

Private Equity as an Enabler of Employment Growth: Evidence From European Buyouts

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Abstract

With a unique dataset of 337 buyouts observed during 2012 to 2015 across several European countries, this study aims to answer whether leveraged buyouts enable or stifle employment growth for targets relative to non-private equity owned controls. Through a series of regressions we are able to conclude that private equity ownership is associated with more growth in employment and other operating metrics. As an additional test, we investigate if the alleviation of credit constraints following a buyout spurs growth in employment and other operating metrics, where we fail to find a significant effect. Lastly, since most prior literature focuses on one particular country or region, it is difficult to infer results across different geographies. Given our pan-European dataset, we are therefore for the first time able to make direct geographical comparisons and study potential differences across countries. For example, we test whether post-buyout employment varies with labour regulation but do not find any such connection.

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1. Introduction

Private equity is sometimes perceived as contentious with dubious societal implications, particularly with regards to job safety, an assumption that is partly backed by older empirical literature focusing primarily on buyouts in the early era of private equity. It should be noted however that the private equity ownership form is relatively new with the foundations having mostly been laid in the eighties, aided by the development of the high-yield bond market at the same time.

With the growth of the industry also came the first body of academic literature studying the private equity phenomenon. Notable studies include Jensen (1989) and Kaplan (1989), praising private equity as a “superior ownership form” through its mechanism of enforcing stricter governance and more disciplined cash flow management. However, critics have sometimes argued that the increase in operating performance could come at the expense of employees. This notion is partly supported by literature, see e.g. Kaplan (1989), Lichtenberg and Siegel (1990) where they find that buyout companies experience less employment growth.

However, most of the literature studies deals that were made in the time prior to the global financial crisis in 2008. In the wake of the crisis, liquidity shortages and covenant breaches led to widespread defaults. This was particularly troubling for the private equity industry as the time preceding the crisis had been marked by record-high deal flow, valuation levels and high levels of debt (MacArthur and Rainey, 2018). As such, many private equity firms have adjusted and typically use lower levels of leverage as a result (Nowotnik et al., 2012). Since the underlying concept of private equity is to pursue value creation through a combination of debt paydown, multiple expansion and cash flow growth (Viscio and Pushner, 2014), the implication is that a larger share of private equity companies now pursue value creation through growth, rather than debt paydown. And while cash flow growth could come from a combination of cost reduction or revenue growth, private equity firms are increasingly targeting the latter (Nowotnik et al., 2012; Mullin and Panas, 2014; MacArthur and Rainey, 2018). As a consequence of this shift in the industry, it becomes increasingly important to question some of the older assumptions about the implications of private equity on target firms. In particular, we believe that the negative connotation of private equity with regards to job creation (or lack thereof) should be challenged.

The purpose of this study is to revisit some of the older assumptions about private equity and employment and add to a growing body of empirical literature that studies private equity-backed firms in a more recent setting. Specifically, we test whether there is an observable

increase or decrease in the number of employees following a buyout, and whether there is growth in other size-related operating metrics as well. Our aim is to substantiate the claim that private equity-backed firms experience more growth in employment, and in turn other operating metrics, as part of a broader growth agenda, as opposed to investing less, downsizing or reducing costs. In addition, we seek to test one particular explanation mechanism for why private equity-backed companies could experience more (or less) growth compared to controls. In line with Boucly et al. (2011), we bring forward the notion of financial dependence and its relationship with post-buyout growth, seeing the private equity ownership primarily as a facilitator of growth through the lessening of credit constraints. Lastly, we aim to supplement previous studies by also focusing on factors that could drive the extent of post-buyout growth in different geographies. Specifically, we want to see if the extent of post-buyout growth can be explained by a country's labour regulation policies.

We gather data on 337 buyouts across nine European countries, with a transaction year between 2012 to 2015. The choice of years is dictated by our focus on data post the global financial crisis and the time span is the result of us choosing to study a six-year time span for each target. The time span for each target or control is two years prior in order to analyse the time before the buyout and three years post to allow for the effects of the buyout to materialize. We then proceed and match each target to at least two firms that are non-private equity owned (our control firms). The matching occurs one year prior to the buyout and is based on size (revenue and employees) and style (industry). In total, we obtain a sample of 1,286 European firms. We then perform a series of regressions and test whether targets experience post-buyout growth relative to controls in several different metrics. To get an indication of operational size we use employment, revenue, EBITDA, total assets, capital expenditures and leverage as dependent variables.

We find that firms undergoing a buyout experience statistically significant growth in not only employment but also in revenue, total assets and capital expenditures. This could be an indication that growth rather than cost reduction in the form of layoffs is a primary motive for private equity firms. To make sure that these results are not reflective of inherent selection bias – the fact that private equity firms could select targets that are already growing or are better poised for growth – we include pre-buyout revenue growth as a control term, in line with Boucly et al. (2011). We also test the change in employment in the undisturbed time period of 2 to 1 years prior to the buyout year as a second measure to ensure that selection bias does not drive our results. After performing these two robustness checks our results still hold. A last concern could be that employment grows as a result of mergers or acquisitions since such

inorganic growth would not be reflective of a de facto increase in employment but rather of a consolidation of entities. To control for this, we conduct a robustness test where we perform the previous regressions on a subset of our total sample where we exclude companies that grow their total assets by more than 100% in any consecutive year—an approach similar to Amess and Wright (2012). We find that our results do not change after this adjustment.

Second, in contrast to Boucly et al (2011), we are unable to confirm the notion that private-to-private buyouts experience growth as a result of the alleviation of (prior) credit constraints. While we do find that the financial dependence of a firm defined at the level of industry is predictive of post-buyout growth in employment, we do not find the same connection for other size-related operating metrics and as such we question the validity of these findings.

Lastly, we test the notion of whether the extent of post-buyout growth across countries could be explained by differences in labour regulation. The connection between pre-existing labour laws and the conditions for post-buyout growth is sometimes highlighted in prior studies, e.g. by Boucly et al (2011) and Olsson and Tåg (2017). The idea is that differences in labour regulation could potentially impact the extent of layoffs in the buyout firm or enact some other impact on employment growth, as well as growth in general. We perform regressions using three commonly used labour indices and find that the differences in labour regulation are not predictive of post-buyout growth.

In sum, our study contributes to the literature of private equity and its effects on employment and growth in two main ways. First, most prior studies have focused on time periods before the financial crisis. Using more recent data, our paper sheds new light on how private equity impacts employment in targets in a more contemporary setting. Second, our pan-European dataset is to the best of our knowledge unique from other studies. As such, our dataset enables us to provide an outlook for countries which have little or no prior precedence in the literature and also test whether previous findings hold on a broader scale. For example, we are first in being able to test the association between financial dependence and buyout growth on a larger pan-European scale. Furthermore, the dataset not only enables us to make comparisons of countries between two different timeframes, it also makes it possible for us to investigate potential reasons for the differing results on a country-to-country basis. For example, this makes us first in being able to test whether labour regulation leads to different degrees of post-buyout employment growth.

This paper is structured as follows. Section 2 reviews the broader empirical literature on private equity, its impact on operating metrics and employment in particular, and also

presents other adjacent theories that bear relevance for this study. Section 3 describes our method, including the choice and construction of variables, sample construction and model specifications. Section 4 presents our results and section 5 outlines our conclusions and summarises all of our findings.

2. Literature Review

2.1 Leveraged Buyouts

A leveraged buyout (LBO) as described by Kaplan and Strömberg (2009) is when a private equity firm uses a smaller amount of equity and larger amount of debt to buy a company. To summarise how the private equity firm specifically achieves return on invested capital, one could take a simplified approach and categorise the sources of return in three ways (Viscio and Pushner, 2014). Since private equity firms acquire firms at a particular multiple to earnings, returns can be enhanced by increasing that multiple toward the exit. Second, returns are generated through the paydown of debt through internal cash flows, resulting in a higher equity portion. Lastly, returns are generated from increasing earnings or cash flow such that applying a multiple to the increased earnings results in a higher enterprise value. Increasing earnings or cash flow, in turn, can either be through a result of increasing revenues or a reduction in costs, or a combination of both.

The first private equity wave emerged in the 1980s. Shortly after the peak in private equity activity in the 1980s, Jensen (1989) writes one of the first studies on the subject. The author argues that private equity firms with the use of LBOs are able to create more efficient and superior organizations than public corporations. Through the use of an LBO, a private equity firm can strengthen the governance structure which enables them to align their interests to the management team of the buyout firm. Firstly, the associated debt incentivises and forces company managers to ensure that they cover their interest payments. As a result, they are less inclined to partake in negative NPV valued ventures or spend money on bonuses or empire building. The second measure is the high equity stake offered to the management team, further incentivising them to pay down debt and generate cash. Lastly, to make sure that the necessary steps are taken by the management team, private equity firms often impose a strict monitoring role, having board members and demanding to receive regular reporting. Management teams that do not perform are often quickly replaced (Kaplan and Strömberg, 2009).

In the early 1990s when the high-yield bond market in the US crashed, a shift in LBO transactions occurred. Public-to-private buyouts disappeared and a new type of buyout became more prevalent. Private equity firms started to acquire private companies and divisions. The public-to-private transactions did not resurface in the US until the mid-2000s, contributing to a record amount of capital raised by the private equity industry (Kaplan and Strömberg, 2009).

The European private equity market experienced a similar growth trajectory, increasing by 50% in 2005 (Amess and Wright, 2007).

Shortly after the financial crisis and the turbulence of the debt markets, private equity activity once again plummeted and then recouped once the markets stabilised. Since 2010, the private equity market has flourished. According to Beltran de Miguel et al. (2020), the private markets assets-under-management has grown by 170% in the past decade and the number of active private equity firms has more than doubled. Not only has the activity grown, private equity companies have also gradually changed their investment style. They have increasingly moved away from leverage to instead focus on improving operational performance (Nowotnik et al., 2012). Furthermore, as a result of the global financial crisis, non-private equity owned companies (that would make potential buyout candidates) have been forced to cut their operational costs. Consequently, private equity firms have been less able to extract value by cutting costs further and instead resort to creating value through growth. A survey conducted by Mullin and Panas (2014) concurs, finding that value is now mainly created by growing the company instead of reducing costs. According to their survey, expanding sales and improving pricing is now seen as the top priority. An alternative explanation as to why private equity focuses more on growth can be derived from the increase in competitiveness. Today, most of the cost-cutting opportunities are described as being “baked into inflated asset prices” or have already been captured by other private equity players (MacArthur and Rainey, 2018). As such, private equity firms need to boost a company through measures that are not already reflected in the price, for example by enacting a growth agenda. Improving the revenue of a company is often considered as a complex process, demanding intricate pricing strategies and developing commercial capabilities. Despite the possible difficulties in achieving it, revenue growth is today considered to be the single most important impact on valuation multiples (MacArthur and Rainey, 2018).

2.2 Buyout Effect on Target

2.2.1 Empirical Evidence of Operating Performance Post-Buyout

In general, LBOs have empirically been shown to increase the operating performance of their targets. The earliest work in this research field is focused on the US, where the results are overwhelmingly positive. Kaplan (1989) studies the effects of operating performance of 76 management buyouts (MBOs) of public companies between 1980 and 1986. The results indicate that companies experience an increase in operating income and net cash flows, while

capital expenditures decrease. These results are supported by Smith (1990) and Lichtenberg and Siegel (1990).

The empirical work associated with the second LBO wave that occurred in the 1990s and 2000s is mostly focused on European transactions that show similar results to the earlier US research presented above. In the UK, Harris et al. (2005) discover productivity gains in UK manufacturing establishments post-buyout by reducing the labour intensity of production through outsourcing. Moreover, Bergström et al. (2007) find that buyout firms create value relative to controls through an increase in EBITDA margin and ROIC during the holding period. Boucly et al. (2011) investigate the change in corporate behaviour in French buyouts. Their evidence suggests that companies become more profitable and grow in both assets and sales by 12% in comparison to their non-buyout peers. A more recent study by Davis et al. (2014) shows the effects of employment, total factor productivity and the earnings of workers on targets of private equity buyouts. In contrast to earlier research, the authors investigate the firms and their establishments. Their findings show that targets close down less productive establishments but later introduce new establishments with higher productivity. As such, the post-buyout effect is not necessarily only going in one direction but could change over the course of time.

The previous literature shows with a high level of empirical consistency that buyouts typically increase operating performance of their targets. However, these findings have also raised questions of where this source of value derives from, opening up for a new field of research. One concern that is often raised is the value and wealth transfers to owners from employees. Shleifer and Summers (1988) are some of the first to argue that buyouts transfer the wealth to investors through employee layoffs and by lowering wages. This warrants the question of whether buyouts seek to create value by enforcing layoffs as part of broader cost reduction initiatives.

2.2.2 Buyout Effect on Employment

Employment change as a result of an LBO is often discussed through the lens of corporate governance literature. As mentioned in the previous section, both Jensen (1989) and Kaplan (1989) argue that through high leverage, managerial incentives and engaging in active governance, private equity firms enable superior performance. Thus, purely from an agency theory perspective private equity firms could be argued to operate more efficiently by reducing sub-optimal investments, such as pursuing an unnecessarily high firm size (Amess and Wright,

2012). This would suggest that private equity targets have lower levels of employment in comparison to their less efficient, non-buyout peers. However some argue that in practice the same measures could instead lead to an expansion in employees. For example, Cressy et al. (2011) argue that the increase in operating performance might also be due to an increase in sales as a result of introducing sales incentives and ratchets, which ultimately would lead to a larger firm size. Amess and Wright (2012) suggest that private equity firms could instead drive employment growth through their experience and expertise. They argue that as private equity firms are actively engaged in the business of their targets, they are able to guide them towards growth opportunities that would otherwise not have been pursued without the experience of the private equity professionals.

Thus, given the previous literature on the subject there is no clear argument that buyout targets should experience an increase or decrease in the number of employees in comparison to their non-private equity owned peers (Amess and Wright, 2012; Cressy et al., 2011).

A separate reason for why there lacks clear consensus on the topic of employment growth following a buyout could relate to the fact that studies have often been conducted in separate countries, making generalisations and comparisons difficult. Wright et al. (2009) identify 17 prior studies that focus on the employment effects of buyout, whereas 16 investigate either the US or UK. The lack of empirical evidence in the broader geographical context has spurred new studies focusing on new countries, sometimes yielding different results.

Employment Effect of Buyouts in the US

The previously mentioned study by Kaplan (1989) on MBOs' effect on operating performance and value is also accompanied with an analysis on employment. By comparing the employment of buyouts to non-private equity owned companies in similar industries, the results suggest that employment grows 12% less for buyout firms. Several other studies covering the 1980s show similar modest net employment effects (Muscarella and Vetsuypens, 1990; Lichtenberg and Siegel, 1990).

In more recent research, Davis et al. (2014) manage to study and track the employment outcome of firms and establishments between 1980 to 2005. The findings from Davis et al. (2014) suggest a 3% and 6% decline in employment for target establishments over a two- and five-year period post-buyout respectively, relative to controls. The evidence also shows that targets, relative to their controls, create jobs at new establishments at a faster rate. However, once consolidated on a firm-level, the employment effects are once again modest. The results

obtained on an establishment-level indicate that private equity firms act as catalysts for a “creative destruction” process where jobs decrease with establishment shutdown but are once again created with new establishments. Davis et al. (2019) extend this study by investigating buyouts between 1980 to 2013. In terms of employment effect, publicly listed target firms experience an employment contraction of 13%, whilst private firms’ employment increase by 13%. The results for Europe are however more mixed, as described in the following section.

Employment Effect of Buyouts in Europe

In the United Kingdom, Amess and Wright (2007) find an insignificant effect on employment growth in line with the research performed in the US. However, the authors argue in line with Davis et al. (2014) that the type of deal influences the outcome of employment. By distinguishing LBOs into two different categories, management buyouts (driven by insiders) and management buy-ins (driven by outsiders), they argue that buyouts driven by insiders tend to experience increases in employment whilst management buy-ins show an opposite effect. In a later study, Amess and Wright (2012) find once again that LBOs do not have a significant effect on employment levels. A study performed by Cressy et al. (2011) yields different results. With a sample of 57 buyouts between 1995 to 2000, the authors find a significant employment contraction post-buyout.

Two studies in Sweden showcase similar findings to prior studies. Bergström et al. (2007) show that employment remains unchanged. Olsson and Tåg (2017) also conclude that buyouts on average do not affect employment. Furthermore, in regard to whether their findings could be generalised towards other countries, they argue that labour protection policies could have an impact. While not testing for this effect, they speculate that any results indicative of more layoffs should represent a “lower bound” relative to countries that have weaker employment protection policies than Sweden.

There are however studies that favour the notion that private equity creates employment growth. In other parts of Europe, the employment growth for private equity buyouts have shown to be mostly positive (Bacon et al., 2013). Toubeau (2006) studies 53 Belgian buyouts during 1998 to 2005 and concludes that buyouts, relative to their control firms, experience higher employment growth. Pellon et al. (2007) show similar findings in Spain. Boucly et al. (2011) study 839 French buyouts where their target firms experience not only a growth in employment but also in sales and assets. The authors mention that two potential reasons for these differing results could relate to France’s relatively more extensive labour regulation –

making layoffs more costly – and relatively weaker credit market. On this last point, they are able to find that firms operating in more financially dependent industries experience disproportionate growth post-buyout. This finding could help reconcile why French buyouts yield differing results to prior studies, mainly in the US and UK, although many continental European countries are broadly similar to France in this regard (i.e. in terms of credit market development).¹

In summary, the literature can be divided into three different categories. First, studies performed in the US and UK suggest that employment levels of buyout targets are lower than their control groups (Kaplan, 1989; Muscarella and Vetsuypens, 1990; Lichtenberg and Siegel, 1990; Cressy et al., 2011). Second, some studies show that employment effects of buyouts are not significant at all (Bergström et al, 2007; Amess and Wright, 2012; Olsson and Tåg, 2017; Davis et al., 2019). The last category of studies reviewed are studies that provide evidence of employment growth (Toubeau, 2006; Pellin et al., 2007; Boucly et al., 2011).

Although the literature seems to show that continental Europe experiences a positive employment growth following a buyout, it is not clear why this would be the case. A possible theory was presented by Boucly et al. (2011) for their French buyout sample. As previously mentioned, Boucly et al. (2011) argue that one potential reason for why French buyouts experience positive employment growth could relate to external financial dependence, and the fact that France's credit market differs from that of the UK or the US where employment growth has mostly been seen to be negative. The rationale underpinning this hypothesis is explained further below.

2.2.3 Employment Effects and Financial Dependence

The relationship between (external) finance and real growth has been studied for a long time but is perhaps most associated with the work of Rajan and Zingales (1998), who study whether economic growth can be affected by financial development. The argument goes that financial markets reduce the cost of external finance by facilitating for companies to overcome problems of moral hazard and adverse selection. Through a development in the financial sector, firms may therefore get cheaper access to external financing. To support this hypothesis, they test whether financially dependent industries are growing more in financial markets that are more mature and developed. Not only is their evidence consistent with this notion, but also shows

¹ According to World Bank data, the ratio of private credit to gross domestic product (GDP) is similar for most continental European countries, being ~20-50% and ~30-60% lower than that of the UK and US respectively.

that industries in developed markets invest more and have larger firms than those in non-developed markets.

Drawing on the methodology of Rajan and Zingales (1998), Boucly et al. (2011) study whether private equity could result in growth as it alleviates credit constraints for financially dependent firms. Their results suggest that private-to-private deals experience higher growth post buyout, in comparison to, for example public-to-private deals. They argue that, as public-to-private firms are less financially constrained than private-to-private firms, private equity firms could be seen as facilitators of growth to firms that are constrained prior to the buyout. Even if the private equity firm does not provide direct financing, it is still plausible that the presence of the private equity owner helps improve relationships with lenders for the target, e.g. through increased credibility or prior relationships. Lastly, although financial dependence is one theory that could explain why a country such as France experiences growth whilst others do not, the concept has not been tested on a broader geographical scale and as such it is not clear whether this result could be generalised across borders.

A separate theory that is occasionally mentioned in the literature is that a country's labour regulation could enact an impact on post-buyout growth, for example through impacting the possibility and cost of employee layoffs. While this factor has not been extensively studied in prior literature, a summary can be provided below.

2.2.4 Employment Effects and Labour Regulation

As mentioned above, the relationship between labour protection regulation and employment growth has been under-emphasized in previous literature but has briefly been discussed in the works of Boucly et al. (2011) and Olsson and Tåg (2017). Boucly et al. (2011) mention that the fact that France has more extensive labour regulation could be an explanatory factor as to why their sample shows positive employment growth post-buyout. The argument goes that as it is relatively more difficult to layoff an employee in France, their buyouts will not experience cost-cutting to the same extent as in the US and the UK, who have less rigid labour regulation. Even though a relationship between the two could not be established in the specific study of Boucly et al. (2011), the discussion can still be extended. According to other literature, there exists a clear link between private equity and local labour laws. Lazear (1990) studies the effect of labour regulation on employment growth. The author argues that labour protective regulations, such as severance pay, may have stifled employment growth in many European countries. Blanchard et al. (1997) agrees and claims that labour rigidities could be a driving

factor of the low employment growth experienced in continental Europe in comparison to countries such as the US and the UK. In more recent research, Kerr and Bozkaya (2009) study how the differences in labour regulation affect the development of private equity markets. Their results indicate that employment protection has a negative effect on private equity entry. They argue that there is a strong correlation between where private equity firms choose to enter and the strength of labour regulation, showing that tough labour laws lead to fewer investments. However, they also claim that private equity formation depends on other country-related factors that are likely omitted if only labour policies are examined. They complement their study by pursuing interviews with private equity professionals. The answers indicate that labour policies are a priority concern when looking for buyout targets, strengthening the claim that labour regulation influences private equity investments.

3. Method

3.1 Hypotheses

Given the prevailing contradictions in prior literature, this study aims to answer the question of whether leveraged buyouts enable or stifle growth in employment relative to their non-private equity owned counterparts and, if so, what the possible reasons for this development could be. To answer this question, three main hypotheses have been formulated.

To frame our first hypothesis, we refer to previous empirical studies and the theories of Jensen (1989) and Kaplan (1989). As presented in section 2, there is no clear argument as to whether buyout targets should experience an increase or decrease in their number of employees relative to controls following a buyout. The corporate governance measures that private equity firms implement have theoretically and empirically been consistent with an improvement in operating profitability. However, what these measures say about the development of employment is less clear. This improvement in operating profitability can be consistent with both employment expansion or employment contraction, as value creation can be due to either an increase in growth of sales and employees as opposed to, or in addition to, traditional cost-cutting through for example layoffs (Amess and Wright, 2012; Cressy et al., 2011). Ultimately, these opposing actions create an ambiguous interpretation of the literature, giving no clear direction as to what can be expected of employment in a buyout firm. As a result, we formulate two alternative hypotheses.

Firstly, one could argue that buyouts lead to negative effects on employment growth. According to the theories of Jensen (1989) and Kaplan (1989), private equity firms use debt, create management incentives, and have an active monitoring role which in theory creates superior value. These corporate governance improvements should from a theoretical perspective reduce sub-optimal investments such as pursuing an unnecessarily large firm size (Amess and Wright, 2012). A reduction in employment growth would also be in line with most of the previous studies taking place in the US or the UK. We therefore formulate our first hypothesis as such:

Hypothesis 1a: Firms subject to a buyout have a significant, negative post-buyout employment growth compared to their non-buyout peers.

Opposite to this first hypothesis, there are arguments for positive effects on employment growth as a result of a buyout. Firstly, as presented by Cressy et al. (2011), the same corporate governance initiatives that could lead to a decrease in employment growth could equally lead

to an increase in employment growth since a higher efficiency could lead to increasing sales and thus a larger firm size. In addition, management incentives in the form of sales targets and ratchets could lead to further performance improvements that could result in an expansion of employee growth. An alternative contributor to growth is mentioned by Amess and Wright (2012). They argue that private equity firms use their expertise to take advantage of growth opportunities, leading to employment growth. Also, there are empirical findings that strengthen the argument of employment growth. Bacon et al. (2013) summarizes the findings of European studies as being more prone to experience employment expansion, in contrast to studies taking place in the US or the UK. The last and possibly most contemporary element that suggests a positive employment development is the indications of a shift in private equity focus. As a result of the financial crisis and an escalation in competitiveness, the private equity industry has increasingly changed its focal point from cutting costs (e.g. through layoffs) to pursuing value creation through growth. This transition is expected to be reflected in our results, as this study focuses on the years post the global financial crisis. With this in mind, we formulate the following hypothesis:

Hypothesis 1b: Firms subject to a buyout have a significant, positive post-buyout employment growth compared to their non-buyout peers.

In line with the idea that the private equity industry has changed from focusing on costs to focusing more on growth, one could conceive that private equity firms enable such growth through various means. One such mechanism could be through the alleviation of credit constraints, a relationship brought forward by Boucly et al. (2011) as mentioned in section 2. The theory relates to Rajan and Zingales (1998) and stipulates that more financially dependent companies are less able to grow without outside capital, and a buyout could therefore be argued to facilitate ex post growth. This extends beyond the possibility of the private equity firm to provide direct capital to the business. For example, private equity firms typically have stronger relationships with lenders and could help firms gain access to credit to help finance growth opportunities. These relationship benefits are also likely to lower the cost of financing. As such, it could be hypothesized that target firms receive better access to external finance and are therefore better able to pursue growth. With this in mind, we formulate the following hypothesis:

Hypothesis 2: Private equity-backed firms that operate in sectors that have higher financial dependence experience more employment growth post-buyout relative to controls.

Separately, an issue with prior literature is the lacking consensus on the relationship between private equity and employment growth across geographies. This concerns the fact that many prior studies focus on one particular country or region and is acknowledged by Wright et al. (2009). The authors note that most prior research have involved the US and the UK, yielding similar results. However, other European studies involving countries such as Belgium, Spain and France have found contradicting results to the studies in the US and the UK (Tobea, 2006; Pellon et al., 2007; Boucly et al., 2011). As such, it is difficult to infer any of these results on a broader geographical level. One arising issue could be that there are inherent institutional differences across geographies that facilitate or stifle growth, explaining why results obtained in certain geographies could be difficult to infer or generalize to others.

One specific institutional factor that could be highlighted is labour regulation. As suggested by previous literature, there is an ambiguous effect of strict labour laws on employment. One could argue, in line with Boucly et al. (2011) and Olsson and Tåg (2017), that stricter labour policies create higher growth for buyout firms as the private equity owners are less able to pursue cost reductions through layoffs than for example in less strict legislations such as the US or the UK. Specifically for Boucly et al. (2011), the significant and positive employment growth in their sample of French buyouts is argued to potentially be reflective of the country's strict labour laws since France has more extensive regulation. Others may argue in the likes of Lazear (1990), Blanchard (1997) and Kerr and Bozkaya (2009), that strict labour policies could in fact stifle employment growth as companies are less inclined to hire new staff that will be costly to lay off in the future. These contradicting views state that there might be a relationship between employment growth and labour regulation, however it is not clear whether it is positively or negatively correlated. As a result, we avoid taking a stance as to whether the relationship is positive or negative and formulate the following third hypothesis:

Hypothesis 3: Labour policies exert a significant influence on employment growth for private equity-backed firms relative to controls.

3.2 Variables

3.2.1 Dummy Variables

Given the nature of our regressions, as will be presented in section 3.2.4, two dummy variables are incorporated. The variable LBO_i distinguishes a company from being a buyout firm and a control. If firm i is a buyout company it takes the value of 1 and 0 otherwise. The variable $Post_{it}$ divides our sample into two different time frames, either representing the period preceding the

buyout or post the buyout. The dummy variable equals 1 when firm i , either buyout firm or a matched control company, is in the years following the buyout and 0 otherwise.

3.2.2 Firm and Industry Variables

For the dependent variable, different operating metrics that can be interpreted as measures of size are used. The metrics include employment, revenue, EBITDA, total assets, capital expenditures and leverage. While this study mainly aims to analyse the effect on employment following a buyout, the other variables will help provide a further context to the results. All operating figures except for leverage and prior year revenue growth are expressed as logarithms, including employment, to account for potential skews.

In addition to these firm variables, we compute the level of financial dependence across sectors, similar to the methodology of Rajan and Zingales (1998) in order to test our second hypothesis. Using data from Bureau van Dijk, we compile data on cash flows and capital expenditures for the universe of firms available across the nine European countries in our sample. Financial dependence is in turn defined as the ratio of capital expenditures not financed through internal cash flows, i.e. capital expenditures minus operating cash flow normalized by capital expenditures. Operating cash flow is defined similarly to Boucly et al. (2011), where we look at the gross operating cash flow by adding back depreciation and amortisation to net income. We then calculate the level of financial dependence across the time span of six years for each firm, remove outliers and calculate the median financial dependence for each two-digit sector. Hence, the score provides an industry-median of financial dependence for each firm based on their industry classification. Since credit constraints also depend on firm size, we choose to only calculate this metric for firms with more than 100 employees, in line with Boucly et al (2011) to better reflect the inherent financial dependence of different sectors. See table 1 for a summary of the chosen variables.

Table 1. A summary of our chosen dummy, firm and industry variables.

Variable	Description
<i>Post</i>	Dummy variable taking a value of 1 for a buyout or matched company in the years following the buyout, and 0 otherwise.
<i>LBO</i>	Dummy variable taking a value of 1 for a buyout and 0 for a matched company.
<i>log(Emp)</i>	The logarithm of number of employees.
<i>log(Rev)</i>	The logarithm of revenue.
<i>log(TA)</i>	The logarithm of total assets.
<i>log(EBITDA)</i>	The logarithm of EBITDA.
<i>log(CAPEX)</i>	Capital expenditures at year <i>t</i> , defined as the change in fixed assets, adjusted for depreciation & amortisation during the year.
<i>RevGr</i>	Revenue growth from <i>T</i> –2 to <i>T</i> –1, where <i>T</i> indicates the buyout year.
<i>Leverage</i>	Defined as debt divided by total assets.
<i>FD</i>	Financial dependence, expressed as the median financial dependence of the firm’s sector (defined by the first two digits) between the years 2010–2018. Financial dependence is calculated as the ratio of capital expenditures not financed through internal cash flows, i.e. capital expenditures subtracted by operating cash flow and divided by capital expenditures.

The above table presents the variables of choice for our regressions as presented in section 3.2.1 and 3.2.2, along with their according definitions and methods of calculation.

3.2.3 Labour Indices

To test our third hypothesis, namely to investigate the effect of labour regulation across countries, a set of index variables reflecting the extent of labour regulation are included in the regressions. Deakin (2018) argues that there are two separate interpretations of regulation as an underlying concept. Firstly, regulation that aims to reflect the “intended effect of a norm or rule”, and secondly regulation that reflects “the impact on a social actor”, in this case the employer. A second point raised by Deakin (2018) is that in order to fully represent one of these two definitions – either focusing on reflecting the purpose of the law or the influence that the law has on a certain actor – the coding has to differ. For example, leximetric coding works

well when the sole purpose is to capture the purpose of the law. However, it will not be able to efficiently capture the impact on actors or how it is perceived in practice.

While there is no standard for comparing labour regulations across countries, we have chosen the most prevalent composite indices that also complement each other. This is done by choosing indices that are based on one of the two distinctions presented by Deakin (2018), ensuring that their data retrieval is adequate and choosing those indices that are most practically relevant or reflective indices of the phenomenon in question (i.e. the ease of enforcing layoffs following a buyout). These choices of labour indices are presented in Table 2 and are further explained below.

OECD's Employment Protection Legislation (EPL)

The EPL index aims to reflect the austerity of the regulation of dismissals and the use of temporary contracts. The index is constructed to measure an economic effect and in this particular case a cost imposed to the employer. OECD complements their leximetric coding by conducting surveys to ensure that the impact that a specific law has on its actors is accurately reflected in the index. In contrast to many other indices, the EPL index is built on the assumption that strict regulation is costly (Deakin, 2018). The EPL score is expressed on a scale of 1 to 6, where a high score is associated with a higher cost imposed on employers and a better protection for employees. The index is compiled of 21 individual variables and can be broadly classified into three summary indicators that represent the strictness of employment protection on “individual dismissals”, “collective dismissals” and “temporary employments” (OECD, 2013) . Given the main purpose of this study, to test the effect of employment development as a result of private equity ownership, we solely use the summary indicator for individual and collective dismissals (EPRC) between 2008 and 2013 and exclude “temporary employment”. The latest available data for the EPL index is from 2013. However, since our buyouts occur in the 2012 to 2015 time span, we believe that the values as of 2013 to be the most reflective, meaning that the EPL score for 2013 will represent the country-score for all transaction years.

CBR Labour Regulation Index

Constructed by the Centre for Business Research (CBR) in Cambridge, the CBR Labour Regulation Index is a dataset consisting of labour laws in 117 countries between the 1970s to 2013. In contrast to the EPL index presented above, the CBR index is a reflection of protective

regulation and formally does not reflect labour “costs” or rigidity. The data retrieval is solely based on legal information. The index consists of five sub-indices, including “the law governing the definition of the employment relationship and different forms of employment”, “the law on working time”, “the law relating to dismissal”, “the law governing employee representation” and “the law relating to collective action”. Together, these five sub-indices generate a total of 40 variables, expressed on a scale of 0 to 1 (Adams et al., 2016). With similar reasoning to the choice of variables for the OECD’s EPL index, we choose to regress the sub-index which we believe to reasonably impact employment growth post-buyout. In this particular case, “the law relating to dismissal” is the only sub-index chosen to be part of this specific test. Similarly to the EPL index presented above, the CBR labour index has the latest data available as of 2013. With the same reasoning regarding the EPL index, we choose the 2013 values of the CBR labour index to represent the time period for our buyouts between 2012 to 2015.

International Labour Office (ILO) Employment Protection Legislation

The International Labour Office (ILO) constructs the so-called ILO “EPLex” indicators. In contrast to OECD’s EPL index and CBR’s Labour Regulation Index, their sole focus is to reflect the regulation associated with “terminating regular contracts” (individual dismissals). Similar to the CBR index, ILO only codes for legal information and does not reflect any impact imposed on specific actors. It is also, just as the CBR index, considered to be a neutral index, not making any judgments as to whether strict labour laws are good or bad (Deakin, 2018). The index is divided into eight sub-indices that construct the EPLex summary indicator. A variable can take on a value between 0 and 1, where a higher score indicates a higher value of employment regulation (ILO, 2019). The EPLex summary indicator is used in this study as it adequately reflects the regulation of dismissing employees. In contrast to OECD and CBR, when using the ILO index we are not restricted by the data availability. The ILO index covers the timespan from 2009 to 2017, enabling us to use the different scores obtained for each year between 2012 to 2015 as we match each target and control to the index score of the buyout year.

Table 2. A list of labour-related index variables and their according sources.

Source	Name	Description
<i>OECD</i>	EPRC	A combination of individual and collective dismissal regulation. The index spans until 2013, where we match this year value to our targets and controls.
<i>CBR</i>	Dism	Variable reflects the sub-index score on “employee dismissals”. The index spans until 2013, where we match this year value to our targets and controls.
<i>ILO</i>	EPLex	Index covering the termination of regular employment contracts (individual dismissals). The index spans until 2017, where we match each index year to the year of the buyout for the targets and controls.

A summary of the labour indices that we use as part of our regressions in Eq. (4). The source, variable name and description for each variable is presented above.

3.2.4 Model Specifications

In order to test our first hypothesis, we perform a regression with firm and year fixed effects and cluster standard errors at the firm level to control for correlation between errors within groups over time. The first regression has the following specification:

$$Y_{it} = \alpha_i + \delta_t + LBO_i + LBO_i \times Post_{it} + Post_{it} + \varepsilon_{it} \quad (1)$$

Where Y_{it} is an operating metric for a target firm i at any time t , expressed as a logarithm. The LBO_i and $Post_{it}$ variables are explained in table 1. The interaction term $LBO_i \times Post_{it}$ acts as the difference-in-difference estimator, which means that its coefficient will explain the effect of the buyout on the dependent variable. Continuing, we acknowledge that a potential confounding factor is that private equity companies aim to select companies with better potential for growth, some of which are already growing at a higher rate. Similar to Boucly et al. (2011), we therefore include a pre-buyout growth control, to account for the fact that some of the growth ex-post could already have been established ex-ante. See Eq. (2) for a modified regression.

$$Y_{it} = \alpha_i + \delta_t + LBO_i \times Post_{it} + RevGr_{it} \times Post_{it} + Post_{it} + \varepsilon_{it} \quad (2)$$

The modified regression has the additional interaction term $RevGr_{it} \times Post_{it}$. We expect this term to be positive, as this would indicate that targets and controls that grow more prior to the buyout also grow more post the buyout.

We confirm our regression results on employment through a Welch's t-test. We do this for two main reasons: First, similarly to Boucly et al. (2011) we want to ensure that the results are not impacted by pooling all of the post-buyout years into one post-period, and second, it works as an additional control to make sure that our results are not driven by selection bias. We therefore test the cumulative employment change in four periods of time, and also test for statistical insignificance of the employment change during the undisturbed time period T-2 to T-1 where T is the buyout year.

Lastly, another factor that could impact the results is the possibility that private equity-backed companies pursue growth through acquisitions, either smaller add-on acquisitions or larger-scale acquisitions. Since we are principally interested in the impact on organic employment growth, we perform a robustness test where we exclude companies whose total assets grow more than 100% at any consecutive year, in line with Amess and Wright (2012).

In testing our second hypothesis pertaining to financial dependence, we use the following regression model with firm and year fixed effects and cluster standard errors at the firm level:

$$Y_{it} = \alpha_i + \delta_t + LBO_i \times FD_i \times Post_{it} + LBO_i \times Post_{it} + FD_i \times Post_{it} + LBO_i \times FD_i + FD_i + Post_{it} + \varepsilon_{it} \quad (3)$$

Here, Y_{it} is the specific operating metric for target firm i at time t . FD_i indicates the median financial dependence of the sector that a firm belongs to. The LBO_i and $Post_{it}$ variables are explained above in table 1. We would expect the coefficient for $FD_i \times Post_{it}$ to be negative if the logarithm of employment is put as a dependent variable, as financially constrained companies are presumed to grow their employees less. For the interaction term $LBO_i \times FD_i \times Post_{it}$, we expect the coefficient to be positive as this would indicate that post-buyout growth could specifically be attributed to prior financial dependence in financially constrained companies.

For our third hypothesis, we use the following regression model with firm and year fixed effects and cluster standard errors at the firm level:

$$Y_{it} = \alpha_i + \delta_t + LBO_i \times Ind_{it} \times Post_{it} + LBO_i \times Post_{it} + Ind_{it} \times Post_{it} + LBO_i \times Ind_{it} + Ind_{it} + Post_{it} + \varepsilon_{it} \quad (4)$$

Where Y_{it} is the logarithm of employees for target firm i and time t . Ind_{it} represents the index of choice and is defined at the level of country for firm i . In accordance with our third hypothesis, we do not make an assumption as to whether the coefficient for the Ind_{it} term should be positive or negative. If the interaction term $LBO_i \times Ind_{it} \times Post_{it}$ is significant a relationship between the two can be established. If the $LBO_i \times Ind_{it} \times Post_{it}$ term has a significant positive coefficient, the result would be in line with Boucly et al. (2011) and Olsson and Tåg (2017), as it would indicate that strict regulation increases employment growth as cost cutting is hard to pursue. If the term has a negative coefficient, the result would be in line with Lazear (1990), Blanchard (1997) and Kerr and Bozkaya (2009), as strict labour policies would lead private equity firms to not hire people to the same extent as in weaker protected countries, leading to a lower growth in employees.

3.3 Data

The collection of data is structured into three different stages: creating the buyout sample, matching the sample of buyouts with a corresponding sample of controls and retrieving accounting data for all firms. These steps are further explained below.

3.3.1 Constructing the Buyout Sample

The sample of buyouts is collected from S&P's Capital IQ (CIQ) and Thomson Reuters' SDC Platinum (SDC). We choose to study the period from 2012 to 2015. We choose this time frame for two reasons. The first is that the majority of the previous literature focuses on older data, and we believe that studying transactions post the global financial crisis is better suited to explain how private equity works today. Second, since we study operating changes over time we need a sufficiently long time span for changes to materialise (our time horizon for each target).

The countries chosen as part of our analysis are made on the grounds of two factors. First, we focus on countries in order of value invested in private equity and prioritise the top countries (Invest Europe, 2019). We then choose the countries that are able to provide us accurate and consistent firm data throughout the time window in order to conduct the analysis.

This procedure results in a sample consisting of the following countries: the United Kingdom, France, Italy, Spain, Belgium, Germany, the Netherlands, Sweden and Finland.

The transaction data includes information on the closing date of the transaction and the names of the relevant parties, i.e. the acquiror(s), the target and the seller(s). Following the methodology of Strömberg (2008), the CIQ sample is created by selecting effective or closed transactions involving majority stakes with secondary transaction features tagged as “Leveraged Buyout” and not “Management Buyout” or “Secondary Buyout”. MBOs are excluded as this study focuses on the effect from private equity actors. In addition, Amess and Wright (2007) find that employment growth differs depending on whether the transaction is driven by insiders (the management team) or outsiders (a private equity actor). As such, if we were to include MBOs it could affect our results and overestimate the positive employment effect that a buyout creates. Secondary LBOs are excluded as we are primarily interested in the effect of first-time private equity ownership, as it could otherwise be the case that potential cost-cutting measures have already been undertaken by the first private equity owner, an approach also in line with Olsson and Tåg (2017). We also exclude public-to-private and divisional buyouts. Lastly, although CIQ is a comprehensive database on buyouts, we also use SDC to help complement our data. Using SDC and applying similar criteria therefore extends our sample by approximately a third.

3.3.2 *Retrieving Firm Data*

The accounting data is retrieved from Bureau van Dijk, where we match our sample of buyouts with the associated company name in their database and link the accounting information. In cases when names between the databases are not the same, we make use of company information through company websites and annual reports. If an accurate match is not found, we exclude that buyout from our sample. Matching the total sample of buyouts with the firm name in Bureau van Dijk results in a considerably smaller sample. As mentioned in section 3.2.2, the company and accounting information that is needed includes metrics such as employment, revenues, EBITDA, total assets, capital expenditures and leverage. Capital expenditures is the only metric which cannot be extracted as a standalone variable from Bureau van Dijk. In line with Faccio, Marchica and Mura (2016), we compute capital expenditures through Eq. (5) below.

$$CAPEX_t = FA_t - FA_{t-1} + D_t \quad (5)$$

Where FA_t is fixed assets at time t and D_t is the depreciation and amortization at time t .

We study a time-window spanning two years before to three years after the buyout, meaning that we gather accounting and company data between 2010 to 2018. The time window is chosen in order to reflect pre-buyout conditions and also provide enough time post buyout for changes to materialize. While the holding period of LBOs is typically longer than three years (Bergström et al. 2007), we believe that a reduction in employees as part of a value creation process is more likely to occur early on—a notion supported by e.g. Davis et al. (2014). Moreover, extending the time window in this study would drastically lower the sample size as our pan-European accounting data is only available from 2010.

To construct the most accurate data sample possible, we only include firms that have mostly complete accounting data for every year in which we study. When data is missing, we investigate whether it could be complemented with data from national company registers. Using *Bundesanzeiger* for German company data and *Companies House* for English company data, we are able to expand our sample size for these two countries. The final sample contains 337 buyouts.

3.3.3 Constructing the Control Group

To estimate the labour effects of buyouts, we need to compare the development of employment of our buyout targets to similar, non-private equity owned peers. Following previous studies (Bergström et al., 2007; Boucly et al., 2009; Cressy et al., 2011; Amess and Wright, 2012; Davis et al., 2014) we perform a control cell match, meaning that the matching company needs to reflect similar firm characteristics. First, the control firm has to belong to the same, or a similar sector as the buyout firm. To ensure that the industrial composition of the control sample is similar to the buyout sample, matching companies need to belong to the same three-digit NACE2 sector classification. As observed by Bergström et al. (2007), certain companies that are acquired are consolidated groups or holding companies. Since these codes are less informative, we either use the code of the main subsidiary of the group or in the few cases where not obtainable, we allocated the most fitting NACE-codes ourselves by researching the group and its activities. The second criteria is to match companies based on size. Similar to Boucly et al. (2011), we stipulate that the number of employees in the control firm has to be in the $\pm 50\%$ range of the target company one year prior to the buyout. However, instead of using ROA as a second metric, we use net sales one year prior to the buyout, similar to Cressy et al. (2011), and apply the same range of $\pm 50\%$. We choose net sales as this minimizes the potential impact of accounting differences across countries, as opposed to using earnings-based metrics,

and also allows us to include firms with more variable earnings (e.g. software or real-estate related companies). The control firms are then generated by minimizing the square distance with respect to these variables, defined as the sum of the square difference in employees and sales, and matching by industry. We make sure to have at least two control firms per buyout firm, otherwise the buyout firm is dropped from the sample (Amess and Wright, 2012). If there are more than two, we keep the three control firms that are nearest to the buyout company.

4. Results and Discussion

4.1 Descriptive Statistics

Table 3 presents our complete sample of European buyouts, distributed by deal year, country and sector. It also contains the control group for the respective categories. In summary, this study yields 337 buyouts between 2012 to 2015. The matching methodology adds 949 control firms, or approximately 2.8 control firms per buyout on average. As can be seen, the number of deals increases over time with 2015 being the dominant transaction year—reflecting the underlying growth in private equity deal volume. In addition, it can be observed that our sample of targets are primarily within the manufacturing sector, similar to Davis et al. (2014).

Table 4 presents the descriptive statistics for our two panels of buyout targets and control firms for the period one year prior to the buyout. The results indicate that, in line with our matching methodology, the buyouts and control firms have similar employee and revenue means. In comparison to previous studies, the size of sales and employment for our buyout firms is similar to the ones found in the UK by Cressy et al. (2011), but is larger relative to other studies—e.g. Boucly et al. (2011) or Olsson and Tåg (2017). This could indicate that our sample has a disproportionate amount of larger buyout firms. Given that we use one single database to obtain our accounting data, one could argue that only the larger firms would report comprehensive data for the years needed. However, this would unlikely be a problem for the rest of our analysis, as the findings from Amess and Wright (2012) indicate that employment effects are not contingent on firm size. In terms of leverage, our buyout sample closely resembles the targets of Boucly et al. (2011), albeit being somewhat lower. The tendency for the targets to have a higher total assets and EBITDA average is in line with the findings of Cressy et al. (2011). Also worth highlighting is the difference in pre-buyout revenue growth between the targets and the control firms. A similar trend is acknowledged by Cressy et al. (2011), Boucly et al. (2011). This shows the importance of controlling for this potential selection bias effect. In summary, the descriptive statistics seem to be in line with previous studies.

Table 3. The total number and share of LBOs and controls by year, country and sector.

Panel	LBOs		Controls		Total	
	N	%	N	%	N	%
Total number and proportion						
Deal year						
2012	52	15%	148	16%	200	16%
2013	62	18%	175	18%	237	18%
2014	89	26%	243	26%	332	26%
2015	134	40%	383	40%	517	40%
Total	337	100%	949	100%	1,286	100%
Country						
United Kingdom	89	24%	246	26%	335	26%
France	46	10%	127	13%	173	13%
Italy	42	57%	123	13%	165	13%
Spain	42	19%	125	13%	167	13%
Germany	30	8%	89	9%	119	9%
Netherlands	13	14%	33	3%	46	4%
Belgium	22	29%	62	7%	84	7%
Sweden	37	27%	100	11%	137	11%
Finland	16	5%	44	5%	60	5%
Total	337	100%	949	100%	1,286	100%
Sector						
Business services	63	19%	186	20%	249	20%
Construction	14	4%	38	4%	52	4%
Consumer-related	61	18%	176	19%	237	18%
Electrical equipment	8	2%	21	2%	29	2%
Energy	2	1%	5	1%	7	1%
Financial services	10	3%	26	3%	36	3%
Manufacturing	78	23%	213	22%	291	22%
Media and communications	14	4%	37	4%	51	4%
Medical	7	2%	20	2%	27	2%
Metals and mining	6	2%	15	2%	21	2%
Technical hardware	9	3%	24	3%	33	2%
Transportation	5	1%	16	2%	21	2%
Wholesale and retail	60	18%	172	18%	232	17%
Total	337	100%	949	100%	1,286	100%

The above table presents the composition of our total sample of buyouts and controls, grouped by year, country, and sector classification. Sector classification is based on the two-digit NACE2 industry code associated with each firm.

Table 4. Summary statistics for firm variables in our sample of LBOs and controls.

Variable	Median	Mean	S.D.	Q1	Q3	Freq.
Panel A: LBOs						
Emp_{T-1}	111	357	1,372	56	273	337
Rev_{T-1}	28.71	66.09	216.10	13.43	60.60	337
$RevGr_{T-1}$	6.53	11.61	38.92	-0.12	16.64	337
$EBITDA_{T-1}$	2.95	7.42	17.92	1.24	7.51	303
TA_{T-1}	22.36	68.16	229.11	9.16	54.23	337
$CAPEX_{T-1}$	0.49	1.74	11.39	0.15	1.95	298
$Leverage_{T-1}$	60.76	63.49	26.42	46.42	79.41	334
Panel B: Controls						
Emp_{T-1}	109	305	1,155	54	256	949
Rev_{T-1}	25.03	52.96	134.14	12.11	53.53	949
$RevGr_{T-1}$	4.32	7.21	19.50	-1.69	12.83	949
$EBITDA_{T-1}$	1.35	4.31	16.52	0.46	3.66	916
TA_{T-1}	16.76	51.52	157.60	7.84	38.39	949
$CAPEX_{T-1}$	0.45	2.11	12.30	0.10	1.78	890
$Leverage_{T-1}$	65.46	65.25	26.11	46.59	84.53	940

Summary statistics for the main operating metrics that are being tested in the regressions. Emp is employment, Rev is revenue, RevGr is revenue growth, TA is total assets, CAPEX is capital expenditures. Leverage is defined as debt divided by total assets. Each variable is expressed one year prior to the buyout year T. A comprehensive definition list of the variables is found in section 3.2.

4.2 Main Results

We use Eq. (1) and perform a regression with firm and year fixed effects and cluster standard errors at the firm level. The results are presented in table 5.

Table 5. Regression results for our pooled sample of LBOs and controls, with specific operating metrics tested as dependent variables.

Dependent variable	log(Empl)	log(Rev)	log(EBITDA)	log(TA)	log(CAPEX)	Leverage
Model	(1)	(2)	(3)	(4)	(5)	(6)
<i>LBO</i> × <i>Post</i>	0.0617*** (0.0093)	0.0517*** (0.0094)	0.0396* (0.0226)	0.0696*** (0.0124)	0.1790*** (0.0308)	1.5454 (1.0835)
<i>Post</i>	−0.0109** (0.0050)	−0.0132** (0.0056)	−0.0027 (0.0163)	−0.0094 (0.0058)	−0.0343 (0.0317)	−0.0149 (0.4839)
Constant	2.1136*** (0.0021)	1.4617*** (0.0023)	0.3495*** (0.0075)	1.3238*** (0.0023)	−0.2619*** (0.0149)	64.056*** (0.2207)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Number of obs.	7,715	7,713	6,114	7,712	6,486	7,536
Adj. R ²	0.9602	0.9456	0.8266	0.9443	0.6597	0.8059

***p<0.01; **p<0.05; *p<0.10

The table illustrates the OLS regression estimates for Eq. (1) for a sample of LBO targets and their control firms. The sample period is 2010-2018. Empl is employment, Rev is revenue, TA is total assets, CAPEX is capital expenditures and Leverage is defined as debt divided by total assets. The LBO term is equal to 1 if the observation is a buyout and 0 otherwise. See section 3.2 for a comprehensive definition list. All regressions are run with firm and year fixed effects. Standard errors in parentheses, where error terms are clustered at the firm level.

Table 6. Regression results for our pooled sample of LBOs and controls, with specific operating metrics tested as dependent variables and including a control for pre-buyout sales growth.

Dependent variable	log(Empl)	log(Rev)	log(EBITDA)	log(TA)	log(CAPEX)	Leverage
Model	(1)	(2)	(3)	(4)	(5)	(6)
<i>LBO × Post</i>	0.0521*** (0.0081)	0.0406*** (0.0080)	0.0288 (0.0220)	0.0592*** (0.0114)	0.1723*** (0.0310)	1.5557 (1.0937)
<i>RevGr × Post</i>	0.0022*** (0.0007)	0.0025*** (0.0007)	0.0042*** (0.0006)	0.0023*** (0.0007)	0.0016** (0.0006)	−0.0023 (0.0162)
<i>Post</i>	−0.0267*** (0.0063)	−0.0315 (0.0072)	−0.0346** (0.0172)	−0.0263*** (0.0069)	−0.0477 (0.0317)	0.0017 (0.4851)
Constant	2.1137*** (0.0023)	1.4618*** (0.0026)	0.3508*** (0.0076)	1.3239*** (0.0025)	−0.2615*** (0.0149)	64.0555*** (0.2206)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Number of obs.	7,715	7,713	6,114	7,712	6,486	7,536
Adj. R ²	0.9635	0.9509	0.8304	0.9477	0.6572	0.8059

***p<0.01; **p<0.05; *p<0.10

The table illustrates the OLS regression estimates for Eq. (2) for a sample of LBO targets and their control firms. The sample period is 2010-2018. Empl is employment, Rev is revenue, TA is total assets, CAPEX is capital expenditures and Leverage is defined as debt divided by total assets. RevGr is the revenue growth of the firm prior to the year of buyout. The LBO term is equal to 1 if the observation is a buyout and 0 otherwise. See section 3.2 for a comprehensive definition list. All regressions are run with firm and year fixed effects. Standard errors in parentheses, where error terms are clustered at the firm level.

Table 5 shows the regression results for Eq. (1), where the *LBO* term is omitted from the regression due to multicollinearity as a result of the fixed effects. We see that the coefficient for the interaction term $LBO \times Post$ is significant and positive on the 1% level for all models except for 3 and 6. As can be inferred from the positive coefficient, firms undergoing a buyout grow their employees, revenues, total assets, capital expenditures and EBITDA more than their controls. This indicates that buyout firms experience more growth relative to controls in not only employment but also other size-related operating metrics, in line with our initial notion that private equity is more concerned with growth. EBITDA is only significant at the 10% level, although it could potentially stem from reporting inconsistencies across countries or missing observations.

As a first robustness test, we investigate whether the above observations hold when we include a control for pre-buyout revenue growth in line with Eq. (2). The results are presented in table 6, where the pre-buyout sales growth interaction term, $RevGr \times Post$, is significant at the 1% level for all regressions except 5 and 6. Since some of the post-period growth could be attributed to the selection of faster growing companies ex-ante, this variable helps to account for such selection bias. In addition, the $LBO \times Post$ interaction term is not significant for leverage in either table 5 or 6. While LBO transactions are made with leverage, it should be mentioned that leverage typically does not appear on the operating company level (Boucly et al. 2011). As such, it is not necessarily expected that the operating company of a target should increase its leverage significantly post-buyout.

As mentioned in section 3, we also use a Welch's *t*-test to verify our results, see table 7. The results indicate that the cumulative employment growth of targets is statistically significant from that of controls for the three periods of time post-buyout: T-1 to T+1, T-1 to T+2 and T-1 to T+3. This change is not significant in the period of T-2 to T-1, suggesting that there is not a relationship between targets and their pre-buyout employment growth. Failing to control for this could mean that any (positive) post-buyout growth could mistakenly be attributed to the actions of the private equity firm instead of the firms chosen for buyout. Since we do not find any significant change in the undisturbed period, we conclude that selection bias is likely not a substantial problem in our sample.

Table 7. Welch's t-test of cumulative employment growth of LBOs relative to controls.

Time period	T-2 to T-1	T-1 to T+1	T-1 to T+2	T-1 to T+3
Test	(1)	(2)	(3)	(4)
Employment	0.1519 (0.1116)	0.2895*** (0.1067)	0.3610*** (0.1122)	0.4902*** (0.1158)

***p<0.01; **p<0.05; *p<0.10

Figures represent the difference in average cumulative employment growth between LBOs and controls. The sample period is 2010-2018. The different columns represent different periods in relation to the buyout year T. Standard errors are in parentheses.

Furthermore, to make sure that our results are not driven by large-scale acquisitions or mergers, we perform a robustness check where we exclude companies, buyouts and non-buyouts, whose total assets grow more than 100% at any given year, an approach similar to Amess and Wright (2012). The adjusted results are presented in table A1 in the appendix. As can be observed, we do not find any material differences in our results after performing this adjustment and are still able to make the same interpretations.

With tables 5, 6, A1 and 7, we are therefore able to confirm hypothesis 1b that post-buyout growth differs significantly from controls on a pan-European scale and reject hypothesis 1a.

In order to provide a direct comparison for our results and previous research, we extend our methodology and use Eq. (2) on a country level for every country with at least 30 buyouts. This includes, UK, France, Italy, Spain, Germany and Sweden. The results are presented in table A2. This yields several findings. Firstly, the results obtained for France and Spain are in line with prior research (Pellon et al. 2007; Boucly et al. 2011). Secondly, for countries that to our knowledge have not previously been examined in isolation – i.e. Italy and Germany – we are able to provide an initial outlook on their development. The last observation pertains to the results from the UK and Sweden. As presented in section 2, previous studies conducted in the UK have either reached a conclusion that LBOs do not have a significant effect on post-buyout employment (Amess and Wright, 2007; 2012) or that they do have a negative significant effect on employment (Cressy et al., 2011). However, our results suggest that UK targets experience a positive employment growth as a result of the buyout. This is to our knowledge the first time that the consistency of UK results has been challenged. A similar conclusion can be drawn from the Swedish data sample. According to our results, Swedish buyouts experience a positive effect as a result of being acquired by a private equity player, contradicting earlier studies performed by Bergström et al. (2007) and Olsson and Tåg (2017). The fact that most of our countries experience a positive employment growth following a buyout, could be an indication

that private equity, at present, primarily focuses on growth – reflected in our first hypothesis. It could also be that this growth occurs indirectly through other means, tested in our second hypothesis below.

To test our second hypothesis, we perform regressions using model (3), with the results presented in table 8. The terms *LBO*, *FD* and $LBO \times FD$ are omitted from the regression due to multicollinearity as a result of the fixed effects. The table shows that the interaction dummy $LBO \times FD \times Post$ is insignificant for nearly all regressions, indicating that we fail to find any connection that targets operating in financially dependent sectors experience disproportionate growth post buyout as a result of being financially constrained prior to the buyout. While this interaction term is positive and significant in the first regression model that tests employment – a result that is similar to Boucly et al. (2011) for private-to-private transactions – we hesitate to infer any particular conclusions from this result. The reason for this is that if the theory holds that financially constrained companies grow disproportionately post-buyout as a result of the alleviation of financial constraints, then we would expect this to hold for most growth metrics in addition to employment. Since this only holds for employment according to table 8, we are cautious to make any definitive conclusions. However, since Boucly’s findings pertain only to French buyouts – and since there lacks sufficient precedence on the topic – it could be difficult to make the claim that these results should necessarily hold on a broader European scale.

Table 8. Regression results including the effect of financial dependence for our pooled sample of LBOs and controls, with specific operating metrics tested as dependent variables.

Dependent variable	log(Empl)	log(Rev)	log(EBITDA)	log(TA)	log(CAPEX)	Leverage
Model	(1)	(2)	(3)	(4)	(5)	(6)
<i>LBO × FD × Post</i>	0.0713** (0.0304)	0.0259 (0.0306)	−0.0094 (0.0774)	0.0197 (0.0333)	−0.0006 (0.0771)	3.8544 (3.6637)
<i>LBO × Post</i>	0.0987*** (0.0272)	0.0627** (0.0262)	0.0490 (0.0623)	0.0609** (0.0295)	0.1559** (0.0719)	3.5114 (2.9000)
<i>FD × Post</i>	0.0010 (0.0164)	0.0139 (0.0149)	−0.0136 (0.0464)	0.0046 (0.0182)	0.0420 (0.0437)	−3.6140** (1.5066)
<i>Post</i>	−0.0143 (0.0143)	0.0028 (0.0121)	−0.0073 (0.0330)	−0.0038 (0.0151)	−0.0008 (0.0422)	−3.1516** (1.2686)
Constant	2.4993*** (0.0028)	1.7339*** (0.0028)	0.6006*** (0.0100)	1.6040*** (0.0031)	0.1222*** (0.0128)	67.2485 (0.3032)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Number of obs.	4,110	4,110	3,318	4,107	3,497	4,008
Adj. R ²	0.9315	0.9301	0.8079	0.9413	0.7656	0.8045

***p<0.01; **p<0.05; *p<0.10

The table illustrates the OLS regression estimates for Eq. (3) for a sample of LBO targets and their control firms. The sample period is 2010-2018. Empl is employment, Rev is revenue, TA is total assets, CAPEX is capital expenditures and Leverage is defined as debt divided by total assets. RevGr is the revenue growth of the firm prior to the year of buyout. The LBO term is equal to 1 if the observation is a buyout and 0 otherwise. FD is the median financial dependence of the industry that the company belongs to. See section 3.2 for a comprehensive definition list. All regressions are run with firm and year fixed effects. Standard errors in parentheses, where error terms are clustered at the firm level.

Lastly, we proceed with testing our third hypothesis relating to whether labour policies as specific institutional factors could help drive country-specific levels of buyout growth compared to controls. Table 9 contains the regression results obtained from Eq. (4).

Table 9. Regression results for select labour index variables, with the dependent variable being the logarithm of employment.

Source	OECD	CBR	ILO
Index variable	<i>EPRC</i>	<i>Dism</i>	<i>EPLex</i>
Model	(1)	(2)	(3)
$LBO \times Ind \times Post$	-0.0099 (0.0192)	0.0089 (0.0085)	-0.0388 (0.1596)
$LBO \times Post$	0.0855* (0.0480)	0.0141 (0.0460)	0.0783 (0.0695)
$Ind \times Post$	-0.0132 (0.0093)	-0.0044 (0.0039)	-0.0576 (0.0811)
$Post$	0.0211 (0.0226)	0.0127 (0.0212)	0.0138 (0.0344)
Constant	2.1136*** (0.0021)	2.1136*** (0.0021)	2.1136*** (0.0021)
Year FE	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
Number of obs.	7,715	7,715	7,715
Adj. R ²	0.9602	0.9602	0.9602

p<0.01***, p<0.05**, p<0.10*

The table illustrates the OLS regression estimates for Eq. (4) for a sample of LBO targets and their control firms. The sample period is 2010-2018. The dependent variable is the logarithm of employment. The LBO term is equal to 1 if the observation is a buyout and 0 otherwise. Ind indicates the index variable of choice. See section 3.2 for a comprehensive definition list of the variables. All regressions are run with firm and year fixed effects. Standard errors in parentheses, where error terms are clustered at the firm level.

Similarly to Eq. (3), several variables are omitted from this test as well. The terms LBO , Ind and $LBO \times Ind$ are omitted from the regression due to multicollinearity resulting from the fixed effects. As can be seen in table 9, the $LBO \times Ind \times Post$ interaction term is not significant in any specification, meaning that a relationship between indices and post-buyout growth cannot be found. Although we are primarily interested in analysing how private equity firms act (whether pursuing a growth agenda or cost-cutting strategy) in different labour regulation environments, it is also interesting to note that the $Ind \times Post$ term is insignificant standalone. This means that firms in general do not vary in employment growth as a result of these indices. To conclude, against our third hypothesis and in line with the findings from Boucly et al.

(2011), we are unable to establish a connection between the extent of buyout growth relative to controls as a result of variations in these specific labour indices. We see three explanations that could motivate these results. First, it could be that country-specific labour regulation, as reflected in these indices, is a weak predictor of relative post-buyout growth. Second, it could be that labour regulation, de facto policies and other related institutional factors on a country-level enact a mediating effect on growth but that these indices fail to reflect them accordingly. Lastly, it could be that this level of abstraction is too high, meaning that labour regulation and standards at e.g. the level of industry could instead be conceived to exert an impact, although this notion is not supported by e.g. Boucly et al. (2011). While this last point could hold, it is nevertheless difficult to conduct leximetric cross-country studies with more detailed data that is not already covered by the labour indices. Regardless of which explanation we rely on, we conclude that our findings fail to indicate any connection with country-specific labour regulation and relative post-buyout growth.

5. Conclusion

This paper investigates the effect of private equity ownership on employment growth on a selection of nine European countries. In total, the sample includes 337 buyouts from the United Kingdom, France, Italy, Spain, Belgium, Germany, the Netherlands, Sweden and Finland in the period of 2012 to 2015.

Drawing on previously used methodologies, the results of this paper illuminates three key findings. Firstly, by testing the effect of being a private equity-owned company, we conclude that on a consolidated European-level, employment growth for buyouts is higher than their control firms – confirming hypothesis 1b and rejecting hypothesis 1a. In addition, as the regressions measuring the effect on other size-related operating metrics than employment are mostly positive, we believe that this could be an indication that employment grows as an effect of a larger growth agenda pursued by the private equity firms. Furthermore, by performing regressions on a country-level we enable comparisons to prior studies, highlighting the contrasting results found in countries such as the UK and Sweden. In accordance with recent research, the positive employment development may merely be a reflection to the change from cost-cutting strategies to instead focusing more on growth. Such growth could also arise through various means, one of which is investigated further in our second hypothesis.

For the second hypothesis, we investigate whether a positive employment development could arise as a result of private equity firms acting as alleviators of credit constraints. Using Eq. (3), we are able to establish a positive significant relationship between financially constrained firms (specifically private-to-private transactions) and post-buyout employment growth. However, since all other models that measure other size-related operating metrics are not significant, we hesitate to make any definitive conclusions. It is also not clear whether this is the expected result as there lacks sufficient precedence for this particular analysis and as such we challenge the validity of the theory proposed by Boucly et al (2011) on a broader European scale.

Our third finding relates to the notion of whether differing labour regulations across countries could lead to differences in post-buyout growth in employment. The connection between labour regulation and supposed post-buyout growth has been proposed as a potential factor by prior studies, e.g. Boucly et al. (2011) or Olsson and Tåg (2017). Using three commonly used labour indices, we are unable to find any such connection between labour regulation and post-buyout growth.

There are a few limitations in our study that are worth noting. The first pertains to the constraint of our data sample. In comparison to prior studies, our sample is relatively small which makes it difficult to draw more definitive conclusions. However, in order to perform a pan-European study we are naturally constrained to using fewer databases, as opposed to accessing single country-level databases used by prior researchers. Doing the latter in a cross-regional context would also be methodologically difficult as the data could be fraught with inconsistencies. As such, it is difficult to obtain a larger sample size for this type of study.

A second caveat relates to selection bias. A principal concern in constructing the control group is to account for the fact that buyouts do not occur randomly, i.e. there is some selection bias. An ideal counterfactual would consist of companies that have been subject to buyout attempts but chosen not to sell. However, this is by nature methodologically difficult to implement. In line with previous research, we control for pre-buyout revenue growth and also perform a Welch t-test on the observed employment change in the time preceding the buyout to mitigate the effect of such bias (Boucly et al., 2011; Cressy et al., 2011). Another way of mitigating selection bias would be to use propensity score matching as a way of generating similar control firms, instead of matching based on distinct firm characteristics. However, since we are interested in specific prior results in a European context that have mostly been generated through the cell-matching methodology, we have employed the same approach to remain consistent. In addition, we note from Davis et al. (2014) that the matching methodology does not materially impact the outcome of their study.

A third issue is the challenge to measure solely organic employment growth. The targets could pursue acquisitions or mergers, creating the illusion that the buyout creates new employment. To control for this, we have performed a robustness check where we have excluded any company that has experienced 100% growth or more in total assets in any consecutive year, where we find that our results still hold. Despite this correction, one could argue that it is difficult to control for the effect of inorganic growth if one does not use establishment-level data (Davis et al., 2014). Due to our broad geographical focus and lacking data, such establishment-level analysis was not feasible.

As a result of this study, we hope to illuminate certain issues that could be the target of future research. In line with our first finding, we believe that the increase in employment growth could be due to the change of focus from cost-cutting strategies to growth. This opens up for several new studies. Firstly, we believe it is relevant to once again test the countries that earlier have been perceived to not have a significant positive relationship with private equity ownership and employment growth, since more recent data could lead to other conclusions.

Secondly, to continue on the notion that the investment style of private equity has changed, one could investigate whether metrics pertaining to cost-cutting or growth has changed during this period of time where this shift is believed to have taken place. In addition, it could be of interest to identify other regulatory factors that could impact post-buyout growth and identify them either by changing the level of abstraction – e.g. looking at the context of industry rather than country – or by changing the methodology, e.g. by conducting in-depth surveys with practitioners. Lastly, we believe it is relevant to continue comparing results between geographies. As suggested by our results, there might be differences between regions. Without being able to conclude where this specific difference derives from, we welcome further studies on the subject.

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7. Appendix

Table A1. Regression results for a subset of the total sample of LBOs and controls, containing only companies whose total assets grow less than 100% at any consecutive year. Specific operating metrics tested as dependent variables and including a control for pre-buyout sales growth.

Dependent variable	log(Empl)	log(Rev)	log(EBITDA)	log(TA)	log(CAPEX)	Leverage
Model	(1)	(2)	(3)	(4)	(5)	(6)
<i>LBO × Post</i>	0.0387*** (0.0077)	0.0373*** (0.0079)	0.0220 (0.0223)	0.0385*** (0.0094)	0.1564*** (0.0411)	1.8267 (1.1643)
<i>RevGr × Post</i>	0.0031*** (0.0003)	0.0035*** (0.0003)	0.0043*** (0.0007)	0.0027*** (0.0004)	−0.0002 (0.0017)	−0.0128 (0.0348)
<i>Post</i>	−0.0248*** (0.0045)	−0.0318*** (0.0060)	−0.0284 (0.0176)	−0.0195*** (0.0048)	−0.0024 (0.0390)	0.1711 (0.5031)
Constant	2.1235*** (0.0021)	1.4726*** (0.0025)	0.3479*** (0.0077)	1.3293*** (0.0019)	−0.3045*** (0.0182)	63.8283*** (0.2137)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Number of obs.	7,127	7,126	5,683	7,125	4,485	6,996
Adj. R ²	0.9693	0.9589	0.8364	0.9687	0.6316	0.8187

***p<0.01; **p<0.05; *p<0.10

The table illustrates the OLS regression estimates for Eq. (2) for a sample of LBO targets and their control firms and excludes firms whose total assets grow more than 100% at any consecutive year. The sample period is 2010-2018. Empl is employment, Rev is revenue, TA is total assets, CAPEX is capital expenditures and Leverage is defined as debt divided by total assets. RevGr is the revenue growth of the firm prior to the year of buyout. The LBO term is equal to 1 if the observation is a buyout and 0 otherwise. See section 3.2 for a comprehensive definition list. All regressions are run with firm and year fixed effects. Standard errors in parentheses, where error terms are clustered at the firm level.

Table A2. Regression results for the pooled sample of LBOs and controls, grouped by countries with more than 30 LBOs.

Country	UK	France	Italy	Spain	Sweden	Germany
Model	(1)	(2)	(3)	(4)	(5)	(6)
<i>LBO</i> × <i>Post</i>	0.0463*** (0.0161)	0.0576*** (0.0192)	0.0721** (0.0283)	0.0989*** (0.0227)	0.0451** (0.0215)	0.0174 (0.0191)
<i>RevGr</i> × <i>Post</i>	0.0038*** (0.0006)	0.0035*** (0.0007)	0.0030*** (0.0010)	0.0026*** (0.0007)	0.0012** (0.0005)	0.0029*** (0.0005)
<i>Post</i>	−0.0551*** (0.0085)	−0.0433*** (0.0127)	−0.0171 (0.0104)	−0.0250 (0.0159)	−0.0375** (0.0162)	−0.0194** (0.0096)
Constant	2.3125*** (0.0037)	1.9986*** (0.0053)	1.8728*** (0.0046)	2.2765*** (0.0088)	1.8335*** (0.0087)	2.3472*** (0.0032)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Number of obs.	2,010	1,038	990	1,002	821	714
Adj. R ²	0.9716	0.9440	0.9584	0.9551	0.9535	0.9648

***p<0.01; **p<0.05; *p<0.10

The table illustrates the OLS regression estimates for Eq. (2) for a sample of LBO targets and their control firms, performed by each country with at least 30 buyouts. The sample period is 2010-2018. Empl is employment, Rev is revenue, TA is total assets, CAPEX is capital expenditures and Leverage is defined as debt divided by total assets. RevGr is the revenue growth of the firm prior to the year of buyout. The LBO term is equal to 1 if the observation is a buyout and 0 otherwise. See section 3.2 for a comprehensive definition list. All regressions are run with firm and year fixed effects. Standard errors in parentheses, where error terms are clustered at the firm level.