflects the strong performance of LBO targets during normal times rather than an identifiable crisis-specific effect.

ROA falls by 17.2 percentage points in regular times following an LBO, at the 5% significance level. But neither the crisis interaction term, which indicates a 7.4 percentage-point increase in ROA, nor the total crisis-year effect is significant. $\log(\text{EBITDA})$ follows the same pattern: 0.031 log points in non-crisis years, -0.011 log points interaction, 0.020 log points total, all insignificant. Leverage rises by 2.4 percentage points in non-crisis years (insignificant), and the total crisis-year effect is 4.0 percentage points (at the 10% level), though the crisis-specific interaction (0.015 log points) is not individually significant.

POST $\times$ CRISES, which captures the average crisis effect in the post-matching period absorbed by the control group, is small and insignificant for all outcomes, as expected, once firm- and year-fixed effects absorb the average downturn. The POST coefficients are negative and significant at the 1% or 5% level for ROA, EBITDA, employment, sales, and capital employed, confirming that the matched controls contract after the matching date.

Those results indicate that leveraged buyout targets continue to grow during economic downturns at rates similar to their non-crisis performance, whereas their matched controls experience contraction. Crisis years do not appear to diminish post-LBO expansion in employment, sales, capital employed, or investment. Notably, the capital expenditure (CAPEX) point estimate suggests that targets may invest more aggressively during downturns, though it lacks sufficient precision to support a definitive conclusion. The crisis-specific analysis is therefore consistent with LBO targets demonstrating resilience to macroeconomic downturns.

Two caveats apply across the crisis specification. First, the marginal increase in operating-company leverage during pooled crisis years is consistent with two opposing scenarios: opportunistic borrowing to finance expansion when constrained competitors pull back, or defensive borrowing to cover cash-flow shortfalls under a revenue shock. Both produce the same net debt outstanding, and our specification cannot distinguish them. The simultaneous expansion in employment and CAPEX supports an opportunistic reading. Second, downturns increase firm exit through default, sale, and restructuring, which alters the composition of both treated and control samples.

While the specification in Table 6 estimates a single average effect of PE-backing across the three downturns, Tables 7–9 estimate the crisis interaction separately for the Great Financial Crisis (2008–2009), the European Debt Crisis (2012), and the COVID-19 recession (2020).

Table 6

Crisis difference-in-differences estimates of the LBO effect, decomposed into a non-crisis-times component ($\hat{\beta}_1$, POST × LBO) and a crisis component for times of economic downturn ($\hat{\beta}_2$, POST × LBO × CRISES). CRISES is a dummy equal to 1 in years when Swedish real GDP growth fall one standard deviation, or more, below the average growth rate of the period 1997–2023 . The sample, definitions of POST and LBO, and outcome variables are as in Table 5. The third row reports the total crisis-year effect ($\hat{\beta}_1 + \hat{\beta}_2$). All regressions include firm and year fixed effects. Of the 212 deals, 117 had post-deal observation in at least 1 crisis year. Standard errors are clustered at the firm level and reported in parentheses.

	ROA	log(EBITDA)	log(Empl)	log(Sales)	log(FA+WC)	Leverage	log(CAPEX)
POST × LBO	-0.172**	0.031	0.312***	0.248***	0.425***	0.024	0.36***
	(0.069)	(0.081)	(0.051)	(0.084)	(0.079)	(0.015)	(0.117)
POST × LBO × CRISES	0.074	-0.011	-0.076	0.009	-0.036	0.015	0.25
	(0.075)	(0.126)	(0.056)	(0.068)	(0.083)	(0.02)	(0.157)
$\hat{\beta}_1 + \hat{\beta}_2$ (total crisis effect)	-0.098	0.02	0.236***	0.257***	0.389***	0.04*	0.61***
	(0.082)	(0.136)	(0.069)	(0.063)	(0.11)	(0.021)	(0.163)
POST × CRISES	-0.056	-0.054	-0.003	0.025	0.012	0.002	-0.097
	(0.054)	(0.075)	(0.026)	(0.046)	(0.043)	(0.009)	(0.111)
POST	-0.079**	-0.176***	-0.084***	-0.135***	-0.091***	-0.001	-0.072
	(0.034)	(0.044)	(0.016)	(0.024)	(0.028)	(0.006)	(0.074)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	7,719	6,401	7,514	7,540	7,588	7,680	6,114
Number of deals	212	212	212	212	212	212	212
Overall Adj. R^2	0.39	0.80	0.92	0.89	0.91	0.63	0.72

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

4.3 Difference-in-differences during the Great Financial Crisis estimates

Table 7 restricts the crisis indicator to 2008 and 2009. Up to 41 LBO targets have at least one post-deal observation in those years. Capital expenditure is the outcome where the GFC interaction is most pronounced. The crisis interaction on CAPEX is increasing by 0.354 log points, at the 10% significance level. The total GFC-period effect ($\hat{\beta}_1 + \hat{\beta}_2$) show an effect of 0.741 log points at the 1% significance level for CAPEX, nearly double the non-crisis effect of 0.387 log points.

ROA shows a comparably large interaction in the opposite direction. In non-crisis post-deal years, LBO targets are 18.0 percentage points below matched controls, displayed at the 1% significance level. The crisis interaction is +21.3 percentage points at the 10% significance level, and the total GFC-period effect is +3.3 percentage points, which is statistically insignificant. The non-crisis ROA gap does not appear in the GFC sample, though the total effect is statistically insignificant.

The remaining outcomes are close to their non-crisis values. Total GFC-period effects on log(employment) (0.260, 5% level) and log(sales) (0.303, 1% level) are similar to the non-crisis effects of 0.301 log points and 0.244 log points. Capital employed (0.282 log points, insignificant) has a lower point estimate than the non-crisis effect of 0.433 log points, though the wide standard error precludes a sharp comparison. The crisis interactions on these three outcomes are individually insignificant. Leverage is marginally significant in both the non-crisis component (2.5 percentage points, 10% level) and the total crisis effect (6.7 percentage points, 10% level), but the crisis-specific interaction (4.3 percentage points) is not individually significant. EBITDA is insignificant.

Boucly et al. (2011) show that the post-buyout gains in size, debt, and capital expenditures are concentrated in targets operating in industries that rely more heavily on external finance, in firms where credit constraints would otherwise bind most tightly. If PE creates value partly by relaxing those constraints, the channel should be most pronounced when credit dries up for the broader economy (Axelson, Jenkinson, Strömberg, and Weisbach (2013)), and the Great Financial Crisis shows exactly such an episode in our Swedish sample. The total CAPEX effect during the GFC is nearly double the non-crisis baseline, while operating-company leverage barely moves; therefore, the financing likely comes from the sponsor rather than from incremental debt at the operating company. The findings on ROA are iterating; the negative gap that characterizes non-crisis years closes during the GFC, with the total effect indistinguishable from zero. We cannot claim a full profitability recovery, since the total ROA effect is also statistically indistinguishable from the non-crisis deficit. But the direction of movement is consistent with one, and the dynamic differs sharply from what we see in non-crisis years or in the European Debt Crisis and COVID-19 episode. This pattern matches Bernstein, Lerner, and Mezzanotti (2019) findings. Studying about 500 UK PE-backed firms in 2008, they find that PE-backed firms invested 6 percentage points more of their assets per year than matched peers during the crisis. They also raised more equity, issued more debt, and paid lower interest expense than their peers. PE-backed firms held on to financing access while comparable independent firms lost it.

Table 7
Crisis-augmented DiD estimates for the GFC (2008-2009) episode under equation (2). $\text{CRISES}_t = 1$ for 2008 and 2009. Specification, sample, and variable definitions are as in Table 6. The third row reports the total LBO effect during the crisis episode ($\hat{\beta}_1 + \hat{\beta}_2$) with delta-method standard errors. Deals with crisis exposure counts distinct LBO targets with at least one post-deal observation in a crisis year. All regressions include firm and year fixed effects. Standard errors are clustered at the firm level and reported in parentheses.

	ROA	log(EBITDA)	log(Empl)	log(Sales)	log(FA+WC)	Leverage	log(CAPEX)
POST × LBO	-0.180***	0.011	0.301***	0.244***	0.433***	0.025*	0.387***
	(0.067)	(0.082)	(0.049)	(0.079)	(0.079)	(0.015)	(0.113)
POST × LBO × CRISES	0.213*	0.146	-0.041	0.060	-0.151	0.043	0.354*
	(0.119)	(0.187)	(0.112)	(0.095)	(0.171)	(0.035)	(0.204)
$\hat{\beta}_1 + \hat{\beta}_2$ (total crisis effect)	0.033	0.157	0.260**	0.303***	0.282	0.067*	0.741***
	(0.114)	(0.186)	(0.120)	(0.081)	(0.179)	(0.035)	(0.207)
POST × CRISES	-0.087	-0.085	-0.023	0.021	0.069	-0.003	-0.035
	(0.065)	(0.112)	(0.042)	(0.071)	(0.062)	(0.011)	(0.144)
POST	-0.087***	-0.179***	-0.085***	-0.135***	-0.104***	-0.000	-0.082
	(0.033)	(0.044)	(0.015)	(0.025)	(0.028)	(0.006)	(0.073)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	7,775	6,445	7,569	7,596	7,641	7,736	6,158
Number of deals	212	212	212	212	212	212	212
Deals with crisis exposure	41	39	40	40	41	41	39
Overall Adj. R^2	0.39	0.80	0.92	0.89	0.91	0.62	0.72

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

4.4 Difference-in-differences during the European Debt Crisis estimates

Table 8 restricts the crisis indicator to 2012. Between 21 and 29 LBO targets have post-deal observations in that year, depending on the outcome. None of the seven crisis interactions is statistically significant. The point estimates are noisy, reflecting the limited number of treated firms with 2012 post-deal observations. The total crisis-year effects on sales (0.272 log points, 5% level) and capital employed (0.455 log points, 1% level) are slightly larger than the non-crisis effects of 0.243 log points and 0.387 log points. Employment remains positive and significant (0.186 log points, 5% level), though somewhat smaller than the non-crisis effect of 0.291 log points. The CAPEX point estimate follows the GFC pattern. The total crisis effect is 0.509 log points versus 0.428 log points in non-crisis years, though imprecisely estimated and insignificant. ROA shows a similar pattern. The non-crisis gap of -15.3 percentage points (5% level) shrinks to -6.8 percentage points in the total crisis-year effect, which is not significant. The remaining outcomes (EBITDA, leverage) are insignificant in both components.

The European Debt Crisis, therefore, produces no significant crisis interactions, and the underlying point estimates move in different directions across outcomes, slightly above non-crisis values for sales and capital employed, slightly below for employment, and statistically indistinguishable from non-crisis values for the remaining outcomes. One possible interpretation concerns how the crisis was transmitted to Sweden. The 2012 episode appears to have put pressure on Swedish firms primarily through their export markets in the affected euro-area economies, rather than through a tightening of Swedish credit supply (Sveriges Riksbank, 2013). If so, the credit-constraint channel that helps explain the GFC result would have had less scope in 2012 than in 2008–2009. LBO firms continued to grow on the operational dimensions where they grew in normal times, and the absence of any sharp crisis dampening would be broadly consistent with weaker external demand affecting both targets and controls roughly evenly. On this reading, the 2012 crisis created neither the credit-tightening environment that defined the GFC nor the acute domestic financial stress that would have most directly tested PE-backed firms' financing advantage. Limited statistical power, however, means we cannot rule out modest crisis-specific effects that the 29-deal subsample is too small to detect.

Table 8

Crisis-augmented DiD estimates for the European Debt Crisis (EUD) episode (2012) under equation (2). $\text{CRISES}_t = 1$ for 2012. Specification, sample, and variable definitions are as in Table 6. The third row reports the total LBO effect during the crisis episode ($\hat{\beta}_1 + \hat{\beta}_2$) with delta-method standard errors. Deals with crisis exposure counts distinct LBO targets with at least one post-deal observation in a crisis year. All regressions include firm and year fixed effects. Standard errors are clustered at the firm level and reported in parentheses.

	ROA	log(EBITDA)	log(Empl)	log(Sales)	log(FA+WC)	Leverage	log(CAPEX)
POST × LBO	-0.153**	0.016	0.291***	0.243***	0.387***	0.026*	0.428***
	(0.063)	(0.080)	(0.049)	(0.071)	(0.078)	(0.015)	(0.110)
POST × LBO × CRISES	0.085	0.156	-0.105	0.028	0.068	0.021	0.081
	(0.126)	(0.152)	(0.078)	(0.101)	(0.135)	(0.037)	(0.327)
$\hat{\beta}_1 + \hat{\beta}_2$ (total crisis effect)	-0.068	0.172	0.186**	0.272**	0.455***	0.047	0.509
	(0.119)	(0.156)	(0.083)	(0.106)	(0.139)	(0.039)	(0.335)
POST × CRISES	0.034	-0.119	0.047	0.039	-0.045	0.010	0.006
	(0.114)	(0.106)	(0.044)	(0.062)	(0.083)	(0.015)	(0.184)
POST	-0.099***	-0.194***	-0.091***	-0.139***	-0.093***	0.000	-0.089
	(0.033)	(0.042)	(0.015)	(0.023)	(0.027)	(0.005)	(0.071)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	7,821	6,486	7,615	7,642	7,690	7,782	6,206
Number of deals	212	212	212	212	212	212	212
Deals with crisis exposure	29	21	28	29	29	29	26
Overall Adj. R^2	0.39	0.80	0.92	0.89	0.91	0.62	0.72

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

4.5 Difference-in-differences during the COVID-19 crisis estimates

Table 9 restricts the crisis indicator to 2020. The COVID-19 episode produces a pattern distinct from both the GFC and the European Debt Crisis: operational performance is preserved, but profitability comes under greater pressure than in either normal post-deal years or the two earlier downturns. None of the seven crisis interactions is statistically significant. The point estimates for ROA (-0.141), EBITDA (-0.340), employment (-0.089), and sales (-0.067) are negative, suggesting greater pandemic pressure on LBO firms, but the standard errors are too wide to support that interpretation individually. The critical difference from the GFC is in the direction of the ROA interaction: whereas the GFC interaction was positive (21.3 percentage points), the COVID interaction is negative (-14.1 percentage points), widening the profitability gap between target companies and its control firms. This directional difference is also reflected in the total COVID-period effects. The total ROA effect ($\hat{\beta}_1 + \hat{\beta}_2$) is -0.27 percentage points at the 10% level, substantially more negative than both the non-crisis years effect of -0.13 percentage points and the GFC total of $+0.033$ percentage points. LBO firms were meaningfully less profitable than their matched controls in 2020, and more so than in any other post-deal period we examine. The EBITDA total effect of -0.278 log points is insignificant. The total COVID-period effects on employment and sales are 0.211 log points at the 1% level and 0.196 at 10%, positive and significant but somewhat below the normal-times effects of 0.299 and 0.262 log points. Capital employed, by contrast, expands at 0.463 log points at the 1% significance level, above the normal-times effect of 0.391. LBO firms therefore kept investing in the asset base through 2020 even as revenue and headcount growth moderated. Investment held up as well: the total CAPEX effect of 0.569 at 10% significance is larger than the normal-times effect of 0.423, though well below the value observed during the GFC.

The 2020 shock was a sudden collapse in revenue, not a tightening of credit (Del Rio-Chanona et al., 2020). The mechanism behind the GFC result, sponsors deploying capital while credit-constrained competitors retrench, does not fit COVID as cleanly. Bernstein et al. (2019) show that PE-backed firms invested more during the financial crisis because sponsors helped relax financing constraints when credit markets were frozen. In 2020, by contrast, the immediate shock was primarily a collapse in demand for many customer-facing activities, with COVID restrictions and infection avoidance sharply reducing demand for contact-intensive services (Del Rio-Chanona et al., 2020).

Profitability fell for PE-backed firms during COVID. The total ROA effect is the most negative we observe in any post-deal period, and the EBITDA point estimate is also negative. This is what we would expect when a leveraged firm runs into a revenue shock. CAPEX is harder to reconcile. Total capital expenditure during COVID exceeds non-crisis levels. The credit-constraint frame does not obviously predict this. If lenders were not the binding constraint, sponsors might have had little reason to invest counter-cyclically, as they appear to have done during the GFC. Two readings could potentially account for the result. PE sponsors may operate on longer horizons than other owners, and investment plans formulated before the shock could have carried through despite the revenue collapse (Kaplan & Strömberg, 2009). Alternatively, COVID may have produced a brief but sharp credit-market dislocation in early 2020, before central bank interventions (Haddad, Moreira, and Muir, 2021), which would have given the credit-constraint channel some scope. Our specification cannot easily distinguish between the two interpretations.

The cross-crisis pattern is consistent with PE ownership working differently depending on the kind of shock. In a credit shock like the GFC, sponsors increase investment sharply. In a revenue shock like COVID, they hold investment steady or expand it but compress on labor. In a downturn that operates through trade rather than credit or revenue, the LBO advantage neither increases nor decreases.

Table 9
Crisis-augmented DiD estimates for the COVID-19 (2020) episode. $CRISES_t$ = 1 for 2020 under equation (2). Specification, sample, and variable definitions are as in Table 6. The third row reports the total LBO effect during the crisis episode ($\hat{\beta}_1 + \hat{\beta}_2$) with delta-method standard errors. Deals with crisis exposure counts distinct LBO targets with at least one post-deal observation in a crisis year. All regressions include firm and year fixed effects. Standard errors are clustered at the firm level and reported in parentheses.

	ROA	log(EBITDA)	log(Empl)	log(Sales)	log(FA+WC)	Leverage	log(CAPEX)
POST × LBO	-0.130**	0.062	0.299***	0.262***	0.391***	0.030**	0.423***
	(0.060)	(0.079)	(0.051)	(0.072)	(0.076)	(0.015)	(0.112)
POST × LBO × CRISES	-0.141	-0.340	-0.089	-0.067	0.073	-0.031	0.146
	(0.124)	(0.265)	(0.072)	(0.099)	(0.119)	(0.026)	(0.309)
$\hat{\beta}_1 + \hat{\beta}_2$ (total crisis effect)	-0.270*	-0.278	0.211***	0.196*	0.463***	-0.000	0.569*
	(0.138)	(0.271)	(0.072)	(0.102)	(0.141)	(0.027)	(0.310)
POST × CRISES	-0.089	0.084	-0.012	0.013	-0.021	0.001	-0.346
	(0.122)	(0.152)	(0.047)	(0.095)	(0.076)	(0.018)	(0.271)
POST	-0.088***	-0.207***	-0.089***	-0.136***	-0.097***	-0.000	-0.066
	(0.033)	(0.042)	(0.015)	(0.022)	(0.027)	(0.006)	(0.071)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	7,839	6,504	7,632	7,660	7,705	7,800	6,218
Number of deals	212	212	212	212	212	212	212
Deals with crisis exposure	47	33	44	46	46	47	36
Overall Adj. R^2	0.39	0.80	0.92	0.89	0.91	0.62	0.72

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

5 Robustness tests

5.1 Industry-by-year fixed effects

If LBO targets cluster in industries with stronger growth or weaker profitability for reasons unrelated to the buyout, the POST × LBO coefficient could capture industry trends rather than firm-level treatment effects. Table 10 replaces the year fixed effects in equation (1) with industry-by-year fixed effects, where industry is defined by a two-digit SNI code. This specification compares LBO targets to matched controls within the same industry-year and absorbs any sector-specific shock common to firms in that industry-year. The baseline findings hold. POST × LBO is positive for employment with 0.283 log points, sales with 0.244 log points, capital employed with 0.393 log points, and CAPEX with 0.405 log points, all significant at the 1% level. It remains negative for ROA at -13.4 percentage points at the 1% significance level. The point estimates are smaller than in Table 5 for most outcomes, as expected once sector-specific shocks are absorbed, but the direction and significance are unchanged. Log(EBITDA) remains insignificant, and leverage is marginally significant, increasing by 2.5 percentage points; this time, leverage is significant at the 10% level compared to 5% in Table 5. The LBO effects, therefore, reflect within-industry differences between targets and matched controls rather than industry composition.

5.2 Alternative matching procedures

A further concern is whether our results are sensitive to the specific matching procedure. We address this with two additional tests that vary the matching bandwidth and algorithm. First, we tighten the matching criterion by restricting both employment and ROA to $\pm 20\%$ of the treated firms. The stricter matching criterion reduces the sample to 195 matched deals, as addressed in Section 2.3. Our results, which can be found in Appendix Table 8, show that the POST × LBO coefficients are close to unchanged: ROA falls by 13.1 percentage points at the 5% significance level, while employment rises by 0.307 log points, sales by 0.277 log points, capital employed by 0.380 log points, and CAPEX by 0.420 log points, all at the 1% significance level. EBITDA remains insignificant. Leverage increases by 0.019 log points but is no longer significant, compared to 0.028 log points at the 5% significance level in Table 5, suggesting that the leverage result is sensitive to the matching window. The other magnitudes are close to the baseline

Table 10
Robustness of the baseline DiD estimates to industry-specific time trends. Year fixed effects are replaced by 2-digit industry × year fixed effects, alongside firm fixed effects. Identification comes from within-industry-year variation, ruling out the concern that LBO targets cluster in industries with idiosyncratic time trends in employment or investment. Standard errors are clustered at the firm level and reported in parentheses.

	ROA	log(EBITDA)	log(Empl)	log(Sales)	log(FA+WC)	Leverage	log(CAPEX)
POST × LBO	-0.134***	0.02	0.283***	0.244***	0.393***	0.025*	0.405***
	(0.047)	(0.073)	(0.042)	(0.063)	(0.07)	(0.013)	(0.101)
POST	-0.053	-0.113**	-0.102***	-0.119***	-0.115***	0.002	-0.012
	(0.037)	(0.048)	(0.017)	(0.027)	(0.031)	(0.006)	(0.085)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry × Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	7,858	6,468	7,650	7,676	7,721	7,819	6,190
Number of deals	212	212	212	212	212	212	212
Overall Adj. R^2	0.41	0.80	0.92	0.89	0.91	0.63	0.71

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

estimates in Table 5, indicating that the tighter pre-deal balance between treated and control firms does not alter the post-deal divergence we document on employment, sales, capital expenditure, capital employed, or profitability.

Second, we replace our baseline matching, which uses full optimal matching via a binary integer program, with nearest-neighbor matching based on Euclidean distance over the same pre-deal characteristics. We use two tolerance settings. The first allows employment to fall within ±20% of the treated firm's value and ROA to fall within ±50% of the treated firm's value, resulting in 212 matched deals. The second allows both employment and ROA to fall within ±50% of the treated firm's value, resulting in 233 matched buyouts. The results are shown in Appendix Table 9 and are similar to our baseline findings in Table 5. ROA falls by 14.3 percentage points at the 5% significance level. Employment rises by 0.294 log points under the tighter tolerance and 0.292 under the looser one, sales by 0.252, and capital employed by 0.408, all at the 1% significance level. Leverage rises by 2.8 percentage points under the tighter tolerance and 2.7 under the looser one, both at the 10% significance level, weaker than the 5% significance found in the baseline. EBITDA remains insignificant. The CAPEX coefficient differs slightly between the two windows, at 0.450 under the tighter tolerance and 0.443 under the looser one, both significant at the 1% level. The baseline results are therefore robust to both the choice of matching bandwidth and the underlying matching algorithm, except for leverage, where precision attenuates somewhat under both Euclidean specifications.

5.3 Permutation test

With 212 treated firms, each matched with up to five controls, our sample is small enough that the asymptotic approximations underlying clustered standard errors may be imprecise. Hence, we perform a permutation test on log(Sales) and log(Employment). Within each matched group, we randomly reassign the LBO indicator, with the remaining firms serving as placebo controls. We then re-estimate equation (1) on the reassigned sample, and store the placebo POST × LBO coefficient. Repeating this procedure 1,000 times yields an empirical distribution of placebo coefficients under the null hypothesis of no treatment effect. Because reassignment occurs within matched groups, the industry, size, and profitability structure of the sample is preserved; any placebo effect must therefore arise from chance rather than systematic differences between treated and control firms. Figure 5 displays the resulting distributions. For both outcomes, the actual estimate lies far to the right of the distribution, and no placebo coefficient across 1,000 reassignments reaches it. For log(Sales), the actual estimate of 0.251 exceeds the 95th percentile of the placebo distribution (0.113) by more than a factor of three. For log(Employment), the actual estimate of 0.291 exceeds the 95th percentile (0.073) by more than a factor of four. The permutation p-value is zero for both outcomes, indicating that the baseline results for sales and employment cannot be attributed chance or the structure of the matched sample.

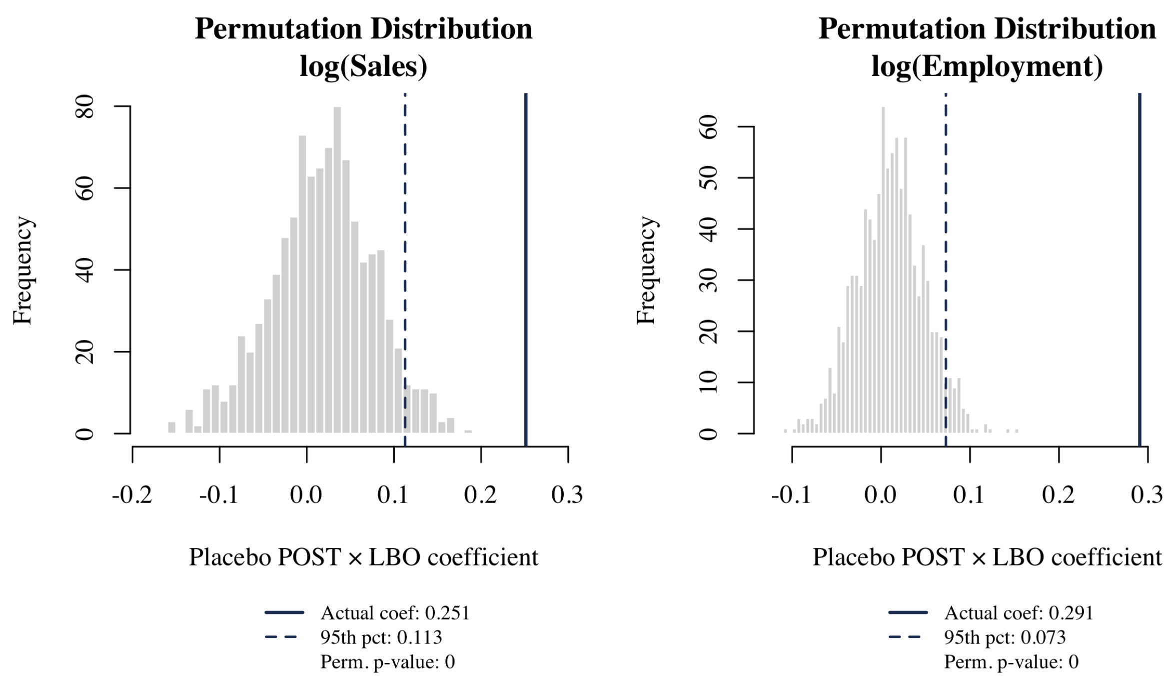

Figure 5. Permutation test for the POST $\times$ LBO coefficients on log(Sales) and log(Employment). Within each matched group, the LBO indicator is randomly reassigned to one firm, with the remaining firms in the group serving as placebo controls. The baseline DiD specification from equation (1) is re-estimated on the reassigned sample, and the placebo POST $\times$ LBO coefficient is stored. The procedure is repeated 1,000 times. Histograms display the resulting distribution of placebo coefficients. The solid black line marks the actual estimate from the un-permuted sample; the dashed blue line marks the 95th percentile of the placebo distribution. The permutation p-value is the share of placebo coefficients that meet or exceed the actual estimate. ($p < 0.001$).

5.4 Industry-by-year fixed effects for crisis periods

Appendix Table 10 re-estimates the crisis DiD by replacing year fixed effects with industry-by-year fixed effects. The coefficients on POST × LBO during non-crisis years are similar to the corresponding estimates in Table 6: ROA falls by 16.2 percentage points at the 1% significance level. Employment increases by 0.300 log points, sales by 0.241 log points, capital employed by 0.425 log points, and CAPEX by 0.337 log points, all at the 1% significance level. The crisis interactions (POST × LBO × CRISES) are insignificant for all outcomes except CAPEX, where the coefficient increases by 0.228 log points at the 10% significance level. This finding is similar to the marginally significant CAPEX interaction found for the GFC specification in Table 7. The total crisis-year effects are not statistically significant, but their point estimates remain close to those observed in Table 6: for employment, 0.238 log points against 0.236, for sales, 0.256 log points against 0.257, for capital employed, 0.393 log points against 0.389, and for CAPEX, 0.565 log points against 0.610. The loss of statistical significance could reflect larger standard errors after absorbing industry-year trends, rather than a change in the underlying magnitudes. The conclusion from Table 6, therefore, holds within industries: LBO firms do not contract during crises, and the pattern is not driven by differential industry exposure to economic downturns.

5.5 Difference-in-differences with alternative crisis definition

A final robustness test varies the definition of economic downturns. Table 11 replaces the crisis specification (equation 2) with one that uses quarterly deviations of Swedish GDP from the long-run trend, following the methodology behind SCB's konjunkturklocka indicator. A quarter is classified as recessionary when GDP is below its long-run trend, and the negative deviation widens. A year is classified as a downturn when at least two of its quarters meet this criterion. This procedure identifies 2001-2003, 2009, 2012-2013, and 2023-2024 as periods of macroeconomic contraction. Notably, 2020, which coincided with the COVID-19 crisis, is not flagged, whereas the early-2000s dot-com downturn (2001–2003) is added (Sveriges Riksbank, 2001). The main findings are robust to this alternative definition. The POST × LBO coefficients during non-crisis years are close to the baseline estimates in Table 6 across all seven outcomes. The total crisis-year effects are 0.238 log points for employment (1% significance level), 0.332 log points for sales (1% significance level), 0.282 log points for capital employed (5% significance level), and 0.544 log points for CAPEX (1% significance level). All broadly consistent with the estimates in Table 6. ROA's total crisis effect is -7.6 percentage points and is statistically insignificant, in line with Table 6. Because the Serrano database ends in 2023, we observe the first year of the 2023–2024 downturn but not the second. Without intervention, LBOs completed in 2020 would have their t = +3 observation in 2023, labeling them as having experienced this downturn while being observed for only half of it. To preserve symmetry with the earlier, fully observed downturns (2001-2003, 2009, 2012-2013), we drop the t = +3 observation for LBOs completed in 2020. All other deals are observed across full downturn episodes or not at all. One notable difference is EBITDA. The crisis interaction (POST × LBO × CRISES) is 0.255 at the 10% level, and the total crisis-year EBITDA effect is 0.246, also at the 10% level, whereas they were insignificant in Table 6. Part of this reflects the negative POST × CRISES coefficient on EBITDA (-0.176, at the 5% significance level), which captures the average crisis-year decline in operating profits absorbed by the control group; against this benchmark, LBO targets' EBITDA remains relatively stable. Under this alternative definition, the crisis window includes the 2001–2003 dot-com downturn and excludes the 2020 COVID-19 downturn. The shift in results suggests that the marginally significant EBITDA crisis effect is sensitive to which downturns are classified as crisis years.

Table 11

Crisis-augmented DiD estimates under an alternative crisis definition: SCB's *konjunkturklocka* flags a quarter as recessionary when GDP is below the long-run trend and the gap widens; a year is a downturn when at least two quarters qualify. Under this rule, downturn years are 2001–2003, 2009, 2012–2013, and 2023–2024 (2020 is not flagged). Specification, sample, and variable definitions are otherwise as in Table 6. The 2023–2024 downturn cannot be fully observed because Serrano data ends in 2023; we therefore drop $t = +3$. Standard errors clustered at the firm $\times$ POST level in parentheses.

	ROA	log(EBITDA)	log(Empl)	log(Sales)	log(FA+WC)	Leverage	log(CAPEX)
POST $\times$ LBO	-0.185***	-0.010	0.298***	0.246***	0.427***	0.014	0.385***
	(0.070)	(0.089)	(0.050)	(0.079)	(0.083)	(0.015)	(0.117)
POST $\times$ LBO $\times$ CRISES	0.110	0.255*	-0.060	0.086	-0.145	0.017	0.159
	(0.082)	(0.138)	(0.063)	(0.076)	(0.101)	(0.022)	(0.184)
$\hat{\beta}_1 + \hat{\beta}_2$ (total crisis effect)	-0.076	0.246*	0.238***	0.332***	0.282**	0.031	0.544***
	(0.078)	(0.146)	(0.078)	(0.077)	(0.110)	(0.024)	(0.193)
POST $\times$ CRISES	-0.025	-0.176**	-0.007	-0.043	0.047	0.015	-0.011
	(0.055)	(0.083)	(0.030)	(0.053)	(0.053)	(0.009)	(0.115)
POST	-0.095***	-0.168***	-0.081***	-0.129***	-0.090***	0.001	-0.072
	(0.034)	(0.044)	(0.015)	(0.024)	(0.028)	(0.006)	(0.072)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	7,561	6,293	7,368	7,389	7,436	7,524	5,989
Number of deals	212	212	212	212	212	212	212
Overall Adj. R^2	0.39	0.80	0.92	0.89	0.91	0.63	0.72

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

6 Limitations and suggestions for future research

6.1 Holding periods and portfolio resilience

Value creation in private equity unfolds over time, as sponsors' strategic activities typically involve replacing management and shifting to a new business model (Gompers, Kaplan, and Mukharlyamov, (2016)). This explains why Boucly et al. (2011) examine a three-year window following the LBO, rather than only observing effects in the year directly following the buyout. However, the view that value is realized over time creates challenges for our extension of the baseline DiD (Equation 1). When extending our DiD regression to include crisis periods, the length of private equity ownership prior to the onset of an economic contraction varies. Hence, a limitation of our study is that certain firms have experienced shorter holding periods under private equity ownership prior to a crisis than others. For example, a firm acquired in 2005 would have had 2 years of PE ownership before the GFC in 2008; in contrast, a firm acquired in 2006 would have had only 1 year of PE ownership before the crisis. This variation matters for the estimated coefficient on the interaction term POST $\times$ LBO $\times$ CRISIS. As observed in Section 4.2, private equity sponsors continue to drive operational growth for targets during crises. However, given that value creation in private equity unfolds over time, we expect targets with a longer PE holding period prior to a crisis to be more resilient. A shorter holding period could indicate that targets are less resilient, as they carry higher leverage without yet benefiting from improvements and fully implemented strategies. Thus, averaging across these groups could underestimate the crisis resilience of more mature PE-owned firms. Alternatively, it could omit differences in how PE ownership affects performance across holding periods. This poses a meaningful extension for future research, which could examine whether resilience to crisis increases with the duration of PE ownership. This could be addressed by introducing an interaction term between the crisis effect and the PE holding period.

6.2 Economic downturn, resilience and long-term competitive advantage

An additional extension of this study would be to examine whether the resilience observed in PE-sponsored firms during periods of crisis translates into a long-term competitive advantage, or whether the effects reverse as economic conditions normalize. Boucly et al. (2011) introduce an interaction term measuring the effects in the period following a crisis. Their post-crisis window extended for five years, 2009-2013. Our specification in equation 2 does not include any such interaction term, leaving a possible research question unexplored: do PE-backed firms' operational growth in periods of crisis translate to future out-performance, or does it simply delay a convergence from non-PE backed firms? One reason to expect non-PE-backed firms to catch up after the crisis is the increase in credit supply that typically follows as economies normalize (Farboodi & Kondor, 2023). Bernstein et al. (2017) show that PE-backed industries grow faster precisely when external financing is scarce for competitors, implying that once credit is no longer scarce, this relative advantage fades and non-PE-backed firms can catch up.

6.3 Selection bias, bankruptcy and corporate events

An additional limitation to this study is the possibility of selection bias, as a result of the requirement that all firms must have complete observations across the full event window. Consequently, if a treated firm or a potential control firm would enter bankruptcy within this window, it would be excluded. If a potential control firm enters bankruptcy, the most financially distressed firms are excluded from the regression. The control group is thus composed of firms that are, on average, more financially stable than the true counterfactual population; all else the same, such an upward distortion would cause the estimated treatment effect to be underestimated. On the contrary, if a treated firm enters bankruptcy within the event window, it is excluded from the treated sample. This removes the firms for which treatment was least effective, consequently inflating the estimated treatment effect. It should be noted, however, that bankruptcy is only one of several reasons a firm may fail to maintain data coverage across the full event window. Corporate events such as mergers and acquisitions, and group restructurings could also be plausible explanations. These events carry different implications for selection bias compared to bankruptcy. For instance, an acquired firm is often financially sound and strategically attractive. Removing these from the control group hence risks overestimating the effect, as above-average performers are removed. Given the wide range of potential explanations for a firm's absence from the data, conducting a case-by-case investigation of the excluded control firms is not feasible.

7 Conclusion

In the 1980's and 1990's, academic literature claimed that PE sponsors improved profitability through financial engineer- ing, rather than operational value creation (Kaplan, 1989). However, in the early 2000's, the consensus began to shift; literature showed that PE-backed firms have positive net employment effects (Davis et al., 2014) and that operating improvements stem from improved management and governance (Bergström et al., 2007). Our reference paper, Boucly et al. (2011), observes that PE-backed firms create value by operational growth driven by the alleviation of pre-existing credit constraints. Our regression estimates extend the evidence for this hypothesis to Sweden over the 2000–2020 period. In contrast to Boucly et al. (2011), we find that LBO targets do not increase their profitability, as proxied by ROA; although a plausible explanation is a lag in profitability following rapid asset growth and an expansionary strategy, we acknowledge that the deviation from Boucly et al. (2011) cannot be fully explained.

Our study identifies a gap in the pre-existing literature by observing if PE sponsors continue to create value through operational and financial expansion in periods when markets contract. Our sample covers three economic downturns, coinciding with the Great Financial Crisis, European Debt Crisis, and COVID-19 crisis. The main contribution of this paper is the finding that PE sponsors do not significantly change their behavior in times of crisis. However, their consistency in firm behavior over time leads to an outperformance in operational growth, in relation to matched non-PE-backed firms. We further observe that the magnitude of operational growth depends on the type of crisis. Specifically, the credit-constraint channel proposed by Boucly et al. (2011) appears most pronounced when external credit tightens, as during the Great Financial Crisis, and less so during shocks transmitted through trade or demand. Our findings contribute to the understanding of how private equity sponsors create value across economic cycles.

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9 Appendices

9.1 Appendix A

Appendix Table 1
Covariate balance between LBO targets and matched controls at $t = -1$ under the wider ±50% employees / ±50% ROA caliper specification. The Difference column reports LBO Target minus Control. The t-statistic tests the null of equal means.

	LBO Target (mean)	Control (mean)	Difference	t-stat	p-value
log(Employees)	3.781	3.602	0.179	1.812	0.071
ROA	0.591	0.562	0.029	0.38	0.704

Appendix Table 2
Covariate balance between LBO targets and matched controls at $t = -1$ under the tighter ±20% employees / ±20% ROA caliper specification. The Difference column reports LBO Target minus Control. The t-statistic tests the null of equal means.

	LBO Target (mean)	Control (mean)	Difference	t-stat	p-value
log(Employees)	3.554	3.285	0.269	2.593	0.01
ROA	0.608	0.584	0.024	0.312	0.755

Appendix Table 3
Covariate balance under the Boucly, Sraer, and Thesmar (2011) matching procedure: greedy nearest-neighbour assignment on raw Euclidean distance over log(Employees) and ROA, with ±20% / ±50% calipers. Euclidean matching and our optimal-matching procedure yield the same number of matched deals but share only 58% of control firms.

	LBO Target (mean)	Control (mean)	Difference	t-stat	p-value
log(Employees)	3.633	3.47	0.164	1.635	0.103
ROA	0.606	0.504	0.103	1.332	0.184

Appendix Table 4
Covariate balance under the Boucly, Sraer, and Thesmar (2011) matching procedure with the ±50% employees / ±50% ROA calipers used in their original specification. The Difference column reports LBO Target minus Control.

	LBO Target (mean)	Control (mean)	Difference	t-stat	p-value
log(Employees)	3.781	3.6	0.181	1.83	0.068
ROA	0.591	0.51	0.08	1.074	0.284

Appendix Table 5
Industry distribution of the matched LBO sample (212 deals, 2000–2020). Industries are classified by 2-digit SNI 2007 codes. The share is the fraction of deals in each industry; Cumul. is the cumulative share.

Code	Industry	*N*	Share (%)	Cumul. (%)
46	Wholesale trade (excl. motor vehicles)	42	19.8	19.8
62	Computer programming & consultancy	23	10.8	30.6
70	Head offices & management consultancy	22	10.4	41.0
43	Specialised construction activities	14	6.6	47.6
47	Retail trade (excl. motor vehicles)	10	4.7	52.3
28	Manufacture of machinery & equipment n.e.c.	9	4.2	56.5
81	Services to buildings & landscape	7	3.3	59.8
56	Food and beverage service activities	5	2.4	62.2
58	Publishing activities	5	2.4	64.6
10	Manufacture of food products	4	1.9	66.5
25	Manufacture of fabricated metal products	4	1.9	68.4
66	Activities auxiliary to financial services	4	1.9	70.3
71	Architectural & engineering activities	4	1.9	72.2
72	Scientific research and development	4	1.9	74.1
20	Manufacture of chemicals	3	1.4	75.5
23	Manufacture of other non-metallic minerals	3	1.4	76.9
26	Manufacture of computer & electronic products	3	1.4	78.3
27	Manufacture of electrical equipment	3	1.4	79.7
29	Manufacture of motor vehicles & trailers	3	1.4	81.1
45	Wholesale & retail trade of motor vehicles	3	1.4	82.5
52	Warehousing & support for transportation	3	1.4	83.9
64	Financial service activities	3	1.4	85.3
31	Manufacture of furniture	2	0.9	86.2
32	Other manufacturing	2	0.9	87.1
33	Repair & installation of machinery	2	0.9	88.0
42	Civil engineering	2	0.9	88.9
49	Land transport & transport via pipelines	2	0.9	89.8
68	Real estate activities	2	0.9	90.7
74	Other professional & scientific activities	2	0.9	91.6
85	Education	2	0.9	92.5
86	Human health activities	2	0.9	93.4
16	Manufacture of wood & wood products	1	0.5	93.9
17	Manufacture of paper and paper products	1	0.5	94.4
18	Printing & reproduction of recorded media	1	0.5	94.9
22	Manufacture of rubber & plastic products	1	0.5	95.4
24	Manufacture of basic metals	1	0.5	95.9
35	Electricity, gas, steam & air conditioning	1	0.5	96.4
38	Waste collection, treatment & disposal	1	0.5	96.9
41	Construction of buildings	1	0.5	97.4
63	Information service activities	1	0.5	97.9
73	Advertising and market research	1	0.5	98.4
77	Rental and leasing activities	1	0.5	98.9
78	Employment activities	1	0.5	99.4
87	Residential care activities	1	0.5	99.9

Appendix Table 6

The table reports the POST LBO coefficients across three clustering assumptions: Firm, Firm x Post, and within each matched pair. Coefficient estimates are identical across specifications, and only standard errors differ. The primary specification clusters standard errors at the firm level to account for serial correlation of residuals within firms over time.

	ROA	log(EBITDA)	log(Empl)	log(Sales)	log(FA+WC)	Leverage	log(CAPEX)
Firm	-0.145**	0.032	0.291***	0.251***	0.393***	0.028**	0.438***
	(0.061)	(0.079)	(0.049)	(0.07)	(0.076)	(0.014)	(0.109)
Firm × POST	-0.145***	0.032	0.291***	0.251***	0.393***	0.028***	0.438***
	(0.043)	(0.058)	(0.035)	(0.051)	(0.055)	(0.01)	(0.079)
Pair	-0.145**	0.032	0.291***	0.251***	0.393***	0.028*	0.438***
	(0.057)	(0.076)	(0.048)	(0.071)	(0.079)	(0.015)	(0.106)

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Appendix Table 7

Sensitivity of the baseline DiD estimates when accounting for pre-LBO sales growth, following of Boucly, Sraer, and Thesmar (2011). The specification adds POST × GR, where GR is the average annual log growth in sales over the three years preceding the transaction ($t = -3$ to $t = -1$). All specifications include firm and year fixed effects. Standard errors are clustered at the firm level and reported in parentheses.

	ROA	log(EBITDA)	log(Empl)	log(Sales)	log(FA+WC)	Leverage	log(CAPEX)
POST × LBO	-0.155**	0.026	0.295***	0.243***	0.38***	0.03**	0.431***
	(0.06)	(0.078)	(0.048)	(0.07)	(0.076)	(0.014)	(0.109)
POST × GR	-0.145*	0.237**	0.105	0.061	0.143	0.009	0.255*
	(0.083)	(0.118)	(0.07)	(0.094)	(0.092)	(0.012)	(0.132)
POST	-0.074**	-0.252***	-0.112***	-0.147***	-0.125***	-0.001	-0.132*
	(0.036)	(0.05)	(0.02)	(0.029)	(0.031)	(0.006)	(0.077)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	7,658	6,399	7,469	7,505	7,526	7,621	6,103
Number of deals	212	212	212	212	212	212	212
Overall Adj. R^2	0.39	0.79	0.92	0.90	0.90	0.62	0.72

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Appendix Table 8

Robustness of the baseline DiD estimates under tighter matching calipers (±20% employees / ±20% ROA). The matched sample shrinks to 195 LBO targets and 730 controls. POST × LBO is the difference-in-differences coefficient on the post-treatment interaction; POST is the standalone post-event indicator. All specifications include firm and year fixed effects. Standard errors are clustered at the firm level and reported in parentheses.

	ROA	log(EBITDA)	log(Empl)	log(Sales)	log(FA+WC)	Leverage	log(CAPEX)
POST × LBO	-0.131**	0.026	0.307***	0.277***	0.38***	0.019	0.42***
	(0.063)	(0.085)	(0.052)	(0.076)	(0.08)	(0.014)	(0.117)
POST	-0.107***	-0.205***	-0.098***	-0.149***	-0.106***	-0.002	-0.08
	(0.038)	(0.048)	(0.017)	(0.027)	(0.03)	(0.006)	(0.082)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	6,348	5,232	6,169	6,206	6,233	6,311	4,941
Number of deals	195	195	195	195	195	195	195
Overall Adj. R^2	0.37	0.80	0.92	0.89	0.90	0.63	0.70

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Appendix Table 9

Robustness of the baseline DiD estimates under the Boucly, Sraer, and Thesmar (2011) matching procedure: greedy nearest-neighbour assignment on raw Euclidean distance over log(Employees) and ROA. Panel A applies the procedure with our preferred ±20% employees / ±50% ROA calipers (212 matched targets); Panel B applies the original ±50% / ±50% calipers (233 matched targets). All specifications include firm and year fixed effects. Standard errors are clustered at the firm level and reported in parentheses.

	ROA	log(EBITDA)	log(Empl)	log(Sales)	log(FA+WC)	Leverage	log(CAPEX)
Panel A: Euclidean ±20% employees / ±50% ROA							
POST × LBO	-0.143**	-0.012	0.294***	0.252***	0.408***	0.028*	0.45***
	(0.06)	(0.083)	(0.05)	(0.072)	(0.079)	(0.015)	(0.115)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	5,826	4,714	5,660	5,683	5,734	5,805	4,574
Number of deals	212	212	212	212	212	212	212
Overall Adj. R^2	0.40	0.81	0.93	0.90	0.91	0.61	0.73
Panel B: Euclidean ±50% employees / ±50% ROA							
POST × LBO	-0.143**	-0.012	0.292***	0.252***	0.408***	0.027*	0.443***
	(0.06)	(0.083)	(0.05)	(0.072)	(0.079)	(0.015)	(0.114)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	5,819	4,704	5,653	5,676	5,727	5,798	4,569
Number of deals	233	233	233	233	233	233	233
Overall Adj. R^2	0.40	0.81	0.93	0.90	0.91	0.61	0.73

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Appendix Table 10

Crisis-augmented DiD estimates with 2-digit industry × year fixed effects in place of year fixed effects, alongside firm fixed effects. Identification comes from within-industry-year variation. Specification, sample, and variable definitions are otherwise as in Table 6. Standard errors clustered at the firm × POST level in parentheses.

	ROA	log(EBITDA)	log(Empl)	log(Sales)	log(FA+WC)	Leverage	log(CAPEX)
POST × LBO	-0.162***	0.017	0.300***	0.241***	0.425***	0.020	0.337***
	(0.054)	(0.075)	(0.044)	(0.075)	(0.072)	(0.014)	(0.106)
POST × LBO × CRISES	0.074	-0.010	-0.062	0.015	-0.032	0.016	0.228*
	(0.067)	(0.115)	(0.045)	(0.060)	(0.078)	(0.018)	(0.138)
$\hat{\beta}_1 + \hat{\beta}_2$ (total crisis effect)	-0.088	0.008	0.238	0.256	0.393	0.037	0.565
	(0.067)	(0.124)	(0.055)	(0.059)	(0.103)	(0.019)	(0.147)
POST × CRISES	-0.043	-0.032	0.026	0.048	0.032	0.002	-0.167
	(0.064)	(0.092)	(0.029)	(0.054)	(0.047)	(0.011)	(0.133)
POST	-0.035	-0.098*	-0.101***	-0.117***	-0.106***	0.000	0.023
	(0.038)	(0.051)	(0.018)	(0.028)	(0.033)	(0.007)	(0.088)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry × Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	7,716	6,348	7,510	7,534	7,583	7,677	6,068
Number of deals	212	212	212	212	212	212	212
Overall Adj. R^2	0.40	0.80	0.92	0.89	0.91	0.64	0.72

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

9.2 Appendix B: Ai usage

Claude Code (Anthropic) was used as an assistive tool to support R coding, table formatting and debugging. AI tools, including Claude (Anthropic) and Grammarly were used to assist with the language and grammar of this paper. All intellectual content, including the ideas, arguments, and analysis, was developed entirely by us.