Private Equity and Local Industry

Estimating the Spillover Effects of Private Equity Buyouts on Local Industry Peers

ABSTRACT

The impact of private equity (PE) buyouts on stakeholders, especially peer firms, has so far received little attention in the literature. This is surprising given the extensive research on the effects on funds and target firm performance. We address this research gap by studying the spillover effects of PE buyout transactions on local industry peers, using data on Swedish PE deals between 2001 and 2019. Our study is two-fold. First, we investigate the impact of PE firms on their portfolio companies. Second, we estimate the spillover effects on local industries. We apply a fixed effects regression model with time-varying treatment effects. Our most notable findings are 1) that buyout targets increase investments in intangible assets and 2) that these investments spill over to local industries. Thus, our results indicate that PE firms might have been contributing to digital transformations in their portfolio companies, rendering ripple effects throughout local industries. In addition, negative spillover effects are observed in employment and sales growth, suggesting that PE firms may be outcompeting local peers. Lastly, we show that spillover effects are stronger when PE deals are large and that the effects vary with the competitiveness within local industries. We add to the literature on PE buyouts' impact on portfolio companies and their broader stakeholder impact, as well as the literature on spillover effects in general.

Keywords: Private Equity, Buyouts, Spillover Effects, Local Industry

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1 Introduction

Private Equity (PE) buyouts have become a dominant force in today's economic landscape. At the end of 2022, buyout capital was the world's largest alternative asset class with 3.3 trillion USD under management, and it is growing fast (McKinsey & Co, 2023). In 2018, assets under management were under 2 trillion USD, and by 2027, they are projected to reach upwards of 5 trillion according to Preqin (Financial Times, 2022). This rapid growth journey is indicative of PE buyouts' superior value-creating abilities, which have been documented in several studies on PE fund returns and their impact on portfolio companies' operating performance (Bergström et al., 2007; Harris et al., 2014). However, much less effort has been put into understanding the impact of PE buyouts on industry peers. Further, very little is known about how PE buyouts affect the local area where the portfolio companies operate. Thus, we study the association between PE buyouts and spillover effects on local industry peers.

Understanding PE buyouts' impact on local industry peers' operational and financial outcomes is important because businesses play a pivotal role in local economic welfare, influencing job creation, innovation, public finances, and community development (Olbert & Severin, 2023). Spillover effects are intriguing because they represent the indirect impacts that a company's actions, like investments or operations, have on nearby firms. These effects are rooted in the concept of agglomeration economies, where firms benefit from being located near each other due to shared expertise, resources, and labor market pooling (Marshall, 1890). However, spillover effects can be both positive, like enhanced productivity and efficiency, and negative, such as increased competition leading to challenges for some firms (Blomström & Kokko, 1998; Leary & Roberts, 2014). Given the size of the PE industry, any effect that buyouts have on local peers becomes paramount when considering the vast number of global PE deals. Further, the PE industry has, in recent years, received heightened attention from regulators, politicians, and labor groups, voicing skepticism of PE funds and arguing that they extract value from other stakeholders (Bernstein et al., 2017; Davis et al., 2014). The PE industry itself, however, argues that their investments positively impact society, yielding economic growth and creating positive spillovers to other companies (AIC & EY, 2019). Thus, it is surprising that we do not know more about how the peers of PE targets are affected. Previous research within this area has mainly investigated PE buyouts' spillover effects on public firms and has found largely positive spillovers within industries regarding financial performance, investments, and employment (Aldatmaz & Brown, 2020; Bernstein et al., 2017). These results may be amplified for local peers, given that spillovers have been shown to occur to a greater extent between firms within close proximity of each other (Giroud et al., 2021). However, they may also be reversed if PE-owned firms steal business from local industry peers or reduce business to local suppliers.

We investigate the effects of PE buyouts on local industries using Swedish data. We do this partly because of the extensive data availability but mainly because Sweden is one the most active buyout markets in the world when put in relation to GDP (Aldatmaz & Brown, 2020; SVCA, 2022), and that Swedish buyouts are representative of buyouts elsewhere in the world (Olsson & Tåg, 2017). In constructing our dataset of PE transactions, we combine buyout deal data from Capital IQ between 2001 and 2019 with rich financial statement data from the Serrano database.

Our analysis is carried out in two steps. First, we examine the firm-level effects on PE buyout targets across investment, employment, and financial performance outcomes. We match PE target firms to similar non-acquired control firms using nearest-neighbor matching. After that, we use a fixed effects regression model with a time-varying treatment variable and panel data to study the buyout effects over time. We observe that buyout targets increase capital expenditure at the beginning of the holding period, mainly driven by investments in intangible assets. The increased intangible capital expenditure indicates that PE firms in our sample may actively pursue digital transformation strategies in their portfolio companies. We also find that buyout targets grow in employment and sales during the first two years of PE ownership, highlighting that growth has been a central strategy for the PE-owned firms in our dataset.

In the second part of our analysis, we investigate the same outcome variables in local industries. We create a panel dataset of aggregated firm data for all firms within the same municipality and industry. We assign a treated status for the municipality-industry based on the presence of a PE deal and match it to similar non-treated municipality-industries. As in our firm-level analysis, we use a fixed effects regression model with time-varying treatment effects to estimate the spillover effects. We find evidence that spillover effects from PE buyouts occur within the local industries. For investment outcomes, the results suggest that spillovers follow the activities of the buyout targets. That is, in the years after the targets increase intangible capex in our firm-level analysis, we observe that local industry peers also increase intangible capex. This also increases the intangible assets ratio for treated local industries compared to the control group, an effect that is sustained five years after the buyout transaction. For employment and sales growth, we observe negative associations indicating that PE firms may steal or otherwise reduce the business of local peers. On the other hand, we find a positive development in EBITDA margin, suggesting that PE firms increase competition, leading to peers becoming more cost-conscious.

In further tests, we show that the spillover effects mainly concentrate on larger PE transactions and that the results are robust to excluding large cities from the data, which are typically subject to more PE buyouts than smaller cities. We also show that intangible capex spillovers mainly occur in low competition settings, whereas increased profitability spillovers tend to occur in high competition settings. These results highlight that the relative competitiveness of the local industry matters for spillover effects.

Our study adds to the literature on PE buyouts, their impact on portfolio companies, their broader stakeholder impact, and the literature on spillover effects in general. Our firm-level analysis provides further evidence to the extensive existing catalog of research on target company effects. By exploring the impact of intangible investments, however, we build on the results of Olsson &

Tåg (2017) and contribute empirical evidence to an otherwise relatively unexplored aspect of PE investments. While the literature is rich with evidence on the operational performance of PE targets and the value-generating abilities of PE funds, much less work has been done within the field of stakeholder impact. There have been relatively few studies on the spillover effects of PE investments on other firms. Aldatmaz & Brown (2020), who investigate the impact of PE investments on industry developments, call for further research on the settings in which spillovers occur. Other studies include Bernstein et al. (2017), who also investigate overall industry developments, and Olbert & Severin (2023) who examine spillovers within municipalities as a part of their broader tax study. We build on the results of Bernstein et al. (2017), Aldatmaz & Brown (2020), and Olbert & Severin (2023) and provide evidence on a previously unexplored topic, namely the spillover effects of PE buyouts on local industries. Further, our study gathers insights on how spillover effects occur between firms in close geographical proximity, which adds to the broader literature on spillover effects.

2 Literature Review

As our study aims to investigate Private Equity's impact on both target companies and local industry peers, we turn to literature within two avenues. First, we set the stage by reviewing the documented research on Private Equity's impact on portfolio companies. In that section, we focus on the channels PE firms use to create value in buyout targets, as well as the financial statement impact such PE intervention has historically had on buyout targets. Second, we review the literature on spillover effects. That section provides an overview of how spillovers occur and then looks at studies that have examined spillover effects attributable to private equity involvement.

Private Equity (PE) is broadly defined as *investments in unlisted companies by professional investors*. This sector is a part of the larger private capital market[1] and consists of five subsegments: Venture Capital (VC), Growth Equity, Buyout, Balanced, and Distress. We focus on buyout funds acquiring controlling interests in mature companies that are typically profitable and cash-flow positive using a combination of equity and debt. PE investors add value to these portfolio companies by fostering growth, increasing efficiencies, or a combination of both. They do so through active ownership and governance during a limited holding period of typically 3-7 years. Exits usually occur through an IPO, sale to a corporate buyer, or through a secondary buyout (Kaplan & Strömberg, 2009; Metrick & Yasuda, 2011; Strömberg, 2007). A notable example of a leveraged buyout is the acquisition of Thule Group's trailer business by Accent Equity Partners in 2014. In this transaction, the company's management co-invested in the buyout, showcasing one way the PE firm employed active governance. Over the next seven years, the company increased sales from 578m SEK to 780m SEK while simultaneously reducing its workforce from 500 to 380 employees. In 2021, the company was sold to the publicly traded investment company Storskogen (Storskogen, 2021; Thule Group, 2014).

2.1 Private Equity's impact on portfolio companies

The consensus in the literature is that private equity transactions generally create value for the shareholders of portfolio companies (Sorensen & Yasuda, 2022). The larger debate in the literature concerns the sources of the increased shareholder value. Sorensen & Yasuda (2022) outline a dichotomy in the private equity literature between the views of Shleifer & Summers (1988) and Jensen (1989) on the impact of private equity transactions. Jensen (1989) argues that shareholder gains stem from efficiency improvements, whereas critics like Shleifer & Summers (1988) have raised concerns that PE funds might not genuinely create value but rather extract it from stakeholders, such as employees or customers. This sentiment is often echoed in the media, where it is suggested that PE buyouts lead to job cuts that boost cash flows in the short term and compromise long-term performance (Financial Times, 2023). By the same logic, PE-owned firms

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[1] The Private Capital market includes five segments: Private Equity, Private Debt, Real Estate, Infrastructure and Natural Resources.

should also reduce capital expenditure to boost short-term cash flows. While the debate is ongoing, several studies have examined the way in which private equity firms operate to generate shareholder gains and the impact it has on various firm and stakeholder outcomes (Sorensen & Yasuda, 2022). As we show in the following paragraphs, the literature largely rejects this criticism, although it offers mixed evidence warranting further exploration.

2.1.1 PE firms' channels to value creation

Kaplan & Strömberg (2009) categorize the value-creating activities of buyout firms into three distinct domains: financial engineering, governance engineering, and operational engineering. Financial and governance engineering were the key components in most leveraged buyouts as the private equity industry emerged in the 1980s (Kaplan & Strömberg, 2009). These practices are centered around aligning management incentives through equity stakes, utilizing leverage to create pressure to meet principal and interest payments, and being active owners. Such practices are easy to copy and have been commoditized to the extent that they are argued to be reflected in transaction prices of buyouts. During the 21st century, operational engineering has become commonplace in private equity firms. The term refers to the industry and operating knowledge that sponsors bring to their portfolio companies. Private equity sponsors typically develop and implement business plans for their holdings to improve the value creation of the investment. Such plans may include a wide range of methods to improve the operational performance of the portfolio company, including cost-cutting, strategic changes, capital expenditure, and company acquisitions (Kaplan & Strömberg, 2009).

The way operating activities are described in the PE literature is typically high-level in category and broad in scope, which may be an inherent consequence of the infinite paths pursued to improve operating performance. In a review of the PE literature, Sorensen & Yasuda (2022) note a trend that authors often distinguish between *public*-to-private and *private*-to-private buyouts. Public buyouts are portrayed as inefficient, mature companies full of wasteful perquisites and surplus headcount, whereas private buyouts are portrayed as being associated with major growth opportunities. Sticking to such a simplified classification can be useful when investigating operational trends in some contexts, but PE transactions are undertaken for a variety of reasons, which may have caused researchers to overlook operating activities associated with both growth and efficiency gains. For instance, in a prominent survey of 74 PE firms on the pre-and post-investment sources of value creation in buyouts, over one third cited improving IT systems (Gompers et al., 2016). Other examples include the introduction of shared services. Such sources would apply equally well for firms targeting growth and efficiency improvements. Thus, in our study, we seek to contribute to the current literature by examining rich financial statement data for operational patterns that apply regardless of the type of case. In the following sections, we investigate the literature which shows that the operating strategies employed have significant consequences on portfolio companies.

2.1.2 Impact on employment and capital expenditure

Studies on employment outcomes have been subject to mixed evidence. The variability likely stems from the fact that each business requires different strategic plans to be employed by PE sponsors. These plans vary widely, from cost-cutting to market expansion, which ultimately should lead to diverse employment outcomes. One of the most prominent studies on employment was conducted by Davis et al. (2014), which analyzed 3,200 manufacturing buyouts and their 150,000 establishments following U.S. buyouts from 1980 to 2005. The results of the studies showed that while the net job losses were minimal (less than 1 percent compared to a control group), there was significant job rotation as PE closed inefficient establishments and opened new ones. In alignment with these studies, Olsson & Tåg (2017) investigate employment outcomes for buyout targets using Swedish PE deals from 2002 to 2008 and find no significant change in net employment following a buyout compared to controls. Building on their earlier work, Davis et al. (2021) expanded their scope to include both manufacturing and non-manufacturing industries, examining US buyouts from 1980 to 2013. In this paper, the authors classified the sample into public-to-private and private-to-private buyouts and found striking differences. Public-to-private buyouts showed a 12.6 percent decrease in employment. This reduction was attributed to resolving agency problems and boosting efficiency, which manifests through reducing headcount, wasteful perquisites, or value-destroying "pet projects". In contrast, private-to-private buyouts, typically reflective of expansion strategies, saw a 12.8 percent increase in employment. Complementing these findings, Boucly et al. (2011) examined 839 French buyouts between 1994 and 2004. Their study revealed an 18 percent increase in employment in the four years following the transaction, predominantly in private-to-private buyouts.

When examining PE's impact on target firms' investments in assets, we find a shift from early trends. Research on buyouts in the 1980s found a decrease in capital expenditure following a transaction (Kaplan, 1989). However, as the PE industry evolved, focusing more on operational engineering, newer studies reveal increases in investments. Boucly et al. (2011) find that leveraged buyout targets had significantly higher levels of capital expenditure than controls, particularly in scenarios where the buyout alleviated previously held credit constraints. In addition, Boucly et al. (2011) also show that increased levels of spending are concentrated in private-to-private deals, as opposed to divisional carveouts or public-to-private transactions. Further supporting this trend, Olbert & Severin (2023) observed 35% higher capital expenditure for buyout targets compared to controls in their study of 11,000 European PE deals from 2001 to 2018. Notably, this study finds that tangible capital expenditure is an important driver of this increase, illustrating the role new machinery and equipment play in operational engineering. In a different study on Swedish PE deals between 2002 and 2008 by Olsson & Tåg (2017), increased intangible assets have been identified as a critical operating lever used by PE firms acquiring *low-productive* firms. The authors regard intangible assets as a proxy for technological investments, emphasizing their role in increasing automation. We find these results noteworthy as the current literature lacks further evidence on the role of intangible investments for PE firms. However, we interpret the results with caution as the observed effect only holds for low productive firms. Furthermore, the tests do not control for the levels of intangible assets in the firms, meaning that it could be an inherent consequence of firms with already high levels of intangible assets, invest more in intangible assets to maintain their existing IT infrastructure. Still, the studies on investments, which examine deals in Europe during the 21st century, suggest that increasing investments is an important avenue of PE firm's value-creation strategies, marking a shift from earlier practices. However, the underlying nature and operational role of these investments have not been extensively explored, warranting further exploration.

2.1.3 Impact on financial performance

The changes implemented by private equity firms, such as increasing investments or employment, are often part of a larger plan intended to increase demand factors and boost firm performance. Previous literature largely indicates that PE funds have been successful in doing so, exampled by the fact that US PE funds have been shown to outperform the stock market by over 3 percent per year (Harris et al., 2014). Further evidence shows that the financial performance of leveraged buyout targets typically increases following a buyout. In Sweden, Bergström et al. (2007) examined the value-creating abilities of private equity sponsors and observed a significant abnormal positive impact on profitability and a subtle increase in sales. The study focused on buyouts from 1993 to 2006 and found a median increase in EBITDA margin of 2.31% and a 0.71% increase in sales. Moving beyond Sweden, the aforementioned study by Boucly et al. (2011) in France observe similar evidence that private equity transactions lead to increases in the targets' EBITDA of 18%. In contrast to Bergström et al. (2007), however, Boucly et al. (2011) also observe an 11% increase in sales over the holding period. These effects are concentrated in private-to-private deals.

In summary, PE firms are increasingly reliant on operational engineering to increase value in their portfolio companies. The literature on PE firms' impact on buyout targets indicates that employment outcomes are mixed, although an observation emerges: private-to-private buyouts tend to yield more favorable employee outcomes than their public-to-private counterparts. Evidence from Europe suggests that the pronounced impact in private-to-private deals extends to capital expenditure, sales, and earnings increases as well. In the next section, we review the theory of spillover effects and share the documented impact private equity firms have on industries.

2.2 Spillover effects and the context of private equity

An important aspect of the ongoing debate on Private Equity and whether it has a positive or negative impact on society is its effect on peers. If the observed value creation in PE funds occurs at the cost of local and industry peers, the net effect of PE activity may be negative. However, the PE industry argues that their investments positively impact society in terms of economic activity and growth, citing strong spillover effects to competitors, suppliers, customers, and employees

(AIC & EY, 2019). Despite the limited literature on PE-related spillover effects, existing evidence supports that operational improvements and changes in a firm can have a significant impact on local and industry peers. In the following sections, we first explore the literature on spillover effects in general and then share the available studies on spillover effects in a PE setting.

2.2.1 General spillover effects

The concept of spillover effects has a long history within the literature and dates back to Marshall (1890), who posited that firms enjoy positive externalities from localizing close to other firms. The externalities arise from the information and expertise that is shared between firms, the ability to share and trade resources as well as labor market pooling. Such agglomeration economies are still present today, as highlighted by Porter's (1998) Harvard Business Review article. His paper examines the phenomena of industry clusters such as Silicon Valley and car manufacturers in southern Germany. Porter (1998) argues that the persistence of geographical clusters highlights the importance and constant occurrence of knowledge exchange through local relationships, leading to competitive advantages. Thus, despite the technological advancements facilitating long-distance communication, proximity between firms can greatly impact their development. This is also shown by Rosenthal & Strange (2020), who, in a review of the literature on agglomeration economies, explain that positive externalities become more pronounced the closer firms localize to each other.

Clusters are centered around one industry, implying that agglomeration effects are mainly concentrated on industry peers. This follows intuitively from Marshall's (1890) observation on externalities occurring due to shared expertise, resources, and labor market pooling, all of which more easily occur within an industry rather than between industries. In a similar vein, Giroud et al. (2021) study the spillover effects of large plant openings in the US on nearby plants and find significant positive productivity spillover effects. These effects are much more prominent if the nearby plants are in the same industry as the newly opened large plant. The authors ascribe the spillovers to knowledge and best practice sharing, which occur between nearby plants and are further propagated by labor market pooling.

The preceding sections describe how local industry knowledge sharing, instigated by new investments, leads to spillover effects. These findings are echoed by Blomström & Kokko (1998), who find significant spillover effects – both positive and negative – from foreign direct investments into a particular industry and area. The authors note that these effects largely occur as firms interact and compete, which aligns well with the literature on competitive dynamics within peer groups. Leary & Roberts (2014) show that corporate decisions, such as investing in new assets or increasing debt, often represent strategic responses to peer behavior. It is not uncommon that these corporate decisions mimic the behavior of peers; for example, if company A increases capital expenditure, then company B should, too. At the very least, industry competitors learn from each other by observing each other's strategic actions (Armstrong et al., 2019). Further literature posits that managerial efforts will be affected by the intensity of competition. Schmidt (1997) shows that competition positively affects managerial efforts in product market competition. At the same time, if the competition becomes too intense, managerial efforts are reduced. Collectively, these studies indicate that spillover effects from PE investments could potentially incentivize peer firms to improve efficiency by following suit. However, if peer firms do not have the same operational freedom in implementing further investments and cannot keep up with the competitive pressure, they may become crowded out, illustrating how negative spillovers can occur.

In summary, the empirical evidence on spillover effects highlights the significant impact of proximity and industry clustering on firm performance, emphasizing the strategic adaptations firms make in response to peer behavior. This context helps us understand the dynamics at play and the potential impact PE targets could have on local industry peers.

2.2.2 Private equity spillover effects

Similar to foreign direct investments and new plant openings, changes implemented by PE firms – whether financial or operational – likely create negative or positive spillover effects for industry or local peers. Therefore, it is necessary to examine the impact on peer firms to understand the full impact of PE transactions. The research in this field is scarce and has been concentrated mainly on public company data.

Hsu et al. (2011) study a set of 178 large PE buyouts (median deal value of $1.2bn) in the US and find that the share price and operational performance of core public competitors to the target decline post the transaction. These effects likely stem from the increased competitiveness of the private equity-owned firms. Increased competitiveness in the industry likely has positive outcomes for consumers, providing one avenue of potential positive spillover effects from PE investments. If, on the other hand, firms are outcompeted, and job losses among the core competitors, the spillover effects may be overwhelmingly negative. In a study of the wider industry effects of PE activity, Bernstein et al. (2017) investigated the correlation between the presence of PE buyouts and industry performance. The dataset included 20 industries in OECD countries from 1991 to 2009, using OECD country-industry data. The authors find that country-industries where PE funds have been active in the past five years grow faster than other country-industries, whether measured using total production (gross output), value added (industry contribution to national GDP), total wages (labor costs), or employment (number of employees). A limitation, however, is that the study uses industry-level data rather than firm-level data, and therefore, it cannot document what effects stem from the target firms and what stems from spillover effects to peer firms.

Aldatmaz & Brown (2020) build on the results from Bernstein et al. (2017) and attempt to isolate the spillover effects from PE investments within country-industries by focusing solely on the effects on public companies. The author's assumption is that all PE-owned firms are privately held, and thus, any effect seen on public industry peers should be due to spillovers from PE investments. The dataset consists of information on the absolute dollar amount of globally invested PE capital matched to aggregated accounting data of listed companies across 19 different industries in 52 countries between 1990 and 2017. Their findings support Bernstein et al. (2017) by showing that PE buyouts are associated with higher growth in employment and profitability among publicly traded industry peers, although they did not see any significant effect on productivity. The authors also highlight that the level of competition and quality of legal institutions are important determinants in creating spillover effects, as country-industries with low competition or countries with poor legal institutions did not show any significant spillovers.

The results of Aldatmaz & Brown (2020) and Bernstein et al. (2017) together indicate the presence of large positive spillover effects from PE buyouts within industries on a national level. Spillovers may, however, as previously highlighted, also occur at more local levels driven by proximity to the PE buyout target. Olbert & Severin (2023), investigate municipal-level spillover effects of PE buyouts as a part of their wider study on tax implications for local governments. The authors aggregate firm-level data on a municipal level and match it to a large sample of European buyout targets between 2001 and 2018 by the municipality of incorporation. They then compare the aggregated firm developments of municipalities that have received PE investments, excluding the PE target firms, with a non-treated control group. Despite estimating spillover effects on firms across all industries in each municipality, the study finds a small but significant increase in capital expenditures in treated municipalities. For employment growth, they observe positive but insignificant coefficients.

While estimating spillover effects from PE buyouts on a municipal level is highly relevant for studies related to municipal tax collection, there is an argument to be made for the incorporation of industries into the spillover section of Olbert & Severin's (2023) study. The results provided by Aldatmaz & Brown (2020) and Bernstein et al. (2017) indicate that most of the spillover effect observed in Olbert & Severin's (2023) results should be concentrated in the industries of the PE treated firms. This would mean that spillover effects on a local industry-level correspond to those reported by Olbert & Severin (2023), only amplified. However, it could also be that the positive within-industry spillover effects observed on a national level by Aldatmaz & Brown (2020) and Bernstein et al. (2017) do not hold for a local industry-level. This could then be due to the proximity between the firms and the potential outcompeting of industry peers in the local market from PE-owned firms. In that case, the positive spillover effects observed on a national level could even be reversed on a local level. However, such negative spillover effects on industry peers may not be visible in Olbert & Severin's (2023) study, where firms across all industries are aggregated. This warrants further analysis of the spillover effects on a local industry-level.

Summarizing the literature on spillover effects from PE buyouts, there is evidence that competition increases, which naturally has some negative effects on close competitors to PE targets. However, the evidence on aggregate industry outcomes indicates that private equity buyouts overwhelmingly have positive spillovers, but there is a need to further understand what occurs at a local industry-level.

3 Hypotheses

Based on the empirical foundation provided in the previous section, the following hypotheses are formulated to investigate the impact on PE investments. Our study is twofold. First, we look at the firm-level outcomes, and second, we look at the spillover effects on local industry.

3.1 Firm-level impact

The empirical evidence is largely consistent in contending that PE buyouts lead to operational performance gains, although the outcomes differ depending on the characteristics of the targets and the strategies employed by PE firms (Boucly et al., 2011; Davis et al., 2021). European studies from the last decade find that buyouts tend to yield significant growth in capital expenditure and employment compared to controls, which could be an effect of the increased focus on operational engineering over the past several years (Kaplan & Strömberg, 2009). In light of these empirical findings and considering our sample, which consists of Swedish single entities, our study hypothesizes that PE firms increase employment and capital expenditure. Along with the arguments put forward by Olsson & Tåg (2017) that intangible assets, defined as information technology, could play a role in developing operating performance, we hypothesize that intangible assets are a key driver when PE buyout targets undertake new investments.

Several studies in a variety of geographies, including Sweden (Bergström et al., 2007), France (Boucly et al., 2011), and the US (Kaplan & Strömberg, 2009), consistently report increases in profitability measures, such as EBITDA, following buyouts. Notably, in addition to profitability, some studies, like Boucly et al. (2011), observe that buyout targets exhibit a higher growth rate in sales compared to non-PE-backed firms. Our study anticipates, and thus hypothesizes, that the observed trends in profitability and sales will be consistent with the previously documented results.

H1a: Private Equity buyout targets experience *increased* levels of *capital expenditure* relative to controls, including *increased* levels of *intangible capital expenditure*.

H1b: Private Equity buyout targets experience *increased* levels of *employment growth* relative to controls.

H1c: Private Equity buyout targets experience *increased* financial performance, measured as *net sales growth* and *EBITDA margin,* relative to controls.

3.2 Spillover effects on local industry

Extending beyond our firm-level hypotheses, we review literature that leads us to hypothesize that spillover effects on local industry peers exist. The theories on proximity and industry clustering that lead to knowledge exchange, resource sharing and labor market pooling (Marshall, 1890; Porter, 1998; Rosenthal & Strange, 2020) indicate that municipality and industry play a vital role in the occurrence of spillover effects. In addition, competitive dynamics literature suggests that peers react to, learn from, and potentially mimic the behavior of peers (Armstrong et al., 2019; Leary & Roberts, 2014; Schmidt, 1997), establishing further motives for spillovers to occur. Furthermore, earlier studies have shown that industry spillover effects from PE transactions occur on a national level (Aldatmaz & Brown, 2020; Bernstein et al., 2017), suggesting the possibility of similar effects occurring at the local level. Perhaps most signaling, a very recent study on municipal spillovers not taking industry into account suggests that PE buyouts lead to significant but modest increases in capital expenditure (Olbert & Severin, 2023).

> **H2:** Private Equity buyouts lead to *spillover effects* on peer companies if located within the same municipality and industry.

Considering our belief that spillover effects occur within local industries, we formulate a series of hypotheses on the anticipated impact on investments, employment and financial performance.

Turning to investments, we anticipate multiple dynamics at play. As previously mentioned, Olbert & Severin (2023) observe a small but significant increase in capital expenditure for firms in a municipality exposed to a PE buyout. While interesting, we anticipate that the effect observed is concentrated, and thus stronger, in the industry of the PE target because spillovers tend to affect peers in the same industry to a larger extent (Giroud et al., 2021). Such effects could be a natural consequence of direct spillover effects from increased levels of capital expenditure exhibited by buyout targets following a transaction. Direct spillovers refer to the impact on local suppliers from new demands by the target, such as upgrading IT systems for supply chain compatibility, or executing subcontracted work, leading to increased investments. However, there may also be indirect effects as indicated by the literature on competitive dynamics (Armstrong et al., 2019; Leary & Roberts, 2014; Schmidt, 1997). Local competitors of the PE target may increase their capital expenditure in response to intensified competition from the PE-backed firm. Furthermore, investments made by PE firms can form knowledge transfer of best practices to suppliers and competitors, given the operational and industry expertise PE firms bring (Kaplan & Strömberg, 2009), which may induce investments among local peers. Based on these dynamics, we hypothesize that capital expenditure should increase for local industry peers following a PE investment in the area.

Our point of departure for employment growth draws on the positive spillovers occurring within industries nationally (Aldatmaz & Brown, 2020; Bernstein et al., 2017) and the positive correlation between capital expenditure and employment growth (deployment of capital could be viewed as a sign of expanded operations and thus requires more employees) established by studies on PE's firm-level impact (Boucly et al., 2011; Davis et al., 2021). By the same logic, local industry peers should witness an increase in employment conditioned on increasing capital expenditure. However, the local industry dynamics could significantly change the outlook, as the literature on competition points out. A PE buyout can initially prompt local industry peers to increase their investments as a strategic response. However, as competition intensifies in the local market, local industry peers may be crowded out and reluctant to expend further capital on new employees. In addition, the anticipated employment growth in PE firms will likely draw employees from a local shared employment pool, which, holding all else equal, should pose a downward effect on employment growth for industry peers. Furthermore, while the sharing of best practices can drive capital expenditure by encouraging investment in critical technologies, this dynamic does not equally apply to employment growth, as such knowledge exchange does not inherently necessitate increased staffing. Thus, employment growth for local industry peers remains an important empirical question and we hypothesize that there is an effect but dare not speculate on what that effect is.

When we examine spillover effects on financial performance, we direct our attention to the outcome variables of sales growth and EBITDA margin. Previous literature has showed decreasing operating performance in public competitors of PE targets (Hsu et al., 2011), illustrating the negative spillovers occurring due to intensified competition. On the other hand, country-level industries targeted by PE grow faster in terms of sales and profitability than those that are not (Bernstein et al., 2017). An argument can be made that intensified competition affects the local industry peers differently as they may consist of suppliers, customers, and competitors. For example, a competitor may find decreased levels of sales from the competitive pressure, whereas a supplier may experience an increase in sales due to direct spillovers by the buyout target. The empirical evidence thus suggests that there are multiple dynamics at play and makes the case for further analysis of financial performance, though we do not speculate on the direction of the spillover effect.

H3a: Private Equity buyouts lead to *increases* in *capital expenditure* of peer companies located within the same municipality and industry, including *increased* levels of *intangible capital expenditure*.

H3b: Private Equity buyouts lead to *spillover effects* on *employment growth* of peer companies located within the same municipality and industry.

H3c: Private Equity buyouts lead to *spillover effects* in *financial performance* of peer companies located within the same municipality and industry.

4 Data

To capture the impact of PE firms on buyout targets and local industry, we create two separate datasets based on identified PE buyouts in Sweden between 2001 and 2019. First, we create a panel dataset consisting of firm-year observations for our initial analysis of the effects in buyout targets. Second, we aggregate financial data for firms incorporated in the same municipality and operating in the same industry (excluding PE-owned companies) to capture effects on local industry peers. The observed treated PE target firms and local industries are subsequently matched to controls who did not undergo treatment. The following sections describe how we collected and constructed the data required to test our hypotheses.

4.1 Constructing treatment groups

We construct our treatment groups by obtaining information from Capital IQ on PE buyout deals in Sweden between 2001 and 2019. The identified target companies are then matched to the Serrano database to obtain financial statement information. Capital IQ was selected as it offers comprehensive breadth in its coverage and is recognized as the premier source for global PE transactions over time (Bernstein et al. 2017). For each transaction, it provides details on involved parties (acquirers, targets, sellers), key dates (announcement, closure), and transaction characteristics (transaction value, if available). The Serrano database is compiled of rich financial statement data from the Swedish Companies Registration Office (*Bolagsverket*). The database holds key features for our study, such as calendar-year-adjusted financials, criteria for active business identification, and detailed records of industries, municipalities, and ownership structures, along with extensive financial statement records.

As a first step of constructing our treatment groups, 550 PE deals were obtained from Capital IQ, all of which met the following criteria, following Bernstein et al (2017):

i. The target company is headquartered in Sweden;
ii. The deal was closed between January 2001 and December 2019;
iii. The buyer is identified as a Private Equity firm in Capital IQ;
iv. The transaction is classified as a "leveraged buyout", "management buyout" or "going private"

The second step included cleaning the dataset of duplicates, minority interest acquisitions, bank or insurance targets, non-PE buyers and secondary PE transactions. To facilitate this process, we use complementary transaction databases Mergermarket and Pitchbook to look up information about buyers, sellers, and missing deal details. We keep a strict definition of PE firms and focus exclusively on firms operating with a fund structure typical for Private Equity firms. Thus, we exclude several public and private investment companies, corporate venture capital arms and family offices. Upon detecting a secondary transaction where a PE firm is the seller, we traced the original deal and manually added the transaction to the dataset if it met the specified criteria and was not already present in the existing dataset. This results in the replacement of seven secondary deals on the transaction list. We exclude buyouts where the target is a bank or insurance company, as their financial statement structure differs substantially from other companies. After cleansing, the dataset contains 380 verified deals.

The third step involves matching the PE buyout targets to financial information in the Serrano database. We elect to study single entity data rather than consolidated financials, which implies that we need to identify the most representative operating entity for each transaction.[2] Although manually selecting a representative operating company is a decision that requires careful judgment, we view it as the most accurate way of handling intricate group setups. To do this, we assess indicators like net sales and employee numbers over several years, which reflect the company's operational scale and typically exhibit stable trends. In most cases, we select the legal entity with the largest net sales and the highest number of employees. If the largest entity, however, experiences substantial changes – such as mergers of other units into the company or the entity serves as a cost center for other group firms, which would be indicated by erratic or missing data – we manually examine the company's group structure and select a more representative company.[3] Several transactions are lost while matching to data in Serrano, either because the firm does not exist in Serrano or because the information in Capital IQ was not sufficient to find a match. Once this process is completed, our dataset contains 315 PE treated firms matched to Serrano by organizational number and transaction year.

The fourth step in constructing our treatment group concerns removing any transaction with missing data on variables used to match PE transactions with comparable control firms. This results in the removal of 25 firms for which we could not observe the necessary data before the transaction occurred. Thus, 290 firms make up our initial treatment group for the firm-level analysis before matching to control firms.

4.1.1 Treatment group for local industry-level tests

To construct our treatment group for the local industry-level tests, we begin with the 315 firms that underwent PE buyouts and are matched to the Serrano database (step three above). All firms incorporated in the same municipality and operating in the same industry as the buyout target are assigned a treated status for the year of the transaction. The industry classification used is a broad two-digit classification provided in Serrano based on the Swedish SNI classification system. Before aggregating the data, we remove companies that are inactive or exhibit sales, assets or employee numbers that are equal to zero or negative. We also remove observations that have missing values

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[2] We opt for single legal entity data due to the complexities associated with consolidated statements, such as adjusting for merger & acquisition activity, non-disclosures of foreign subsidiaries and inability of collecting the PE deal type carveouts. Further, when studying local outcomes, single entity level data should provide a more accurate view of activity in the municipality of incorporation than consolidated financials.

[3] In a select few instances, a representative company could not be identified due to the target being a group consisting of a high number of small individual entities.

for any of our matching, control, or outcome variables. We then aggregate the data at the municipality-industry level for each year, including our key outcome variables, control variables, and variables for matching. Once the data is aggregated, we proceed to remove all duplicate 'municipality-industry-year' observations. This process results in a refined panel dataset representing local industries from 1998 to 2021.

In our newly constructed dataset, we observe a total of 175 unique local industries where a PE buyout has occurred during the observed period. This observation reveals that out of the 315 transactions identified at the firm-level, 140 occurred in local industries that had already experienced a buyout. In our methodology, a local industry (or municipality-industry) is considered 'treated' from the moment the first PE transaction takes place within it. Consequently, we do not account for subsequent PE transactions in the same local industry. These 175 local industries with at least one PE buyout form our initial treatment group, before matching to controls. Table 1 presents the sample construction process forming the treatment groups and Figure 1 illustrates the distribution of PE deals across industries and year, respectively.

Table 1. PE Deals Sample Construction

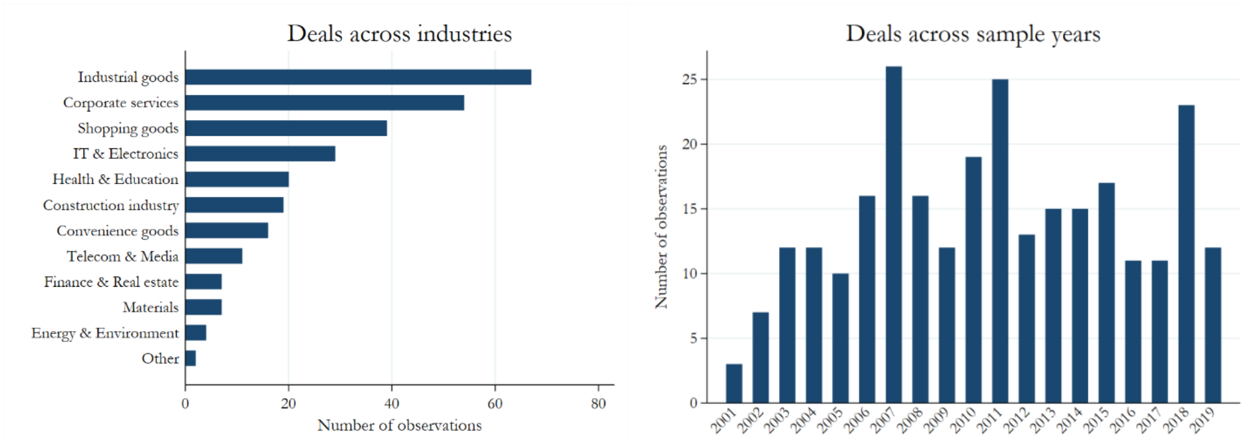
	Private Equity Deals		Deal Value (m SEK)	
Panel A: Firm-level PE deals	Obs	Loss	Obs	Mean
(1) Deals that fulfill pre-defined criteria in Capital IQ	550		151	2,737
(2) Deals verified for buyer, seller, acquisition stake and errors	380	30.9%	94	2,211
(3) Deals with identified representative operating company	315	11.8%	82	2,346
(4) Target companies with non-missing information on matching variables[4]	290	4.5%	82	2,346
(5) Deals able to match with control groups	278	2.2%	74	2,260

	Treated		
Panel B: PE deals local industry	Obs	Loss	Share of total
(1) Unique deals (step 3 in panel A)	315	-	n/a
(2) Uniquely treated municipality-industries	175	44.4%	5.6%
(3) Treated municipality-industries able to be matched with controls	149	8.3%	4.6%

The table presents the construction process of our PE deal data sample for our firm-level analysis and local industry-level analysis. The deal data is collected from Capital IQ, dated January 2001 to December 2019. *Panel A* includes observation counts at each step, and the relative loss from the original deal sample. The panel also presents the count of observations with available information on deal value and the respective average deal values for our firm-level PE deal sample. *Panel B* illustrates the construction of our municipality-industry deal data, includes observations at each step, and the relative loss from the original deal sample. It also presents the share of municipality-industries that become treated.

[4] Complete list of matching variables and definitions is available in Appendix A.

Figure 1. Sample PE Deals across industries and over time



The figure shows the distribution of PE deal observations across target firm industries (left) and sample years (right).

4.2 Constructing control groups

To accurately capture the impact of PE buyouts on target companies and local industries we create matched control groups. These samples should ideally mirror the pre-treatment characteristics of companies and local industries affected by PE buyouts. To achieve this, we perform nearest-neighbor matching using the Mahalanobis distance metric. Mahalanobis is the most commonly used distance metric for nearest-neighbor matching and is considered the most robust estimator under different settings (Zhao, 2004), which suits our scope of matching two scenarios (firm and local industry). It also accounts for correlation between covariates (Zhao, 2004), which likely exists for financial statement data. We match on discrete and continuous variables one year before the transaction occurs. Our use of nearest-neighbor matching is informed by prior research, specifically leveraging established determinants of PE buyouts like pre-buyout size, profitability, and growth variables (Guo et al., 2011). This method allows precise selection of these key criteria for matching, directly addressing the characteristics central to PE acquisition scenarios. Table 2 presents the matching statistics for the matched samples one year prior to the buyout and demonstrates the matching quality achieved in our procedure.

4.2.1 Control groups for firm-level analysis

Our data preparation for the firm-level matching procedure begins with cleaning the Serrano dataset, adhering to the methodology described in section 4.1.1. First, we exclude any entities not recognized as limited liability companies. Then, we remove any inactive companies and observations with zero or negative assets, sales, or employees. Following this, we exclude observations with missing values for any of our matching and outcome variables. To avoid any bias that might arise from matching treated entities with other entities under PE ownership, we remove all companies that were connected through a shared parent company with any of our treated entities during the holding period.

Our matching strategy involves pairing each treated firm with the two most similar non-treated firms from our dataset. We adopt a one-to-two matching approach to balance bias reduction and matching precision (Imbens & Wooldridge, 2009). The process of matching is carried out using several key variables. The discrete matching variables used are industry and the year before the transaction. All discrete variables must be exactly matched, meaning that the control firm must exhibit the exact same values for each discrete variable. For instance, a firm undergoing a PE buyout in 2015 will be matched in the preceding year (2014). If the buyout target is operating in the industrial goods industry, only firms operating in that industry in 2014 are eligible to be matched. By using the discrete variables industry and year, we ensure that our matched sample faces the same macroeconomic conditions, regulation, and industry trends. The continuous matching variables used are related to assets, profitability and growth during the preceding two years and are intended to reflect characteristics PE firms look for in potential buyout targets (Guo et al., 2011). The matching variables used are listed in panel A of Table 2.

In conducting the matching, we do not use any replacement of control firms. This leads to 15 treated observations being excluded as some control firms were matched to two or more treated observations. Following this matching procedure, our final dataset comprises 278 treated observations and 556 matched control firms. The quality of the matching procedure is shown in panel A of Table 2 by the relative and standardized differences in means between the treated and control groups. In general, our firm-level matching indicates high-quality matches with small differences between the treated and control group.

4.2.2 Control groups for local industry-level analysis

For our local industry-level analysis, we use the dataset created in section 4.1.1 with aggregated firm-level data on a municipality-industry level. Think of these local industries (or 'municipality-industries') as the sum of the financial data for all firms incorporated in a particular municipality and industry. By considering 290 Swedish municipalities and 11 industries, we end up with 3,147 unique local industries after adjusting for industries not represented in municipalities. Out of these local industries, 4.6 percent have received PE treatment. We analyze the annual developments of these industries from 1998 to 2021 and end up with a dataset consisting of 80,818 'municipality-industry-year' observations. The objective of the matching procedure is to identify local industries that maintain similar characteristics in terms of size, asset base, profitability levels, and growth. Similar to our firm-level approach, we require that a control municipality-industry is in the same industry as the treated and that they are matched in the same year. Thus, a given treated municipality-industry will be matched to the same industry in a different municipality. The continuous matching variables used are the total number of firms, aggregated total assets, intangible assets, EBIT, and the asset growth for the preceding two years in the municipality-industry. Panel B in Table 2 presents the matching statistics for our local industry analysis.

An issue with analyzing local outcomes of private equity investments is that larger cities, due to their size, typically receive more PE investments and thus are more likely to be treated. As we are investigating Swedish data only, we are limited to drawing from the 290 existing municipalities in Sweden when matching. Therefore, we use one-to-one matching for the local industry-level analysis. Of the 175 treated municipality-industries, 149 are uniquely matched to a control municipality-industry. Given the nature of our data, there is an imbalance where the mean of the treated municipality-industries is higher than the mean of the control municipality industries for each matching variable. To understand the magnitude of the imbalance between our treated and control group we turn to the standardized differences in mean. In a review of the literature on matching methods, Stuart (2010) outlines that the absolute value of the standardized difference in means (SMD) should ideally be as low as possible for each matching variable. However, in the absence of a big pool of potential high-quality matches, the author suggests limiting the number of variables with large SMDs as much as possible. A large difference is defined as greater than 0.25. We follow this approach, and in our final matched sample, only two variables have an SMD of above 0.25. While this indicates a noteworthy imbalance in our dataset, it does not prohibit us from proceeding with our analysis and interpreting the results (Stuart, 2010). We further address this issue in section 7.2.

Table 2. Matching statistics for the matched firm sample one year prior to the buyout

	Target firms (N=278)		Control firms (N=556)		Differences in means	
Panel A: Firm-level	Mean	Median	Mean	Median	Rel.	Std.
Total Assets (m SEK)	311.26	99.74	248.50	52.31	0.20	0.05
EBIT (m SEK)	21.73	13.97	15.53	7.78	0.29	0.07
Intangible Assets (m SEK)	9.16	0.00	6.06	0.00	0.34	0.10
Asset CAGR (2-year)	15.6%	10.0%	15.5%	10.6%	0.01	0.00
Sales CAGR (2-year)	19.0%	10.0%	15.9%	10.1%	0.16	0.08
	Target local industry (N=149)		Control local industry (N=149)		Differences in means	
Panel B: Aggregated local industry	Mean	Median	Mean	Median	Rel.	Std.
Count of companies (#)	368.60	121.23	160.01	70.28	0.57	0.29
Total Assets (m SEK)	31,160.13	2,632.35	5,192.01	1,679.42	0.80	0.20
EBIT (m SEK)	1,040.35	130.04	237.49	60.28	0.77	0.22
Intangible Assets (m SEK)	271.72	19.70	81.72	5.92	0.69	0.36
Asset CAGR (2-year)	14,6%	6,8%	13,2%	5,8%	0.10	0.02

The table presents matching statistics for the matched firm sample one year prior to the buyout. Controls are matched to treated firms based on the pre-buyout year, industry, and positive earnings. We employ nearest neighbor matching on continuous variables listed, using the Mahalanobis distance method. The table presents mean and median values for the target and control firms, alongside their relative and standardized differences, demonstrating the matching quality of our approach (Imbens and Woolridge, 2009).

4.3 Description of final datasets

Table 3 provides summary statistics for the variables that form the data sample used in subsequent tests and analysis, covering the period from 1998 to 2021. The table details the number of observations, mean, median, minimum, maximum, and standard deviation for each variable. The outcome variables investigated are exclusively ratios and growth rates, ensuring comparability between firms and local industries. The controls, on the other hand, include log-transformed variables to account for size effects. For in-depth definitions and calculations of each variable we refer to Appendix A. See also Appendix E for further details on the rationale behind log transformations. Panel A in the summary statistics table is used to examine the impact on buyout targets following PE-ownership, while panel B focuses on the spillover effects impact on local industry, aggregating the observations from the same timeframe. In Panel B we have excluded all the PE buyout target firms and any affiliated firms that are majority owned by the identified target's parent from the aggregated data. We do this to ensure that any observed impact on local industries from PE buyouts does not include the effect of the PE targets themselves.

Table 3. Summary statistics

Panel A: Firm-level	Obs	Mean	Min	Median	Max	SD
Capital Expenditure/Assets (%)	6,713	6.77	-26.61	2.33	99.45	16.00
Intangible CapEx/Assets (%)	6,605	1.44	-1.62	0.23	26.39	3.83
Intangible Assets ratio (%)	6,838	3.08	0.00	0.00	48.67	8.22
Employment growth (%)	6,754	3.48	-174.31	1.90	193.16	21.92
Sales growth (%)	6,754	6.52	-65.04	5.98	82.02	21.74
EBITDA margin (%)	6,804	10.15	-24.90	8.03	57.73	12.17
(2) Control Variables						
Log. Assets	6,838	11.37	8.24	11.30	15.15	1.48
Log. Intangible Assets	6,840	3.26	0.00	0.00	12.28	4.22
EBIT over Assets (ROA, %)	6,753	15.48	-35.69	12.29	91.47	20.29
Leverage ratio (%)	6,837	68.16	13.70	71.23	99.80	20.49
Cash ratio (%)	6,838	14.48	0.00	6.89	74.95	17.94
Group situation (dummy)	6,840	0.20	0.00	0.00	1.00	0.40
Panel B: Aggregated local industry						
Capital Expenditure/Assets (%)	2,617	6.10	-12.90	4.80	39.30	7.20
Intangible CapEx/Assets (%)	2,617	0.90	-0.70	0.50	8.40	1.30
Intangible Assets ratio (%)	2,624	2.20	0.00	0.70	30.30	4.50
Employment growth (%)	2,617	1.40	-200.00	1.50	200.00	23.20
Sales growth (%)	2,617	4.10	-91.90	4.90	89.40	21.70
EBITDA margin (%)	2,624	8.90	-7.70	7.30	35.60	6.90
(2) Control Variables						
Log. Assets	2,628	14.28	8.30	14.32	19.59	2.00
Log. Intangible Assets	2,628	8.94	0.00	9.47	15.28	3.55
EBIT over Assets (ROA, %)	2,624	7.80	-14.30	7.10	36.00	7.40
Leverage ratio (%)	2,624	56.20	20.70	56.50	85.50	12.80
Cash ratio (%)	2,624	13.60	0.50	11.40	54.30	10.10

The table presents summary statistics for our regression analysis variables, including the number of observations, mean, median, minimum, maximum and standard deviation for each variable. Detailed variable definitions are in appendix A. Ratios and indicator variables are stated in percentage terms. Panel A used for PE target firm impact spans three years pre- and five years post-intervention for each treated and control firm from 1998 to 2021. Given the one-to-two matching, treated firms represent 33% and control firms 67% of the sample. Financial variables are winsorized at 1% and 99% to mitigate extreme value effects. Panel B used for spillover impact of PE investments consists of observations from year 1998 to 2021. Local industry is matched on a one-to-one basis, making treated and control firms represent 50% respectively.

5 Empirical Strategy

In this section, we outline the empirical strategy we use to test our hypotheses. To isolate the effect of PE buyouts, one would ideally first randomize the targets that PE firms acquire, capture the development over time and, second, compare the results to the development of the exact same firm (or local industry peers) in a parallel universe where the PE firm did not complete the acquisition. While this fictive and idealized empirical setting cannot be constructed, it eloquently conveys the main challenges related to our study. In the following paragraphs, we explain these challenges and the deliberate choices we make to increase the reliability of the results. Then we move on to the models used to test firm-level outcomes and local industry-level outcomes, respectively.

Endogeneity concerns pervade empirical research, but understanding the setting-specific endogeneity concern in PE is critical (Borysoff et al., 2023). The reason is that the direction of causality of discretionary actions such as a PE investment can go two ways. Bernstein et al. (2017) express it neatly: “Does the presence of PE lead to higher production or do PE investors invest where they anticipate industries to grow?” in their study on spillover effects from PE investments. Because PE firms choose their buyout targets deliberately (Sorensen & Yasuda, 2022), a critical question arises: How can we discern whether the outcomes observed in PE targets are due to the influence of the PE firm or merely a result of the target's inherent potential that led to its selection? A definite answer is elusive because it is impossible to observe the counterfactual trajectory of the PE target had the acquisition not taken place. Thus, we do not attempt to establish causality through our study but rather investigate any associations present while attempting to alleviate endogeneity concerns to the greatest extent possible.

The closest approximation to the ideal for our observational setting is performing a matching procedure to construct control groups of matched firms and local industries to our treated observations. The control and treatment groups should follow similar trends and exhibit similar characteristics one year before the buyout occurs. For an in-depth explanation of our matching procedure, refer to the data section. The rationale behind matching is that the more reminiscent the control observations are of the treated observation prior to the transaction, the likelier it is that they could have been the target of a PE fund as well. This indicates that any subsequent divergence in outcome can be more confidently attributed to the impact of the PE firm's intervention rather than pre-existing trends. In the subsequent sections, we explain how we construct our models and discuss the rationale behind our control variables.

5.1 Firm-level outcomes

In the first part of our study, we examine firm-level impact after being acquired by a PE firm. We estimate the effects on PE buyout targets over time using time-varying treatment variables for the pre and post treatments periods. First, we study the total effects during the pre- and post-period for three years prior and three years post the transaction. This event window is in line with previous research and is intended to capture effects from operational changes typically implemented early in the holding period (Boucly et al 2011; Olbert & Severin 2023). Second, we employ a leads and lags setting to study the yearly development and extend the analysis to five years after the transaction to capture potential time delays until operational changes have been implemented and the effect shows.[5] For both analyses we use the same regression framework shown in Equation 1.[6] Thus, our firm-level tests are conducted using the following fixed effects regression model, which follows that of Olbert & Severin (2023):

$$Y_{it} = \sum_{t=-3}^{T=5} \beta_t \times Treated_i \times D_{it} + \sum_{t=-3}^{T=5} \gamma_t D_{it} + \eta \times Controls_{it} + \alpha_i + \delta_t + \varepsilon_{it} \qquad (1)$$

where Y_{it} denotes the outcome variable of interest, i indexes firm and t indexes year relative to transaction. The coefficient of interest β_t measures the differential impact of being a PE-owned firm in each specific time period. By including time-specific treatment effects, the model can capture how the impact of PE intervention evolves over time, possibly identifying immediate effects, short-term adjustments, and longer-term consequences on the outcome variable of interest. $Treated_{it}$ is an indicator variable that takes the value of 1 for treated firms. D_{it} is a vector of dummy variables indicating the event years t = -3 until T = 5 which is included to capture the effect of shocks and trends that are unique to each time period surrounding the transaction. The term $Controls$ is a vector of control variables further described below with the coefficient η for each variable. We include firm (α_i) fixed effects control for all unobservable characteristics that are unique to each firm and constant over time, such as corporate culture, brand strength or geographic location. We also include year (δ_t) fixed effects to account for any external factors affecting all firms uniformly in a given year, such as economic trends, market conditions or regulatory changes. ε_{it} represents the error term for firm i at time t, and captures the random disturbance or the unexplained variation in our outcome variable Y_{it} after accounting for the effects of the observed variables included in the model. We winsorize financial variables at the 1% and 99% levels to mitigate extreme value effects and prevent results from being driven by values not representative of the population. Standard errors are clustered at the firm-level to account for potential correlation in the standard errors within each firm.

Control variables are critical in our analysis of the performance of buyout targets following a transaction, as they allow us to distinguish the improvements attributable to PE rather than other factors that could explain our outcome variable of interest. In our model, we control for total assets, ROA, cash ratio, intangible assets, leverage ratio and group situation. *Total assets* are included to

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[5] Including up to 5 years post-acquisition period in our analysis, despite the typical 3-to-7-year exit horizon for PE firms, serves as a critical benchmark for comparing firm-level impact with spillover effects on local industry peers in the second part of our study. In addition, the event window in equation 1 can be manipulated to facilitate a +/- 3 year period, typically observed in similar studies that aim to identify immediate effects.

[6] The only difference in model specification between the pre and post and leads and lags model is that the vector of variables D_{it} is replaced by two dummy variables indicating the pre and post period.

account for size-related factors that can influence a firm's growth and performance, such as economies of scale and bargaining power. *ROA* controls for the overall financial performance of a firm. *Cash ratio* captures firms' liquidity, which reveals its ability to meet short-term obligations, seize investment opportunities and weather economic downturns. *Intangible assets* signal the value of a firm's intellectual property and brand strength, which contributes to its competitive advantage. The amount of intangible assets present in a firm is likely also an important driver for further investments into intangible assets that we wish to control for. *Leverage ratio* is a measure of financial risk and an indicator of a firm's ability to finance new investments. *Group situation* indicates whether a company is an independent entity or parent company in a group. We include this variable as single entities and parent companies may carry costs and take on investments that are not borne by subsidiary companies. Each of these variables posits explanatory power in the tests we perform, and by incorporating them into our model, we aim to reinforce the credibility and robustness of our results.

5.2 Local industry-level outcomes

In the second part of the study, we want to capture the spillover effects to local industry peers as a PE buyout occurs using our dataset of aggregated municipality-industry data. The aggregated data excludes all identified PE buyout targets and other firms that are majority-owned by the target's parent. In this way we can ensure that any observed associations in our analysis can be attributed to spillover effects. Similar to the approach in the previous section, we first study the total effects during the pre- and post-period from three years prior until five years after the transaction. We include all five years in the analysis to capture the potential time lag in the occurrence of spillovers. Secondly, we employ a leads and lags model to study the yearly effects. We estimate the association between PE buyouts and local industry spillovers using the same regression equation as in (1) adapted to the municipality-industry level analysis:

$$Y_{mt} = \sum_{t=-3}^{T=5} \beta_t \times Treated_m \times D_{mt} + \sum_{t=-3}^{T=5} \gamma_t \times D_{mt} + \eta \times Controls + \alpha_m + \delta_t + \varepsilon_{mt} \tag{2}$$

where Y_{mt} represent the aggregate impact on the outcome variable of interest in municipality-industry *m* and year relative to transaction *t*. We maintain the same event window as in our firm-level analysis. The coefficient of interest β_t measure the differential impact on the local industries subject to a buyout, and thus potential following spillover effects. $Treated_m$ is a time-invariant dummy variable indicating whether at least one firm incorporated in the municipality-industry *m* is acquired by a PE firm. D_{mt} is a vector of dummy variables indicating the event years *t=-3* until *T=5*. Municipality-industry fixed effects α_m control for all time-invariant characteristics of the local industry economies that may occur. δ_t denotes year fixed effects. ε_{mit} represents the error term for municipality-industry *m* at time *t*, capturing any unexplained variation in our outcome variable Y_{it}. We winsorize financial variables at the 1% and 99% levels and cluster standard errors at the municipality-industry level.

The control variables included in the local industry analysis are the same as in the firm-level analysis with the exception that the variable *Group situation* is removed. The rationale for including the same variables as in the firm-level analysis is that our controls maintain the same logic for aggregated data. For example, the leverage ratio will indicate the financial risk and financing ability of the companies, on average, within a municipality-industry in the same way as it does for a single company. The control variables included are total aggregated assets, ROA, leverage ratio, cash ratio and aggregated intangible assets.

To further alleviate endogeneity concerns and add robustness, we conduct three additional tests following our analysis. First, we exclude any impact of large cities that may have caused inconsistent matches and thus influence our outcomes. Second, we test the effects based on PE deal size. Third, we examine how spillover effects vary across different levels of market competition. These are further described in section 7.2.

6 Firm-level results and discussion

In the following section, we present and discuss PE firms' impact on buyout targets. The section serves two purposes. First, it adds to the documented effects on operating performance in buyouts. Second, it sets the stage for our subsequent chapter on spillover effects in local industries. Our investigation seeks to not only serve as a complement to earlier findings but also deepen the understanding of the specific operational levers employed by PE firms in our sample. By identifying patterns of how PE firms intervene in targets, we can better understand the rationale behind spillover effects occurring in the local industries.

In column 1 of Table 4, we find that PE-backed companies increase investments more than non-PE-backed companies following a buyout. The post-treatment effect on capital expenditure, running from year $t = 0$ to $t = 3$, is statistically significant and large in magnitude. The increase of 2.91% in capital expenditure scaled by assets is equivalent to an average increase in capital expenditure of ~43% or 7.7m SEK relative to controls.[7] Annual effects, depicted in Figure 2, indicate a spike in investment directly following an acquisition and in the first full year of ownership. This positive effect, however, dissipates from the second year onwards, becoming statistically non-significant.[8] These findings are consistent with both the direction and magnitude established by prior literature, such as Olbert & Severin's (2023) study of 11,000 European deals, which reported a 35% increase in a similar context.

We find a positive significant post-effect for the intangible capital expenditure coefficient in column 3, supporting our hypothesis. Figure 2 shows that buyout targets exhibit a similar trajectory for investments in intangibles to the one identified in capital expenditure, with a statistically significant impact starting immediately following the transaction. In the first full year of ownership, buyout targets more than double their investments in intangible assets compared to controls, with an average increase of 4.2m SEK.[9] Corresponding evidence is supported in the alternative outcome variable intangible asset ratio (see Appendix E), where buyouts exhibit a 55% higher ratio than the control group, rendering robustness to our results. A subtle difference is that the higher intangible ratio persists over time, meaning that while the investments in intangible assets do not markedly exceed those of the control group in the subsequent years, the proportion of intangible assets to total assets does. In the context of our study, intangible assets are indicative of a targeted investment in information technology. This interpretation is in line with (Olsson & Tåg, 2017), who noted a similar trend in IT investments, and further backed by evidence from a survey study of PE firms conducted by Gompers et al. (2016). In this survey, 33.5% of respondents specified upgrades in IT as a pivotal source of value creation in their buyouts. This evidence strongly suggests that

[7] For reference, these effects are calculated by comparing the coefficient (2.91) to the mean (6.77), and by multiplying the coefficient (2.91) with the average total assets (264m SEK) for firms in our sample.
[8] Full disclosure on variable coefficients, standard errors, and significance for leads and lags model is found in Appendix C and D.
[9] Note that annual effects are computed by multiplying the event year coefficient (1.59) with average total assets in the year of interest, i.e., in this case $t = 1$ (265m SEK).

Table 4. Firm level: Pre- and post-buyout outcomes with and without visible control variables

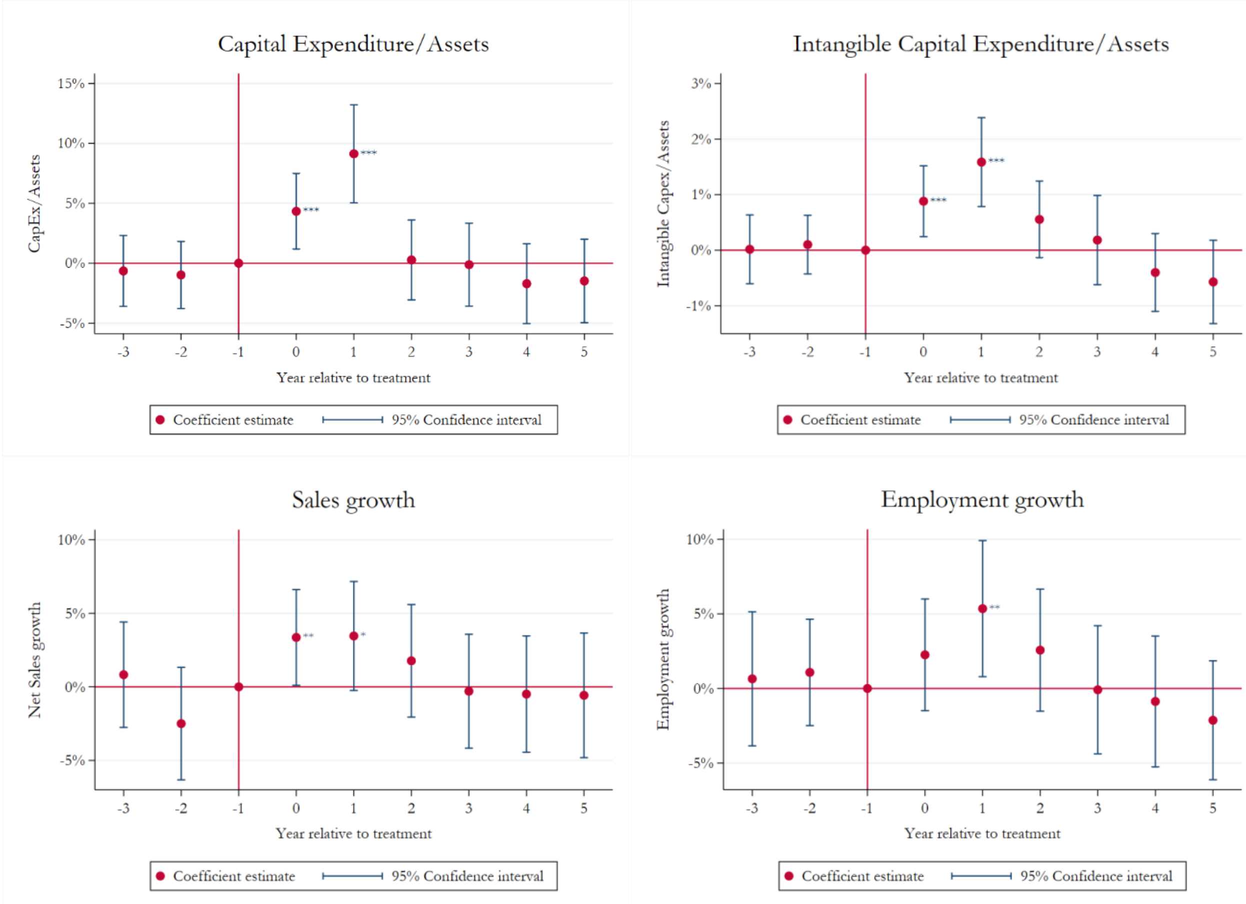
	CapEx/Assets (%)		Int. CapEx/Assets (%)		Sales growth (%)		Empl. growth (%)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Pre * Treated PE	-0.87 (1.31)	-0.72 (1.39)	0.08 (0.26)	0.05 (0.28)	-0.74 (1.63)	-0.85 (1.76)	0.91 (1.73)	1.00 (1.76)
Post * Treated PE	2.91** (1.35)	6.50*** (1.40)	0.70*** (0.26)	1.50*** (0.29)	1.73 (1.47)	3.47** (1.59)	2.00 (1.66)	4.16** (1.63)
Log. Assets	9.12*** (1.01)		0.96*** (0.23)		7.85*** (1.39)		8.85*** (1.24)	
Log. Int. Assets	0.49*** (0.13)		0.47*** (0.04)		0.17 (0.16)		0.38** (0.19)	
ROA (%)	12.74*** (2.32)		1.40*** (0.48)		51.93*** (3.24)		12.60*** (2.90)	
Leverage ratio	2.26 (2.75)		-0.37 (0.58)		18.34*** (3.42)		9.80*** (3.65)	
Cash Ratio	24.84*** (2.42)		-2.35*** (0.63)		-10.14*** (3.06)		-0.98 (3.54)	
Group Situation	1.53 (1.18)		0.47* (0.25)		2.98* (1.78)		3.25 (2.11)	
Observations	5,505	5,505	5,406	5,406	5,538	5,540	5,538	5,540
Adj. R^2	0.19	0.12	0.36	0.28	0.26	0.14	0.13	0.10
Controls	Y		Y		Y		Y	
Firm FE	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y

This table presents the estimates from a time-varying fixed effects regression model. All specifications include firm and year fixed effects. Odd numbered columns include firm-level control variables. Columns report estimates for capital expenditure/assets, intangible capital expenditure/assets, sales growth (%), and employment growth (%). Refer to Appendix A for variable definitions. The event window used runs from 3 years prior until 3 years post. To facilitate interpretation of our findings, the event years t = -3 to t = *-2* from equation (1) are aggregated into a single *Pre* and *Pre * Treated PE* dummy. Similarly, event years t = 0 to t = 3 are aggregated into a corresponding *Post* and *Post * Treated PE* dummy. Event year t = -1 is omitted due to collinearity and event years t = 4 to t = 5 are excluded to capture immediate post-buyout effects. *Treated PE* is equal to 1 for firms subject to a PE buyout and 0 for controls. Ratios are multiplied by 100 to facilitate the interpretation of coefficients in percentage terms. We report the standard error clustered on firm-level within parentheses. Asterisks denote the significance of coefficients ($* \, p < 0.1,\ ** \, p < 0.05,\ *** \, p < 0.01$).

investments in information technology are not merely incidental but a strategic focus for PE firms. As we have noted, our investigation finds empirical data to back these statements up. It also makes intuitive sense: the context of our study – Swedish PE deals between 2001 and 2019 – coincides with technological advancements and a generally growing emphasis on digital transformations in business operations. Thus, it is plausible that PE firms, recognizing this trend, have actively pursued the digital transformation of their portfolio companies to create value for shareholders.

The observed sales growth in the post-buyout period for targets is 1.73% greater than comparable controls (column 5), but not statistically significant. Though, the first full year post-acquisition

Figure 2. Firm-level outcome in variables of interest



The figure shows the coefficient estimates and confidence intervals before and after a buyout occurs for our outcome variables of interest. All estimates include firm and year fixed effects as well as control variables. Ratios are multiplied by 100 to facilitate the interpretation of coefficients in percentage terms. Asterisks denote the significance of coefficients ($*\ p < 0.1,\ **\ p < 0.05,\ ***\ p < 0.01$).

shows a 3.46% significant growth in sales relative to similar firms. If anything, these results could be an effect of PE firms targeting companies with inherent sales growth potential, as further indicated by the annual average sales growth of 6.52% in our sample. Such a subtle sales increase is perhaps more in line with prior evidence from Bergström et al. (2007) who observed a 0.71% increase in sales, and less pronounced than the 11% increase observed by Boucly et al. (2011). Further, we observe no significant effect on the EBITDA margin in the post-buyout period or annual years (see Appendix D). These findings contrast those of Bergström et al. (2007) and Boucly et al. (2011), and could be an inherent consequence of looking at single-entity data. Single entity data does not capture the financial impact in other operating units if separate group entities exist, nonetheless the results on financial performance are not in line with our hypothesis.

When considering PE firms' effect on the number of employees in the target firm in column 7, we observe a positive but non-significant coefficient in the post-buyout period, compared to controls. However, we do observe an increase of employees of 5.35% in the target firm in the first full year of ownership, statistically significant at the 5% level (see Figure 2) which lends credence to our hypothesis. This effect, however, does not persist beyond the first full year, as indicated by the declining trend emerging. We acknowledge that the annual net change in employment cannot distinguish PE targets that replace its workforce during the year, which prevents us from fully capturing all employment impact. We do note that following year three, the coefficient, while non-significant, turn negative, indicating that, on average, buyout targets grow the number of employees less than comparable firms. This could indicate a decreased focus on growth and increased focus on profitability in the later stages of the holding period.

When control variables – representative of the firm-specific financial health – are excluded, the magnitude and significance of the identified effects in outcome variables are generally amplified. The control variables show significant effects for at least one of the outcome variables, indicating that selected controls help reveal positive correlations, thereby adding to the predictive power of our models' effects. Our control variable outcomes generally follow our expectation, e.g., size and profitability being positively correlated with all outcome variables.

Our analysis reveals relatively wide confidence intervals and low precision, which can be partly attributed to the limited number of PE deals in our sample. To provide comfort in our findings, we test statistically significant outcome variables using alternative metrics. In doing so, we test that our results are consistent across various performance measures. Generally, the alternative metrics are consistent with our main results. Refer to Appendix E for a full analysis of the data construction and outcome of alternative results.

To conclude, our findings reinforce the idea that PE firms typically increase investments and employees immediately, with a notable increase in intangible assets. These findings help us anticipate spillover effects on local industries. The expanded investments and employment may serve as a signal to competitors within the local industry of a new, assertive operational approach. Using the arguments of Schmidt (1997), such signals can channel a variety of responses from competitors, influenced by the intensity of competition. Managerial effort increases as competition increases, indicating that spillover effects from PE investments could potentially incentivize peer firms to improve efficiency by following suit. However, if competition becomes too intense, managers may experience a decline in motivation, which could illustrate a channel leading to a lack of responsive actions by local industry competitors. Increased investments by PE-backed firms may also cause direct spillover effects to non-competitors. For example, the addition of a new local facility can have direct positive impacts on local businesses through subcontracting arrangements. The influence of sales growth is not straightforward. While an uptick in sales could disadvantage local peers if the market share is being redistributed, it may not adversely affect them if the growth is due to market expansion. Thus, the net effect on local industries partly hinges on whether the buyout targets are capturing existing demand or creating new demand.

7 Local industry results and discussion

In the following sections, we present and discuss the results of our local industry-level analysis. For this section, we exclude the PE targets and any affiliated companies' financials, implying that this section is entirely dedicated to the spillover effects on other non-PE-acquired firms. The chapter is divided into two parts. First, we examine the effect on local industry. Second, we compute additional tests rendering robustness to our results and discuss the impact of market concentration.

7.1 Results of main analysis

The results confirm our main hypotheses that PE buyouts are associated with spillover effects to peer firms in the local industry, as we find significant effects across multiple outcome variables. The observed effects are, however, only significant at the 10% level when looking at the total effects during the post-treatment period with varying magnitude. The table with yearly estimates and control variable coefficients is presented in Appendix F.

Beginning with investment outcomes, we find a positive but insignificant association between a PE buyout in the local industry and capital expenditure, as illustrated in column 1 of Table 5. For intangible capex in column 3, on the other hand, we observe a significant increase in local industries where a PE buyout has occurred compared to the control group. The increase amounts to 0.27 percentage points, which is relatively large given the sample mean of 0.9% intangible capex over assets. The absolute effect is, however, fairly small, representing an increase of circa 4.5m SEK per year in the median local industry.[10] Figure 3 highlights that the effect occurs mainly in years three and four, indicating that there is a time lag in the effects. The observed time lag and increase in intangible capex are further strengthened by our analysis of the intangible ratio, which shows a similar pattern in Figure 3 and a positive increase that is significant at the 10% level in column 5 of Table 5. The intangible ratio remains elevated for treated local industries until year five in Figure 3, suggesting that the observed increase compared to controls is sustained over time. In year five we observe a 0.9 percentage point increase in the ratio compared to the control group. This is equal to an addition of 14.8m SEK of intangible assets to the median municipality-industry balance sheet.[11]

The results are in line with our hypothesis that PE buyouts lead to increased intangible investments in the local industry. However, this is not the case for total capex over assets. Although the sign is as expected, the result does not align with our hypothesis or the results of Olbert & Severin (2023) who find small but significant increases in aggregate municipality capex after a PE buyout. The discrepancy may stem from differences in our firm-level observations. Our firm-level analysis aligns well with our results for intangible capex on a local industry-level, which indicates that

[10] When calculating the absolute impact, we use the median asset base for local industries in our sample. This is due to some treated local industries being much larger than controls as outlined in section 4.2.2 which affects the mean. The calculation performed is thus the observed coefficient (0.27) times the median asset base (1,648 m SEK)

[11] Calculated by taking the percentage point increase (0.9) times the median asset base (1,648 m SEK)

Table 5. Local industry: Pre- and post-buyout outcomes with and without control variables

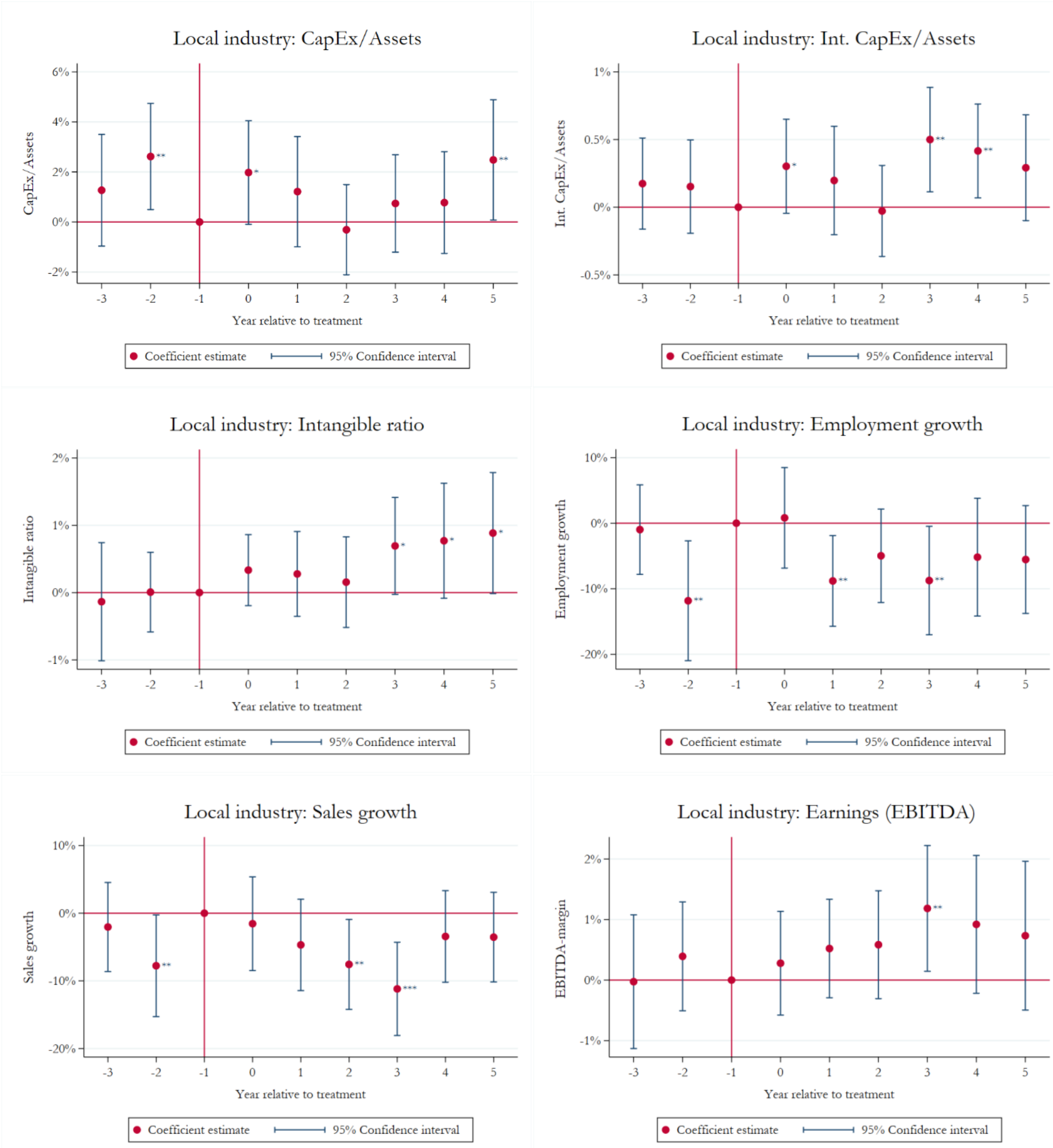
	CapEx/Assets (%)		Intangible CapEx/Assets (%)		Intangible Assets ratio	
	(1)	(2)	(3)	(4)	(5)	(6)
Pre * Treated PE	1.95**	1.97**	0.16	0.15	-0.06	-0.16
	(0.92)	(0.93)	(0.16)	(0.00)	(0.33)	(0.39)
Post * Treated PE	1.13	1.56**	0.27*	0.33**	0.50*	0.75**
	(0.80)	(0.79)	(0.14)	(0.00)	(0.30)	(0.37)
Observations	2,616	2,619	2,624	2,628	2,624	2,624
Adj. R^2	0.16	0.10	0.29	0.25	0.73	0.64
Controls	Y		Y		Y	
Firm FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
	Employment growth (%)		Sales growth (%)		EBITDA margin (%)	
	(7)	(8)	(9)	(10)	(11)	(12)
Pre * Treated PE	-6.49*	-6.74*	-4.93	-4.89	0.18	0.37
	(3.32)	(3.78)	(3.04)	(3.29)	(0.47)	(0.60)
Post * Treated PE	-5.35	-4.67	-5.32*	-4.29	0.69*	0.66
	(3.42)	(3.51)	(2.76)	(2.90)	(0.40)	(0.47)
Observations	2,616	2,617	2,616	2,617	2,624	2,624
Adj. R^2	0.08	-0.05	0.10	0.01	0.81	0.68
Controls	Y		Y		Y	
Firm FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y

This table presents the estimates from a time-varying fixed effects regression model. All specifications include firm and year fixed effects. Odd numbered columns include local industry-level control variables Log. assets, Log. intangible assets, Ebit over assets (ROA, %), leverage ratio, and cash ratio. Columns report estimates for capital expenditure/assets, intangible capital expenditure/assets, intangible asset ratio, employment growth (%), sales growth (%), and EBITDA margin (%). Refer to Appendix A for variable definitions. The event window used runs from 3 years prior until 5 years post. To facilitate interpretation of our findings, the event years t = -3 to t = -2 from equation (2) are aggregated into a single *Pre* and *Pre * Treated PE* dummy. Similarly, event years t = 0 to t = 5 are aggregated into a corresponding *Post* and *Post * Treated PE* dummy. Event year t = -1 is omitted due to collinearity. *Treated PE* is equal to 1 for firms subject to a PE buyout and 0 for controls. Ratios are multiplied by 100 to facilitate the interpretation of coefficients in percentage terms. We report the standard error clustered on local industry-level within parentheses. Asterisks denote the significance of coefficients ($*\ p < 0.1,\ **\ p < 0.05,\ ***\ p < 0.01$).

investment spillover effects follow the activities of the PE target, i.e., if the PE target increases intangible capex, so do local peer firms. In contrast to our study, Olbert & Severin (2023) find large increases in tangible capex on a firm-level, which typically is larger than intangible capex, implying greater total capex spillover effects.

The observation that investment spillovers follow PE target activity supports both the idea of knowledge exchange and competition driving spillover effects. Building on Schmidt (1997), who suggests that peer firms react to increased competitiveness, it may be that local peers follow the buyout targets in increasing intangible capex to be able to survive intensified competition in the

Figure 3. Local industry-level outcome in variables of interests



The figure shows the coefficient estimates and confidence intervals before and after a buyout occurs for our outcome variables of interest. All estimates include firm and year fixed effects as well as control variables. Asterisks denote the significance of coefficients (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$).

local market. However, as shown by Giroud et al (2021) local peers may also learn from and adopt best practice standards implemented by the PE firms such as new IT systems or increased R&D. The low absolute level of investment spillovers could further suggest that the effect is concentrated to a few firms within the local industry rather than permeating throughout all firms. Arguably, only firms who have the capacity and need would pursue such intangible investments. We further explore the settings under which these spillovers occur in section 7.2. Regardless of the specific dynamics, the results show a clear albeit small effect of PE buyouts spurring on intangible investments. This could indicate that PE buyouts facilitate digital transformations in local industries, which can be seen as a positive spillover that makes the local industry more competitive on a national level.

Moving to our results on employment and sales growth in columns 7 and 9, we find similar negative trends in both variables, although this is only significant for sales and only at the 10% level. During the treatment period, we observe a decrease of 5.3 percentage points in the sales growth rate of treated local industries compared to the control group. Figure 3 illustrates that the negative effect on sales growth intensifies until year three when the coefficient indicates an 11.2 percentage point decrease in sales growth compared to the control group. Similarly, we find in Figure 3 a negative effect on employment growth equivalent to 8.8% in year one and three. These are fairly large differences on an aggregated municipality-industry level. Further, the results do not align with the observations made by Aldatmaz & Brown (2020) and Bernstein et al. (2017) who both observe positive growth on a country-industry level as a result of PE buyouts. One explanation of the negative effects on employment could be that PE targets are attracting employees from peers as they expand, given that they likely share the same labor market. Such reallocation of workers is not necessarily a negative outcome for the local industry but is unlikely to explain the full magnitude of the observed effect. Another explanation would be that PE-owned firms toughen negotiations with local suppliers and potentially reduce their business. The results further suggest that there may be competitive effects in line with Hsu et al. (2011) who observe that the operating performance of core competitors of PE-acquired firms declines after the transaction.

To further understand the observed employment and financial performance development, we turn to the EBITDA margin. Our results shown in column 11 of Table 5 indicate a positive impact in treated observations significant at the 10% level, which is contrary to our result on sales growth. The effect amounts to 0.69 percentage points representing an added 15.2 million SEK of profit per year for the median municipality-industry.[12] This result seems plausible given Bergström et al. (2007) who observe a 2.31% increase in EBITDA margin in PE-acquired firms. However, it does seem somewhat counterintuitive that there would be a negative effect on sales growth but a positive on profitability in local industries from PE buyouts. These effects can best be reconciled by considering competitive responses, knowledge spillovers and the typical actions of a PE firm. As observed in our firm-level section as well as previous studies (e.g., Boucly et al., 2011) buyout firms often pursue growth strategies. An important aspect of this is whether the PE-owned firms take market share from rival firms or create new demand. Our negative results on sales growth indicate that there may be some market share captured by PE targets in the local market. On the

[12] Calculated using the increase of 0.69 percentage units times the median sales value (2.2b SEK)

other hand, intensified competition, as well as best practice spillovers, may lead local peers to slim down their organizations and become more cost-conscious, ultimately leading to improved profitability. This could potentially explain the negative trend in employment growth as well. To further reconcile the observed effects, one can study where and when they occur, as each effect may be more or less prevalent depending on the market setting, which we do in section 7.2.

A noteworthy observation on sales growth and EBITDA margin is that variables become non-significant when the control variables are excluded, which can be seen in Table 5. The sign and the approximate magnitude of control variables persist, however, showing that the association still holds. This highlights that the control variables have a significant effect on the analysis. The full table showing all control variables is presented in Appendix F. In general, we see that ROA drives total capex over assets whereas Log Intangible Assets is more important for intangible capex over assets. The size indicated by Log Assets, is strongly associated with growth in employment and sales as well as the profitability of the local industry. An interesting observation is that Log Assets are not associated with higher capex levels as it were in the firm-level analysis, indicating that local industries of various sizes invest proportionally the same.

In our results, there are a few further statistical observations worth noting. The adjusted R-square is very low for sales and employment growth, indicating that the explanatory power of our model is poor. Low adjusted R-square values for growth variables in the range of 0.1 or below are, however, in line with previous literature (Olbert & Severin, 2023). A limitation of this study on the other hand is that we do not account for macro factors such as GDP and demography developments in specific municipalities and regions which may provide further insight into local industry developments. Another important observation is that there are some significant pre-trends in our results for capex over assets and employment growth. This indicates a risk that the treatment and control groups are not sufficiently similar prior to the treatment. As observed in section 4.2.2. there is a fairly large difference in means between our treatment and control groups in size due to the limited dataset of Swedish municipalities and inability to match the largest local industries to equivalent counterparts. Thus, there is an inherent risk that the treatment and control group follow different trends prior to the transaction. Zooming in on the observed pre-trends in Figure 3 we see that they are concentrated to year $t = -2$ and not a general trend for the entire pre-period. This provides us with some comfort, but to further alleviate concern that our results in general would be due to pre-existing trends and not PE, we dedicate the following section to further analysis.

7.2 Robustness & further analysis

Because our study on local industries is the first of its kind, we place additional emphasis on robustness and further analysis.[13] Our sample contains a wide array of different local industries when it comes to the size and composition, and because PE buyouts are not random, it is crucial to understand how and where the observed spillover effects occur. We build robustness and extend our analysis in three steps. First, we estimate the impact of excluding the three largest cities in Sweden. Second, we estimate the impact of PE deal size. Third, we estimate the impact of market concentration. These tests are computed for the three outcome variables with the strongest spillover effects: intangible capex, sales growth and EBITDA margin (as illustrated by section 7.1).[14]

7.2.1 Excluding large cities

As outlined in section 4.2.2, the treated local industries are, on average, larger than the control units. This is not surprising considering the high concentration of PE deals in big cities. To alleviate any worry of a potential bias that larger cities drive the impact of spillover effects, we compute additional tests excluding the three largest cities Stockholm, Göteborg and Malmö. These tests reveal trends consistent with our prior findings with two minor exceptions: EBITDA becoming marginally stronger, and intangible capital expenditure decreasing slightly. Thus, any captured spillover effects are unlikely a consequence of comparing large municipalities to smaller ones. Refer to Appendix G for tests, analysis and additional comments.

7.2.2 The effect of PE deal size

To further limit endogeneity concerns from pre-existing trends, we test whether there is a difference in spillover effects between small and large PE deals. If PE buyouts are made in local industries that are already subject to observed spillover effects, i.e., rising profitability, investments and so on, we would not expect there to be a difference in spillovers between small and large PE deals. On the other hand, if the observed spillovers occur as a consequence of PE buyouts, we would expect large deals to show greater spillover effects. To compute this test, we divide the dataset based on the median sales of the buyout targets in the matching year (t = -1). We only include buyout targets that are used to treat local industries.[15] The lower half of the sample consists of local industries subject to a PE deal where the target had below 190m SEK in sales and their matched control group. Similarly, the upper half consists of PE deals with targets of above 190m SEK in sales. Our test renders the exact same regression analysis as in section 7.1 and results are presented in Table 6.

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[13] While Olbert & Severin (2023) use a similar empirical strategy, they focus on municipalities. Our approach is distinct in that we combine municipalities and industries, rendering several implications for accurately handling the empirical strategy.

[14] Intangible asset ratio also showed significant effect in section 7.1, but is excluded from the following tests due to its similarity to intangible capex.

[15] Because local industry treatment is based on the presence of a PE deal, any subsequent deals in treated local industries are excluded from the subgroups split on the median.

Table 6. Local industry-level outcome variation by PE deal size

	Intangible CapEx/Assets (%)		Sales growth (%)		EBITDA margin (%)	
Small deal size	(1)	(2)	(3)	(4)	(5)	(6)
Pre * Treated PE	0.08 (0.19)	0.05 (0.20)	-11.87*** (4.30)	-10.10** (4.55)	0.75 (0.64)	1.58* (0.86)
Post * Treated PE	0.02 (0.18)	0.05 (0.18)	-8.37** (4.19)	-6.89 (4.30)	0.34 (0.59)	0.29 (0.70)
Observations	1,314	1,314	1,313	1,313	1,318	1,318
Adj. R^2	0.23	0.21	0.10	0.00	0.84	0.71
Controls	Y		Y		Y	
	Intangible CapEx/Assets (%)		Sales growth (%)		EBITDA margin (%)	
Large deal size	(7)	(8)	(9)	(10)	(11)	(12)
Pre * Treated PE	0.26 (0.25)	0.24 (0.26)	0.38 (4.47)	0.43 (4.72)	-0.52 (0.70)	-0.88 (0.81)
Post * Treated PE	0.56** (0.23)	0.60** (0.24)	-3.29 (3.76)	-1.65 (3.86)	1.05** (0.50)	1.03* (0.60)
Observations	1,303	1,303	1,303	1,304	1,306	1,306
Adj. R^2	0.32	0.26	0.13	0.02	0.76	0.64
Controls	Y		Y		Y	

This table presents the estimates from a time-varying fixed effects regression model. All specifications include firm and year fixed effects. Odd numbered columns include local industry-level control variables Log. assets, Log. intangible assets, Ebit over assets (ROA, %), leverage ratio, and cash ratio. Columns report estimates for intangible capital expenditure/assets, sales growth (%), and EBITDA margin (%) for small PE deals in columns numbered 1 to 6, and for large PE deals in columns numbered 7 to 12. The PE deal size groups are split by the median sales for the buyout target during the matching year, i.e., t = -1. Refer to Appendix A for variable definitions. The event window used runs from 3 years prior until 5 years post. To facilitate interpretation of our findings, the event years t = -3 to t = *-2* from equation (2) are aggregated into a single *Pre* and *Pre * Treated PE* dummy. Similarly, event years t = 0 to t = 5 are aggregated into a corresponding *Post* and *Post * Treated PE* dummy. Event year used for matching, t = -1, is omitted due to collinearity. *Treated PE* is equal to 1 for firms subject to a PE buyout and 0 for controls. Ratios are multiplied by 100 to facilitate the interpretation of coefficients in percentage terms. We report the standard error clustered on local industry-level within parentheses. Asterisks denote the significance of coefficients ($* p < 0.1,\ ** p < 0.05,\ *** p < 0.01$).

Our results show that larger deals have greater spillover effects for intangible capital expenditure (in column 1 and 7) and EBITDA margin (in column 5 and 11), limiting any concern that spillover effects are due to pre-existing trends. For these two variables, essentially all spillover effects are concentrated in large PE deals, with highly significant positive values in the post-buyout period for large deals and coefficients around zero for small deals. From the coefficients on sales growth, we can infer that much of the negative pre-trends observed in section 7.1 are concentrated to local industries with small PE deals. This could be a random anomaly, which we do not dare speculate in, other than to conclude that our matching procedure was unable to capture this pre-trend in the treated sample.

7.2.3 Effect of market concentration

Finally, we look at how market concentration impacts spillovers from PE buyouts in the local industry. We do this for two reasons. Firstly, it can provide insights into how and where spillover effects occur. Secondly, it can further limit concerns that observed effects are not due to the PE buyouts. If there is a difference in spillover effects based on market dynamics unrelated to the observed outcome variables, it could suggest that the observed effects are due to spillovers. Furthermore, market concentration can act as a proxy for competition with low levels of concentration indicating higher competition. Previous studies suggest the level of competition can impact spillovers (Aldatmaz & Brown, 2020). We estimate market concentration by taking the average market share of the three largest companies in each treated municipality-industry during the three years preceding the buyout (t = -3 to t = -1). We subsequently divide the dataset into two based on the median market concentration and perform the same regression analysis as in section 7.1. The results are presented in Table 7.

The analysis shows that the spillover effects occur differently depending on the outcome variable that we look at, but the general trend is that the spillover effects for each variable occur to a greater extent in one of the market concentration states. For EBITDA margin, we can see that the spillover effects to peers are stronger in low concentration markets, proxying a highly competitive market. This is in line with the results of Aldatmaz & Brown (2020). For intangible capex and sales growth on the other hand, we see that the effect is stronger in local industries with high market concentration than low concentration. The differing result indicate that there are different mechanisms at play in creating the respective spillover effects.

Aldatmaz & Brown (2020) argue that competitive pressures are an important factor in creating spillover effects. This is in line with Schmidt (1997) who finds that managerial efforts increase in a competitive setting, i.e., competitive pressures make managers more inclined to adopt operational changes. When a PE firm enters a competitive market and implement operational improvements, local peers will potentially mimic the PE-owned firms' changes or implement their own. In a low competitive market, peer firms may not be as reactive to the actions of the PE-owned firms, which would explain the observed effect on EBITDA margin.

When it comes to intangible capex it may stem from a lack of competitive pressures. It is possible that firms in local industries with high market concentration to a greater extent than other firms are underinvested when it comes to intangible assets. Thus, it is first when a PE firm enters the local industry and increases the competitive pressures that they feel inclined to take on such investments. Alternatively, it could also be that it is easier for knowledge spillovers and best practice sharing to occur due to lower competition levels. Similarly, for sales growth, it is possible that the high concentration and historically low competition better enable PE firms to capture market shares.

Table 7. Local industry-level outcome variation by market concentration

High market concentration	Intangible CapEx/Assets (%) (1)	(2)	Sales growth (%) (3)	(4)	EBITDA margin (%) (5)	(6)
Pre * Treated PE	0.09	0.08	-7.04	-7.35	0.54	0.62
	(0.21)	(0.22)	(4.87)	(5.23)	(1.08)	(1.08)
Post * Treated PE	0.35*	0.37*	-6.91	-8.14*	1.17	0.35
	(0.19)	(0.19)	(4.49)	(4.80)	(0.73)	(0.79)
Observations	1,311	1,311	1,310	1,311	1,311	1,311
Adj. R^2	0.27	0.24	0.09	0.00	0.65	0.62
Controls	Y		Y		Y	
Low market concentration	**Intangible CapEx/Assets (%) (7)**	**(8)**	**Sales growth (%) (9)**	**(10)**	**EBITDA margin (%) (11)**	**(12)**
Pre * Treated PE	0.24	0.21	-2.36	-2.33	0.12	0.08
	(0.23)	(0.25)	(3.72)	(3.92)	(0.51)	(0.51)
Post * Treated PE	0.17	0.28	-2.48	-0.42	0.72	0.98**
	(0.22)	(0.24)	(3.15)	(3.13)	(0.44)	(0.46)
Observations	1,306	1,306	1,306	1,306	1,312	1,312
Adj. R^2	0.31	0.24	0.09	0.05	0.78	0.78
Controls	Y		Y		Y	

This table presents the estimates from a time-varying fixed effects regression model. All specifications include firm and year fixed effects. Odd numbered columns include local industry-level control variables Log. assets, Log. intangible assets, Ebit over assets (ROA, %), leverage ratio, and cash ratio. Columns report estimates for intangible capital expenditure/assets, sales growth (%), and EBITDA margin (%) for local industries with high market concentration in columns numbered 1 to 6, and for low market concentration in columns numbered 7 to 12. High market concentration occurs when the three largest companies represent at least 51% of the average local industry sales from event year t = -3 to t =-1. Low market concentration occurs in the opposite scenario. 51% is selected because it represents the median market concentration among *Treated PE*. Refer to Appendix A for variable definitions. The event window used runs from 3 years prior until 5 years post. To facilitate interpretation of our findings, the event years t = -3 to t = -2 from equation (2) are aggregated into a single *Pre* and *Pre * Treated PE* dummy. Similarly, event years t = 0 to t = 5 are aggregated into a corresponding *Post* and *Post * Treated PE* dummy. Event year t = -1 is omitted due to collinearity. *Treated PE* is equal to 1 for firms subject to a PE buyout and 0 for controls. Ratios are multiplied by 100 to facilitate the interpretation of coefficients in percentage terms. We report the standard error clustered on local industry-level within parentheses. Asterisks denote the significance of coefficients ($* p < 0.1$, $** p < 0.05$, $*** p < 0.01$).

8 Conclusion

PE investments have increased dramatically during the last two decades, making it a dominant force in today's economic landscape. During this time, research has focused on how PE impacts their portfolio companies, with less emphasis on stakeholder impact. Local industries are one important stakeholder to consider when assessing the impact PE firms have on society and the economy at large. However, identifying spillover effects is intricate and necessitates an initial understanding of the direct impact PE firms exert on their buyout targets. In our study, we first do a matched panel data comparison to examine firm-level impact using financial statement data and 278 Swedish PE deals. Second, we aggregate financials into local industries and use the same approach to study the impact in 149 unique local industries subject to PE buyouts.

We find that spillover effects on local industries exist following PE investments. More specifically, our evidence suggests that PE investments lead to increased investments in intangible assets among local industry peers. This effect is underpinned by the significant increase observed on a firm-level. Intangible assets likely reflect investments in information technology, suggesting that PE firms have actively pursued digital transformation opportunities in their portfolio companies, prompting local industry peers – competitors and non-competitors alike – to react. Additionally, we find that the intensified competition buyout targets invoke on local industries result in increased cost consciousness and improvement in earnings. Previously documented nation- and industry-wide spillover effects show that PE capital drives growth and productivity, which we find is not the result of spillover effects occurring on a local industry-level. Instead, we find that the effects of the expansion strategies employed in buyout targets can occur at the cost of local industry peers, resulting in adverse effects on sales and employment. Despite the seemingly modest magnitute of the impacts identified on local industries per case, the cumulative effect becomes paramount when considering the vast number of PE deals occurring globally. We add robustness and limit endogeneity concerns through evidence from three additional tests. First, we find that excluding large cities does not significantly impact our results. Second, we show that greater spillover effects occur in larger PE deals, enabling us to attribute the impact of spillover effects to PE buyout interventions more confidently. Third, we find variations in spillover effects across different competitive environments, further alleviating endogeneity concerns.

While most empirical evidence since the dichotomy between Jensen (1989) and Shleifer & Summers (1988) find that PE firms have a positive effect on other stakeholders, our study presents contrasting evidence, suggesting that spillover effects are not exclusively positive. These findings adds merit to the somewhat refuted views presented by Shleifer & Summers (1988) proclaiming that the value gains are extracted from other stakeholders. Thus, in addition to contributing to the limited literature on spillover effects from PE capital, we add to the academic debate on value creation versus extraction, which is not entirely settled. Furthermore, in the absence of clear one-directional effects, informing any policy decisions becomes a delicate task. We suggest awaiting further evidence before drawing any major conclusions on how local policy should be formulated to stimulate adequate outcomes.

We acknowledge that there are some limitations our study does not control for. One such limitation is that while our financial statement data is calendar-year-adjusted, the timing of the PE transaction is not. The deals in our study can therefore occur at any point in time between the beginning of January and end of December during the transaction year. This prevents us from distinguishing the pre-trend from PE impact in that year. This matters because buyout targets often run 100-day programs immediately following the transaction aimed at implementing sweeping operational changes. Nonetheless, while we cannot exclude the impact of the pre-trend in year $t = 0$, we fully capture any impact induced from PE firm intervention during the transaction year. Furthermore, our study might not fully capture the spillover effects of PE-backed firms on local industries when their operating units are located in different municipalities from their headquarters. This could potentially lead us to misinterpret the magnitude of spillover effects, if we attribute a rise in employment to the headquarters' municipality, while it occurs in other municipalities with operating units. However, even if we were able to track the municipalities where the operating units are located, it would significantly complicate the methodology for how we match treated and non-treated local industries.

We look forward to future research that classifies local industry peers into groups of competitive and non-competitive positions. One limitation of our study, as it investigates the aggregated impact, is that we do not capture which companies that are subject to the spillover effects. We acknowledge that a supplier may be subject to very different spillover effects than a direct competitor. By classifying local industry firms, tangible evidence on the rationale driving the spillovers can more effectively be obtained, i.e., are spillover effects primarily driven by competitive dynamics or knowledge transfers?

Grouping companies by municipality *and* industry, increases the relevance of local industry peers. However, due to our limited sample, we were forced to use a broad industry classification of 11 different industries. Such broad classification may limit the relevance of peers included as they may not be directly related to the PE buyout target, which could partially explain the limited magnitude of the spillover effects identified in our study. Thus, another avenue for further research would be to use a larger sample, potentially looking beyond the borders of Sweden, and use more narrow industry classification. This could also generate better matching statistics, thus providing additional comfort in the results.

9 Appendix

Appendix A. Variable definitions

Panel A: Firm-level	
(1) Dependent variables	
Capex / Assets	(fixed assets – lagged fixed assets + depreciation) / lagged total assets
Intangible capex / assets	(intangible fixed assets – lagged intangible fixed assets +(lagged intangible fixed assets / fixed assets)* depreciation) / lagged total assets
Intangible assets ratio	Intangible fixed assets / total assets
Employment growth (%)	(number of employees – lagged number of employees) / (0.5 × (number of employees + lagged number of employees)), following Davis et al. (2014), Antoni et al. 2019 and Olbert and Severin (2023)
Sales growth (%)	(net sales – lagged net sales) / (0.5 × (net sales + lagged net sales))
EBITDA margin	(operating profit + depreciation) / net sales
Log. Capital Expenditure	ln(1 + fixed assets – lagged fixed assets + depreciation)
Log. Intangible Capital Expenditure	ln(1 + intangible fixed assets – lagged intangible fixed assets + (lagged intangible fixed assets / fixed assets)* depreciation)
Log. Net sales	ln(1 + net sales)
Log. EBITDA	ln(1 + operating profit + depreciation)
(2) Control Variables	
Log. Assets	ln(1 + total assets)
Cash ratio	Liquid assets / total assets
Log intangible assets	ln(1 + intangible fixed assets)
Leverage ratio	(Total assets – total equity) / total assets
Group situation	Indicator equal to 1 if company is a parent in a group or single entity
Panel B: Municipality-industry level – All inputs represent firm numbers aggregated for each municipality-industry	
Capex / Assets	Capex / lagged assets
Intangible capex / assets	Intangible capex / lagged assets
Intangible assets ratio	Intangible fixed assets / total assets
Employment growth (%)	(number of employees – lagged number of employees) / (0.5 × (number of employees + lagged number of employees))
Sales growth (%)	(net sales – lagged net sales) / (0.5 × (net sales + lagged net sales))
EBITDA margin	EBITDA / net sales

The table outlines the variables used in our analysis. Panel A presents definitions for the firm-level sample. Data on PE deals are sourced from the Capital IQ database, while financial statement data is obtained from Serrano. For more specific definitions of underlying financial data components, please refer to the Serrano documentation file (2015). Panel B represents the dependent variables used for the local industry-level analysis. All inputs represent information that is calculated at the firm-level and then aggregated together for each municipality-industry. I.e., intangible capex is calculated for each firm based on the formula in Panel A before it is aggregated in Panel B. Control variables for the municipality-level analysis is calculated in the same as for the firm-level analysis but with the aggregated values of the inputs.

Appendix B. Distribution of discrete matching variables

	N	%
Industry selection		
10 - Energy & Environment	4	1.4
15 - Materials	7	2.5
20 - Industrial goods	67	24.3
22 - Construction industry	19	6.9
25 - Shopping goods	39	14.2
30 - Convenience goods	16	5.8
35 - Health & Education	20	7.2
40 - Finance & Real estate	7	2.5
45 - IT & Electronics	29	10.5
50 - Telecom & Media	11	4.0
60 - Corporate services	54	19.6
98 - Other (excluded in local industry analysis)	2	0.7

The table presents the distribution of treated firms across the discrete matching variables excluding year.

Appendix C. Firm-level investment outcomes with and without control variables

	Investment outcome variables					
	CapEx/Assets (%)		Int. Capex/Assets (%)		Intangible Ratio (%)	
	(1)	(2)	(3)	(4)	(5)	(6)
Event (t=-3) * Treated PE	-0.64 (1.51)	-0.29 (1.58)	0.02 (0.32)	0.00 (0.34)	0.14 (0.38)	0.22 (0.41)
Event (t=-2) * Treated PE	-0.98 (1.42)	-1.01 (1.51)	0.10 (0.27)	0.09 (0.29)	-0.11 (0.26)	-0.10 (0.29)
Event (t=-1) * Treated PE	.	.	.	.	.	.
Event (t=0) * Treated PE	4.33*** (1.61)	5.28*** (1.71)	0.88*** (0.32)	1.07*** (0.37)	0.75** (0.36)	1.02** (0.43)
Event (t=1) * Treated PE	9.13*** (2.08)	12.18*** (2.22)	1.59*** (0.41)	2.22*** (0.46)	2.21*** (0.53)	3.58*** (0.69)
Event (t=2) * Treated PE	0.27 (1.70)	4.18** (1.70)	0.55 (0.35)	1.43*** (0.37)	2.57*** (0.55)	4.55*** (0.73)
Event (t=3) * Treated PE	-0.12 (1.76)	4.31** (1.72)	0.18 (0.41)	1.27*** (0.42)	2.27*** (0.55)	4.62*** (0.72)
Event (t=4) * Treated PE	-1.71 (1.70)	3.07* (1.64)	-0.40 (0.36)	0.88** (0.36)	1.58*** (0.57)	4.36*** (0.74)
Event (t=5) * Treated PE	-1.48 (1.77)	3.68** (1.67)	-0.57 (0.38)	0.86** (0.39)	0.83 (0.58)	3.90*** (0.70)
Log. Assets	6.87*** (0.90)		0.64*** (0.17)		0.63* (0.36)	
Log. Int. Assets	0.55*** (0.11)		0.44*** (0.03)		1.09*** (0.08)	
ROA (%)	11.30*** (2.01)		0.83* (0.43)		-2.86*** (0.74)	
Leverage Ratio	2.79 (2.34)		0.00 (0.50)		-0.49 (1.04)	
Cash Ratio	-21.35*** (2.22)		-1.65*** (0.51)		-4.41*** (1.06)	
Group Situation	1.32 (1.10)		0.45* (0.27)		1.80** (0.71)	
Observations	6,711	6,712	6,600	6,601	6,750	6,838
Adj. R^2	0.18	0.12	0.35	0.27	0.74	0.64
Controls	Y		Y		Y	
Local industry FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y

This table presents the estimates from a time-varying fixed effects regression model. All specifications include firm and year fixed effects. Odd numbered columns include firm-level control variables. Columns report estimates for capital expenditure/assets, intangible capital expenditure/assets, and intangible asset ratio. Refer to Appendix A for variable definitions. *Events* indicate the year *t* relative to the transaction year for the PE buyout, with year t-1 omitted due to collinearity. *Treated PE* is equal to 1 for firms subject to a PE buyout and 0 for controls. Ratios are multiplied by 100 to facilitate the interpretation of coefficients in percentage terms. We report the standard error clustered on firm-level within parentheses. Asterisks denote the significance of coefficients ($* p < 0.1$, $** p < 0.05$, $*** p < 0.01$).

Appendix D. Firm-level financial performance outcomes with and without control variables

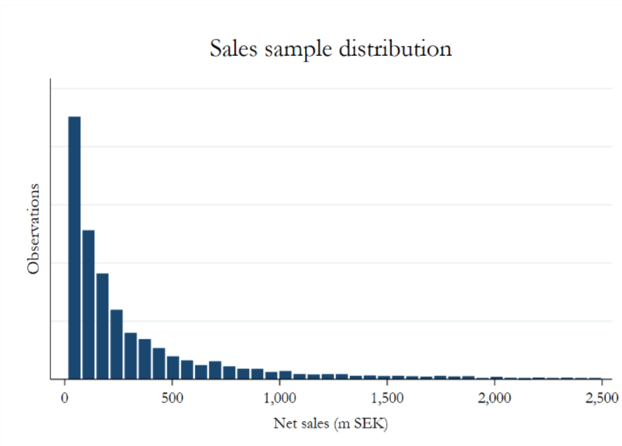
	Employment and financial performance outcome variables					
	Employment growth (%)		Sales growth (%)		EBITDA margin (%)	
	(1)	(2)	(3)	(4)	(5)	(6)
Event (t=-3) * Treated PE	0.65 (2.29)	0.73 (2.34)	0.83 (1.83)	0.64 (2.01)	0.23 (0.44)	-1.05* (0.63)
Event (t=-2) * Treated PE	1.08 (1.82)	1.20 (1.83)	-2.50 (1.95)	-2.31 (2.05)	0.66* (0.35)	0.46 (0.48)
Event (t=-1) * Treated PE	.	.	.	.	.	.
Event (t=0) * Treated PE	2.26 (1.91)	2.57 (1.89)	3.36** (1.66)	3.86** (1.82)	-0.02 (0.43)	0.48 (0.57)
Event (t=1) * Treated PE	5.35** (2.32)	6.95*** (2.34)	3.46* (1.89)	4.83** (2.08)	-0.58 (0.55)	-0.47 (0.78)
Event (t=2) * Treated PE	2.57 (2.08)	4.63** (2.09)	1.77 (1.95)	3.40 (2.08)	0.15 (0.56)	0.03 (0.79)
Event (t=3) * Treated PE	-0.09 (2.19)	2.32 (2.18)	-0.30 (1.97)	1.65 (2.18)	0.44 (0.62)	0.50 (0.88)
Event (t=4) * Treated PE	-0.87 (2.23)	1.80 (2.19)	-0.49 (2.01)	1.49 (2.21)	0.72 (0.66)	0.40 (0.98)
Event (t=5) * Treated PE	-2.13 (2.03)	1.29 (2.01)	-0.58 (2.16)	2.49 (2.20)	0.60 (0.79)	0.82 (1.16)
Log. Assets	7.15*** (0.96)		6.70*** (1.08)		2.54*** (0.39)	
Log. Int. Assets	0.29* (0.15)		0.23* (0.13)		0.00 (0.05)	
ROA (%)	14.13*** (2.54)		50.28*** (2.81)		35.28*** (1.22)	
Leverage Ratio	8.74*** (3.01)		19.88*** (2.93)		-2.82*** (1.08)	
Cash Ratio	-0.89 (3.00)		-8.02*** (2.81)		-0.19 (1.06)	
Group Situation	4.18** (1.86)		4.48*** (1.58)		-0.02 (0.51)	
Observations	6,750	6,754	6,750	6,754	6,716	6,804
Adj. R^2	0.14	0.11	0.25	0.13	0.84	0.67
Controls	Y		Y		Y	
Local industry FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y

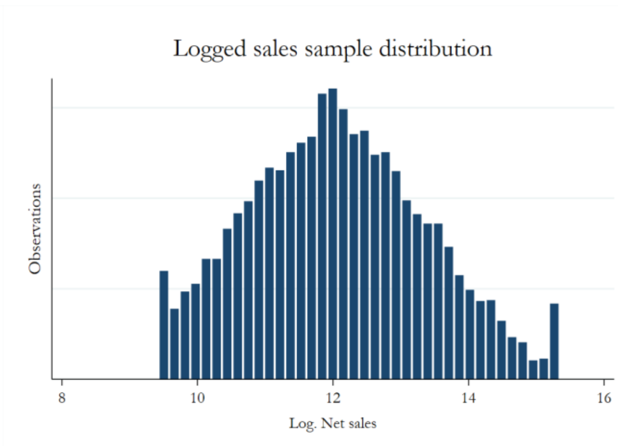
This table presents the estimates from a time-varying fixed effects regression model. All specifications include firm and year fixed effects. Odd numbered columns include firm-level control variables. Columns report estimates for employment growth (%), net sales growth (%), and EBITDA margin (%). Refer to Appendix A for variable definitions. *Events* indicate the year *t* relative to the transaction year for the PE buyout, with year t-1 omitted due to collinearity. *Treated PE* is equal to 1 for firms subject to a PE buyout and 0 for controls. We report the standard error clustered on firm-level within parentheses. Asterisks denote the significance of coefficients ($* p < 0.1,\ ** p < 0.05,\ *** p < 0.01$).

Appendix E: Validation of PE buyout impact on firm-level outcomes using alternative metrics

The following section describes how we transformed variables to form alternative outcome variables, list summary statistics for those variables and finally show the alternative variables outcome, serving as a robustness test. A concern inherent in studying the impact on companies using financial statement data is the potential distortion caused by the skewed distribution of financial variables. Because companies can only lose a fixed amount of money but have unlimited potential in earnings and size, financial variables are often skewed to the right as illustrated by the sales distribution in Figure 4. To address this, our alternative outcome variables incorporate log transformations, which is a method also embraced by prior researchers (Aldatmaz & Brown, 2020; Olbert & Severin, 2023). In doing so, we provide rigorous alternative outcome metrics to complement our original findings, which we discuss further on the next page.

Figure 5. Illustration sample distribution change of transformation of sales to logged sales





The figure shows the sample distribution of observed net sales (left) and capital expenditure (right) in m SEK in our dataset between the period 1998 and 2021.

Table 8. Additional summary statistics for alternative log outcome variables

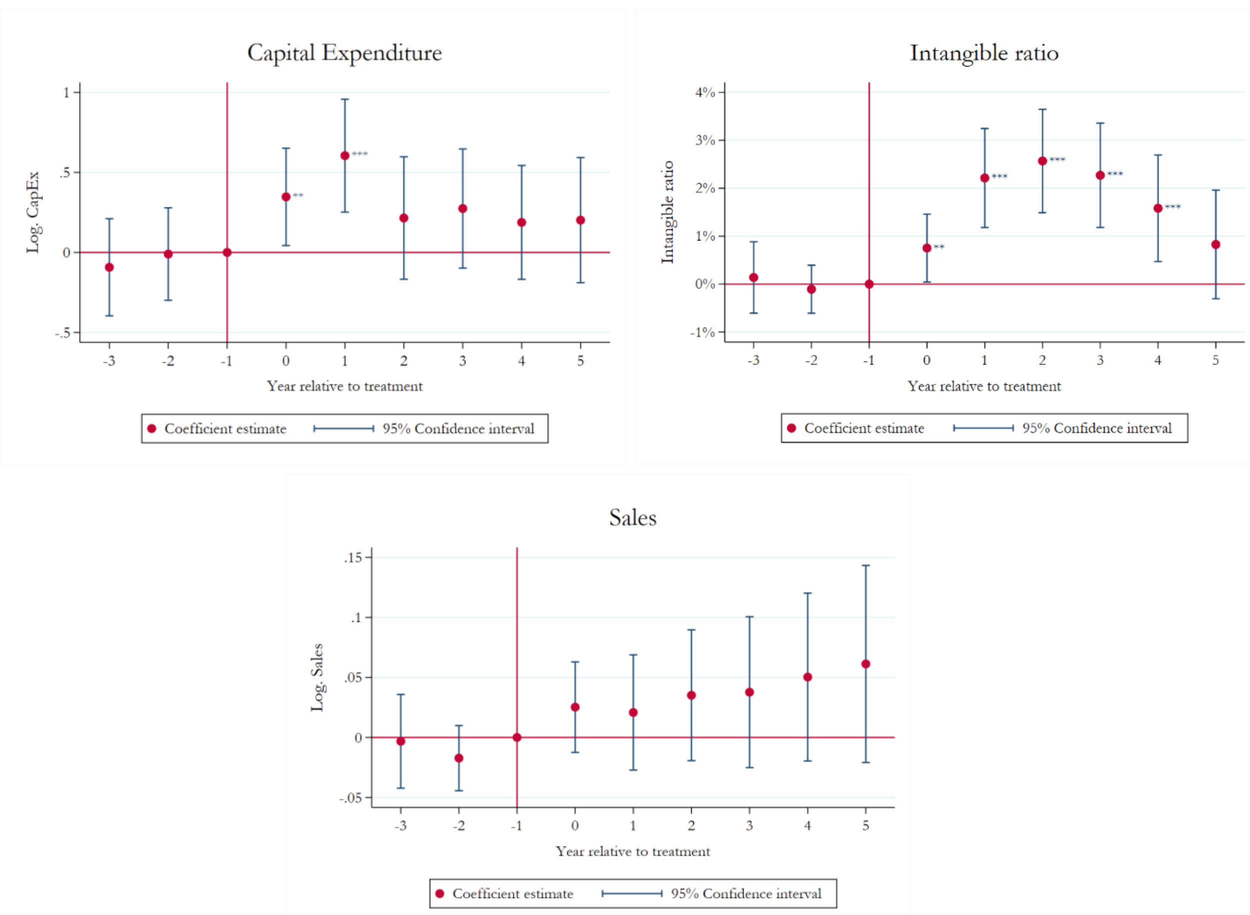
Panel A: Firm-level	Obs	Mean	Min	Median	Max	SD
Log. Capital Expenditure	5,673	7.57	0.00	7.88	12.88	2.82
Log. Intangible Capital Expenditure	6,215	4.83	-4.14	5.29	11.32	3.37
Log. Net sales	6,840	11.92	9.38	11.85	15.25	1.31
Log. EBITDA	6,050	9.45	0.69	9.51	12.75	1.52

The table presents summary statistics for our additional regression analysis variables, including the number of observations, mean, median, minimum, maximum and standard deviation for each variable. Detailed variable definitions are in appendix A. Panel A used for PE target firm impact spans three years pre- and five years post-intervention for each treated and control firm from 1998 to 2021. Financial variables are winsorized at 1% and 99% to mitigate extreme value effects.

Figure 6 below represent alternative outcome variables to complement our findings in the main analysis. For disclosure, when testing alternatives variables, we hold every other input in our model constant. Capital expenditure transformed using log shows that investments remain significant and robust, as illustrated by the graph and underscoring our main results. Intangible ratio, measures as total intangible assets as a percentage of total assets, show that buyout targets exhibit a significantly higher ratio relative to a control group. Thus, these results remain large in magnitude and significance, which aligns with our main findings and supports the robustness of our results.

We use logged net sales as an outcome variable to cross-check the impact PE ownership has on buyout and find that it lacks statistical significance throughout the event window but reveals a non-significant yearly growth trajectory. These findings are not entirely consistent with the ones in our main study, where we find significant effects immediately following the buyout. One reason for this could be the different compounding effects at play when comparing percentage growth and absolute growth. Sales growth is typically more pronounced in percentage terms initially and then stabilizing over time, whereas absolute terms represent the opposite. Asterisks denote levels of statistical significance, which are detailed here for completeness: *p<0.1, **p<0.05, ***p<0.01.

Figure 6. Illustrated results of alternative outcome variables



Appendix F. Local industry-level outcomes with and without control variables

	Investment and financial performance outcome variables					
	CE / A	Int. CE/A	Int. R	Empl Gr	Sales Gr	EBITDA(%)
Event (t=-3) * Treated PE	1.27 (1.13)	0.17 (0.17)	-0.14 (0.45)	-0.98 (3.47)	-2.04 (3.34)	-0.03 (0.56)
Event (t=-2) * Treated PE	2.62** (1.08)	0.15 (0.18)	0.01 (0.30)	-11.84** (4.64)	-7.77** (3.82)	0.39 (0.46)
Event (t=-1) * Treated PE	.	.	.	.	.	.
Event (t=0) * Treated PE	1.98* (1.05)	0.30* (0.18)	0.33 (0.27)	0.81 (3.90)	-1.55 (3.52)	0.28 (0.44)
Event (t=1) * Treated PE	1.21 (1.12)	0.20 (0.20)	0.28 (0.32)	-8.81** (3.52)	-4.68 (3.43)	0.52 (0.41)
Event (t=2) * Treated PE	-0.31 (0.92)	-0.03 (0.17)	0.16 (0.34)	-4.98 (3.62)	-7.57** (3.38)	0.58 (0.45)
Event (t=3) * Treated PE	0.74 (0.99)	0.50** (0.20)	0.69* (0.37)	-8.75** (4.20)	-11.19*** (3.50)	1.18** (0.53)
Event (t=4) * Treated PE	0.77 (1.03)	0.42** (0.18)	0.77* (0.43)	-5.18 (4.57)	-3.44 (3.44)	0.92 (0.58)
Event (t=5) * Treated PE	2.48** (1.22)	0.29 (0.20)	0.88* (0.46)	-5.55 (4.18)	-3.54 (3.37)	0.73 (0.62)
Log. Assets	-0.03 (0.59)	0.01 (0.07)	-0.27 (0.25)	16.88*** (3.62)	13.29*** (1.65)	1.43*** (0.47)
Log. Int. Assets	0.21 (0.13)	0.20*** (0.03)	1.04*** (0.15)	0.35 (0.53)	0.41 (0.49)	0.04 (0.09)
ROA (%)	19.35*** (4.30)	-0.38 (0.58)	-3.77* (2.15)	17.08 (12.52)	54.69*** (10.88)	43.80*** (3.12)
Leverage Ratio	4.35 (3.24)	-0.53 (0.41)	0.27 (1.71)	10.65 (9.64)	15.39* (9.19)	1.26 (1.71)
Cash Ratio	-19.07*** (5.07)	-0.61 (0.62)	-1.30 (1.90)	-1.59 (23.04)	-14.80 (14.66)	5.77** (2.74)
Observations	2,506	2,514	2,514	2,506	2,506	2,514
Adj. R^2	0.1664	0.3208	0.7712	0.1139	0.1371	0.8382
Controls	Y	Y	Y	Y	Y	Y
Firm FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y

This table presents the estimates from a time-varying fixed effects regression model. All specifications include firm and year fixed effects. Columns report estimates for capital expenditure/assets, intangible capital expenditure/assets, intangible asset ratio, employment growth (%), sales growth (%), and EBITDA margin (%). Refer to Appendix A for variable definitions. *Events* indicate the year *t* relative to the transaction year for the PE buyout, with year t-1 omitted due to collinearity. *Treated PE* is equal to 1 for firms subject to a PE buyout and 0 for controls. Ratios are multiplied by 100 to facilitate the interpretation of coefficients in percentage terms. We report the standard error clustered on local industry-level within parentheses. Asterisks denote the significance of coefficients ($* p < 0.1,\ ** p < 0.05,\ *** p < 0.01$).

Appendix G. Robustness test of excluding large cities from sample

As outlined in section 4.2.2, the treated municipality-industries are, on average, larger than the control group. Furthermore, as illustrated in Figure 7 below, our sample of PE deals are highly concentrated on Stockholm and to some extent Malmö and Gothenburg. Effectively, part of our treated sample will consist of almost every industry in the largest municipality. This introduces two concerns: First, as we match local industries on size of assets and number of companies, we may not find an equal match for the local industries in Stockholm. Second, because we treat local industries on the mere *presence* of a PE deal, we do not reflect that large cities such as Stockholm could be subject to a much more intense PE deal treatment. Consequently, we worry that any spillover effect identified are heavily influenced by the presence of large cities. Such concerns are valid because large cities often experience greater overall growth, which entail additional endogeneity concerns.

To address these concerns, we carry out additional tests where we exclude Stockholm, Göteborg and Malmö from our sample and compute the same analysis as in section 7.1. In doing so, we ensure that any prior results are robust to excluding these cities. The results of the additional tests are graphed in Figure 8 on the next page. The graphs indicate that results from section 7.1 are robust to excluding the largest cities, with all observed trends being almost identical to those observed in the main analysis. Two minor diversions arise. First, the positive effect on earnings becomes slightly stronger, entailing that earnings spillover are stronger in smaller local industries. Such a result may reflect the fact that a lower concentration of local peers is better able to capture the positive earnings spillover effects. Second, the effect of intangible capital expenditure decreases slightly. This could be a consequence that larger cities place more emphasis on digital transformation compared to smaller municipalities, but those different only appear to be subtle. In summary, these findings are deemed negligible and renders robustness to the results presented in section 7.1. Asterisks denote levels of statistical significance, which are detailed here for completeness: *p<0.1, **p<0.05, ***p<0.01.

Figure 7. PE Deal distribution across municipalities

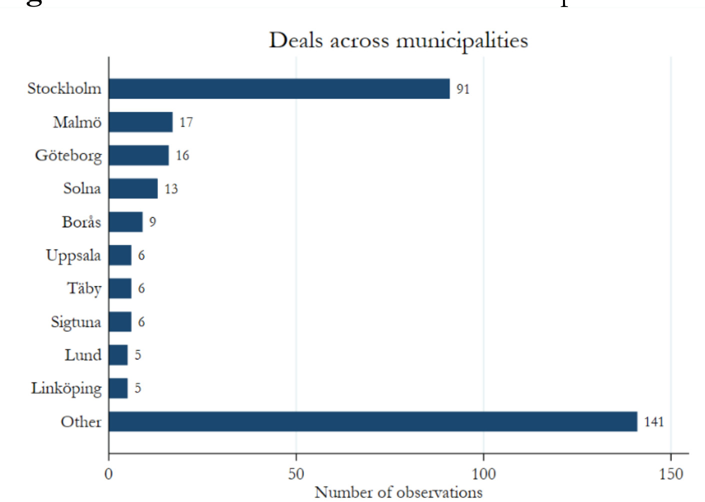
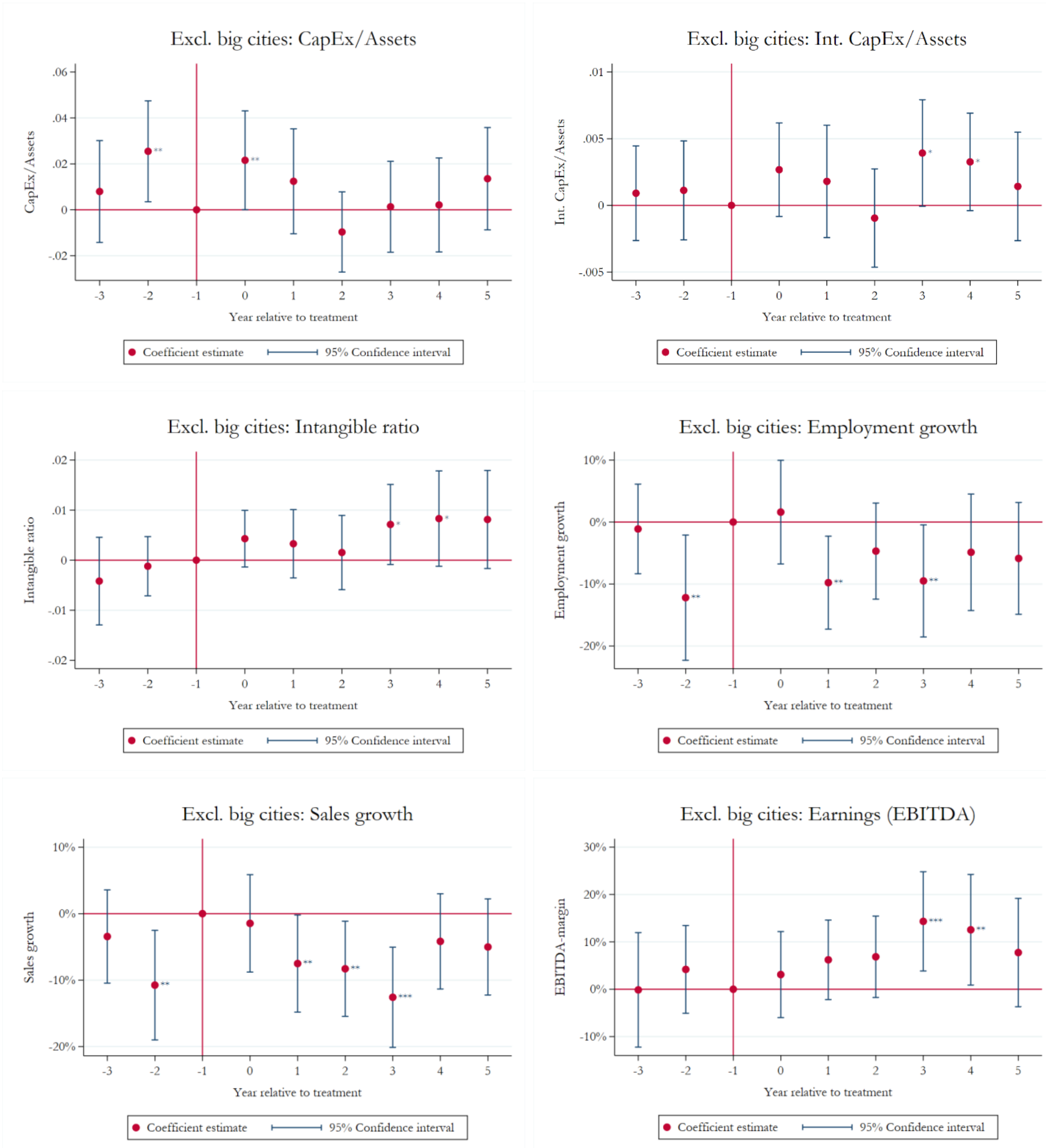


Figure 8. Local industry-level outcomes excluding Stockholm, Göteborg and Malmö



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