PRIVATE EQUITY AND LONG-TERM INVESTMENT: EVIDENCE FROM SWEDEN

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ABSTRACT

There is a long-lasting debate on whether private equity (PE) firms invest long-term in portfolio firms, or if their short investment horizon lead to long-term growth being sacrificed to boost short-term performance. In this thesis, we examine three types of long-term investments: changes in asset investments, innovative investments and investments in personnel. Using a sample of 176 Swedish PE-backed leveraged buyouts (LBOs) and a carefully constructed control sample we find that acquired firms invest significantly more than controls in physical assets (mainly fixed assets but also capital expenditure) and personnel (employees and wages) during the three years following a buyout. Controlling the observed increases for growth in firm size, we show that while investments in personnel grow in line with firm size, physical assets grow disproportionately more. The post-buyout growth in assets is concentrated among private-to-private and divisional buyouts, i.e. deals in which the seller is an individual or a conglomerate. In contrast, secondary transactions and public-to-private transactions (sellers are PE firms or public companies) show no effect on asset investments. Our results indicate that PE firms help targets alleviate financial constraints and take advantage of unexploited investment opportunities. These results diverge from existing evidence that LBO targets invest less or downsize after being acquired by a PE firm.

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1. Introduction

Leveraged buyouts (LBOs) by private equity (PE) firms have played a central role in corporate finance for more than three decades. Dating back to Jensen (1989), advocates of private equity have reported benefits of LBOs and its use of concentrated equity ownership, strong monitoring and improved discipline stemming from high leverage. More recent empirical studies have documented positive effects of PE ownership on operating performance (see e.g. Cumming, Siegel and Wright, 2007).

Although it is generally accepted that operating performance improves, the sources of these gains remain controversial. Numerous policymakers and influential investors assert that PE firms earn their returns not by creating value in the companies they acquire, but by transferring wealth from other financial claimants to themselves (Harford and Kolasinski, 2013). Correspondingly, PE firms are regularly accused of asset stripping, cost cutting, short-termism and layoffs (e.g. Wright, Gilligan and Amess, 2009; Davis et al., 2014). Critics argue that PE firms' short-term investment horizon promotes the generation of short-term profits and that the prevalence of high leverage in buyout transactions diverts cash away from long-term investments towards servicing debt (Rappaport, 1990). Whilst most studies confirm LBOs' improvements to financial performance, it is not clear whether PE firms create value in their portfolio firms or—as claimed by critics—increase short-term profits at the expense of long-run value.

Whether PE firms invest long-term in their portfolio firms or sacrifice long-term growth to boost short-term performance has big implications for firms that are potential targets of an LBO. Every year, an increasing number of firms become targeted by PE funds (Bain, 2019). We argue that existing literature does fully

not capture the long-term effects of being acquired by a PE firm. Our thesis aims to narrow this gap by shedding light on how these firms are impacted in the long run by their PE owners.

While LBOs have been studied thoroughly, little attention has been given to the effect on long-term investments. A small set of literature has focused on LBOs' effect on innovative activity, arguing that such activity is well-suited to indicate long-term investments (e.g. Lerner, Sorensen and Strömberg, 2011; Amess, Stiebale and Wright, 2015). In this area, several studies have examined the effects of LBOs on research and development (R&D) expenditure (e.g. Long and Ravenscraft, 1993), while others have studied patenting activity (e.g. Amess et al., 2015). Another area of literature has reviewed PE firms' effect on firm growth. Boucly, Sraer and Thesmar (2011) show that target firms grow faster than controls, issue additional debt and increase capital expenditure in the three years following an LBO. A third area of literature has studied the effect of buyouts on employment and wages (e.g. Amess and Wright, 2007).

In this thesis, we take a broader perspective on long-term investments in comparison to existing literature, aiming to understand how buyouts change their "long-term" behavior following an LBO. To capture the long-term focus of an acquired firm, we examine three forms of long-term investments: changes in innovative investments, asset investments and investments in personnel. We use the number of granted patents as a measure of innovation, capital expenditure (capex) and fixed assets as proxies for investments in physical assets and employee count and wages to measure investments in personnel. If critics are correct regarding PE firms prioritizing short-term cost-cutting, we expect to observe a decrease in these forms of investments following an LBO.

Patents have been employed as a measure of innovation activity in several recent studies (e.g. Bena and Li, 2014) and are a widely accepted measure of innovative activity (Lerner et al., 2011). One disadvantage of using patents as an indicator of innovation is that not all inventions become patented. For example, many inventions are protected as trade secrets. We choose, however, to rely on patent count as a measurement, because it is a well-established indicator of innovation (Amess et al., 2015). Further, capex and fixed assets are commonly used in the literature to measure asset investments (e.g. Kaplan, 1989; Olsson and Tåg, 2015). Even though capital expenditure and fixed assets are closely related to each other we use both measures in our estimations. Capex is the most common measure of long-term investments of the two, but fixed assets include financial fixed assets, which are interesting to include as these contain equity holdings in, and loans to, subsidiaries. Further, several studies have focused on PE-backed LBOs and its effects on employment and wages (e.g. Olsson and Tåg, 2015). Although these studies evaluate employment effects from a labor market perspective, we argue that the analyses have properties of and implications for long-term investments. As an extreme example, most firms would struggle to grow over time without personnel. Thus, reducing headcount—typically involved in cost-cutting—could indicate short-termism. Conversely, firms that increase employment following a buyout invest in future growth, which indicates long-term rather than short-term focus. Boucly et al. (2011) estimate the effects of LBOs on employment and find strong growth following a buyout.

For empirical evidence, we look to Sweden. Sweden has an active private equity market that has evolved similarly to other active private equity markets (Olsson and Tåg, 2015). Holding periods and performance of buyouts are relatively similar

between Swedish and international LBOs. Besides, accounting data are available for *all* private and public firms in Sweden.

Using two separate sources of data, we identify 176 Swedish PE-backed buyouts during 1999-2014. We follow these firms and their corporate behavior before and after the deal using accounting data from the Swedish Companies Registration Office (SCRO). We compare the firms' development to similar firms that are not subject to an LBO. Each target firm is matched to one control firm with similar pre-buyout characteristics (e.g. size and profitability), in the same industry group and during the same year as the buyout.

In line with Boucly et al. (2011), we find that LBO targets grow significantly more than comparable non-LBO firms following a buyout, in terms of physical assets and personnel. We find that growth in fixed assets, employment and wages are statistically significant at the 1% level and numerically large. Between one year preceding the buyout (t-1) and three years after (t+3), fixed assets, employment and wages of the targets increase by, respectively, 85%, 33% and 31% relative to control firms. The results are robust to different sampling methods and variable definitions. The other measure we employ for physical assets, capex, is also associated with a large positive but insignificant effect (63%) following a buyout. Although the effect is not significant three years after an LBO, by t+2 the positive coefficient of 91% is significant at the 5% level. Overall, the magnitude of the effects raises some concern, as it could be that the effects are simply driven by target size growing following an LBO. To control for this, we both compare growth in size (total assets) to growth in the outcome variables and run separate regressions in which we normalize the variables by size. First, we find that capex, fixed assets and employees grow disproportionately more than total assets for buyouts. Second, using the normalized variables—filtering out firm

growth—we find that capex and fixed assets are associated with statistically significant post-LBO growth for targets. Conversely, we find no significant effect on employees normalized by size. Further, in contrast to existing studies, we find that LBOs have no significant effect on patent count in our sample. Our estimated patent coefficients suggest a positive impact, although statistically insignificant.

Generally, our results indicate that LBOs are associated with large increases in physical assets and personnel. Whereas growth in personnel seems to be proportionate to and driven by growth in firm size, physical assets grow disproportionately more. This indicates that firms going through an LBO shift towards a long-term focus after the buyout. Although our main results are in line with Boucly et al. (2011), they are in contrast to earlier literature: Kaplan (1989) shows that LBO targets cut down on investment and sell off assets and Davis, Haltiwanger, Jarmin, Lerner and Miranda (2011) find that LBO targets experience employment reduction following an LBO. We also find that targets issue additional debt following the deal to finance asset growth: leverage increases by 3.6 percentage points between one year before and three years following a deal. The magnitude is similar to the 2.6 percentage point growth found by Boucly et al. (2011). The result is significant at the 5% confidence level and robust to different sampling methods.

The observed effects are dependent on targets' pre-transaction ownership structure. The post-buyout increase in physical assets is concentrated among private-to-private and divisional transactions. Private-to-private transactions are deals in which the seller is an individual (typically a family business) and divisional buyouts are divested by large conglomerates. While the effect of private-to-private LBOs on fixed assets is significant at the 1% level for all three years, the positive treatment effect for capex is significant only for t+1 and t+2.

The treatment effect for divisional buyouts are statistically significant with both asset measures. In contrast, secondary transactions (where the seller is a private equity firm) and public-to-private transactions (where the target is publicly listed) show no effect on investments in assets. These results are partly intuitive. Private-to-private buyouts are more likely to be credit-constrained prior to the transaction than publicly held firms and firms that have already been owned by a PE firm, as those firms are relatively small and reliant on internal finance (Carpenter and Petersen, 2002; Amess et al., 2015). Our interpretation of the results for private-to-private deals is that PE firms help alleviate financial constraints and take advantage of unexploited investment opportunities. What is less intuitive is that divisional buyouts also lead to increases in physical assets, in contrast to what e.g. Boucly et al. (2011) find. We argue that one explanation could be differences in samples. Another, more intuitive explanation is that divisions suffer from underinvestment due to capital misallocation within internal capital markets and that PE firms can reduce such underinvestment by facilitating access to external finance (Amess et al., 2015). Further, our estimates indicate a positive effect on employment and wages for private-to-private, divisional and secondary deals. Only public-to-private deals, which involve the largest firms in our sample, see no significant effect on personnel after a buyout.

This thesis presents evidence that Swedish LBOs experience strong growth in physical assets and employment following a buyout, in particular for firms that are likely to be financially constrained before the deal. Our interpretation is that PE firms help targets alleviate financial constraints and take advantage of unexploited investment opportunities. These results diverge from existing evidence that LBO targets invest less or downsize after being acquired by a private equity firm.

2. Literature review

Previous research has generally studied the relationship between LBOs and acquired firms' performance from two perspectives. The first perspective has focused on acquired firms' financial performance while the second perspective has studied non-financial measures of productivity. Although LBOs traditionally have been associated with short-termism and cost-cutting behavior, recent literature establishes a positive effect on firm growth for firms that are acquired by PE firms (see e.g. Cumming et al., 2007 for a review). Improvements are attributed to a combination of concentrated ownership, strong managerial incentives and active monitoring, leading to higher firm efficiency (Jensen, 1989).

Less attention has been given to the effect of LBOs on firms' long-term investments. A number of studies have focused on LBOs' effect on innovative activity, arguing that such activity is well-suited to indicate long-run investments. While costs for innovative activities commonly occur immediately, the benefits are not likely to be observed for numerous years (Lerner et al., 2011). In this area, a number of studies have examined the effects on R&D expenditure. Long and Ravenscraft (1993) study the impact on R&D for firms following an LBO and find a 40% drop in R&D intensity. Conversely, Bruining and Wright (2002) find that management buyouts are followed by large increases in new product development. Further, several recent studies have examined LBOs' effect on patenting activity, which is a widely accepted measure of innovation (e.g. Lerner et al., 2011; Ughetto, 2010). In contrast to R&D expenditure, patents reflect the quality and extent of a firm's innovation. Studying patents, Lerner et al. (2011) find no evidence that LBOs sacrifice long-term investments—conversely, target firms' patents are more cited after a buyout. Similarly, studying UK deals, Amess

et al. (2015) find that LBOs have a positive causal effect on patent stock three years after a deal.

Another area of literature has reviewed PE firms' effect on firm growth. Boucly et al. (2011) provide evidence that target firms grow faster than peers, issue additional debt and increase capex in the three years following an LBO. A third area of literature has studied the effect of buyouts on employment and wages. Amess and Wright (2007) find that PE-backed LBOs have no effect on employment, but a negative effect on wage growth. Davis et al. (2011) find that employment declines in target firms by three percent over three years relative to controls. Opler (1992) finds small increases in employment after an LBO.

Several studies on LBOs argue that private equity firms play an important role in relaxing portfolio firms' financial constraints and thus relax issues of underinvestment (e.g. Boucly et al., 2011). PE firms are able to mitigate capital market imperfections, leading to increases in investments following an LBO (Engel and Stiebale, 2014). Boucly et al. (2011) recognize that PE firms facilitate access to finance through financial know-how and improved corporate governance. Private equity firms' financial expertise is reassuring to creditors, which increases the likelihood that they will provide funds for investment. Besides, the improved corporate governance associated with buyouts that stems from e.g. active monitoring of senior management and board representation, provides lenders with confidence that funds are used productively. Consequently, PE-owned firms are less likely to suffer from underinvestment than firms not owned by PE funds (Amess et al., 2015).

3. Data and Empirical Strategy

3.1. Data construction

Our data set is constructed from three sources: Thomson Reuters Eikon, Serrano and PAtLink. First, we retrieve transactions from Thomson Reuters Eikon with the following characteristics: (i) the deal is completed between January 1997 and December 2017, (ii) the acquired firm is incorporated in Sweden and (iii) the transaction is classified as a majority investment. We choose acquirers that are classified as "Private Equity", "Closed-End Funds" and "Alternative Financial Investments" in the database, in a first step to remove non-financial buyers. We continue by (i) manually removing all Swedish buyers that are not members of the Swedish Private Equity & Venture Capital Association (SVCA) and classified as "buyout" and (ii) dropping all non-Swedish buyers that are not defined as private equity buyout firms according to their websites.[1] If there is any ambiguity regarding a buyer's classification, we turn to third-party sources (e.g. Bloomberg). At this point, there are 386 deals in the sample. We identify and remove 56 duplicates: these are additional transactions of target firms already in our sample. We improve our coverage by adding transactions from the Capital IQ database and Bergström, Grubb and Jonsson (2007) that were not identified by the Thomson Reuters dataset. This leaves us with a total of 346 deals.

Second, we obtain financial statements from the Serrano database. Serrano contains financial statement data from the Swedish Companies Registration

[1] The Swedish Private Equity & Venture Capital Association is the industry association for the private equity industry in Sweden. Most Swedish PE firms are members, however not all as membership is voluntary. SVCA provides a list of all its members, defining them as 'buyout', 'growth' or 'venture capital'.

Office (SCRO) covering the period 1997-2017. Swedish law requires all limited liability firms to report annual accounts to the SCRO.

One issue arises as our transaction and accounting data do not have the same identifiers. We solve this by manually identifying firm identifiers for the targets from annual reports and company websites. For all targets, we focus on the main entity with the most real activity, rather than on a holding firm that owns various subsidiaries but has no real operations. We control the quality of our methodology by comparing sales and employment figures from annual reports and firm websites. When a buyout does occur at the holding level, all subsidiaries are marked as targets and thus removed from any potential control group. The total number of observations from the Serrano database, containing both treated and non-treated firms, adds to 7,305,552, corresponding to 809,899 unique firm identifiers. We make two adjustments to the raw data set: (i) we drop all observations that are not limited companies, e.g. economic associations and (ii) remove firms with no data on industry (SNI) codes.[2]

One concern at this stage is that we struggle to identify firm identifiers for all LBO targets. Particularly divisional buyouts, i.e. spin-offs from industrial firms, are in some cases not possible to track as the targets may not be independent legal entities before the transaction.[3] After removing the deals that we fail to identify firm identifiers for, 288 transactions remain. We remove all firm-year observations that correspond to financial years that are longer or shorter than 12 months for comparability reasons. We proceed by removing deals in which targets are property portfolios. Following our method, in which buyouts' development is analyzed from one year preceding the buyout to three years after, transactions

[2] The Swedish Standard Industrial Classification (SNI) is based on the industry standard classification system used by the European Union, NACE Rev.2.

[3] See Appendix A for an explanation of how we deal with such cases.

occurring before 1998 and after 2014 are removed. Finally, we drop targets for which critical accounting data, e.g. sales, for the period around a buyout is missing. This leaves us with a total of 176 deals.

The third data source we use is PAtLink, which holds all patents granted to Swedish firms during 1990-2018. The patent data are extracted from PATSTAT, developed by the European Patent Office (EPO) and the Organization for Economic Co-operation and Development (OECD). The data contain information on application numbers, country, filing years and legal firm identifiers.

We merge the three datasets and complement the data by computing measures of financial performance: capex, return on assets (ROA) and leverage. We define capex as the change in property, plant & equipment (PPE) and intangible fixed assets plus depreciation and amortization, ROA as earnings before interest expenses (EBIE) divided by total assets and leverage as debt (interest-bearing liabilities) divided by total assets. It should be noted that the leverage measure uses the accounting information of the target, which is different from the debt raised for the LBO itself. The debt issued for an LBO operation is typically borne by a holding company that owns the target. Consequently, it does not appear in our data. Similar to Boucly et al. (2011), we believe that target debt is, nevertheless, interesting as it informs us on a firm's ability to raise debt after a buyout, beyond what has originally been raised to finance the transaction.

Our final target sample consists of 176 buyout targets ranging from 1999 to 2014, as shown in Table A1 in the appendix. The 176 targets constitute 51% of the PE transactions extracted from Thomson Reuters Eikon, Capital IQ and Bergström et al. (2007). Comparably, Olsson and Tåg (2017) match 61% of the transactions from Capital IQ and Davis et al. (2014) find firm records for 65% of the identified buyouts. The main reason for the lower share in our sample is that

we adjust for the time period—removing all 44 deals from 1997, 2015, 2016 and 2017—at a later stage than comparable studies. Adjusting for the mentioned deals would result in a matching success rate of 58%, in line with the literature.[4]

Table 1 presents pre-buyout descriptive statistics for targets (columns 1 and 4) and the universe of potential control firms (2 and 5). The samples are dissimilar on all variables. Buyouts are larger (in sales, assets and employees) and more profitable (net income and EBIT). The dataset is heavily unbalanced with 176 treated firms and 347,556 potential unique control firms. In our attempt to study LBOs' effect on a set of outcome variables, we want to compare the targets to similar firms that did not go through an LBO. We address the concern regarding the unbalanced data by using a matching method, described in Section 3.2.

Table 1. Descriptive statistics: targets and potential control firms

Variable	Mean			Median		
	Treated	All firms	Controls	Treated	All firms	Controls
Sales	486.13	27.37	471.96	202.50	2.20	208.91
Total assets	389.31	54.01	302.75	127.58	1.83	114.30
Fixed assets	167.98	27.20	133.02	30.77	0.28	21.36
Net income	17.36	1.79	18.96	8.61	0.05	2.90
EBIT	19.10	1.84	25.46	15.01	0.11	7.01
ROA (%)	15.14	6.21	19.74	14.56	7.18	9.73
Leverage (%)	8.70	14.87	8.97	0.00	0.00	0.00
Employees	251.53	9.84	214.15	88.50	2.00	71.00
Age	29.26	16.22	29.07	23.00	13.00	22.00
Observations	176	4 022 846	176	176	4 022 846	176

Notes: (1) This table shows descriptive statistics for all treated firms, the universe of potential control firms and all control firms after matching; (2) Variables for the treated and control firms are measured by one year prior to the transaction (t-1), "all firms" include all firm-year observations of potential control firms; (3) All figures are in million SEK, except for ROA, leverage, employees and age; (4) EBIT is earnings before interest expenses and taxation, ROA is earnings before interest expenses (EBIE) divided by total assets, leverage is interest-bearing liabilities divided by total assets. Other variables are self-explanatory.

[4] In contrast to e.g. Olsson and Tåg (2017) and Davis et al. (2014) we extract deals for a different time period to what we use in our analysis. Initially, we obtain deals for the full period covered by our accounting data (1997-2017). In a later stage, we remove the deals for 1997 and 2015-2017. Conversely, other studies make such adjustments already when extracting the transaction data.

3.1.1. Swedish LBOs in an international context

Figure 1 shows the yearly distribution of LBOs in our sample. Buyout activity grows steadily between 1999-2007, stagnates in 2009 and resumes in 2010. The development of LBOs is similar to the evolution recorded by Olsson and Tåg (2015), in their sample of US, UK and French buyouts.

Our Swedish data are also comparable to existing studies in terms of deal types. A majority of the deals in our sample, 55%, are "private-to-private" transactions.[5] This is slightly above what similar studies find: Both Strömberg (2008) and Amess et al. (2015) record 47%, whereas Boucly et al. (2011) report 52%. Divisional buyouts comprise 32% of our sample, against 31%, 28% and 27% respectively in the mentioned studies. Our share of secondary buyouts, 7%, is lower than the studies, which record 13-16%. Lastly, we record 6% of our deals as public-to-private, similar to Boucly et. al (2011), 4%, and 7% by Strömberg (2008).

The main differences—slightly more private-to-private and divisional transactions and less public-to-private deals in our sample—have several potential explanations. First, our sample of 176 transactions is notably smaller than comparable studies: Amess et al. (2015), Boucly et al. (2011) and Strömberg (2008) study 407, 839 and 21,379 deals respectively. The size differences could impact the distribution of deals. Second, the samples are from different geographies and periods: British LBOs between 1998-2008, French deals during 1994-2004 and global LBOs between 1970-2007. A third reason is related to data collection. As discussed in 3.1, we drop numerous divisional and public-to-private buyouts due to lack of consistent accounting data. Luckily, our concern that we

[5] Following Boucly et al. (2011), we define private-to-private transactions as deals where the seller is an individual, as opposed to divisional buyouts and public-to-private LBOs where the seller is a private or a public firm.

had missed many divisional and public-to-private buyouts did not come true; our sample contains a higher share of such buyouts than similar studies.

The LBO sponsors in our sample are representative of the global universe of private equity firms. Among the 47 sponsors backing the deals in our sample, there are both small local sponsors (e.g. PEQ Invest and Karnell) and large international players (e.g. 3i and CVC). Deals in our sample are mostly local: 65% have a Swedish sponsor and 19% are bought by non-Swedish Scandinavian firms. British deals account for 10% and US deals 4%. Compared to Boucly et al. (2011), our sample holds more local transactions (65% vs. 58%) and less UK and US deals (14% vs. 28%), indicating a more developed local PE market in Sweden compared to in France. Altogether, our Swedish sample appears to be representative of the global LBO activity.

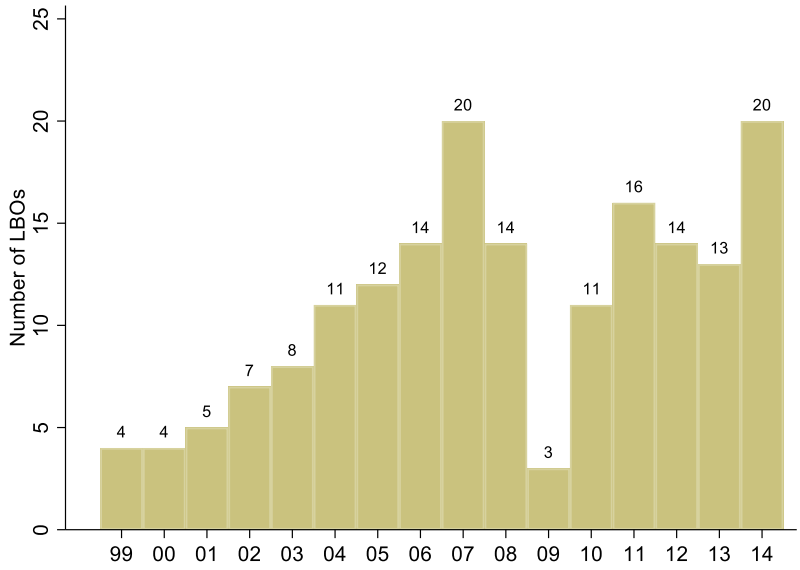

Figure 1. Number of LBOs per year for the sample of 176 LBOs for which accounting data are available. 1999-2014.

3.2. Empirical strategy

3.2.1. Propensity score matching

The main econometric concern for us is that PE firms do not choose targets randomly. Hence, targets are different than other firms in terms of observable and unobservable characteristics. If these characteristics correlate with the variables of our investigation, there might be a risk of selection bias. We attempt to mitigate such bias by relying on a matching method together with difference-in-differences regressions, similar to Smith and Todd (2005). Our comprehensive data set allows us to perform propensity score matching, proposed by Rosenbaum and Rubin (1983). Propensity score matching is especially useful when there is a large number of potential controls, as in our setting (Heckman, Ichimura and Todd, 1997).

The propensity score is, by definition, the conditional probability of assignment to a particular treatment given a vector of observed covariates. We predict the probability (propensity score) of being subject to an LBO, $\Pr(\mathrm{PE}_t = 1|\mathrm{X}_{t\text{-}1})$, with a Probit model. The vector $\mathrm{X}_{t\text{-}1}$ contains pre-treatment firm characteristics.

3.2.2. Estimation strategy

Our empirical strategy aims to identify the causal effect of PE-backed buyouts on a number of performance outcomes. We use propensity scoring to construct the counterfactual and combine this with a difference-in-difference estimator.

In the absence of a randomized experiment, we rely on statistical control strategies to reduce selection bias. Following Amess et al. (2015), we aim to recover the average treatment effect on the treated (ATT) s periods after the treatment period t by comparing the actual performance of a treated firm and a

situation in which the same firm had not been treated. The specification is expressed as:

$$\text{ATT} = \text{E}[\text{Y}^1{}_{t+s}|\text{X}_{t-1}, \text{PE}_t = 1] - \text{E}[\text{Y}^0{}_{t+s}|\text{X}_{t-1}, \text{PE}_t = 1] \quad (1)$$

where $\text{Y}^1{}_{t+s}$ is the performance outcome of a portfolio firm in period $t+s$, measured as number of patents, capital expenditure, fixed assets, employees and wages respectively in our main regressions. Following Olsson and Tåg (2015), we take the log of all variables except for patents to mitigate the impact of outliers. $\text{Y}^0{}_{t+s}$ is the outcome of a buyout if it had not been subject to an LBO, i.e. the counterfactual. X_{t-1} is a vector of control variables and PE_t is a dummy variable that equals one for all firms that were acquired by a PE firm in any respective year.

Our difference-in-differences estimator measures the effect of a firm being subject to an LBO on a set of outcome variables. The estimator is expressed as:

$$\text{E}[\text{Y}^1{}_{t+s} - \text{Y}_{t-1}|\text{X}_{t-1}, \text{PE}_t = 1] - \text{E}[\text{Y}^0{}_{t+s} - \text{Y}_{t-1}|\text{X}_{t-1}, \text{PE}_t = 0]. \quad (2)$$

We obtain the difference-in-differences estimator by applying weighted least squares to the matched data set, using the change in the different outcome variables as dependent variables:

$$\Delta\text{Y}^1{}_{i,t+s} = \alpha + \theta\text{PE}_{it} + \eta_t + \varepsilon_{it}. \quad (3)$$

In this specification, θ is the difference-in-differences estimator of the average treatment effect on the treated, η_t represents year dummies and ε_{it} is the error term. From this specification we analyze heterogeneous effects across characteristics of portfolio firms using the estimation equation:

$$\Delta\text{Y}^1{}_{i,t+s} = \alpha + \theta_0\text{PE}_{it} + \theta_1\text{PE}_{it}Z_{i1t} + \ldots + \theta_K\text{PE}_{it}Z_{iKt} + \eta_t + \varepsilon_{it} \quad (4)$$

where $Z_{iKt,}$ k=1,…K, are the characteristics of portfolio firms.

3.2.3. Outcome variables

The firm-level outcomes we analyze are chosen to indicate outcomes in three forms of long-term investments: investments in physical assets, personnel and innovation.

Physical assets

Capex and fixed assets are used as proxies for investments in physical assets. Although capex is the most common measure of long-term investments of the two, we choose to include fixed assets as it is more well-covered in our data. While fixed assets are available in the accounting data, capex is generally not reported in private companies. Hence, we calculate a proxy with the formula:

$$Capex_t = (PP\&E_t - PP\&E_{t-1}) + (IA_t - IA_{t-1}) + D\&A_t$$

where PP&E is property, plant and equipment, IA is intangible assets and D&A is depreciation and amortization. Ideally, we would calculate capital expenditure as the change in property, plant and equipment (PP&E) adjusted by adding back depreciation, but as the accounting data do not distinguish between depreciation and amortization, we include investments in intangible assets to offset the effect from amortization. One limitation of using this proxy is that the D&A item could include adjustments (impairments or amortizations) to equity holdings in subsidiaries. While this induces a risk of overestimating capex, we argue that it is the most suitable solution. Assets in subsidiaries are generally included in the fixed assets of the parent firm through equity holdings (financial fixed assets) and therefore it is interesting to also include fixed assets as a proxy.

Personnel

Employee count (FTEs) and total wages are used to measure investments in personnel. Both items are available from our accounting data.

Innovation

Patent count is used as a measure of innovative activity. The patent data are available from PaTLink and include all patents belonging to Swedish firms from 1990-2018. Patent grants are organized by firm identifiers.

3.3. Creating the control group

We use propensity score matching to create our control group. The control group is composed of firms that share similar financial characteristics as the buyout firms prior to the transaction. The main steps of the matching are as follows.

We estimate propensity scores for all firms that remain in our sample after the adjustments described in 3.1., using a Probit model. The pre-buyout firm characteristics we match on are firm size (the log of sales, ln_sales), labor productivity (the log of sales per worker, ln_labprod), skill intensity (the log of average wage, ln_av_wage), leverage (interest-bearing liabilities divided by assets), fixed assets (in million SEK), profitability (net income divided by sales, profit_sales) and age (log of firm age, ln_age). Following Amess et al. (2015), we also include industry dummies (SNI, section-level) and time dummies in the Probit model.[6] The choice of variables in our matching procedure is mainly based on previous literature (e.g. Davis et al., 2014; Amess et al., 2015). We also make sure to control for the pre-buyout values of our outcome variables, to ensure that our results are not affected by PE firms choosing targets based on previous outcomes. We include at least one conditioning variable per sort of long-term investment that should be related to the outcomes: fixed assets for assets, labor productivity and average wage for personnel and patent count for innovation. As

[6] The Swedish Standard Industrial Classification (SNI) consists of 21 different industries on the section level, ranging from A-U.

the yearly distribution of granted patents is not expected to be a perfect indication of innovative activity—a firm that has started activities at t is not expected to be granted patents already at t+1—we choose to aggregate the number of patents for the three years before the buyout for all firms. We create a dummy variable that equals one for all firms that have at least one patent during the three years before the buyout and use this as a control for innovation outcome.

Out of the different matching algorithms available, we choose one-to-one nearest neighbor matching without replacement, following Olsson and Tåg (2015). One-to-one matching means that each treated firm is matched with one unique control firm that is statistically similar to the treated firm. We choose a one-to-one matching as the pool of good matches for several firm characteristics is a function of those characteristics. As an example, although company size is a good predictor of the likelihood of undergoing a buyout, the distribution of the variable is likely skewed with many small firms and few large firms. Accordingly, we expect to find several more good matches (control firms with similar propensity scores) for smaller than for larger firms. One limitation of one-to-one matching is that the reduced bias from using one—the most similar—control firm comes at the expense of increased variance (Smith and Todd, 2005). Yet, it is the most common matching procedure in the literature. Further, we match our target firms to control firms in the same industry (SNI, section-level) and during the same year as the buyout. Our extensive dataset of potential control firms allows us to match without replacement—there is no reason to use any control firm more than once. This allows us to improve the precision of our propensity score estimates.

We start with all 176 firms that are subject to PE-backed LBOs during 1999-2014. The pool of potential controls consists of all limited-liability firms in Sweden, after implementing the adjustments mentioned in 3.1. We estimate

propensity scores for a total of 3,096,805 firm-year observations, translating into 349,208 unique firms.

3.4. Assessing the control group

Table 2 presents results for the estimated propensity scores. We perform two different Probit models in which the dependent variable takes the value of one if a firm is subject to a buyout and zero otherwise. The first Probit model is based on a pre-matched sample of 3,096,805 firm-year observations, for which we estimate propensity scores. Column 1 shows that the likelihood of a buyout tends to increase when a firm is relatively large in terms of sales, has lower levels of fixed assets, has higher average wages and lower leverage. Further, having patents increase the likelihood for a firm to be subject of a buyout at a statistically significant level of 1%. In contrast, labor productivity, profitability and firm age do not have a statistically significant impact on a firm being targeted for a buyout. While the results are interesting in themselves, the main purpose of the Probit regression is to generate propensity scores that can be used to match treated firms to similar firms that were not subject of an LBO.

Column 2 of Table 2 presents the results after the one-to-one nearest neighbor matching, which leaves us with a final sample of 176 treated and 176 control firms. Running a Probit regression with the final post-matched sample, we find that no variables are statistically significant and that the pseudo R-squared is considerably smaller than in column 1. The insignificant variables together with the low explanatory power of the model are comforting; they indicate that there are no systematic differences in the distribution of covariates between the treatment and control group (Sianesi, 2004). Our results from the pre- and post-matched Probit regressions are similar to what Olsson and Tåg (2015) find.

Finally, we use a two-sample t-test (proposed by Amess et al., 2015) to examine if there are any significant differences in covariates for the treatment and control group. The differences in means (shown in the appendix, Table B1) are generally smaller and less significant for the matched sample, indicating that the matching procedure has been successful in controlling for pre-LBO firm-specific differences. Similarly, the descriptive statistics displayed in Table 1 confirms that treated and control firms are similar on all variables. Further assessments of the samples are available in Appendix B, in which we observe parallel trends in the covariates.

Table 2. Propensity score estimation, Probit model (Dependent variable = PE)

Variable	Pre-match	Post-match
ln_sales	0.287***	-0.012
	(0.014)	(0.066)
ln_labprod	-0.014	-0.049
	(0.013)	(0.048)
ln_av_wage	0.029***	0.003
	(0.011)	(0.035)
leverage	-0.059***	-0.414
	(0.020)	(0.336)
fixedassets_msek	-0.000***	0.000
	(0.000)	(0.000)
profit_sales	0.000	-0.031
	(0.000)	(0.114)
ln_age	-0.017	-0.005
	(0.027)	(0.085)
dummy_cum_pat_count	0.292***	0.093
	(0.072)	(0.208)
Year Fixed Effects	Yes	Yes
Industry Fixed Effects	Yes	Yes
Observations	3 096 805	352
Pseudo R squared	0.266	0.007

Notes: (1) This table presents results from the pre- and post-matching Probit regressions; (2) Column 1 shows the results before restricting the sample to only targets and matched controls, Column 2 shows the results after matching; (3) labprod is sales divided by employees, av_wage is total wage divided by employees, leverage is interest-bearing liabilities divided by total assets, multiplied by 100, profit_sales is net income divided by sales, multiplied by 100, dummy_cum_pat_count is a dummy variable taking the value of one if a company has been granted any patents during the three years preceding the buyout. Other variables are self-explanatory; (4) Z-values in parentheses; (5) *** p<0.01, ** p<0.05, * p<0.1.

4. Results

4.1. Long-run activity

4.1.1. Physical assets

We start off by assessing the effect of LBOs on assets in Table 3. The regression results in Panel A show the treatment effect on capital expenditure. Capex of a target firm is compared to capex of a similar firm in the same industry and during the same year. We display the effects for up to three years following an LBO, where t is the transaction year. The estimates suggest that capital expenditure increases drastically after an LBO, for all three years. By t+2, capex rises by 91% (65 log points) relative to t-1 for the treatment group, compared to control firms. The increase is statistically significant at the 5% confidence level. The average effect on capex is positive also for t+1 and t+3, although without any statistical significance.[7] The results indicate that buyouts invest more than similar firms that did not go through an LBO. The estimated effects are notably larger than the 24% increase that Boucly et al. (2011) find. While our estimates are larger, it is comforting that we also find positive effects. The magnitude of the estimates raises some concerns, as one explanation to the observed increase could be that target firms simply grow in size following an LBO. We deal with this in Section 4.1.4 in which we control capex and the other outcome variables for firm size.

In Panel B, we estimate positive effects on fixed assets during the observable period. The impact is positive and statistically significant at the 1% level for all years. By the third year following an LBO, fixed assets of buyouts increase by

[7] In Appendix A, we explain why the number of observations in the regressions deviate from the full sample (352 firms). In addition, we explain how this could impact our results.

over 80%. One interpretation of the observed growth in physical assets is that it reflects a switch to a more long-term focus for firms acquired by PE firms.

Further, it is not surprising that Panel A and B follow similar patterns as the accounting items are linked. Fixed assets are related to capex: they grow with capex and fall with depreciation. Hence, the similar trends are comforting.

Table 3. The effect of LBOs on assets

Panel A: Capex	t+1	t+2	t+3
PE	0.331	0.647**	0.487
	(0.250)	(0.253)	(0.307)
Observations	190	206	198
R-squared	0.090	0.093	0.061
Year FE	Yes	Yes	Yes
Panel B: Fixed assets			
PE	0.517***	0.596***	0.619***
	(0.121)	(0.136)	(0.149)
Observations	344	342	342
R-squared	0.087	0.078	0.073
Year FE	Yes	Yes	Yes

Notes: (1) The dependent variable is the change in (logged) capital expenditure in Panel A; (2) in Panel B, the dependent variable is the change in (logged) fixed assets; (3) PE is a dummy variable that equals one after a private equity backed LBO, zero otherwise; (4) Robust standard errors in parentheses; (5) *** p<0.01, ** p<0.05, * p<0.1.

4.1.2. Personnel

Table 4 presents the treatment effect of LBOs on employment and wages. Panel A displays the effect on the number of employees during the three years following a buyout. The coefficients show a positive and significant effect at the 1% confidence level. Column 3 shows that LBOs are associated with employment growth of approximately 33% (28 log points) three years after an LBO. Evidence from the LBO literature is mixed with respect to employment consequences. Boucly et al. (2011) find that the employment growth of LBO targets between the four years preceding the transaction and the four subsequent years is 18% higher than their control firms, generally in line with our estimates. Opler (1992)

also finds small increases in employment after an LBO. Contrarily, Kaplan (1989) and Smith (1990) find that buyouts do not expand employment in line with industry averages. Amess, Girma and Wright (2008) find no employment effects.

In Panel B, we estimate the treatment effect on wage levels and observe significant increases for the treated firms relative to the control group. On average, target firms experience 31% (27 log points) higher wage levels three years after an LBO. The estimates are statistically significant at the 1% level. Empirical evidence of LBOs and wages is less divided than employment studies. While Amess et al. (2007) find that LBOs have significantly lower wage growth than non-LBOs, Bergström et al. (2007) find no effect on wages, studying 69 Swedish buyouts between 1993 and 2005. Similar to our comments on assets, our concern is that the observed employment and wage growth is affected by firms growing in size following an LBO. We control for this in Section 4.1.4.

We note similar trends in employment and wage growth. Employment grows slightly more than wages, which could indicate that buyouts increase headcount rather than wage levels for existing personnel. Amess et al. (2008) argue that estimating wage and employment separately does not fully capture the interdependence of employment and wages; the two are set simultaneously. We try to control for this and separate the two effects in Table D1 in the appendix, in which we estimate effects on average wages. The results indicate that LBOs have weak negative, if any, effect on post-buyout development of average wages. While the negative effect is significant at the 10% level for t+1, coefficients are insignificant for the remaining years. This strengthens the suggestion that PE funds grow firms by adding personnel rather than by increasing existing wages.

Table 4. The effect of LBOs on personnel

Panel A: Employees	t+1	t+2	t+3
PE	0.130***	0.201***	0.280***
	(0.045)	(0.061)	(0.068)
Observations	342	340	338
R-squared	0.069	0.058	0.097
Year FE	Yes	Yes	Yes
Panel B: Wage			
PE	0.119**	0.220***	0.270***
	(0.056)	(0.067)	(0.082)
Observations	348	334	338
R-squared	0.045	0.057	0.061
Year FE	Yes	Yes	Yes

Notes: (1) The dependent variable is the change in (logged) number of employees in Panel A; (2) in Panel B, the dependent variable is the change in (logged) wages; (3) PE is a dummy variable that equals one after a private equity backed LBO, zero otherwise; (4) Robust standard errors in parentheses; (5) *** p<0.01, ** p<0.05, * p<0.1.

4.1.3. Innovation

Table 5 shows the effect of LBOs on the number of patents. Although the coefficients suggest a consistent positive effect for all years, we detect no statistically significant impact on patenting in portfolio firms. The results diverge from existing studies. Amess et al. (2015) find a significant increase in patenting activity for their sample of buyouts in the UK: by year t+3, the patent stock increases by about one-third of a patent. The effect is, however, concentrated among private-to-private transactions. Similarly, both Ughetto (2010) and Lerner et al. (2011) find that innovation increases after LBOs.

There could be various reasons for the insignificant results. One potential reason is that our sample is notably smaller than similar studies. Out of the 176 treated firms in our sample, only 32 were granted any patent during the three years before a buyout. In the control group, patent activity was slightly lower: 28 firms received at least one patent before the buyout. Consequently, only a small share of the firms in our sample appears to be involved in innovative activity.

While the sample size is difficult to deal with, we manage the imbalance between the groups by matching with a forced dummy, in which all 32 treated firms are matched to firms with a history of patent activity. The regression, reported in Table C1 in the appendix, shows similar results: coefficients are positive yet insignificant. In Table C2, we make a similar estimation to the original regression in Table 5 but match on a patent dummy based on the cumulative number of patents up to *ten* years before a buyout, instead of three. The results are virtually unchanged although the number of targets with patents increases to 48 (37 control firms). Another potential explanation to our results follows the empirical evidence that if leverage increases, it could affect innovative activity negatively. The link between leverage and reduced innovation is well-known, even though the direction of causality is unclear (Hall, 1992). We estimate the effect on leverage in Section 4.2. Finally, as discussed in Section 3, there are numerous downsides to using patents as an innovation indicator. For instance, not all inventions become patented. Besides, there should be large variation in the value of patented innovations, which is not accounted for in our estimations.

Table 5. The effect of LBOs on the number of patents

Patent stock	t+1	t+2	t+3
PE	0.926	1.625	2.062
	(0.775)	(1.375)	(1.886)
Observations	352	352	352
R-squared	0.149	0.143	0.142
Year FE	Yes	Yes	Yes

Notes: (1) The dependent variable is the change in the cumulated stock of patents; (2) PE is a dummy variable that equals one after a private equity backed LBO, zero otherwise; (3) Robust standard errors in parentheses; (4) *** p<0.01, ** p<0.05, * p<0.1.

4.1.4. Controls for firm size

The magnitude of the estimated effects raises some concerns. One explanation could be that target firms simply grow in size, in contrast to the interpretation

that our results reflect a shift to a long-term focus for firms after an LBO. Following Kaplan (1989), we control for this concern by normalizing our outcome variables by firm size, to filter out the effect of pure firm growth in our estimates.

First, we estimate the LBO growth in total assets to examine whether targets actually grow more than controls. In Table D2 in the appendix, we find that targets grow significantly more than controls in terms of total assets following a buyout. By t+3, total assets increase by approximately 30% in target firms, relative to controls. The magnitude of the growth in total assets is in line with our estimates on investments in personnel, indicating that such growth is mainly driven by size increasing post-LBO. The growth in size is, however, less than the estimated growth in capex (63%, not significant) and fixed assets (85%). Although firms grow following a buyout, which is expected for PE-backed buyouts, the results indicate that firms grow disproportionately more in terms of physical assets.

In Table 6 we estimate the effect on capex, fixed assets, employment and wages normalized by size. Panel A and B confirm that capex and fixed assets do grow disproportionately more than firm size. The results indicate that PE-owned firms start to invest more in physical assets after a buyout. In the table, coefficients are ratios, i.e. the coefficient of 0.023 in Panel A indicates a growth of two percentage points for capex normalized by total assets. Estimates in Panel C and D confirm that investments in personnel do not grow, relative to firm size.

Table 6. The effect of LBOs on targets' behavior, controlling for firm size

Panel A: Capex / Total assets	t+1	t+2	t+3
PE	0.029**	0.023*	0.023*
	(0.012)	(0.012)	(0.012)
Observations	320	320	320
R-squared	0.073	0.040	0.042
Year FE	Yes	Yes	Yes
Panel B: Fixed assets / Total assets			
	0.042**	0.046**	0.044**
	(0.017)	(0.019)	(0.020)
Observations	350	352	352
R-squared	0.042	0.044	0.037
Year FE	Yes	Yes	Yes
Panel C: Employees / Total assets			
	-0.043	0.022	-0.014
	(0.093)	(0.106)	(0.100)
Observations	348	346	344
R-squared	0.078	0.068	0.076
Year FE	Yes	Yes	Yes
Panel D: Wage / Total assets			
	-0.029	0.015	-0.012
	(0.021)	(0.029)	(0.025)
Observations	352	352	352
R-squared	0.072	0.084	0.085
Year FE	Yes	Yes	Yes

Notes: (1) The dependent variable in Panel A is the change in capex normalized by size, expressed as capex divided by total assets; (2) in Panel B, the dependent variable is the change in fixed assets normalized by size, expressed as fixed assets divided by total assets; (3) in Panel C, the dependent variable is the change in number of employees normalized by size, expressed as number of employees divided by total assets (in msek); (4) in Panel D, the dependent variable is the change in wage normalized by size, expressed as total wage divided by total assets; (5) PE is a dummy variable that equals one after a private equity backed LBO, zero otherwise; (6) Robust standard errors in parentheses; (7) *** p<0.01, ** p<0.05, * p<0.1.

4.2. Leverage

In this section, we estimate the effect of LBOs on leverage. While Rappaport (1990) argues that high leverage diverts cash away from longer-term investments towards servicing debt, Boucly et al. (2011) find that the observed growth in target leverage finances firms' asset growth. Table 7 reports our estimates on target leverage, which is based on the target's accounts only, i.e. it excludes LBO-

related debt. Buyouts are associated with a positive effect on leverage, significant at the 1% level for one year after a buyout and at 5% during the following two years. By t+3, target leverage increases by about 3.6 percentage points. The positive effect is far from surprising given the LBO literature. Our estimates are similar to the 2.6 percentage point growth found by Boucly et al. (2011). The results indicate that, after the deal, targets issue additional debt beyond what was raised to finance the deal. In addition, the growth in leverage could help explain our lack of significant results on patents, as literature has documented a link between higher leverage and reduced innovation (e.g. Hall, 1992).

Table 7. The effect of LBOs on leverage

Leverage	t+1	t+2	t+3
PE	0.037***	0.034**	0.036**
	(0.014)	(0.014)	(0.014)
Observations	352	352	352
R-squared	0.081	0.067	0.043
Year FE	Yes	Yes	Yes

Notes: (1) The dependent variable is the change in leverage; (2) leverage is measured as debt divided by total assets; (3) PE is a dummy variable that equals one after a private equity backed LBO, zero otherwise; (4) Robust standard errors in parentheses; (5) *** p<0.01, ** p<0.05, * p<0.1.

4.3. Financial constraints

Our results from the estimations in Sections 4.1-4.2 are partly in line with previous studies. We find that target firms tend to invest more than control firms in physical assets, but also increase the number of employees and total wage levels following a buyout. Similar to Boucly et al. (2011), we argue that our results could depend on the targets' pre-buyout ownership structures. Boucly et al. (2011) argue that French buyout targets are likely to be credit-constrained firms with growth opportunities and that PE funds alleviate the constraints by helping targets with improved access to different funding solutions. This suggests that we

should expect targets that are financially constrained before an LBO to experience more increase in long-term investments than unconstrained firms. Although the presence of financial constraints is not directly observable, literature suggests that such constraints are linked to ownership structure. Consequently, we split our sample into four transaction types to assess this hypothesis: private-to-private, public-to-private, secondary and divisional transactions.

In Table 8, we present regression results for the four deal types. To reduce the amount of presented information, we report effects for the period t+3 only. First, we find large differences in capex and fixed assets outcomes among the deal types. The positive effect on capex and assets is concentrated among private-to-private and divisional buyouts, whereas secondary and public-to-private buyouts do not have any impact on physical assets; coefficients indicate a negative but insignificant effect. While the effect on fixed assets in private-to-private deals is significant at the 1% confidence level, the positive effect on capex is insignificant. The effect is, however, consistently positive and statistically significant for t+1 and t+2 at the 10% and 1% confidence level respectively. Divisional buyouts lead to statistically significant growth of both capex and fixed assets.

Although the positive effects for private-to-private deals is expected—these firms are typically small family businesses that are likely to be financially constrained prior to a buyout—the growth of divisional buyouts is surprising. The result is also dissimilar to Boucly et al. (2011) who find small but statistically insignificant asset growth. Finance literature suggests that subsidiaries in large group structures benefit from internal capital markets (Hoshi, Kashyap, and Scharfstein, 1991), which implies that divisional buyouts should be less prone to financial constraints. There is, however, doubt regarding the efficiency of such internal markets. As divisional capital budgets depend on not only economics but

also politics, misallocation of resources is a common issue. Large divisions may have the most bargaining power and receive generous capital budgets while small divisions with growth opportunities can be overlooked (Brealey, Myers, Allen and Mohanty, 2018). If this is true and prevalent in our sample, it could explain why divisional and private-to-private buyouts behave similarly in our regressions.

Another potential explanation to our results in comparison to Boucly et al. (2011) could be differences in samples. In Table D3 in the appendix, we present figures of our targets by deal type. As expected, private-to-private deals are the smallest firms in terms of median sales, employees and assets. While Boucly et al. (2011) "bundle" divisional, secondary and public-to-private buyouts together as large firms that are unlikely to be credit-constrained, we notice large in-between differences in pre-buyout characteristics. Public-to-private buyouts stand out as largest on all size indicators (sales, employees, total assets), followed by divisional buyouts and secondary buyouts. Unfortunately, we are unable to compare these figures to Boucly et al. (2011) as they do not report similar statistics. We are, however, able to compare the divisional buyouts qualitatively. While Boucly et al. (2011) describe divisional buyouts as former divisions of large firms or conglomerates, we note that several buyouts in our sample are acquired from small, local investment firms (e.g. CapMan's acquisition of Yrkesakademin, formerly owned by Fagerberg & Dellby). If this distinguishes our sample, it could help explain why divisional buyouts behave differently to Boucly et al. (2011) and more similar to private-to-private deals in our estimations.

Table 8 also shows that private-to-private and divisional LBOs lead to statistically significant increases in employee count and wages. The magnitude is greatest for private-to-private buyouts: the positive effect on employment is 40% for t+3, compared to 33% for buyouts overall. This strengthens the hypothesis

that LBOs alleviate financial constraints in private and family-owned companies. Private-to-private and divisional buyouts behave similarly also for changes in personnel. One further potential explanation to the growth could be that divisional firms need to be more stand-alone following a buyout and e.g. have their own overhead functions instead of relying on conglomerate structures. Finally, we find evidence that employment and wages also increase following a secondary buyout, in line with Boucly et al. (2011). Our interpretation is that targets previously owned by PE firms do not shift towards a more "long-term" focus, but nevertheless continue to grow following a second buyout.

In column 5, we present effects of the different deal types on leverage. The coefficients indicate that the effect on leverage is concentrated among private-to-private and divisional buyouts. Contrarily, secondary and public-to-private deals are not associated with leverage growth: coefficients indicate a negative but insignificant effect. Similar to Boucly et al. (2011), we argue that the absent effect of secondary buyouts on leverage could indicate that the debt raised for the first transaction, along with retained earnings, is enough to meet the capital needed for future long-term investments. This interpretation is strengthened by Table D3 in the appendix, which shows that secondary buyouts on average have the highest leverage of all deal types prior to an LBO. Generally, our results show that when PE firms acquire firms that are likely to be credit-constrained (i.e. family firms and divisional buyouts suffering from internal capital market frictions), they appear to help targets to increase debt levels, which in turn allows targets to increase investments in assets and personnel.

We present a robustness test in Table C5 in the appendix, in which we match control firms to targets based on 45 instead of 21 industries. Results show that private-to-private LBOs are positively associated with all outcome variables,

which strengthens the hypothesis that PE firms alleviate constraints for private firms. The results appear to be less robust for divisional buyouts; only fixed assets grow significantly. Results for public and secondary deals are virtually unchanged.

Table 8. Heterogeneous effects by deal type

Variables	Capex t+3	Fixed assets t+3	Employees t+3	Wages t+3	Leverage t+3
PE x Priv2Priv	0.517	0.629***	0.331***	0.329***	0.038**
	(0.359)	(0.175)	(0.090)	(0.107)	(0.017)
PE x Divisional	0.836**	0.867***	0.163**	0.158*	0.060***
	(0.340)	(0.212)	(0.074)	(0.084)	(0.023)
PE x Secondary	-1.068	-0.126	0.233***	0.283***	-0.024
	(0.869)	(0.331)	(0.076)	(0.083)	(0.060)
PE x Pub2Priv	-0.483	0.084	0.522	0.329	-0.040
	(1.120)	(0.782)	(0.397)	(0.415)	(0.051)
Observations	198	342	338	338	352
R-squared	0.085	0.092	0.107	0.066	0.063
Year FE	Yes	Yes	Yes	Yes	Yes

Notes: (1) The dependent variable is the change in the outcome variables in the respective columns three years following a buyout. All variables except for leverage are logged; (2) PE is a dummy variable that equals one after a private equity backed LBO, zero otherwise; (3) Priv2Priv is a binary variable for private-to-private buyouts, Divisional is a binary variable for divisional buyouts, Secondary is a binary variable for secondary buyouts and Pub2Priv is a binary variable for public-to-private buyouts; (4) Robust standard errors in parentheses; (5) *** p<0.01, ** p<0.05, * p<0.1.

A final source of cross-sectional heterogeneity we explore to test the financial constraints hypothesis is firm size. Finance research suggests that the size of a firm is a reliable indicator of credit constraints (e.g. Fazzari, Petersen, and Hubbard, 1988). Accordingly, we split our sample by pre-buyout target size, defined by total assets. Large targets are target firms that have total assets above the median of all 176 targets one year prior to the transaction; small firms are the rest. Results for t+3 are reported in Table D4 in the appendix. As expected, we find strong and statistically significant evidence that post-LBO growth in capex, fixed assets, employees and wages is higher in small firms compared to large firms. The results are in line with Boucly et al. (2011) who find similar

estimates for employees and capital employed. We interpret the results as a further indication that credit-constrained firms (mainly private and small firms) are likely to experience increases in long-term investments after a buyout.

4.4. Robustness tests

We conduct several robustness tests to assess if our results are robust to different sampling methods and measurement of key variables. These results are displayed in Appendix C.

The robustness tests are based on the following sampling methods: (1) matching with a forced patent dummy, resulting in equal number of targets and controls with patents (2) matching with another patent dummy that is based on the cumulative number of patents for all years up to *ten* years preceding the buyout and (3) matching on a more narrow industry classification, to improve accuracy of industry matches (from SNI section levels to two-digit SNI codes, leading to 45 industries instead of 21). In one final test, to control for the concern that the selection mechanism of private-to-private buyouts differ from other deals, we consider the treatment to be a private-to-private buyout and produce a separate propensity score estimation. The estimations are based on 194 observations (97 targets and controls) as all non-private deals are removed from the sample.

The tests show that our results are robust towards using different sampling methods for the patent variable, using a narrower classification of industries and considering the treatment to be private-to-private buyouts. For instance, LBOs have a positive effect on fixed assets that is statistically significant at the 1% level for all years in all checks. The only deviations we note are in the test for deal types, in which significant effects on several outcomes in divisional buyouts lose significance. The results are, however, robust for private-to-private deals.

5. Additional discussion

5.1. General discussion

Our results indicate that private equity funds help privately owned firms to grow out of financial constraints, allowing them to shift towards a more long-term focus. These results have important implications for the growing number of private firms that become approached by PE firms every year. Although interest from a private equity firm probably is appreciated by most entrepreneurs, it is difficult for these individuals to comprehend the long-term consequences of becoming acquired. Our results indicate that PE firms act as a long-term partner for privately owned and small firms. This is in line with how several PE firms present themselves; for instance, the largest Swedish PE firm EQT states on its website that "…EQT helps the acquired companies grow and prosper, both under EQT's ownership and with future owners". We believe that this thesis provides a comprehensive and clear picture of the long-term consequences for firms that are acquired by PE firms.

One interesting aspect of our findings is that they contrast with the negative public view of PE firms, that portray them as cost-cutting, short-term and risk-seeking owners. This raises the question whether opponents have reason for their criticism. One explanation of the negative public view could be that it is based on early PE activity from the 1980's and 1990's. Most of the literature from this period argues that LBOs' main source of value creation is cost reductions (Boucly et al., 2011). While the 1980's was a period of intense corporate restructuring, resulting in financial pressure to implement cost cutting (Jensen, 1993), Strömberg (2008) argues that the business model of the PE industry may have changed since. We hope that this thesis can complement the early literature. Our

results suggest that PE firms, contrarily to the criticism, play an important part in the Swedish economy, allowing firms previously unable to invest long-term to make such investments.

There are several areas of buyouts that we have not investigated in this thesis. While our results are in contrast with critics, we have not focused on negative consequences of LBOs. Even if increased focus on long-term investments in private firms is favorable for many stakeholders, there could also be negative consequences that we have not addressed in this thesis. One concern is the use of leverage. The high debt levels used to finance PE transactions could lead to several unfavorable outcomes: higher risk, intensified financial constraints and possibly even distressed portfolio companies. Although this is not part of the scope of the thesis, it could have important implications for PE-owned firms.

5.2. Limitations and areas for future research

There are several limitations associated with this thesis that should be mentioned. One set of limitations could affect our estimates of innovative performance whereas other limitations could help explain the large magnitude of our estimates. A third set of limitations is related to the methodology used in this thesis.

First, existing literature suggests that the propensity to patent differs widely between sectors, for example due to the relatively more high-tech nature of certain industries (Ughetto, 2010). Out of the 176 target firms in our sample, only 32 (48) were granted any patent during the three (10) years preceding the buyout. This suggests that PE funds in our sample acquire firms in industries with low propensities to patent. In the light of this interpretation, it is particularly interesting that the estimates from the Probit regression in Table 2 show that having patents increases the likelihood for a firm to become acquired by a PE

firm. If PE firms do select firms based on previous patent activity, one explanation for the unobserved growth in patents could be that most patents are filed before the LBO. Second, measuring innovative effort is inherently difficult and patents are only one of several indicators of innovation. Although patent count is a widely used indicator of innovation, it has well-known limitations, as discussed in Section 3. Third, most of the LBO literature studying innovation employs more than one output measure—typically a combination of granted patents, patent applications, patent citations and R&D expenses—to better capture innovative activity. Due to data availability we are unable to employ other measures than granted patents, which is a clear limitation. Fourth, it is common in the literature to restrict buyout samples to firms with a history of patenting around the time of the buyout (e.g. Amess et al., 2015). We are, however, unable to follow such a methodology because our sample of target firms active in patenting is small.

Further, one limitation that could explain the magnitude of our estimates is the "buy-and-build" strategy commonly employed by PE firms. The strategy involves acquiring a platform company and making subsequent add-on acquisitions to that platform. As some of the targets in our sample are holding companies, they appear as consolidated financial accounts. If this strategy is employed by PE firms in our sample, a part of the large observed growth could be acquired growth. Our data do not allow us to determine whether the observed growth arises from organic growth or from acquiring companies. If targets in our sample grow more than controls due to acquired growth, the interpretations of our results should be different. There may also be limitations to our data collection process since it is difficult to know if we capture the "correct" financials of firms within group structures. If there are subsidiaries within a group structure that e.g. hold more assets and employees than their respective parent company,

it is necessary for us to include the most operative firm in the structure. Otherwise, if an LBO is followed by a portfolio simplification that leads to a consolidation of assets and employees, we could overestimate the difference-in-differences estimators. We try to mitigate this issue by manually checking all targets' financials and their group structures to ensure that we capture the most operative firm. Another concern is the limited reliability of certain firms' reported financial performance. Small, privately held firms could have incentives to underreport earrings to avoid income tax. If underreporting stops after a buyout, earnings should increase as a consequence. Manipulation of accounting items is a possible source of bias that could affect the magnitude of our observed treatment effects.

A final limitation of our study is related to our matching method and the notion that LBOs are not exogenous events. We are unable to formally distinguish whether PE firms cause the observed effects in our estimations, or if they simply target firms based on certain unobserved characteristics that have an impact on the very outcomes. For example, PE funds could target firms that are on the verge to expand, which should impact the interpretation of our results. Although we control for observable pre-buyout characteristics, we are not able to fully resolve the causation problem.

These limitations act as a good foundation for future research. First, our concern regarding the sample size could be mitigated by either broadening the scope (e.g. include patent applications or citations), expanding the time-period (start from the late 1980s when the first PE firms established in Sweden) or extend the geography of the study. It would be interesting to make a similar study on buyouts in Scandinavia. As PE markets are relatively developed in Scandinavia—especially in Denmark, Finland and Norway—and similar to

Sweden, such research should be feasible. Further, a more comprehensive study ranging over a longer time period could help understand if (and how) long-term focus has changed over time. Do PE firms invest differently today than twenty years ago? Have PE firms' focus changed? Also, how is long-term focus and performance connected? These are all interesting questions to deal with in future research.

6. Conclusion

Private equity critics argue that PE funds' short investment horizon promotes the generation of short-term profits at the expense of long-term growth. In this thesis, we study changes following PE-backed leveraged buyouts in portfolio firms' long-term investments. We examine three sorts of long-term investments: asset investments, innovative investments and investments in personnel. We use a sample of 176 Swedish PE-backed leveraged buyouts during 1999-2014 and compare the changes in long-term behavior to a carefully constructed control sample. Our estimates show that Swedish LBO targets invest significantly more than controls in physical assets (mainly fixed assets, but also capital expenditure) and personnel (employees and wages) during the three years following a buyout. Between one year preceding the buyout and three years after, fixed assets, capex, employees and wages of the targets increase by, respectively, 85%, 63%, 33% and 31% compared to control firms. While estimated treatment effects on fixed assets, employees and wages are statistically significant for all three years after a buyout, the effect on capex is not.

Overall, the magnitude of the estimated effects raises some concern, as it could be that effects are simply driven by target size growing following an LBO. Controlling for firm size, we find that acquired firms are associated with post-LBO growth in fixed assets and capex, statistically significant at the 5% and 10% level respectively. Conversely, we find no significant effect on employment normalized by size. This indicates that while personnel growth is mainly driven by increase in size, physical assets grow disproportionately more than size. Further, in contrast to existing studies, we find that LBOs have no statistically significant effect on patent count in our sample.

We also show that the observed effects are dependent on targets' pre-transaction ownership structure. The post-buyout growth in physical assets is concentrated among private-to-private and divisional buyouts, i.e. deals in which the seller is an individual or a large conglomerate. In contrast, secondary transactions and public-to-private transactions (sellers are PE firms or public companies) show no effect on investments in assets. These results are partly intuitive. Private-to-private buyouts are more likely to be credit-constrained prior to the transaction than publicly held firms and firms that have already been owned by a PE firm, as those firms are relatively small and reliant on internal finance. What is less intuitive is that divisional buyouts also lead to increases in physical assets. One explanation is that divisions suffer from underinvestment due to capital misallocation within internal capital markets and that PE firms can reduce such underinvestment by facilitating access to external finance.

This thesis presents evidence that Swedish LBOs experience strong growth in physical assets and employees following a buyout, in particular for firms that are likely to be financially constrained before the deal. Our interpretation is that PE firms help targets alleviate financial constraints and take advantage of unexploited investment opportunities. These results diverge from existing evidence that LBO targets invest less or downsize after being acquired by a private equity firm.

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Appendix

A. Data

Table A1. Data construction

	# firms	Comment
Thomson Reuters Eikon database	**672**	Acquisition of Swedish firms during 1997-2017. Search criteria: "Private Equity", "Closed End Funds" and "Alternative Financial Investments".
Non-PE transactions	-286	Exclusion of e.g. venture capital and investment companies.
Duplicates	-56	Removal of duplicates from the Thomson Reuters data.
Bergström et al. (2007)	11	Adding transactions from Bergström et al. (2007) not present in our sample.
Capital IQ	5	Adding transactions from Capital IQ not present in our sample.
Total PE deals	**346**	
Missing accounting data	-58	For some transactions, it is not possible to find accounting data for various reasons e.g. unable to find firm identifiers.
Inconsistent financial periods	-30	Removal of firms that report one or more financial years that are longer or shorter than 12 months, for comparability reasons.
Properties	-9	Exclusion of deals involving property portfolios.
Before 1998 & after 2014	-44	Exclusion of deals occurring before 1998/after 2014 as accounting data available 1997-2017 and analysis based on data from 1 year before LBO until 3 years after.
Incomplete data	-29	Removal of firms without consecutive data from the year before the deal (t-1) to three years after (t+3).
Final sample	**176**	**Final sample used in the analysis.**

Target identification

Divisional buyouts, i.e. spin-offs from industrial firms, are sometimes not possible to track as the targets may not be independent legal entities before the transaction (but just a division of the selling firm). Although we manage to find identifiers for most divisional buyouts, we have to drop a significant share. Our approach is as follows: we try to (manually) find firm identifiers for the buyouts by searching for the firms in our accounting data (via Retriever Business). For firms that we (initially) fail to find firm identifiers for, we look at the subsidiaries within the selling group structure. If we find a good match, we compare the subsidiary's financials (sales and employees) with the acquiring PE firm's website. If the financials match, we use the subsidiary.

Similarly, it is not possible to identify historical financial data from targets that were integrated into new group structures at the time of investment. Lacking a good solution, we are forced to drop this type of deal.

Number of observations

Although the sample used in this thesis consists of 352 observations, corresponding to 176 LBOs and 176 control firms, the number of observations is lower in several regressions. The deviations occur as some of the variables that we log contain negative and zero values. As it is not possible to log negative and zero values, those observations become omitted. This also explains why observations differ between years within panels. As an example, the number of observations on capex are different between the three years post-LBO since the prevalence of negative and zero values for capex changes over time. This could also explain why significance levels on capex is not robust to different time periods—a drop in observations could reduce the likelihood of estimating significant results. Further, as we examine the treatment effects pairwise, observations are omitted if their assigned pair has negative or zero values of capex during that year.

Although we acknowledge that using variables that has different number of observations for different years could affect our results, we choose not to remove such observations as it might introduce selection bias if the occurrence of non-positive values is not randomly distributed.

B. Additional assessments of the control group

Table B1. Balancing property

		Mean		t-test	
Variable	Sample	Treated	Control	t	p-value
pscore	U	0.002	0.000	48.90	0.000
	M	0.002	0.002	-0.01	0.990
ln_sales	U	19.184	14.765	32.02	0.000
	M	19.184	19.194	-0.07	0.947
ln_labprod	U	14.607	11.666	7.66	0.000
	M	14.607	14.765	-0.90	0.370
ln_av_wage	U	12.280	8.927	8.13	0.000
	M	12.280	12.310	-0.11	0.910
leverage	U	0.573	0.685	-0.16	0.872
	M	0.573	0.599	-1.11	0.269
fixedassets_msek	U	167.980	28.023	1.88	0.060
	M	167.980	133.020	0.72	0.474
profit_sales	U	0.079	0.050	0.00	0.999
	M	0.079	0.059	0.25	0.802
ln_age	U	3.032	2.393	9.48	0.000
	M	3.032	3.030	0.03	0.979
dummy_cum_pat_count	U	0.182	0.008	26.26	0.000
	M	0.182	0.159	0.57	0.572

Notes: (1) This table presents two-sample t-tests of covariates used in Probit regressions; (2) In this table U represents the unmatched sample and M represents the matched sample.

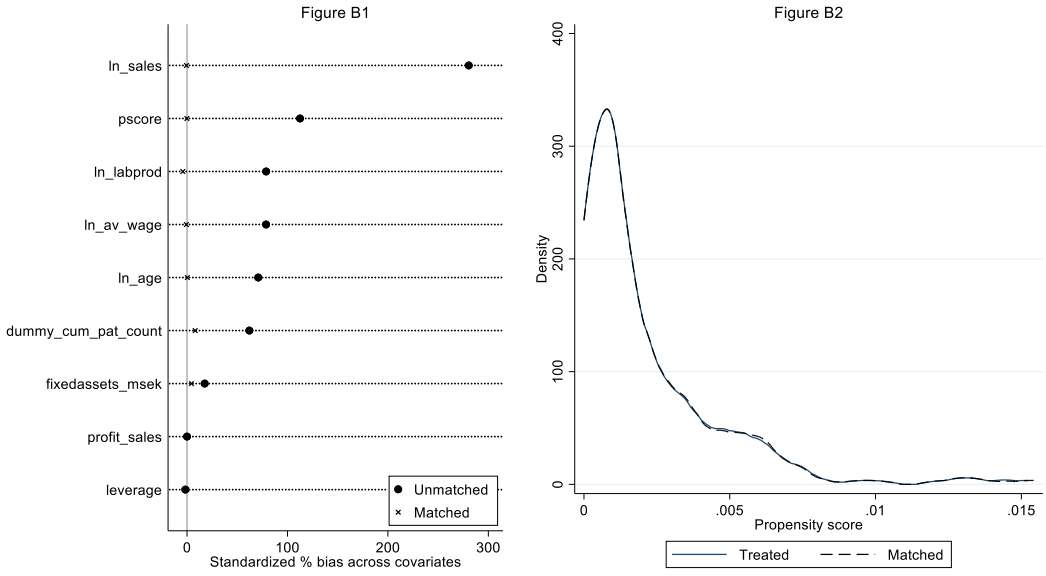

Figure B1. Visualization of balancing property result.

Figure B2. Propensity score distribution: treated and control firms.

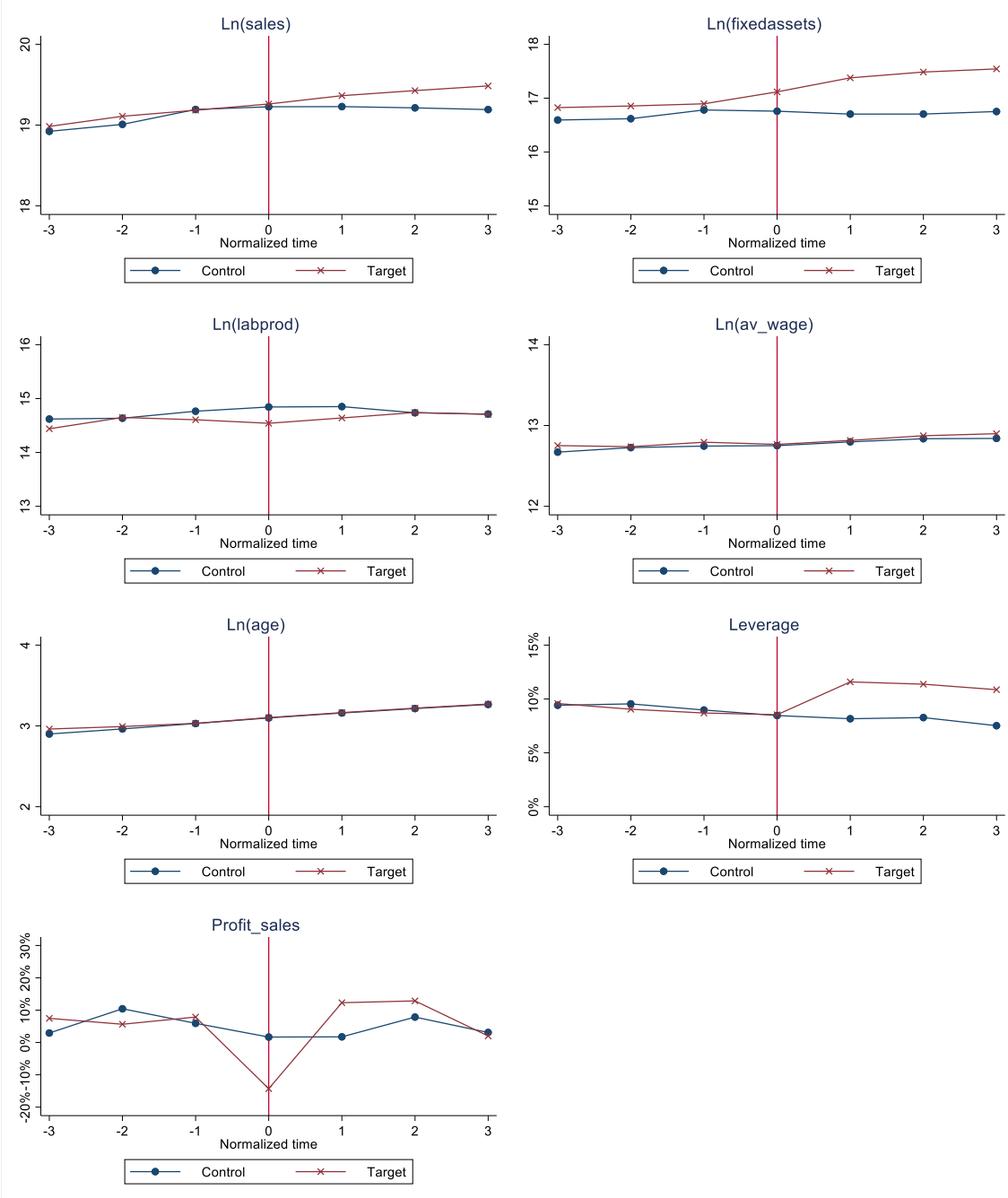

Figure B3. Trends in firm-level variables. The figure displays the pre- and post-treatment trends in normalized average firm-level variables for treated and control firms. Variable description is available in Table 2. The buyout is announced at $t = 0$.

C. Robustness tests

Table C1. Robustness test with forced patent dummy

Panel A: Capex	t+1	t+2	t+3
PE	0.450*	0.664***	0.474
	(0.245)	(0.243)	(0.302)
Observations	194	210	198
R-squared	0.094	0.095	0.062
Panel B: Fixed assets			
PE	0.462***	0.588***	0.617***
	(0.118)	(0.136)	(0.150)
Observations	344	342	342
R-squared	0.074	0.075	0.071
Panel C: Employees			
PE	0.154***	0.237***	0.336***
	(0.049)	(0.060)	(0.072)
Observations	344	346	340
R-squared	0.102	0.098	0.119
Panel D: Wage			
PE	0.105**	0.215***	0.321***
	(0.047)	(0.062)	(0.088)
Observations	348	338	340
R-squared	0.088	0.084	0.075
Panel E: Patent stock			
PE	0.153	0.199	0.091
	(1.006)	(1.770)	(2.469)
Observations	352	352	352
R-squared	0.093	0.089	0.087
Panel F: Leverage			
PE	0.031**	0.033**	0.033**
	(0.014)	(0.014)	(0.015)
Observations	352	352	352
R-squared	0.062	0.052	0.040

Notes: (1) The dependent variable is the change in (logged) capital expenditure in Panel A, the change in (logged) fixed assets in Panel B, (logged) employees in Panel C, (logged) wages in Panel D, number of patents in Panel E; leverage in Panel F; (2) PE is a dummy variable that equals one after a private equity backed LBO, zero otherwise; (3) In this specification, the sample is matched with a forced patent dummy; (4) All regressions include year fixed effects; (5) Robust standard errors in parentheses; (6) *** p<0.01, ** p<0.05, * p<0.1.

Table C2. Robustness test with patent dummy based on 10 years

Panel A: Capex	t+1	t+2	t+3
PE	0.469**	0.316	0.495*
	(0.237)	(0.232)	(0.257)
Observations	192	194	198
R-squared	0.119	0.055	0.108
Panel B: Fixed assets			
PE	0.407***	0.444***	0.624***
	(0.117)	(0.127)	(0.148)
Observations	344	338	342
R-squared	0.088	0.072	0.108
Panel C: Employees			
PE	0.130***	0.209***	0.301***
	(0.046)	(0.057)	(0.062)
Observations	336	334	330
R-squared	0.090	0.091	0.184
Panel D: Wage			
PE	0.079*	0.207***	0.272***
	(0.047)	(0.060)	(0.078)
Observations	338	326	324
R-squared	0.085	0.084	0.102
Panel E: Patent stock			
PE	0.994	1.835	2.528
	(0.767)	(1.339)	(1.813)
Observations	352	352	352
R-squared	0.179	0.163	0.170
Panel F: Leverage			
PE	0.018	0.026*	0.024
	(0.014)	(0.015)	(0.015)
Observations	352	352	352
R-squared	0.063	0.066	0.047

Notes: (1) The dependent variable is the change in (logged) capital expenditure in Panel A, the change in (logged) fixed assets in Panel B, (logged) employees in Panel C, (logged) wages in Panel D, number of patents in Panel E; leverage in Panel F; (2) PE is a dummy variable that equals one after a private equity backed LBO, zero otherwise; (3) In this specification, the sample is matched with a patent dummy taking the value of one if a firm has any patents during the 10 years prior to a buyout; (4) All regressions include year fixed effects; (5) Robust standard errors in parentheses; (6) *** p<0.01, ** p<0.05, * p<0.1.

Table C3. Robustness test with narrow industry groups

Panel A: Capex	t+1	t+2	t+3
PE	0.491**	0.641***	0.491**
	(0.226)	(0.238)	(0.248)
Observations	204	204	186
R-squared	0.094	0.100	0.137
Panel B: Fixed assets			
PE	0.405***	0.390***	0.544***
	(0.127)	(0.125)	(0.141)
Observations	338	336	338
R-squared	0.082	0.078	0.088
Panel C: Employees			
PE	0.107**	0.137***	0.231***
	(0.043)	(0.053)	(0.059)
Observations	336	336	334
R-squared	0.069	0.060	0.111
Panel D: Wage			
PE	0.092**	0.116**	0.206***
	(0.046)	(0.055)	(0.060)
Observations	338	326	322
R-squared	0.055	0.039	0.088
Panel E: Patent stock			
PE	0.210	0.773	1.364
	(0.983)	(1.559)	(2.028)
Observations	352	352	352
R-squared	0.108	0.118	0.123
Panel F: Leverage			
PE	0.036**	0.041***	0.035**
	(0.015)	(0.015)	(0.016)
Observations	352	352	352
R-squared	0.074	0.067	0.037

Notes: (1) The dependent variable is the change in (logged) capital expenditure in Panel A, the change in (logged) fixed assets in Panel B, (logged) employees in Panel C, (logged) wages in Panel D, number of patents in Panel E; leverage in Panel F; (2) PE is a dummy variable that equals one after a private equity backed LBO, zero otherwise; (3) In this specification, the sample is matched on a 2-digit industry code (SNI2007), resulting in 45 industries; (4) All regressions include year fixed effects; (5) Robust standard errors in parentheses; (6) *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table C4. Robustness test with private-to-private deals as treatment

Panel A: Capex	t+1	t+2	t+3
PE	0.457	0.618*	0.676*
	(0.346)	(0.314)	(0.342)
Observations	106	100	102
R-squared	0.088	0.131	0.186
Panel B: Fixed assets			
PE	0.399***	0.476***	0.575***
	(0.153)	(0.170)	(0.192)
Observations	190	186	190
R-squared	0.089	0.112	0.133
Panel C: Employees			
PE	0.187***	0.321***	0.385***
	(0.069)	(0.093)	(0.099)
Observations	188	192	188
R-squared	0.099	0.112	0.124
Panel D: Wage			
PE	0.155**	0.313***	0.309**
	(0.070)	(0.103)	(0.121)
Observations	190	188	184
R-squared	0.084	0.109	0.080
Panel E: Patent stock			
PE	0.124	0.247	0.577
	(0.237)	(0.337)	(0.448)
Observations	194	194	194
R-squared	0.182	0.211	0.211
Panel F: Leverage			
PE	0.020	0.032*	0.031*
	(0.015)	(0.016)	(0.017)
Observations	194	194	194
R-squared	0.051	0.090	0.090

Notes: (1) The dependent variable is the change in (logged) capital expenditure in Panel A, the change in (logged) fixed assets in Panel B, (logged) employees in Panel C, (logged) wages in Panel D, number of patents in Panel E; leverage in Panel F; (2) PE is a dummy variable that equals one after a private equity backed LBO, zero otherwise; (3) In this specification, only private-to-private deals are included; (4) All regressions include year fixed effects; (5) Robust standard errors in parentheses; (6) *** p<0.01, ** p<0.05, * p<0.1.

Table C5. Robustness test: heterogenous deal types with narrow sector groups

Variables	Capex t+3	Fixed assets t+3	Employees t+3	Wage t+3	Leverage t+3
PE x Priv2Priv	0.752**	0.540***	0.296***	0.296***	0.036*
	(0.293)	(0.165)	(0.076)	(0.077)	(0.019)
PE x Divisional	0.516	0.819***	0.092	0.071	-0.030
	(0.339)	(0.205)	(0.063)	(0.065)	(0.060)
PE x Secondary	-1.087	-0.250	0.152**	0.228***	0.058**
	(0.739)	(0.336)	(0.067)	(0.082)	(0.023)
PE x Pub2Priv	-0.194	0.013	0.476	0.113	-0.026
	(0.886)	(0.799)	(0.369)	(0.378)	(0.052)
Observations	186	338	334	322	352
R-squared	0.175	0.112	0.130	0.105	0.052
Year FE	Yes	Yes	Yes	Yes	Yes

Notes: (1) The dependent variable is the change in the outcome variables in the respective columns three years following a buyout. All variables except for leverage are logged; (2) PE is a dummy variable that equals one after a private equity backed LBO, zero otherwise; (3) Priv2Priv is a binary variable for private-to-private deals, Pub2Priv is a binary variable for public-to-private deals, Secondary is a binary variable for secondary buyouts and Divisional is a binary variable for divisional buyouts; (4) In this specification, the sample is matched on a 2-digit industry code (SNI2007), resulting in 45 industries; (5) Robust standard errors in parentheses; (6) *** p<0.01, ** p<0.05, * p<0.1.

D. Additional tables

Table D1. The effect of LBOs on average wage

Average wage	t+1	t+2	t+3
PE	-0.040*	-0.009	0.012
	(0.021)	(0.031)	(0.020)
Observations	342	332	332
R-squared	0.078	0.048	0.060
Year FE	Yes	Yes	Yes

Notes: (1) The dependent variable is the change (logged) average wage; (2) PE is a dummy variable that equals one after a private equity backed LBO, zero otherwise; (3) Robust standard errors in parentheses; (4) *** p<0.01, ** p<0.05, * p<0.1.

Table D2. The effect of LBOs on total assets

Total assets	t+1	t+2	t+3
PE	0.163***	0.179***	0.278***
	(0.047)	(0.059)	(0.075)
Observations	352	352	352
R-squared	0.085	0.085	0.074
Year FE	Yes	Yes	Yes

Notes: (1) The dependent variable is the change in (logged) total assets; (2) PE is a dummy variable that equals one after a private equity backed LBO, zero otherwise; (3) Robust standard errors in parentheses; (4) *** p<0.01, ** p<0.05, * p<0.1.

Table D3. Descriptive statistics by deal types

PE x Priv2Priv	Variables	N	Mean	Median	S.D.	Q1	Q4
	Sales (msek)	97	293.12	154.24	459.91	85.97	274.08
	Total assets (msek)	97	190.76	89.60	364.49	42.23	181.13
	Fixed assets (msek)	97	69.27	11.95	197.36	2.79	52.83
	Net income (msek)	97	10.42	8.35	42.12	3.05	14.70
	ROA (%)	97	17.71	17.95	17.83	7.56	30.78
	Leverage (%)	97	8.56	0.16	14.19	0.00	9.71
	Employees (fte)	97	108.94	55.00	149.52	29.00	115.00
	Age (y)	97	21.86	17.00	16.02	11.00	32.00
PE x Divisional							
	Sales (msek)	57	754.14	302.72	1,144.30	175.18	842.00
	Total assets (msek)	57	549.23	185.18	1,097.42	110.83	600.84
	Fixed assets (msek)	57	197.14	56.40	584.38	14.27	148.44
	Net income (msek)	57	12.68	9.74	34.78	1.00	20.87
	ROA (%)	57	11.84	8.89	13.52	3.79	19.33
	Leverage (%)	57	7.31	0.00	15.13	0.00	3.62
	Employees (fte)	56	504.05	150.00	1,043.35	77.00	498.50
	Age (y)	57	39.46	33.00	28.31	17.00	53.00
PE x Secondary							
	Sales (msek)	12	814.79	216.04	2,142.34	105.13	304.11
	Total assets (msek)	12	764.56	182.18	1,267.83	119.79	792.11
	Fixed assets (msek)	12	422.57	94.33	678.80	40.81	516.50
	Net income (msek)	12	39.67	4.35	87.61	0.31	49.89
	ROA (%)	12	18.60	17.24	15.91	8.53	30.26
	Leverage (%)	12	18.81	4.16	23.53	0.00	42.41
	Employees (fte)	12	256.00	81.00	578.60	23.50	160.50
	Age (y)	12	32.50	26.50	22.25	20.50	41.50
PE x Pub2Priv							
	Sales (msek)	10	436.32	396.68	323.47	145.09	713.74
	Total assets (msek)	10	953.52	229.85	1,788.57	127.72	876.37
	Fixed assets (msek)	10	653.74	74.57	1,471.29	35.69	439.98
	Net income (msek)	10	84.64	22.21	206.54	0.00	48.09
	ROA (%)	10	4.92	13.20	24.19	10.84	15.75
	Leverage (%)	10	5.89	0.00	15.15	0.00	0.06
	Employees (fte)	10	240.40	183.00	199.65	111.00	374.00
	Age (y)	10	39.00	31.00	26.23	20.00	53.00

Notes: This table shows descriptive statistics of the sample of LBO targets for the four different deal types. All variables are calculated for the year prior to a buyout, i.e. *t*-1. All figures are in million SEK except for ROA, leverage, employees and age. ROA is earnings before interest expenses (EBIE) divided by total assets. Leverage is debt divided by total assets. Other variables are self-explanatory.

Table D4. The effects of LBOs on target firms: implications of firm size

Variables	Capex t+3	Fixed assets t+3	Employees t+3	Wages t+3	Leverage t+3
PE x Large LBOs	0.210	0.493***	0.183**	0.231***	0.044**
	(0.352)	(0.176)	(0.074)	(0.081)	(0.021)
PE x Small LBOs	0.833**	0.743***	0.375***	0.310***	0.029*
	(0.366)	(0.200)	(0.094)	(0.120)	(0.016)
Observations	198	342	338	338	352
R-squared	0.070	0.077	0.107	0.063	0.044
Year FE	Yes	Yes	Yes	Yes	Yes

Notes: (1) The dependent variable is the change in the outcome variables in the respective columns three years following a buyout. All variables are logged except for leverage; (2) PE is a dummy variable that equals one after a private equity backed LBO, zero otherwise; (3) Large LBOs is a dummy variable taking the value of one if the pre-LBO total assets of the target is above the median of all 176 targets, Small is a dummy variable taking the value of one if the pre-LBO total assets of the target is below the median of all 176 targets; (4) Robust standard errors in parentheses; (5) *** $p<0.01$, ** $p<0.05$, * $p<0.1$.