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THE EFFECTS OF PRIVATE EQUITY ON TARGETS: MAJORITY VERSUS MINORITY INVESTMENTS

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Abstract

This paper investigates the differential effects on performance of majority and minority Private Equity (PE) investments. By using a difference in difference approach, we compare a sample of 191 firms in the years following the PE investment with a control group constituted by firms that are the most similar to targets in the years preceding the deal. We find that, in the three years following PE investments, targets achieve higher profitability, higher sales and employ more than their control counterparts, and this is more so for minority deals. We also show that PE targets experience a significantly higher board turnover than controls, and that changes are more pronounced in majority investments where both the CEO and the chairman are replaced. Moving to targets ownership types, we find that PEs are especially effective when they acquire a minority interest in family firms or, to some extent, when they take a majority stake in non-family firms. These results suggest that when dealing with family firms PEs are particularly beneficial when they tend to complement rather than substitute the incumbent human capital, namely the entrepreneurs/owners serving as CEO or chairman before the PE steps in.

Keywords: Private Equity, Minority Investments, Private Firms, Firm Performance, Corporate Governance

JEL codes: G32, G34

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1. Introduction

The empirical literature assessing the effects of private equity (PE) on firm performance overlaps, to a large extent, with the one that studies the impact of leveraged buyouts (LBOs) on firm efficiency and growth. LBOs hit the news for the first time in the 1980s, when a wave of highly levered hostile takeovers took private a number of public companies in the United States. The effects of LBOs have been lively debated, with advocates describing LBOs as a superior form of governance, and opponents depicting them as a means to transfer value from employees to barbarian corporate raiders at the expense of firm long-term growth and profitability. This controversy generated a stream of literature investigating the effects of LBOs on firm performance and, at the same time, drew attention on PE firms, that is on the investors specialized in LBOs.

As most of the available evidence on the role of PEs follows from this literature, our knowledge on their effectiveness largely refers to deals carried out as LBOs. This paper marks somewhat of a departure, and contributes to the literature by examining the effect of PEs depending on the stake they acquire in the target, which we interpret as a proxy for the power of the investor in affecting target management and, ultimately, performance. To this end, we group PE deals in *majorities*, when one or more PE houses acquire at least a 50% interest in the equity of the target, and *minorities*[1], when the stake acquired is lower than 50%[2].

As a matter of fact, PE firms have changed considerably since the 1980s, extending their scope from LBOs, that entail almost always the acquisition of a majority stake, to minority investments as well. The rising relevance of this type of deals is underlined by Kaplan and Stromberg (2009), who speculate that, having the current economic crisis led to a substantial growth of buyout related interest rates, PEs will increasingly take minority stakes. Thanks to the experience gained in governance and operational engineering, they should be able to provide value without full control. The non-trivial role played by minority investments is also discussed in Lerner et al. (2009), who study a large sample of

[1] Since in the firms belonging to our sample ownership is highly concentrated, when one ore more PEs acquire a minority stake in the equity of the target, there is always a blockholder or a coalition of blockholders controlling the majority of the voting rights.

[2] We exclude from our study early-stage ventures (seed and start-up), i.e. the subset of investments where the PE (usually referred to as Venture Capital firm) funds companies in their primary development stage.

deals completed worldwide by PEs from 1984 to 2008. Minority investments account for 72.7% of transactions, and are largely predominant in venture capital (99.5%), growth capital (99.3%), and public equity (98.9%) investments, fairly common in private placement (59.9%) and other PE-backed acquisitions (56.8%), and are almost completely missing in LBOs (0.9%). According to Lerner et al. (2009), growth capital and private investments in public equity (i.e. investments that are almost completely undertaken by means of minority stakes) account for 23.4% of all PE-backed transactions worldwide (24.5% in Western Europe and 24.3% in North America)[3].

Notwithstanding the widespread occurrence of minority deals, as well as their increasingly higher relevance in the activity of Private Equity firms (World Economic Forum Reports, 2008-2010), the empirical evidence on their effects is still scant. To the best of our knowledge, the only exception is the paper by Chen et al. (2012) who, considering a sample of 123 US minority investments in publicly held targets carried out from 1990 to 2006, find only weak evidence that PEs increase firm profitability. In contrast with the literature that investigates either minority deals or LBOs, we examine a sample of both majority and minority investments undertaken by PEs in Italy between 1995 and 2004. Overall, our sample is made up of 90 majority and 101 minority targets that, for the most part, are family and private and belong to manufacturing industries.

We offer various contributions to the empirical literature, which can be summarised as follows. First, by studying both majority and minority targets we are able to show how the two groups of firms compare in the years before the deal, underlining whether PE choice between acquiring a minority or a majority stake is predictable on the basis of target observables.

Second, we provide new evidence on the effect of minority deals on firm performance. We find sharp differences in the effects of PEs depending on whether we consider

[3] In building our argument about the importance of minority investments we do not consider the data provided by Lerner et al. (2009) on Venture Capital investments (VC), since we have excluded from our analysis early stage ventures. Further, we discuss Lerner et al. (2009) data on the number of transactions but not on deal dollar volume. While the latter gives a clear snapshot about the relevance of LBOs, it should be taken with some caution. As Lerner et al. (2009) underline, the transaction value for an M&A (such as an LBO) is measured by the enterprise value of the company acquired, while the transaction value for private placements (such as growth capital investments) is measured by the capital provided by the investors, i.e. LBOs should be overestimated with respect to private placements.

minority or majority operations. Our results are suggestive of substantial variability along this dimension, whose origin is unveiled thanks to the quality of our data.

Third, by contrasting majority with minority investments we contribute to the LBO literature investigating whether debt financing, that in principle should be higher for majority deals, affects firm performance. One could speculate *ex ante* that majority deals (which for the most part are LBOs), bringing to the target a relatively large amount of debt, should create value by improving efficiency. Because of this, earnings should increase thanks to cost cutting rather than sales growth. Minority deals, on the contrary, most likely should create value by pushing sales, being in principle less levered. For example, by studying a large sample of PE-backed LBOs undertaken in France from 1994 to 2004, Boucly et al. (2011) find that firms experience significant sales and profitability growth after the deal, similarly to what one would expect ex-ante for minority targets. Therefore, whether or not majority and minority targets perform differently after the deal is still an empirical matter worth of investigation.

Fourth, we contribute to the growing body of literature exploring the drivers of PE effects (e.g., Acharya et al., 2012), by contrasting the extent of changes in the board of target firms for minority vis-à-vis majority investments. Arguably, we expect changes after the deal to reflect the proportion of equity invested.

Fifth, we employ a *difference in differences* approach to evaluate the effect of PEs on a number of performance indicators, governance and ownership features. While this technique has already been used to assess PE impact on targets performance (Boucly et al., 2011; Chung, 2011), to the best of our knowledge we are the first to combine this technique with *propensity score matching* (see, for example, Heckman and Vitlacyl, 2007) to assess PE effect on operating performance, governance and ownership changes, thus refining the accuracy of our empirical findings. Our methodological contribution largely draws from the literature on programme evaluation, where interest lies in the causal effects of an intervention on certain outcomes.

The main results of the paper can be summarised as follows. First, we show that majority and minority targets are not significantly different in the years before the deal. Namely, the two types of firms are not different in term of ownership, sector and age and the same goes for a number of other observables, ranging from sales, EBITDA, EBITDA margin (i.e., EBITDA over sales) and working capital, to leverage, capital expenditure and

number of employees. Furthermore, we don't find any difference in growth opportunities, measured as the change in sales, EBITDA and EBITDA margin in the two years before the PE steps in. This evidence suggests that the PE acquires a majority or minority stake depending on its investment strategy or on the previous owner willingness to sell the control of the target. Since minority and majority targets (MIN and MAJ, respectively, in what follows) are very similar before the deal, it is particularly interesting to compare the two groups of firms after the investment to identify the determinants of the PE effect.

Second, we find that PEs overall boost firms' growth and that this effect is markedly larger for MINs if compared to that of MAJs. Similar evidence holds when the number of employees is considered: MAJs and MINs have more employees than controls after the deal, but the effect of PE is larger for MINs. The difference between the two types of targets is even larger when it comes to profitability: while for both EBITDA is significantly higher than controls, MINs clearly outperform MAJs.

Third, we document that PEs boost growth by increasing capital expenditure, mainly in the first year after the deal, both in MAJs and MINs. In this respect, our evidence is consistent with that provided by Boucly et al. (2011) and Chung (2011), who show that targets after the LBO, beyond growing more and being more profitable, invest more than their peer group. On the contrary, the seminal paper by Kaplan (1989) and the more recent study by Harford and Kolasinski (2012) find that capital expenditures significantly decrease in the years following the investment.

Fourth, consistently with Boucly et al. (2011), we show that targets' degree of vertical integration, as measured by value added over sales, is not affected by the deal. In particular, we find that costs for intermediary inputs, such as raw material, lease and external services, are not significantly different for targets and controls. Moreover, we show that PEs don't enhance efficiency for both MINs and MAJs: after the deal, EBITDA over sales, sales over number of employees, and value added over number of employees are not significantly different between targets and controls. Besides, our results suggest that PEs don't bring any improvement in working capital management.

Fifth, we find that MAJs are much more levered than MINs, even if way less than the public-to-private LBOs studied by Guo et al. (2011). In particular, one year after the deal MAJs, unlike MINs, present a much higher ratio of debt to EBITDA than controls. While MINs also expand their debt to sales ratio, thanks to a substantial EBITDA growth they

keep debt over EBITDA at the same level of controls. Overall, debt doesn't seem to play a relevant disciplining role: both in MINs and in the more levered MAJs, PEs pursue EBITDA growth far more by boosting sales than by cutting costs. If anything, debt appears to slow down growth, since the more levered majority targets, even if outperforming their controls, don't increase sales and EBITDA as much as MINs.

Finally, we show that PEs bring substantial changes to the board after the deal, and this is more so in majority investments. We consider changes that take place in the most influential board roles (CEO, chairman and vice-chairman) from one year before to one year after the deal[4] and we find that MAJs experience a significantly higher board and CEO turnover. We find that after the deal MAJs boards are made up of younger and less 'local' directors. In the empirical analysis, we take as proxy of localness the distance in kilometres between the place of birth of the director and the place where the firm is headquartered (see Battistin et al., 2012, for a similar approach). Results are appreciably different for MINs: boards do change more in MINs than controls, but the magnitude of the change induced by PEs is way lower than the one produced on MAJs. There is some evidence that PEs manage to appoint a new CEO and chairman also when they acquire only a minority interest in the target, but this seems to happen less frequently than in MAJs. Most importantly, when it comes to CEO/chairman turnover the difference between MINs and controls is statistically negligible (significance is just at the 10% level). Furthermore, in contrast to what we observe in MAJs, we don't detect any major change in MINs board after the deal in terms of directors' age and localness.

While the evidence about PE effect on target governance is not strikingly surprising, once it is coupled with the effect of PE on target operating performance it does deliver some interesting results. In sum, majority and minority targets, that before the deal are not significantly different from one another, seem to be approached by PEs very similarly from an operational point of view, while there are some significant differences in the financial and governance engineering. In both cases PEs boost growth, which is achieved by the acquisitions of other firms more frequently in minority than majority targets, while don't vary significantly efficiency and labour productivity. As it turns out, MINs perform

[4] For sake of simplicity, in the following we will use the terms 'board' or 'directors' interchangeably, although our study is focused on a subset of directors, namely those serving as CEO, chairman and vice-chairman.

better than MAJs, although PEs are understandably more actively involved in managing MAJs than MINs. In some respect, our evidence is consistent with the prediction of Kaplan and Stromberg (2009): PEs are able to create value at the company level without having full control of targets thanks to the their experience in operational engineering.

However, we can further elaborate on this argument, exploiting the differences that we detect by comparing MAJs to MINs. Firms in our sample and in the control group have a high degree of localness, i.e. they tend to be run by CEOs, chairman and/or vice-chairman who are born very close to the place where the company is incorporated. This evidence is consistent with that of Battistin et al. (2012) who, analysing a sample of Italian Banks, show that the distribution of the distance between the city of birth of bankers and the city where their banks are headquartered is heavily skewed toward zero. However, in some respect our results are in contrast with those provided by Battistin et al. (2012) and Giannetti et al. (2012). The former study finds that localness doesn't improve performance, and, in a similar vein, the latter, studying a sample of Chinese firms, shows that local directors with foreign experience positively affect firm performances. On the contrary, we find that targets whose localness has been mitigated by PEs (i.e. MAJs) are associated with lower operating performance. One could argue that in our sample, that by and large is made up of small and medium family and private firms, a high degree of localness stands for the presence on the board of people who have played or still play a pivotal role in the development of the firm itself. In such a setting, PEs prove to be beneficial particularly when they tend to complement (minority investments) rather than substitute (majority investments) existing management.

As a matter of fact, our evidence shows that targets perform better in MINs, when PEs assign to their representatives on the board a monitoring role – supplementing rather than substituting existing human capital –, rather than in MAJs – when PEs take on the executive role by appointing new CEOs. This is particularly true when dealing with family and private firms: in such a setting PEs are especially effective in providing incumbent entrepreneurs/owners with suitable support to exploit growth opportunities. This result is consistent with the literature on the impact of large blockholders (Holderness, 1996; Maury and Pajuste, 2005; Attig et al. 2011) and shareholder activism (Brav et al. 2008; Klein and Zur, 2009), as long as it demonstrates a positive impact of active minority shareholders on corporate governance efficiency and firm performances. Moreover, it allows us to speculate that PE minority investments generate particularly effective

governance structure, that help in limiting private firms managers' opportunism (Lerner, 1995; Schulze et al., 2002; Morck and Yeung, 2003) while preserving key idiosyncratic competencies of the former management. In non-family firms, on the contrary, PEs prove to be more effective in MAJs, as in this type of firms human capital is less firm specific and more easily substitutable.

The remainder of the paper is organized as follows. Section 2 describes our data sources and the sample, section 3 introduces the research design, section 4 presents the empirical results and section 5 concludes.

2. Data

The aim of this section is to describe the various data sources that we employ in the empirical exercise. Complementary information from *five* different databases is integrated to identify PE transactions and to gather financial statements and other non-financial information on targets. More specific details on the data collection process are fully documented in Appendix B.

2.1 Targets

We use two main sources to collect information on deals: Private Equity Monitor (PEM), a data archive from the University of Castellanza, and Mergermarket. The former offers extensive coverage of PE investments completed in Italy from 2000, while the latter reports PE deals from 1999, even if its coverage is widespread only after 2002. Since these two sources leave substantially uncovered the years before 1999, we obtained from the University of Castellanza an additional list of PE deals completed in the period 1995-1999.

We considered only PE deals carried out before 2004. Since we require that target's operating performances are tracked up to 3 years after the deal, at the time we started collecting data, target's financial reports were available up to 2007. Furthermore, we required that the target is headquartered in Italy and backed by a PE for the first time. Overall, our sources list 455 PE deals targeted at Italian firms in the period 1995-2004.

Financial statements and other non-financial data for targets were obtained from Telemaco, a database administered by the Italian Chamber of Commerce[5]. For each target we considered financial reports from two years before to three years after the deal, thus ensuring data comparability over time (see Appendix B for further details). Finally, data on governance, i.e. name and tax code of CEOs, chairmen and vice-chairmen sitting on the board of targets at time -1 and +1, were gathered from Cerved, our second source of financial and non-financial information on PE-backed and control firms.

After discarding deals with missing or poor quality information, our working sample consists of 90 majority and 101 minority investments completed from 1995 to 2004. From Figure 1 and the evidence presented in Appendix B, it is reassuring to notice that the distribution over time of PEs in the sample is markedly similar to our best estimate of the total number of deals in Italy for the period considered. This finding bodes well for representativeness of the conclusions drawn from our empirical exercise.

[Figure 1 about here]

There are 104 PEs involved in the 191 deals in our sample, 27.7% of which (53 cases) are club deals. These figures are comparable to those in Guo et al. (2011), who find that the 192 US public to private investments in their sample are undertaken by 120 PEs and that 27.7% of the 94 transactions for which they have additional data are club deals. The median equity stake acquired by PEs in majority investments is 73%, ranging from a minimum of 50%[6] to a maximum of 100%, whereas in minority deals the median equity stake is 22.5%, with a minimum of 5% and a maximum of 49.5%.

Most of the targets in our sample are medium-sized, manufacturing and privately held firms (see Table 1, Panel A). 142 out of 191 belong to manufacturing industries, while 86% are family and private (see Table 1, Panel B). Only 4 firms are listed in the stock exchange at the time of the deal and they are all targets of minority investments. Subsidiaries account for 18 targets and are mostly involved (14 cases) in majority

[5] Telemaco contains a broad range of financial and non financial information about Italian limited liabilities companies. Among others, Telemaco provides individual and consolidated financial statement from 1993 on, information on shareholders, board, merger plans and the likes from 1996.

[6] In two out of 191 deals the PE firm acquires an equity stake of 50%. Even if technically these two transactions are neither majority nor minority investments, we classify them as majority deals to underline the relevant influence exerted by PE in the target.

investments. On the whole MAJs and MINs are not significantly different with respect to industry and ownership structure.

[Table 1 about here]

At time -1, the median sales of sample targets are 38.65m € and the median number of employees is 174 (see Table 2, Panel A). Firms in our sample are slightly larger in size than the ones studied by Boucly et al. (2011): the 839 (mostly private) French targets making up their sample have median sales of 13.1m € and a median number of employees of 64. Similarly, the 1009 UK private targets examined by Chung (2011) have median sales of 10.4m £. On the contrary, our sample firms are smaller than the 317 US LBOs investigated by Cohn et al. (2012), whose median sales are 220.7m $: targets in Cohn et al. (2012), however, are understandably larger than ours since they examine public to private deals, while we study almost only private to private investments. Furthermore, our targets have a median sales growth of 0.11, that is very close to the 0.13 detected by Chung (2011) and higher than the 0.08 growth rate found by Boucly et al. (2011).

[Table 2 about here]

In the years before the deal majority and minority targets in our sample are very similar (Table 2, Panel B and Panel C) in terms of size, profitability, and growth. Leverage (i.e. the ratio of net debt over EBITDA) is slightly higher for MINs than for MAJs, but the difference is not significant. More generally, none of the financial measures displayed in Table 2 are significantly different between MAJs and MINs before the deal. The same result applies to governance: in both MAJs and MINs, CEOs, chairmen and vice-chairmen are in their mid fifties and are strongly connected to the firm.

In results not reported, we formally tested for the equality of MINs and MAJs characteristics in the years before the deal, running a regression of the outcome of interest on a dummy for being a minority target, a set of solar year dummies and dummy equal to 1 a time -1 and 0 at time -2. Our results pointed to no detectable differences for the various dimensions considered, thus suggesting that targets were all comparable in terms of size, profitability, growth opportunities, capital expenditure, leverage before experiencing the PE.

2.2. The control group

A group of firms similar to those in the target population was selected to define a suitable control group for the analysis. We followed a *two step* procedure to obtain a working sample in which target and control firms present similar distributions for a large number of variables that arguably are good predictors of performance.

At first we stratified targets according to their industry and sales one year before the deal. The classification was obtained from the cross tabulation of 2 digits NACE codes and quartiles of the sales distribution. This criterion yielded 133 different cells defined over the two dimensions considered. As potential controls, for each cell we considered all firms in the population defined by the Cerved-Data Bank archive.

We refined our definition of controls by considering only firms with similar EBITDA margin in -1 and -2. We made this rule operational by implementing the following strategy. At first we selected all controls with EBITDA margin between 70% and 130% or within a ± 0.03 window centered at the target's EBITDA margin. In many instances, the number of controls for each target proved sufficiently large. In some cases, however, we were forced to apply increasingly coarser criteria. We started by extending the profitability range to 50%-150% and to ± 0.05. Then we used the profitability filter only in -1 or, if the number of potential controls was still too small, we used no profitability filter. Finally, if the number of potential controls was still small, we removed also the filter on sales but, in this case, used a narrower definition of NACE codes (i.e. 3 instead of 2 digits). This procedure eventually resulted in a total of 45,617 cases, not involved in any PE, to be used as potential controls.

After having selected controls, the working sample was constructed by controlling for additional sources of heterogeneity with respect to target firms. We employed a procedure that defines a distance between targets and controls as a function of sales, EBITDA and EBITDA margin in the years preceding the deal, and matches to each target the most similar control firm along these dimensions. Building upon the well established literature on programme evaluation (see, for example, Heckman and Vytlacil, 2007), we employed the *propensity score metric* to define the degree of similarity between target to control firms. After pooling observations for the two groups, we estimated the propensity score by running a logistic regression of the dummy for being a target firm on sales, EBITDA and EBITDA margin in the two years before the deal, as well as NACE code and year of deal

dummies. The inclusion of the former set of variables was motivated by the need of controlling for both levels and changes over time in performance in the years preceding the deal. This procedure is consistent with Lie (2001), who shows that test statistics designed to detect abnormal operating performance are more powerful when control firms are selected on the basis of both levels and changes in performance in the pre-event years.

Using well known results from the literature on *matching* in statistics (see, for example, Rubin, 2006), it is possible to show that target and control firms sharing the *same* value of the propensity score also share, on average, the *same* levels of the variables used to estimate such quantity. Building upon this result, we matched firms in the two groups to find, amongst the 45,617 potential controls defined in the first step, the most similar firms. These we used to define our final control sample. Following most of the empirical papers, we decided to match each firm undergoing PE to the closest 25 potential controls whose propensity score is at most one percentage point apart from that of the corresponding target. In most papers this procedure is referred to as 'caliper matching', such caliper allowing for a one percent coarseness in the propensity score metric. In those cases where the number of controls falling within the caliper was larger than 25, we selected at random 25 of them. On the contrary, for those targets with less than 25 controls, we kept them all in the final sample. Intuitively, the larger the number of control firms found within the caliper, the most likely it is to find firms similar to targets in the sample of controls.

By means of this procedure we select 2,826 control firms[7] (on average 2,826/191 = 15 per target), for which we retrieve: (i) financial data (the same as those gathered for targets) from time -2 to time +3, if available; (ii) name and tax code of CEOs, chairmen and vice-chairmen at time -1 and +1.

As it is confirmed from the figures presented in Table 2 (Panel A and Panel D), by construction targets and controls are very similar in the years before the deal. This 'common trend' condition in pre-intervention periods is required for the validity of the difference in differences procedure that will be used in the empirical analysis. The median control, with sales of 28.34m € and 136 employees, is somewhat smaller than the median

[7] 2,826 is the overall number of controls. Since we match controls to targets with replacement, the same firm in the same solar year can be matched to more than one target. Therefore, the number of unique firm-year controls is slightly lower (2,509).

target which has, respectively, sales of 38,65m € and 174 employees. Median EBITDA is smaller for controls (2.73m €) than targets (4.92m €), whereas EBITDA margin is rather close in the two groups (0.12 for targets vs 0.10 for controls). Net debt as a percentage of sales is almost identical in controls and targets (0.13), while Net debt as a multiple of EBITDA is slightly larger for the former (1.41 vs 1.01). In the two years before the deal, both targets and controls achieve non-trivial sales and EBITDA growth, even though the former performs a little better than the latter. Finally, directors of the 2,397 controls (2,104 unique firm-year controls) for which we have governance data in -1 and +1 are as old as and as connected to the firms as targets' directors[8]. As we will make clear in the next section, residual pre-deal differences across target and control firms will be accounted for in the estimation.

3. Methods

3.1 Effects on operating performance

To quantify the effects of PE, we compare targets with control firms before and after the deal through a *difference in differences* approach. Using results from the programme evaluation literature, the implicit assumption is that in the absence of the deal target and control firms, selected as explained in Section 2, would have presented parallel patterns over time of the variables of interest (see Heckman and Vytlacil, 2007). For the context at hand, the assumption appears plausible because of the procedure that we adopted in the definition of the control sample. Firms in this group were selected to present time series of sales, EBITDA and EBITDA margin which are the most similar to those of target firms in the two years before the deal, other than operating in the same industry. The assumption of parallel trends in the counterfactual scenario of no intervention for the target and control units is, by far, the most widely employed approach in empirical studies that assess causal relationships from observational data. The combination of difference in differences

[8] The degree of similarity between targets and the selected controls is confirmed by running a multivariate (probit) regression of a dummy for target firms on the various dimensions considered (see Table A1 of Appendix A). For both MINS and MAJs, the variables considered do not serve as good predictors of being a target versus a control firm.

estimation and propensity score matching has a longstanding tradition in empirical micro-economics (see Heckman et al., 1997).

At first we assess the effect of PE on all targets by estimating the following regression:

$$y_{it} = \beta_i + \gamma_0 p_{it} + \gamma_1 p_{it} d_i + \alpha_t + \varepsilon_{it}, \quad (1)$$

where y_{it} represents the outcome (e.g. performance) for firm i in period t, the latter index ranging from 2 years before to 3 years after the deal. The variable d_i is a dummy that identifies target firms, while p_{it} is a dummy for post-deal periods. Finally, β_i are (unobserved) firm fixed effects, and α_t are time effects. We added as additional regressors time dummies for the year of the deal, thus controlling for business cycle effects. Equation (1) sets out the comparison of target and control firms, where the time series of the outcome for the target is contrasted to that of the control firm from before to after the deal. Any difference in the outcome after the deal is measured by the parameter γ_1, which in some empirical specifications is allowed to be period-specific (first, second and third year after the deal). Outcome levels for the two groups may differ because of firm-specific unobserved factors that are captured by β_i.

We experimented with alternative specifications of equation (1), all yielding to similar conclusions. In presenting our results, we will run separate regressions for minority and majority deals. Standard errors presented are robust to heteroskedasticity and clustered at the firm level, thus allowing for general forms of serial correlation. Estimation results obtained from (1) are presented in Tables 3, 6 and A2-A4.

3.2 Effects on governance

After having estimated the direct, 'reduced form' effect of PE on operating performance through equation (1), we investigate which are the mediating factors that may have fuelled such effect. In particular, we consider the casual channel that passes through changes in the governance brought by the PE. As the board composition, both in terms of number of directors and their characteristics, most likely affects operating performance at the firm, it may well be that changes in performance are mediated by important changes in the board. As the extent of such changes is *ex ante* expected to differ between minority and majority deals, the interplay with PE type is certainly a dimension worth considering.

To this end, we will present results obtained from the following regressions:

$$b_i = \delta_0 + \delta_1 d_i + x_i + v_i, \quad (2)$$

where b_i represents the change in the board composition of firm i between -1 and 1, d_i is again a dummy for target firms, and x_i is a set of regressors that are pre-determined with respect to the deal. In our preferred specification, the latter set includes lagged values of net debt over sales, (logged) sales and EBITDA. The change in the board composition is proxied by numerous indicators that are presented in Section 4.3, that we use to check the sensitivity of our conclusions to the outcome employed. The use of changes from -1 to 1 is motivated by the availability of information on the board that we were able to retrieve - for both target and control firms - only for these two years. Thus equation (2), differently form equation (1), makes use of only one observation per firm. Results of this analysis are reported in Tables 4 and 5, and inference is carried out by using heteroskedasticity robust standard errors.

4. Results

4.1. Operating performances

The presentation of results is organised into two parts. We first discuss the effects on a variety of outcomes, and then investigate which are the drivers of these effects. We start from EBITDA, as it is not affected by asset step-up - that is typical in LBOs - and is of paramount importance for PEs, since targets price is often determined as a multiple of EBITDA. We then move to sales, which provide a first insight into the business strategy adopted by PEs. On a similar vein, EBITDA margin sheds further light on the business strategy implemented after the deal to improve target overall capacity to generate earnings. Finally, we consider the number of employees, which allows us to explore one of the most debated issues related to PE activity.

We then address the following four questions: 1) do PEs boost capital expenditures or do they decrease them to improve short-term cash flow reducing, at the same time, long-term targets profitability? 2) Do PEs change the way operations are organized by substituting employees with outsourcing? 3) Do PEs extract value from employees by reducing their

wages? 4) Thanks to their operational skills, are PEs able to improve target efficiency by boosting labour productivity?

Sales, profitability and employment

The results of the analysis are presented in Table 3. For brevity, we report only the coefficients that refer to the PE effect, which we allow to vary over time in the first three years after the deal[9]; p-values for the hypothesis that effects are constant over time are reported in the last row of the table. The first panel of the table reports results obtained by pooling targets, without distinguishing between MINs and MAJs. The two remaining panels present results obtained by stratifying by type of deal.

We find that PEs boost growth for target firms and, specifically, that after the deal PE-backed firms achieve higher EBITDA, higher sales and employ more people than their control counterparts. In the three years after the deal EBITDA is 3.2 - 4.0m € higher than in controls and this difference is significant at the 1% confidence level - see column (1) of Table 3. Considering that median value of EBITDA for targets at time -1 is 4.9m €, the post-deal difference between PE and non PE-backed firms that we document is economically relevant. The increase in EBITDA seems to be driven by the expansion of sales – see column (2). Sales growth is associated with a significant increase in the number of employees – see column (3): PE-backed firms employ more people than controls and this difference is significant at the 1% confidence level. In contrast, EBITDA margin is not different between the two groups of firms – see column (4) - leading us to argue that PEs tend to pursue EBITDA growth more by developing new sales, which in turn boosts employment, than by improving profitability of existing sales.

[*Table 3 about here*]

Overall, these results are consistent with those provided by other recent empirical studies. The comparison with the current literature, though, should be taken with caution since other studies focus only on LBOs. Boucly et al. (2011) find that targets increase EBITDA and sales, respectively, by 18% and 12% more than controls. The PE-backed firms investigated by Chung (2011) outperform controls by a similar magnitude in both sales and EBITDA. However, the subsample of privately held targets, for which Chung (2011)

[9] The full set of regressions is available from the authors upon request.

reports also EBITDA margin, experiences a decrease in this ratio. This result is qualitatively consistent with our evidence, since it entails that EBITDA growth is completely driven by the expansion of sales and not by the improvement of sales profitability. In contrast with our evidence, Guo et al. (2011) show that EBITDA margin increases in the years after the deal. For the subsample of firms for which Guo et al. (2011) have post-buyout financial data, they find that targets improve operating performance after the deal but the magnitude of this improvement is substantially smaller than the one detected by Kaplan (1989). The latter, in particular, provides evidence on PE effects that is rather different from that documented in this paper. Kaplan (1989) shows that, in the first two years after the deal, targets don't significantly change EBITDA, have a slower sales growth than industry controls, but significantly enhance EBITDA margin. This pattern of post-buyout performance is affected by post-deal divestitures: post-transaction sales and EBITDA decrease or grow less than in controls because some of the target assets are sold shortly after the deal, whereas EBITDA margin, accounting for asset stripping, does increase. On the contrary, consistently with Boucly et al. (2011) and, particularly, Chung (2011), our results show that PEs tend to increase EBITDA way more by expanding sales than by improving return on sales, leading us to argue that in more recent and private to private deals PEs seem more effective in freeing growth potential of targets than in improving efficiency.

The same result holds for employment. Boucly et al. (2011) and Chung (2011) show, as we do, that after the deal the number of employees significantly increases, whereas Amess and Wright (2012) find that both PE-backed and non PE-backed deals have no significant effect on employment. Muscarella and Vetsuypens (1990) find that median number of employees falls after the transaction and that the employment reduction is attributable mainly to post-LBO divestiture activities[10], similarly to the evidence provided by Kaplan (1989). Davis et al. (2011) show that the level of employment in targets declines after the deal, even though PE-backed firms create more jobs at new establishments than controls. In sum, while evidence on this issue is all but conclusive, the private to private European deals examined by Boucly et al. (2011), Chung (2011) and in our paper seem to indicate that PEs, boosting sales, are also beneficial to employment: this result is in contrast with

[10] More precisely, Muscarella and Vetsuypens (1990) have employment data for a subsample of 26 firms.

early criticisms that LBOs improve targets profitability cutting jobs, i.e. at the expense of employees.

Once we split our sample into MINs and MAJs, it turns out that the effect on EBITDA is much larger in the latter group. The central and the right hand side panels of Table 3 report the breakdown by type of deal. We know that, in the years before the transaction, MAJs and MINs are not different from each other on a number of observables, ranging from the type of ownership to industry, from size and profitability levels at time -1 to growth ratios. In the years following the investment, however, in MAJs EBITDA is at best 2.2m € larger than in controls – see column (9) of Table 3 - whereas in MINs it is always at least about 5.0m € larger than in control firms – see column (5). Sales and employment grow in both groups of targets, even though more in MINs than in MAJs, whereas in neither group EBITDA to sales is different from controls[11]. Quite interestingly, we find that the effects on employment and sales present different patterns over time for the two types of deal (see the p-values reported in the last row of Table 3). While for MAJs the effect is statistically constant in the three years after the operation, for MINs the pattern of the effect varies over time, increasing more sharply from the first to the third year.

Overall these results suggest that PEs approach MAJs and MINs similarly, increasing EBITDA by developing sales, which in turns translate into more jobs, and leaving substantially unchanged return on sales. The effect, however, proves to be larger in minority targets and to evolve differently over time in the two groups of firms.

Sources of profitability and growth

We then turn our attention to possible sources of post-deal growth in size and profitability. The results of the analysis are fully documented in Tables A2, A3 and A4 of Appendix A. In what follows, we only present a summary of the main findings.

[11] After the deal, a non-trivial part of MINs and MAJs acquire other companies. Namely, 57% of MINs (i.e., 58 out of 101) and 39% of MAJs (i.e., 35 out of 90) engage in one or more acquisitions in the time period spanning from the deal and the following three years. Since data on external growth weren't available for controls, we can't evaluate its effect within a difference in differences research setting. However, given the magnitude of the M&A activities carried out by targets after the deal, it is rather sensible to assume that it plays a role in explaining the growth in size.

Firstly we examine the evolution of expenditure in property, plant and equipment. Studies analysing the first wave of US PTP for the most part (Bull, 1989; Kaplan, 1989; Muscarella and Vetsuypen, 1990; Smith, 1990) find that capital expenditure declines after the transaction. However, as mentioned before, the rationale of that wave of deals was mainly to improve efficiency, removing, among others, investments with negative net present value (Jensen, 1989). In contrast with this stream of empirical studies, Harford and Kolasinski (2012), who investigate 226 PTP deals completed in the US from 1993 to 2001, show that PEs do not reduce investments by their portfolio companies. Furthermore, two recent papers on (mainly) private to private European buyouts (Boucly et al., 2011; Chung, 2011) provide evidence that PEs increase capital expenditure of targets firm as a means to expand sales. Since we detect a strong PE effect on growth, we expected that capital expenditures would increase as well. Actually, investments in property, plant and equipment do increase after the deal also in our sample, both in absolute euro value and as a percentage of sales, and the difference between targets and controls is statistically and economically significant[12] (see columns (1) and (2), Table A2). The PE effect on investment is concentrated mainly in the first year after the deal, meaning that PEs boost growth right after the acquisition is completed, consistently with their need to improve financial performance in a relatively short period of time. Economically, the effect is about the same size in both minority and majority targets, although it is statistically more significant in the latter (columns (4) and (7), Table A2). Since MINs grow more than MAJs, it follows that capital expenditure in MINs are more effective.

Secondly, PEs might improve profitability by outsourcing targets production to more cost effective suppliers. We measure the degree of outsourcing by the ratio of value added over sales and, consistently with Boucly et al. (2011), we find that in the years following the deal minority and majority targets don't rely on subcontractors more than controls (columns (3), (6) and (9), Table A2). We further examine whether PEs change the way targets operations are organized looking at the rates of raw material consumptions, external services and lease over sales, even if, understandably, the overall effect on

[12] This result should be taken with a grain of salt due to the way we measure capital expenditure. We derive investments in property, plant and equipment as the difference between tangible asset at time *t* and at time *t-1*, net of depreciation. This means that asset step-up, if adopted, is computed as investment. However, since asset step-up should take place immediately after the deal (i.e., at time 0), investments at time 1 should be only marginally affected, if anything, by this accounting policy.

profitability of these potential changes cannot be significant, since value added as a percentage of sales doesn't vary after the deal. None of these three ratios shows any relevant variation over the three post-transaction years, meaning that PEs don't bring in any visible changes in the relationships between targets and their suppliers.

A third potential source of value creation concerns wages and labour productivity. While we have already shown that, in contrast to early evidence, our sample-targets hire more people than controls, PEs could improve profitability by reducing wages and by enhancing employees' productivity. Elaborating on the hypothesis theorized by Jensen (1989) that LBOs achieve efficiency by replacing monitoring by large staff with incentives and compensations, Lichtenberg and Siegel (1990) argue that at the shop floor level LBOs should determine a reduction of the ratio of white-collar workers and an increase in the compensation of blue-collar workers. However, the evidence provided by the authors is that compensation of nonproduction workers declines after the deal, while that of production worker is unchanged. In a somewhat similar vein, Amess and Wright (2007) find that after the deal targets experience lower wage growth than controls. In contrast with the scant evidence about PE effect on wages, we show that compensation, measured as the ratio of personnel costs to the average number of employees, is higher for PE-backed firms than controls (columns (1), (5) and (9), Table A3). However, this difference, though statistically significant, is economically negligible, being about 1 thousand € per year. Since our sample-targets significantly increase employment in the years after the deal, the overall effect on wages could originate from the recruitment of new, more skilled (and slightly more costly) people to cope with the organizational demands caused by the larger size.

While *ex ante* PE effect on wages is not univocally definable, PE investments, and particularly LBOs, are expected to increase efficiency thanks to a number of governance mechanisms that motivate managers to eliminate waste and to pursue value creation. We have already shown that the ratio of value added to sales doesn't change after the deal, meaning that targets are not better at procurement than controls. Another possible source of efficiency gain is the improvement of labour productivity. Lichtenberg and Siegel (1990) show that plant productivity significantly rises in the years after the deal and similar conclusions are drawn by Harris et al. (2005). On the whole we don't detect any major effect of PE on efficiency, measured as sales and value added per capita (columns (2), (6) and (10), Table A3). Overall, the increase in the number of employees coupled

with a slight growth of wages and with the lack of any productivity gain translates into a significant increase in the ratio of personnel costs over sales, which in the years after the deal is about 2% higher in targets than controls (columns (3), (8) and (11), Table A3).

A fourth source of value might come from working capital management. While reduction in working capital doesn't immediately increase EBITDA or sales, it can expand the amount of resources available to finance growth projects or, for highly leveraged deals, to service the debt. The reason why PEs are expected to improve cash management are related to the way LBOs are realized: by burdening targets with highly levered financial structures, PEs urge managers to improve cash management in order to repay the debt. The extant evidence about PE effect on working capital management is still rather scarce: Smith (1990) finds that management of working capital is significantly tightened after the deal; on the contrary, Weir and Lang (1998) provide only limited support for the thesis that PEs reduce working capital. We show that the ratio of working capital to sales is not different between targets and controls (column (4), Table A3). One could argue that the pressure to decrease working capital should be higher in majority than minority targets, since the former should be more levered than the latter (see below). In contrast with this line of reasoning, though, one could argue that if PEs have distinctive competencies in managing working capital, they should exploit them whether the target is highly levered or not. Once we split our sample into majority and minority targets we find that in the three years after the deal, both groups reach a ratio of working capital over sales not different from that of controls (columns (8) and (12), Table A3). A possible explanation is that working capital and cash management, contrary to what probably occurred at the time of the early LBOs, is now a well-established technique and therefore PEs are no more better skilled than other types of owners/managers in this area.

Overall, PEs don't seem to bring to targets any major change, with only one notable exception: the growth in capital expenditure, particularly in the very first year after the deal. On the other hand, outsourcing remains substantially unaltered, as well as working capital management. In contrast to early evidence, wages increase after the deal, even though the growth is economically negligible, and labour productivity continues to be at the same level of that of controls. Furthermore, PEs appear to approach majority and minority targets in the same way at the operational level: qualitatively we don't find any significant difference between the two groups of firms. Thanks to their competences in operational engineering, PEs are able to boost target growth without sacrificing EBITDA

margin, and this effect is even more noticeable in minority deals where most likely PEs manage to cooperate successfully with incumbent entrepreneurs (see below).

In the followings, in the attempt to understand the reasons behind the effect of PE on growth and profitability, we examine the changes made by PEs in the board of targets, focusing in particular on the most influential roles, such as chairman, vice-chairman and CEO (Section 4.3). Before moving on to target's governance, we examine the financial structure of majority and minority deals and the PE effect on taxes (Section 4.2.).

4.2. Deal financial structure and PE impact on target taxes

As expected, PEs strongly affect targets' leverage: financial debts as a percentage of sales are significantly higher in targets than in controls and this difference is substantially stable over the three year-period examined in our study (column (1), Table A4). Contrasting majority with minority investments reveals that the former use much more debt than the latter (columns (5) and (9), Table A4). The trend in the ratio of debt to EBITDA further clarifies the difference between the two types of transactions: this ratio is significantly higher in majority targets than in controls, whereas it is at similar levels in minority backed and control firms (columns (6) and (10), Table A4). In MINs the growth in debt is completely counterbalanced by the strong increase in EBITDA that takes place since the very first post-deal year, meaning that PEs substantially expand the financing capacity of targets. On the contrary, in MAJs the larger (with respect to MINs) portion of debt brought in by PEs is associated with a weaker growth in EBITDA, leaving this type of target steadily more levered than its control group. It should be noted, however, that MAJs in our sample, while more levered than MINs, are less levered than LBOs in other studies: at time +1, the median debt to EBITDA ratio of majority deals in our sample is 3.3, compared to a median value of 6.0 detected by Guo et al. (2009) and 4.1 by Acharya et al. (2012).

A common argument against PEs, since the first wave of US LBOs, is that they transfer value from the state to themselves (e.g., Kaplan 1989). The rationale is that, by writing-up assets and by using highly levered financial structure, PEs make targets to pay fewer taxes. While it is behind the scope of our paper to study the impact of tax benefits on PEs returns, we provide evidence about PE effect on target taxes. Overall we find that MINs actually

pay more taxes than their control group (column (7), Table A4) in euro value: the difference (about 1m €) is slightly significant and stable over the three year-period following the deal and, most likely, it is due to the strong increase in sales and EBITDA experienced by MINs. On the other hand, we don't detect any difference between MAJs and their control firms with respect to euro value taxes (column (11), Table A4), suggesting that the increase in EBITDA is offset by a reduction of the tax rate. The ratio of taxes to EBITDA actually decreases (columns (4), (8) and (12), Table A4) and this effect is largely due to majority deals which are burdened with a larger amount of interests on debt and, being more frequently than minority investments carried out by the incorporation of a new company (77.8% of the case against 21.8%), can more easily amortize goodwill and other intangibles. In sum, while PEs effectively manage to reduce the tax rate, at worse PE-backed firms pay as much taxes as controls (majority deals) and, at best, they pay more taxes than their counterpart non PE-backed firms (minority deals), thanks to the strong growth in sales and profitability underwent after the transaction. Therefore, we don't find any support to the usual argument that PE houses extract value from the State by significantly reducing the amount of taxes paid.

4.3. Governance

Governance engineering is one of the sets of changes applied by PEs to targets (Kaplan and Strömberg, 2009) in order to affect operating performance and, broadly speaking, it encompasses all the initiatives brought in by PEs to control portfolio company boards. Governance engineering has been studied mainly by comparing PE-backed firms with publicly held company boards (Acharya et al., 2009) or by examining the changes made by PEs in public to private deals (Cornelli and Karakas, 2012), whereas there is still little evidence about the effect of PEs on governance in private to private transactions.

After having investigated the direct effects of PEs on performance, in this section we study which factors are likely drivers of the results documented so far. To this end we investigate whether changes to the boards associated with PEs vary between majority and minority targets, since the two groups of firms, in the years after the deal, achieve rather different operating performance: clearly, in general we expect that PEs are more involved in the management of majority than minority targets.

We focus on the more influential board roles, and namely on the roles of CEO, chairman and vice-chairman. We gathered board data one year before and one year after the deal. Consistently with the research procedure adopted to evaluate PE effect on operating performance, the very same information is collected also for controls. Overall, considering that we require that board information are available both at time -1 and time +1, our sample is made up of 186 targets and 2,104 controls, covering almost 5,200 directors.

Table 4 shows the effect of PE on boards of majority and minority targets. We start by examining the following variables: *turnover*, that measures the percentage of new board members; *new CEO*, which equals to one if there is at least one new CEO at time +1 and zero otherwise; *new chairman*, which is equal to one if there is at least one new chairman or vice-chairman at time +1[13].

Overall, we find that PEs significantly affect board composition, both changing role of existing directors and placing new representatives. As expected, the change is more substantial for MAJs, turnover being almost 3 times larger than for MINs - see the results in column (1) vis-à-vis column (4). In MINs, PEs appoint new chairman and, to a lower extent, CEO, but the change induced by PEs is only weakly significant (at the 10% level). Since our data set contains information only on CEO, chairman and vice-chairman, and since it is highly unlikely that PEs forgo to nominate their representative in the boards, our finding suggests that in MINs PEs tend to leave the roles of CEO and chairman/vice-chairman to incumbent owners/managers, and to designate some directors, who are outside the spectrum of our analysis. In results not presented, we also find that in MINs PEs tend to move directors serving as CEO one year before the deal to the role of chairman or vice-chairman, suggesting that when PEs change CEO they keep her/him in the board with a different role. This effect, however, is significant only at the 10% level. On the other hand, when it comes to MAJs, PEs do nominate new CEO and chairman/vice-chairman and the effect, beyond being large, is statistically different from zero at the conventional levels.

The PE effect on MAJs boards doesn't come as a surprise: other empirical studies on LBOs, that for the most part are majority deals, show that PEs tend to substitute CEO more frequently than other type of owners. Gong and Wu (2011) find that in 51% of the

[13] If a chairman serves also as CEO, she is counted as CEO.

cases the CEO is replaced within two years from the deal; Guo et al. (2011) show that in 35 out of the 95 LBOs making up their sample (37.2%) the CEO is changed within one year from the investment, and Acharya et al. (2012) find that 39% of the CEOs in their sample of 66 case studies are substituted within the first 100 days.

[*Table 4 about here*]

By appointing new chairmen, vice-chairmen and CEOs PEs can potentially change board size and demographics. In particular, we are interested in examining whether PEs modify the number of directors, their age, gender and localness. By documenting whether changes to the board reflect also changes in the characteristics of the directors appointed, we can shed light on how human capital is replaced within the firm as a result of the PE.

The results of this analysis are presented in Table 5. The following dimensions are considered: average age of members, percentage of local members and percentage of male members. For each variable, we first study how its value differs between target and control firms at time -1 (see columns labelled 'before deal' in the table). We then consider the effects of PE on the change of this variable as explained in Section 3.2 (see columns labelled 'effect').

Overall, we find that board characteristics of minority targets are not altered with the entry of the PE: none of the columns that consider the effects of PEs present statistically significant results for the target dummy. On the contrary, we find that boards of firms involved in majority investments significantly change once the PE steps in. The importance of this result is reinforced by the fact that, before the deal, target and control firms did not differ along the various dimensions considered, and this apply to both majority and minority deals.

In particular, we find that after the deal boards of majority deals are younger (2 years and 5 months younger than their controls counterpart) and contain a higher quota of male directors. Furthermore, the percentage of local (i.e., born in the same province where the company is headquartered) CEOs, chairmen and vice-chairmen is significantly lower in majority target boards than in those of controls.

These results are largely consistent with what we could expect *ex ante*: in majority deals, having more power PEs appoint a higher number of new directors, thus changing board demographics. The effects on demographics, with one exception, are not surprising: on the

one hand, median age of people sitting in majority target boards at time -1 is almost 55 (Table 2, Panel B), so it is rather reasonable that new directors are slightly younger; on the other hand, it is not strikingly clear how the already high mean quota of male directors (91% at time -1) manages to reach an even higher value one year after the deal.

[*Table 5 about here*]

The variable that measures the percentage of directors who are born in the province where the company is incorporated deserves further discussion. Targets in our sample show, before the deal, a high degree of localness: since our sample firms are largely medium sized, family and private, it is reasonable to argue that in such a setting a high degree of localness within boards comes from the strong ties that connect the people at the top of the firm, who often belong to the same family(s). At the same time, it is predictable that directors appointed by PE are not necessary connected to the target and, therefore, are born, on average, farer away from the place the firm is headquartered than the directors they replace. Empirical papers about effect of social connections on performance show that strong ties between executives/directors tend to be detrimental to firm performance. Battistin et al. (2012), for example, who study 1,736 managers and 739 Italian banks over the period 1993-2001, find that local connections don't improve bank performance: if anything, localness hurts operating performance of some types of banks. The authors argue that strong social ties might negatively affect performance in two ways: by lowering skill level of people working in the firm, since recruitment is (at least) partly driven by localness; by preventing poor performing managers from being fired. In a somewhat complementary vein, Giannetti et al. (2012), examining 33,707 directors of 1,733 listed Chinese companies in the years 1999-2009, find that directors with foreign experience are beneficial to firms, since they improve company profitability and corporate governance. According to this stream of research, we would expect that, whenever PEs weaken ties between target board members, company operating performance should improve because the non-connected directors help removing poor performing managers or bring in new valuable experience to the firm. Our evidence, on the contrary, suggests that PE impact on target operating performance is stronger the fewer the changes in the board. In particular, as discussed above, in minority targets we find only weak evidence that PEs appoint new chairman/vice-chairman and new CEO, leaving, as a result, board demographics substantially unchanged. On the contrary, in majority targets PEs nominate both new CEO and new chairman/vice-chairman, significantly modifying board characteristics. As a

matter of fact, the former group of firms performs better than the latter, even though in both groups PEs seem to employ rather similar strategies.

We conjecture that our empirical evidence should be interpreted in light of firm ownership. The large part of our sample consists of relatively small, family and private firms. For these, high localness of CEO, chairman and vice-chairman might indicate that these individuals have contributed to the foundation/development of the firm, and might still be central for it. In this context, PEs seem more effective when, by leaving the roles of CEO and chairman/vice-chairman to incumbent owners/entrepreneurs, they play more a monitoring and advisory role. This is the situation often encountered in the case of minority investments.

To provide empirical ground for such explanation, we collected, whenever available, ownership data at time -1 and +1 for all control firms in our working sample for which we had information on boards (41.5% of the initial sample). We then defined as family and private all controls whose equity in both years was controlled by natural persons or by limited partnerships/sole proprietorships. This definition is somewhat conservative, since it could well be that a family and private firm is owned by a corporation that, in turns, is controlled by one or more natural persons (i.e., the entrepreneur and/or members of her family). However, since the definition we adopted points to a very simple ownership structure, it should be a reasonably good proxy of firms whose shareholders are involved to some degree in the management of the company. We set the threshold at the 50% level, i.e. we defined a control firm as family and private when 50% or more of the equity was owned by natural persons or limited partnerships/sole proprietorships. Results discussed in the paper remain qualitatively unchanged setting the equity threshold at the 55%, 60%, 65%, or 70% level. Because of this, in what follows we only report results obtained from the 50% threshold.

We then run the regression presented in section 3.1 for (A) the subset of family and private targets and controls and for (B) the subset of non-family targets and controls. It is worth noting that the dummy for family ownership is not collinear with the PE deal type, so that we have enough variability in the data to replicate the main analysis after stratification. Overall, results reported in Table 6 confirm that PEs are more effective when they acquire a minority stake in the equity of the target and when they invest in family and private firms. More precisely, when dealing with family and private firms

(Table 6, Panel A), PEs manage to boost profitability and growth only if they acquire a minority interest. On the contrary, if their interest in the target is larger than 50% (MAJs), they don't provide any meaningful change in EBITDA and sales; if anything, they significantly lower EBITDA margin. The picture is somehow symmetric for non-family targets and controls (Table 6, Panel B): PEs don't affect MINs, which in the years following the deals are not distinguishable from their controls, while they significantly increase MAJs sales. Even in MAJs, though, there is no effect on profitability.

[*Table 6 about here*]

These results can be discussed in the context of the literature on the impact of large blockholders (Holderness, 1996; Maury and Pajuste, 2005; Attig et al. 2011) and shareholder activism (Brav et al. 2008; Klein and Zur, 2009) on corporate governance efficiency and firm performances. This literature has focused much of the attention on large listed companies and thus links the possible benefits mainly to an active monitoring role and, ultimately, to the reduction of public companies' agency cost. Our sample is almost completely composed by unlisted companies - with a large prevalence of private and family controllers - with a limited degree of separation between ownership and control. The governance structure of such firms is generally regarded as relatively efficient, as the risks of shareholders expropriation are limited. However, how Schulze et al. (2002) point out, it is too simplistic to presume that all these firms are less vulnerable to the agency cost since it overlooks the complexity of exchanges that occur among the private firm's decision agents.

Owners and management vary in degree of overlap even within family-owned firms, which can range from the small owner-managed (with perhaps few co-owners) to large extended family-owned firms where many family shareholders have no link with the internal operations of the firm (Uhlaner at al., 2007). However, there are several agency problems that can affect also the most concentrated ownership private firms. First, owner controlled firms, may be severely financially constrained as the owners are generally unwilling to diminish their control on the firm. Therefore they may be unable to fully deploy the firm's growth potential. In addition, the concentrated ownership may limit the ability to compete in the labour market for the best managers and employee, since the upper management positions tend to be occupied or reserved for owners or member of their family. Moreover these firms may either be unable or unwilling to use equity as part

of their compensation packages, further reducing the incentives for the best applicants. Finally, owner-managers could adopt non-economically motivated behaviour taking actions driven by personal preferences or taste, either egoistic of altruistic (Schulze et al., 2002). This happens, for instance, as the owner managers use their position for helping friends, follow their passions or interests (e.g. support the local sport team) or refuse to change the firm's business model because it might threaten the status quo, require too much effort, or imply redundancies of long time collaborators.

Private equity funds may help reducing these problems as they are structured for providing intensive oversight of their portfolio firms, both through board participation and informal visit, and involvement in key strategic decisions. The evidence presented for the family and private targets subset suggest that PE-backed family-controlled firms may constitute a 'superior' governance structure compared both to PE takeover (majority investments) and a family ownership without PE involvement (control group). The PE fund acquisition of a minority stake may further reinforce private firms governance, thanks to the specialist competencies (e.g. financial competencies) brought in by the PE managers and a monitoring and spurring role. The substitution of the former controllers, as in majority investments, on the contrary, do not affect performances, possibly because a trade off exist between the gain in specialist competencies brought in by PE managers and the loss of firm specific human capital with the exit of former entrepreneur/managers. Finally, when the target is a non family and private company, the acquisition of a majority stake is on average preferable, since the competencies of incumbent managers are easier to substitute.

5. Conclusions

This paper has compared the impact of majority and minority PE investments and hence has provided insight into the mechanism through which PEs contribute to the value creation of the target firms.

PE investments have been long-time associated to LBOs and Going Private deals and as such viewed as a way to implement drastic restructuring in reluctant target companies and, sometimes, as a means to transfer value from employees to corporate riders. More recent contributions on privately held firms, in contrast, suggest that LBOs might be an engine of

growth, being the PEs 'better owners' than individuals and families for relaxing credit constraints and taking advantage of unexploited growth opportunities (Boucly et al., 2011). We build on that research and compare for the first time the post investment performances of both majority (largely overlapping to LBOs) and minority investments, thus providing evidences on the overall PE impact on portfolio companies. Interestingly majority and minority targets in our sample are statistically undistinguishable as for industry, size, age and pre-acquisition performance and therefore the only difference between the two groups seems to be the share acquired by the private equity funds. This confirms that majorities and minorities are not distinct and inherently different class of financial operations but, in contrast, are variants to a common genus of investment. That is to say that the share acquired by the PE fund is not determined by distinctive features of the target firm, but only by the fund's preference and, possibly to a larger extent, by the owners' resolution to hand over control.

Our results confirm that PE investments contribute to the value generation of portfolio companies trough the promotion of growth more than trough restructuring and efficiency seeking measures. PE targets experience strong growth in sales, profitability and employment. Moreover, we find strong evidence that this effect is way larger in minority investments, where the PEs complement and not substitute the previous ownership and back exiting corporate executives rather than substitute them. Therefore PE contribution to the value of target firms seems to be more associated with their operational engineering competences and a monitoring and spurring role rather than with an 'institutional' superiority as control subject, while additional advantage comes from the retention of the human capital brought by the subject that have contributed to the foundation or the development of the firm before the PE investment.

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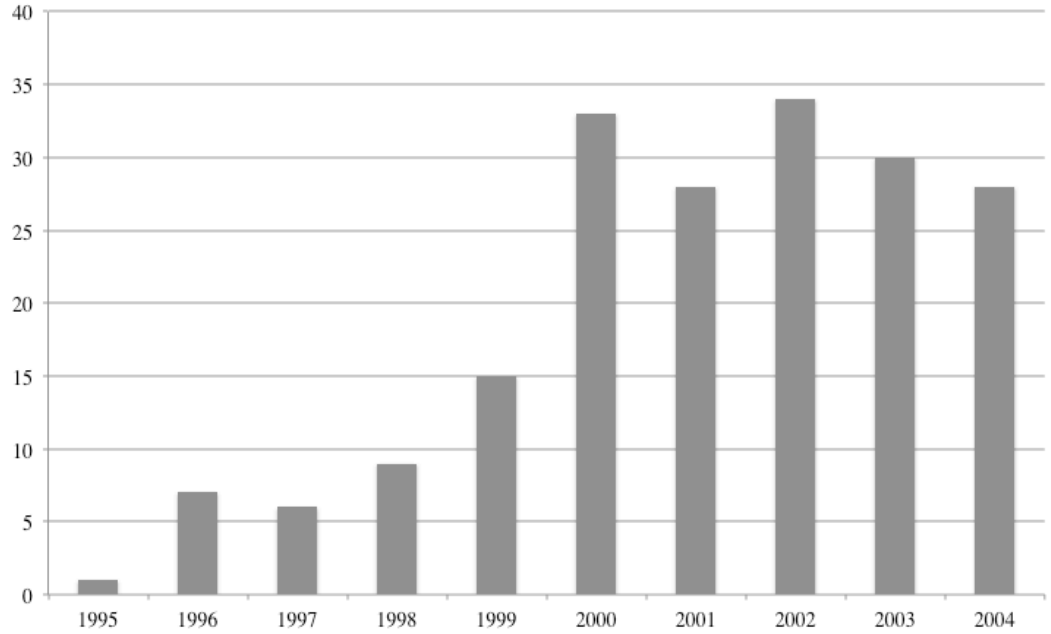
Figure 1: distribution of deals in our sample over the period 1995-2004.



Note. The "sample size" refers to the number of (minority and majority) deals used in the analysis. More details on the sample and on the population of deal it has been extracted from are provided in Appendix B.

Table 1: targets' industry and type of ownership.

Panel A: target industry	All targets	Majorities	Minorities
Manufacturing	142	72	70
Wholesale and retail	11	7	4
Information and telecommunication	20	6	14
Other	18	5	13
Total	191	90	101

Panel B: target type of ownership	All targets	Majorities	Minorities
Listed	4	0	4
Family and private	164	75	89
Subsidiary	18	14	4
Other	4	1	3
Not known	1	0	1
Total	191	90	101

Table 2: target and control firms. All summary statistics are measured at year -1, with the exception of sales growth and EBITDA growth, measured as the change from year -2 to year-1.

Panel A: all targets	mean	median	sd	1st quartile	3rd quartile	# obs
Sales (m €)	106.95	38.65	207.35	18.52	83.92	189
EBITDA (m €)	14.51	4.92	32.84	2.10	10.07	189
EBITDA/sales	0.10	0.12	0.47	0.08	0.18	189
working capital (m €)	34.05	8.43	122.49	3.42	22.15	189
net invested capital (m €)	55.40	13.78	133.89	4.77	33.67	189
capital expenditure (m €)	-5.75	-0.85	18.22	-4.18	-0.21	181
# employees	595.65	174.00	1354.02	78.00	488.00	170
net debt/sales	0.01	-0.13	2.16	-0.22	0.02	189
net debt/ebitda	-7.51	-1.01	81.68	-2.37	0.09	182
sales growth	0.25	0.11	0.75	0.01	0.22	181
ebitda growth	0.38	0.12	2.64	-0.13	0.34	181
# of directors (*)	1.98	2.00	1.03	1.00	2.00	186
mean age of directors	54.72	55.00	9.60	47.50	61.00	186
directors' localness	0.54	0.50	0.44	0.00	1.00	186
% of male directors	0.92	1.00	0.20	1.00	1.00	186

Panel B: majorities	mean	median	sd	1st quartile	3rd quartile	# obs
Sales (m €)	82.31	38.39	190.08	20.40	67.84	89
EBITDA (m €)	12.01	4.64	32.64	2.74	8.14	89
EBITDA/sales	0.14	0.15	0.15	0.09	0.21	89
working capital (m €)	29.01	7.83	155.28	3.36	16.90	89
net invested capital (m €)	42.91	12.99	115.69	5.35	29.74	89
capital expenditure (m €)	-5.36	-0.92	18.59	-3.55	-0.33	87
# employees	349.42	162.00	702.68	83.00	294.00	81
net debt/sales	-0.11	-0.11	0.29	-0.21	0.04	89
net debt/ebitda	-0.78	-0.70	2.85	-1.67	0.22	85
sales growth	0.21	0.09	0.92	0.01	0.18	86
ebitda growth	0.45	0.07	3.18	-0.17	0.28	86
# of directors (*)	2.11	2.00	1.17	1.00	3.00	85
mean age of directors	54.56	54.50	8.50	49.00	60.50	85
directors' localness	0.55	0.50	0.44	0.00	1.00	85
% of male directors	0.91	1.00	0.22	1.00	1.00	85

Panel C: minorities	mean	median	sd	1st quartile	3rd quartile	# obs
Sales (m €)	128.88	39.73	220.21	14.26	121.47	100
EBITDA (m €)	16.73	5.13	33.03	1.06	14.77	100
EBITDA/sales	0.07	0.12	0.62	0.07	0.16	100
working capital (m €)	38.52	9.56	83.75	3.44	34.46	100
net invested capital (m €)	66.52	14.43	147.92	4.01	48.84	100
capital expenditure (m €)	-6.11	-0.76	17.97	-5.28	-0.14	94
# employees	819.75	184.00	1722.04	78.00	659.00	89
net debt/sales	0.12	-0.14	2.97	-0.26	-0.02	100
net debt/ebitda	-13.42	-1.21	111.79	-2.97	-0.19	97
sales growth	0.28	0.13	0.56	0.03	0.35	95
ebitda growth	0.32	0.13	2.05	-0.13	0.49	95
# of directors (*)	1.87	2.00	0.89	1.00	2.00	101
mean age of directors	54.86	55.50	10.47	46.50	61.33	101

directors' localness	0.54	0.50	0.44	0.00	1.00	101
% of male directors	0.93	1.00	0.18	1.00	1.00	101

Panel D: controls	mean	median	sd	1st quartile	3rd quartile	# obs
Sales (m €)	55.36	28.34	109.90	12.97	63.95	2826
EBITDA (m €)	5.67	2.73	10.30	0.91	6.06	2826
EBITDA/sales	0.11	0.10	0.14	0.06	0.14	2826
working capital (m €)	18.23	7.15	92.98	2.16	17.37	2630
net invested capital (m €)	30.46	13.28	80.77	4.83	30.31	2471
capital expenditure (m €)	-2.77	-0.66	8.93	-2.46	-0.13	2815
# employees	278.98	136.00	560.60	62.00	295.50	2482
net debt/sales	-0.18	-0.13	0.54	-0.30	0.00	2471
net debt/ebitda	-2.80	-1.42	14.88	-3.60	0.00	2403
sales growth	1.75	0.08	34.07	-0.01	0.21	2826
ebitda growth	-0.25	0.07	31.38	-0.14	0.36	2826
# of directors (*)	2.18	2.00	1.29	1.00	3.00	2397
mean age of directors	55.06	55.00	9.93	48.00	61.67	2397
directors' localness	0.54	0.50	0.44	0.00	1.00	2397
% of male directors	0.89	1.00	0.25	1.00	1.00	2397

(*) Director stands for CEO, chairman o vice-chairman.

Table 3: PE effect on targets' size and profitability.

	Targets				Minorities				Majorities			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Variables	Ebitda	Ln sales	Ln employee	Ebitda /sales	Ebitda	Ln sales	Ln employee	Ebitda /sales	Ebitda	Ln sales	Ln employee	Ebitda /sales
1st year effect	3.200***	0.183***	0.151***	-0.0120	4.883***	0.248***	0.242***	-0.0133	1.303*	0.120**	0.0498	-0.00961
	(0.853)	(0.0334)	(0.0331)	(0.00821)	(1.442)	(0.0450)	(0.0522)	(0.0111)	(0.733)	(0.0478)	(0.0334)	(0.0123)
2nd year effect	3.453***	0.250***	0.221***	-0.0148	5.486***	0.335***	0.308***	-0.00516	1.170	0.162***	0.121***	-0.0255*
	(0.904)	(0.0418)	(0.0382)	(0.00981)	(1.544)	(0.0541)	(0.0568)	(0.0142)	(0.744)	(0.0620)	(0.0454)	(0.0132)
3rd year effect	3.971***	0.267***	0.252***	-0.0100	5.457***	0.345***	0.361***	-0.00224	2.230**	0.185**	0.129**	-0.0194
	(1.038)	(0.0497)	(0.0429)	(0.00901)	(1.739)	(0.0621)	(0.0607)	(0.0127)	(0.915)	(0.0776)	(0.0548)	(0.0126)
Average effect	3.533***	0.233***	0.206***	-0.0123	5.271***	0.308***	0.302***	-0.00688	1.547**	0.155***	0.0979**	-0.0182*
	(0.850)	(0.0392)	(0.0359)	(0.00795)	(1.447)	(0.0498)	(0.0542)	(0.0115)	(0.697)	(0.0596)	(0.0401)	(0.0107)
Observations	14,201	14,130	12,322	14,135	8,377	8,281	7,094	8,348	5,824	5,849	5,228	5,787
R-squared	0.032	0.064	0.054	0.047	0.045	0.087	0.073	0.045	0.030	0.046	0.039	0.054
Number of id	2,692	2,685	2,459	2,692	1,557	1,552	1,394	1,557	1,135	1,133	1,065	1,135
Firm Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Time Controls	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Clustered SE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Prob > F	0.454	0.009	0.000	0.794	0.787	0.019	0.003	0.543	0.185	0.326	0.063	0.287

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Note. Reported are estimates from equations that control for firm fixed effects, by target and PE type. The available data for targets refer to outcomes as measured from 2 up to 3 years after the deal. For firms in the control group the time series are centred at the year of the deal for the corresponding target (which varies between 1995 and 2004). Standard errors, in parentheses, are clustered at the firm level, allowing for heteroskedasticity and serial correlation.

Prob > : P-value for the equality of PE effect in the first, second and third year after the deal.

EBITDA is measured in million €; for descriptive statistics of the outcomes see Table 2.

Table 4: PE effect on targets' board roles.

	Minorities			Majorities		
	(1)	(2)	(3)	(4)	(5)	(6)
Variables	turnover	new ceo	new chairman	turnover	new ceo	new chairman
Dummy for treated	0.169***	0.103*	0.126*	0.503***	0.297***	0.463***
	(0.0423)	(0.0558)	(0.0692)	(0.0443)	(0.0633)	(0.0780)
Net debt over sales 2 years before the deal	0.113**	0.183**	0.143	0.109**	0.159*	0.190
	(0.0522)	(0.0814)	(0.144)	(0.0516)	(0.0812)	(0.143)
Net debt over sales 1 year before the deal	-0.0815*	-0.199**	-0.0273	-0.0755	-0.181**	-0.0494
	(0.0490)	(0.0820)	(0.151)	(0.0496)	(0.0813)	(0.151)
Log sales 2 years before the deal	-0.0228	-0.00576	-0.0141	-0.0301	-0.0270	-0.00593
	(0.0344)	(0.0460)	(0.0768)	(0.0331)	(0.0459)	(0.0734)
Log sales 1 year before the deal	0.0547	0.0503	0.0570	0.0662*	0.0798*	0.0575
	(0.0347)	(0.0465)	(0.0747)	(0.0339)	(0.0464)	(0.0715)
EBITDA 2 years before the deal	0.00201	0.00161	0.000512	-0.00160	0.00185	-0.0171**
	(0.00244)	(0.00304)	(0.00773)	(0.00314)	(0.00429)	(0.00706)
EBITDA 1 year before the deal	-0.00121	-0.000656	-0.00112	-0.000137	-0.00343	0.0118
	(0.00245)	(0.00303)	(0.00691)	(0.00333)	(0.00413)	(0.00726)
Constant	0.199	0.109	-0.169	0.430***	-0.483***	0.666***
	(0.375)	(0.420)	(0.124)	(0.0907)	(0.134)	(0.137)
Observations	1,832	1,832	1,832	1,819	1,819	1,819
R-squared	0.170	0.128	0.122	0.258	0.143	0.142
Sector Fixed Effects	YES	YES	YES	YES	YES	YES
Province Fixed Effects	YES	YES	YES	YES	YES	YES
Time Controls	YES	YES	YES	YES	YES	YES
Robust SE	YES	YES	YES	YES	YES	YES

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Note. Reported are estimates from equations that make use of one observation per firm. For firms in the control group, the time series of outcomes are centered at the year of the deal for the corresponding target (which varies between 1995 and 2004). Standard errors, in parentheses, are robust to heteroskedasticity. *Turnover*: percentage of new board members; *new CEO*: equals to one if there is at least one new CEO at time +1 and zero otherwise; *new chairman*: equals to one if there is at least one new chairman or vice-chairman at time +1.

Table 5: PE effect on targets' board characteristics.
Panel A: minority targets

	average age		**percentage of locals**		**male quota**	
	(1)	(2)	(3)	(4)	(5)	(6)
Variables	before deal	effect	before deal	effect	before deal	effect
Dummy for treated	-0.607	-1.110	-0.000774	-0.0296	0.0134	0.00444
	(1.127)	(0.787)	(0.0479)	(0.0350)	(0.0254)	(0.0148)
Debt over sales 2 years before deal	-0.832	0.464	-0.0285	-0.0206	-0.00498	-0.0251
	(1.946)	(1.045)	(0.0857)	(0.0378)	(0.0481)	(0.0231)
Debt overs sales 1 years before deal	2.331	-0.322	-0.0139	0.0278	-0.0377	0.0154
	(1.848)	(0.982)	(0.0859)	(0.0346)	(0.0453)	(0.0210)
Log sales 2 years before deal	0.848	-0.572	0.0148	0.0288	0.0425*	-0.0216
	(1.015)	(0.581)	(0.0463)	(0.0207)	(0.0242)	(0.0154)
Log sales 1 year before deal	-0.573	0.301	-0.0766*	-0.0192	-0.0186	0.0246
	(1.007)	(0.591)	(0.0465)	(0.0208)	(0.0242)	(0.0153)
EBITDA 2 years before deal	-0.00647	0.0312	0.00533	-0.00602***	-0.00239	0.00153**
	(0.0667)	(0.0491)	(0.00342)	(0.00214)	(0.00148)	(0.000674)
EBITDA 1 year before deal	0.0615	-0.0357	-0.00216	0.00329	0.00214	-0.00160**
	(0.0635)	(0.0491)	(0.00315)	(0.00228)	(0.00134)	(0.000634)
Constant	55.52***	-14.04	0.273*	0.0608	-0.0267	0.0151
	(9.754)	(10.97)	(0.144)	(0.0556)	(0.0750)	(0.0310)
Observations	1,832	1,832	1,832	1,832	1,832	1,832
R-squared	0.161	0.092	0.202	0.099	0.151	0.096
Sector Fixed Effects	YES	YES	YES	YES	YES	YES
Province Fixed Effects	YES	YES	YES	YES	YES	YES
Time Controls	YES	YES	YES	YES	YES	YES
Robust SE	YES	YES	YES	YES	YES	YES

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note. Reported are estimates from equations that make use of two observations per firm. For firms in the control group, the time series of outcomes are centered at the year of the deal for the corresponding target (which varies between 1993 and 2007). Outcomes in odd columns refer to values for target and control firms in the year before the deal. Even columns refer to changes in the outcome for target and control firms from the year before to the year after the deal. Standard errors, in parentheses, are robust to heteroskedasticity. *Average age*: average age of CEO, chairman and vice-chairman; *percentage of locals*: percentage of CEOs, chairmen and vice-chairmen who are born in the same province where the firm is headquartered; *male quota*: percentage of male CEOs, chairmen and vice-chairmen.

Panel B: majority targets

Variables	average age		percentage of locals		male quota	
	(7)	(8)	(9)	(10)	(11)	(12)
	before deal	effect	before deal	effect	before deal	effect
Dummy for target	-1.733*	-2.336**	0.0405	-0.232***	0.0332	0.0764***
	(1.047)	(1.107)	(0.0549)	(0.0543)	(0.0266)	(0.0242)
Net debt 1 year before deal	0.181	-0.106	-0.0566	0.00209	-0.0196	-0.0121
	(1.913)	(1.006)	(0.0865)	(0.0400)	(0.0494)	(0.0249)
Net debt 2 years before deal	1.162	0.149	0.00695	0.0187	-0.0186	-0.00298
	(1.841)	(0.958)	(0.0866)	(0.0382)	(0.0461)	(0.0230)
Logged sales 1 year before deal	0.622	-0.662	0.0375	0.00243	0.0424*	-0.0146
	(0.972)	(0.563)	(0.0455)	(0.0206)	(0.0241)	(0.0155)
Logged sales 2 years before deal	-0.475	0.391	-0.0951**	0.000699	-0.0208	0.0207
	(0.979)	(0.583)	(0.0462)	(0.0203)	(0.0243)	(0.0154)
EBITDA 1 year before deal	-0.0210	0.108**	-0.00134	-0.00107	-0.00289*	0.00106
	(0.0759)	(0.0524)	(0.00398)	(0.00194)	(0.00162)	(0.000879)
EBITDA 2 years before deal	0.0934	-0.0826*	0.00183	0.000130	0.00281*	-0.00186**
	(0.0728)	(0.0500)	(0.00385)	(0.00194)	(0.00159)	(0.000792)
Constant	55.67***	-8.202***	-0.0405	0.251***	-0.107	-0.0579
	(3.211)	(1.963)	(0.124)	(0.0673)	(0.0677)	(0.0396)
Observations	1,819	1,819	1,819	1,819	1,819	1,819
R-squared	0.166	0.097	0.198	0.157	0.153	0.115
Sector Fixed Effects	YES	YES	YES	YES	YES	YES
Province Fixed Effects	YES	YES	YES	YES	YES	YES
Time Controls	YES	YES	YES	YES	YES	YES
Robust SE	YES	YES	YES	YES	YES	YES

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 6: PE effect on targets size and profitability.
Panel A: family and private targets and controls.

	Minorities				Majorities			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Variables	Ebitda	Ln sales	Ln employee	Ebitda /sales	Ebitda	Ln sales	Ln employee	Ebitda /sales
1st year effect	3.827**	0.235***	0.231***	-0.0177	0.295	0.105	0.0750*	-0.0192
	(1.485)	(0.0448)	(0.0611)	(0.0126)	(0.533)	(0.0739)	(0.0448)	(0.0140)
2nd year effect	5.737***	0.297***	0.299***	-0.0136	0.522	0.130	0.133**	-0.0413**
	(1.675)	(0.0572)	(0.0637)	(0.0133)	(0.784)	(0.0952)	(0.0564)	(0.0167)
3rd year effect	6.273***	0.301***	0.340***	-0.00975	1.914*	0.137	0.130*	-0.0317**
	(1.947)	(0.0711)	(0.0692)	(0.0131)	(0.996)	(0.111)	(0.0677)	(0.0150)
Average effect	5.212***	0.276***	0.286***	-0.0138	0.838	0.123	0.109**	-0.0300**
	(1.612)	(0.0535)	(0.0616)	(0.0115)	(0.687)	(0.0888)	(0.0500)	(0.0130)
Observations	2,127	2,093	1,800	2,094	1,726	1,728	1,525	1,714
R-squared	0.116	0.210	0.213	0.074	0.055	0.090	0.064	0.088
Number of id	389	387	349	389	329	329	307	329
Firm Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES
Time Controls	YES	YES	YES	YES	YES	YES	YES	YES
Clustered SE	YES	YES	YES	YES	YES	YES	YES	YES
Prob > F	0.100	0.125	0.039	0.784	0.065	0.823	0.158	0.192

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note. Reported are estimates from equations that control for firm fixed effects, by target and PE type. The available data for targets refer to outcomes as measured from 2 up to 3 years after the deal. For firms in the control group the time series are centred at the year of the deal for the corresponding target (which varies between 1995 and 2004). Standard errors, in parentheses, are clustered at the firm level, allowing for heteroskedasticity and serial correlation.
Prob > : P-value for the equality of PE effect in the first, second and third year after the deal.
EBITDA is measured in million €; for descriptive statistics of the outcomes see Table 2.

Panel B: non-family targets and controls.

	Minorities				Majorities			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Variables	Ebitda	Ln sales	Ln employee	Ebitda /sales	Ebitda	Ln sales	Ln employee	Ebitda /sales
1st year effect	14.57**	0.157	0.180	0.0799	5.504	0.165**	-0.00816	0.0276**
	(6.179)	(0.245)	(0.190)	(0.0501)	(3.641)	(0.0768)	(0.0416)	(0.0136)
2nd year effect	4.014	0.256	0.185	0.104	3.410	0.265***	0.0844	0.0272
	(6.721)	(0.243)	(0.256)	(0.0814)	(2.259)	(0.101)	(0.104)	(0.0218)
3rd year effect	0.458	0.297	0.336	0.0895	1.215	0.274**	0.0438	-0.00475
	(7.348)	(0.185)	(0.265)	(0.0753)	(1.947)	(0.118)	(0.104)	(0.0224)
Average effect	6.970	0.226	0.226	0.0911	3.382	0.235***	0.0411	0.0166
	(5.913)	(0.213)	(0.234)	(0.0676)	(2.230)	(0.0868)	(0.0747)	(0.0147)
Observations	1,591	1,564	1,468	1,586	1,260	1,292	1,195	1,281
R-squared	0.098	0.075	0.089	0.084	0.061	0.024	0.027	0.083
Number of id	270	266	260	271	238	240	231	241
Firm Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES
Time Controls	YES	YES	YES	YES	YES	YES	YES	YES
Clustered SE	YES	YES	YES	YES	YES	YES	YES	YES
Prob > F	0.163	0.639	0.176	0.535	0.0984	0.505	0.621	0.284

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Appendix A

Table A1: multivariate regression of the dummy for target firms on a number of financial measures in the two years before the deal.

	Minorities	Majorities
	(1)	(2)
Sales two years before the deal	-0.000460*	0.000324
	(0.000260)	(0.000478)
Sales one year before the deal	0.000350	-0.000473
	(0.000280)	(0.000428)
EBITDA two years before the deal	0.00145	0.00317
	(0.00149)	(0.00243)
EBITDA one year before the deal	0.00261*	0.000408
	(0.00138)	(0.00243)
EBITDA/sales two years before the deal	0.0140	0.0373
	(0.0674)	(0.117)
EBITDA/sales one year before the deal	-0.0621	0.187*
	(0.0519)	(0.108)
Net debt/sales two years before the deal	0.000282	0.00916
	(0.00119)	(0.0122)
Net debt/sales one year before the deal	0.0153	-0.0271
	(0.00970)	(0.0276)
Observations	1,518	1,031
R-squared	0.097	0.146
Sector Fixed Effects	YES	YES
Region Fixed Effects	YES	YES
Time controls	YES	YES

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A2: PE effect on capital expenditure and outsourcing.

	Targets			Minorities			Majorities		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Variables	Capex	Capex/sales	Value added/sales	Capex	Capex/sales	Value added/sales	Capex	Capex/sales	Value added/sales
1st year effect	-2.889***	-0.0168**	0.000499	-2.887*	-0.0115*	-0.00652	-2.920**	-0.0225*	0.00911
	(0.970)	(0.00700)	(0.00680)	(1.485)	(0.00661)	(0.0106)	(1.191)	(0.0128)	(0.00817)
2nd year effect	-1.284	-0.00148	0.00335	-1.337	-0.000751	0.00556	-1.242	-0.00181	0.000878
	(0.960)	(0.00635)	(0.00893)	(1.578)	(0.00750)	(0.0138)	(0.981)	(0.0104)	(0.0107)
3rd year effect	-1.307**	-0.00320	0.00631	-2.084**	-0.00931	0.00825	-0.392	0.00556	0.00408
	(0.619)	(0.00645)	(0.00825)	(0.838)	(0.00708)	(0.0116)	(0.915)	(0.0111)	(0.0118)
Average effect	-1.865***	-0.00750	0.00333	-2.117**	-0.00715	0.00245	-1.580**	-0.00716	0.00474
	(0.623)	(0.00538)	(0.00720)	(0.960)	(0.00535)	(0.0110)	(0.761)	(0.00961)	(0.00889)
Observations	11,134	11,063	14,129	6,541	6,517	8,339	4,593	4,546	5,790
R-squared	0.011	0.024	0.010	0.009	0.027	0.008	0.021	0.024	0.016
# of id	2,691	2,691	2,686	1,555	1,558	1,558	1,136	1,133	1,128
Firm Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES	YES
Time Controls	YES	YES	YES	YES	YES	YES	YES	YES	YES
Clustered SE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Prob > F	0.269	0.0736	0.668	0.766	0.373	0.206	0.128	0.0447	0.621

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes to Tables A2-A4. Reported are estimates from equations that control for firm fixed effects, by target and PE type. The available data for targets refer to outcomes as measured from 2 up to 3 years after the deal. For firms in the control group the time series are centred at the year of the deal for the corresponding target (which varies between 1995 and 2004). Standard errors, in parentheses, are clustered at the firm level, allowing for heteroskedasticity and serial correlation.
Prob > : P-value for the equality of PE effect in the first, second and third year after the deal.
Capex: investments in property, plan and equipment - measured in million €.

Table A3: PE effect on wages, productivity and working capital.

	Targets				Minorities				Majorities			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Variables	Payroll/empl.	Sales/empl.	Payroll/sales	Working capital/sales	Payroll/empl.	Sales/empl.	Payroll/sales	Working capital/sales	Payroll/empl.	Sales/empl.	Payroll/sales	Working capital/sales
1st year effect	0.00102**	-0.0113	0.0147**	0.0168	0.00126*	-0.0131	0.0210*	0.0118	0.000760	-0.00923	0.00710	0.0220
	(0.000448)	(0.00983)	(0.00649)	(0.0127)	(0.000681)	(0.0138)	(0.0108)	(0.0185)	(0.000562)	(0.0140)	(0.00633)	(0.0170)
2nd year effect	0.00109**	-0.0157	0.0184***	0.0157	0.00104	-0.00977	0.0184**	0.00808	0.00118*	-0.0218	0.0182*	0.0250
	(0.000535)	(0.0120)	(0.00657)	(0.0144)	(0.000810)	(0.0167)	(0.00867)	(0.0201)	(0.000669)	(0.0169)	(0.0100)	(0.0205)
3rd year effect	0.00143**	-0.0189	0.0208***	-0.00145	0.00113	-0.0178	0.0231*	-0.0122	0.00172**	-0.0212	0.0180**	0.0123
	(0.000626)	(0.0132)	(0.00770)	(0.0148)	(0.000942)	(0.0183)	(0.0121)	(0.0214)	(0.000777)	(0.0188)	(0.00875)	(0.0203)
Average effect	0.00117***	-0.0151	0.0179***	0.0106	0.00114*	-0.0134	0.0208**	0.00284	0.00119**	-0.0171	0.0143*	0.0200
	(0.000448)	(0.0106)	(0.00644)	(0.0123)	(0.000686)	(0.0141)	(0.00986)	(0.0178)	(0.000549)	(0.0157)	(0.00793)	(0.0167)
Observations	12,318	12,316	14,135	13,447	7,124	7,115	8,351	7,827	5,194	5,201	5,784	5,620
R-squared	0.244	0.012	0.050	0.020	0.229	0.015	0.057	0.026	0.272	0.014	0.044	0.017
Number of id	2,468	2,461	2,671	2,651	1,406	1,399	1,551	1,528	1,062	1,062	1,120	1,123
Firm Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Time Controls	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Clustered SE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Prob > F	0.756	0.633	0.386	0.193	0.938	0.890	0.802	0.263	0.356	0.242	0.109	0.715

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Note. empl. stands for number of employees.

Table A4: PE effect on leverage and taxes.

	Targets				Minorities				Majorities			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Variables	Debt/sales	Debt/Ebitda	Taxes	Taxes/Ebitda	Debt/sales	Debt/Ebitda	Taxes	Taxes/Ebitda	Debt/sales	Debt/Ebitda	Taxes	Taxes/Ebitda
1st year effect	-0.220***	-1.172***	0.368	-0.0546***	-0.0851***	-0.175	0.992*	-0.0301	-0.376***	-2.392***	-0.332	-0.0808***
	(0.0284)	(0.315)	(0.297)	(0.0166)	(0.0290)	(0.465)	(0.512)	(0.0204)	(0.0450)	(0.372)	(0.237)	(0.0266)
2nd year effect	-0.232***	-1.034***	0.348	-0.0754***	-0.0791***	0.162	0.856*	-0.0731***	-0.413***	-2.533***	-0.221	-0.0759***
	(0.0314)	(0.326)	(0.284)	(0.0149)	(0.0304)	(0.422)	(0.486)	(0.0200)	(0.0515)	(0.455)	(0.237)	(0.0222)
3rd year effect	-0.200***	-1.476***	0.587*	-0.0770***	-0.0871***	-0.269	1.061*	-0.0302	-0.337***	-2.965***	0.0332	-0.127***
	(0.0289)	(0.404)	(0.341)	(0.0203)	(0.0290)	(0.529)	(0.582)	(0.0215)	(0.0496)	(0.589)	(0.282)	(0.0345)
Average effect	-0.218***	-1.227***	0.432	-0.0690***	-0.0837***	-0.0939	0.968**	-0.0452***	-0.376***	-2.626***	-0.178	-0.0942***
	(0.0273)	(0.292)	(0.269)	(0.0127)	(0.0270)	(0.395)	(0.464)	(0.0164)	(0.0443)	(0.383)	(0.207)	(0.0192)
Observations	13,026	12,299	14,197	14,194	7,671	7,278	8,354	8,349	5,355	5,021	5,843	5,845
R-squared	0.052	0.024	0.019	0.025	0.013	0.015	0.024	0.024	0.139	0.057	0.030	0.032
# of id	2,638	2,614	2,695	2,700	1,520	1,509	1,559	1,563	1,118	1,105	1,136	1,137
Firm Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Time Controls	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Clustered SE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Prob > F	0.219	0.356	0.614	0.405	0.909	0.263	0.906	0.0403	0.0397	0.610	0.342	0.427

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Note. Debt is computed as the sum of cash and cash equivalent minus short and long term financial debts. Taxes are measured in million €.

Appendix B

In this Appendix we provide further details on the sources used to construct the working sample. We also discuss empirical issues related to the quality and comparability of the data employed in the empirical analysis.

The sample of deals was obtained according to the following steps.

1. Data about PE deals come from two main sources: Private Equity Monitor (PEM) and Mergermarket. As discussed in Section 2.1, neither of these sources provides information about deals carried out by PE in Italy before year 2000. To give a comprehensive pictures of the PE effect from its inception (i.e., substantially from middle 1990s), we managed to get from the University of Castellanza, that hosts a research centre specialized in monitoring PE investments, a list of deals completed in Italy from 1995 to 1999. Overall, these three sources allowed us to identify a population of 455 PE deals carried out over the period 1995-2004.
2. From this population, we kept only deals (1) that involved target firms backed by a PE for the first time and (2) with financial reports available from two years before to three years after the investment. Furthermore (3) we kept only deals that ensured comparability of financial data from before to after the operation.

A few comments can clarify the information content of the latter condition. For example, comparability is a cause of concern when, before the deal, the target is made up of a group of non-consolidated companies: while after the investment, performances are usually properly detectable thanks to the consolidated financial statement drawn by the PE-backed company that acquires the targets, before the deal we are left with a number of individual financial statements, none of which provides a comprehensive picture of the group. In this case, comparing the post-deal consolidated financial report (FR) of the controlling entity with the non-consolidated FR of the most representative pre-deal target could overestimate the PE effect. Alternatively, comparing the post-deal individual FR of the controlling entity with the pre-deal individual FR of the most relevant target could be equally misleading since the two can be significantly different even if the group, as a whole, hasn't changed much after the deal. Furthermore, pre and post deal comparability is at stake when the PE acquires a holding company that is controlled by another holding company (i.e. a conglomerate): if this is the case, the target might not draw the

consolidated FR before the deal, leaving us without a pre-deal benchmark to be compared with post-deal performances.

By applying the selection criteria described, we were able to include in our working sample 191 deals completed in the period 1995–2004. To cross check the representativeness of our sample, we used as benchmark the population of deals that is collected in Capital IQ, since this database is employed as a control or as one of the primary sources of data by other empirical studies on PEs and LBOs (Boucly et al., 2011; Chung, 2011). In particular:

a) We selected from Capital IQ all deals, either closed or effective, carried out by PE/Venture Capital (Investment Firm Type) in the 1995-2004 period;
b) We discarded secondary buyouts (M&A features), and deals classified (Transaction Primary Features) as private investments in public equity (PIPE) or as Venture Capital, since by research design they are not examined in our study;
c) If a target was listed more than once in the same year, we counted it only once.

We identified 463 deals matching these criteria in Capital IQ, that is about the same number of investments we detected from the sources used in our empirical analysis. Figure B1 presents the number of deals in Capital IQ and in our sample over time. Our sample understates the number of deals with respect to the population represented by Capital IQ, but the evolution of the two time series is remarkably similar over time.

As a further check, we looked at the distinction between minority and majority deals. Unfortunately, to the best of our knowledge nor Mergermarket, nor Capital IQ, nor other sources provide extensive overage of this deal feature. However, we know from Lerner et al. (2009) that LBOs are by and large carried out as majority deals. Therefore, we checked the representativeness of our sample of majority deals comparing it with the population of LBOs listed in Capital IQ. To this end:

a) We selected from Capital IQ the LBOs (Merger/Acquisition Features) either closed or effective in the 1995-2004 period, and
b) If a firm was listed more than once per year, we counted it just one time.

Figure B2 shows that the evolution over time of majority deals in our sample mirrors the evolution of LBOs listed in Capital IQ, therefore confirming the accuracy of our sample.

Figure B1: number of PE deals (in logs) listed in Capital IQ and in our sample over the period 1995-2004.

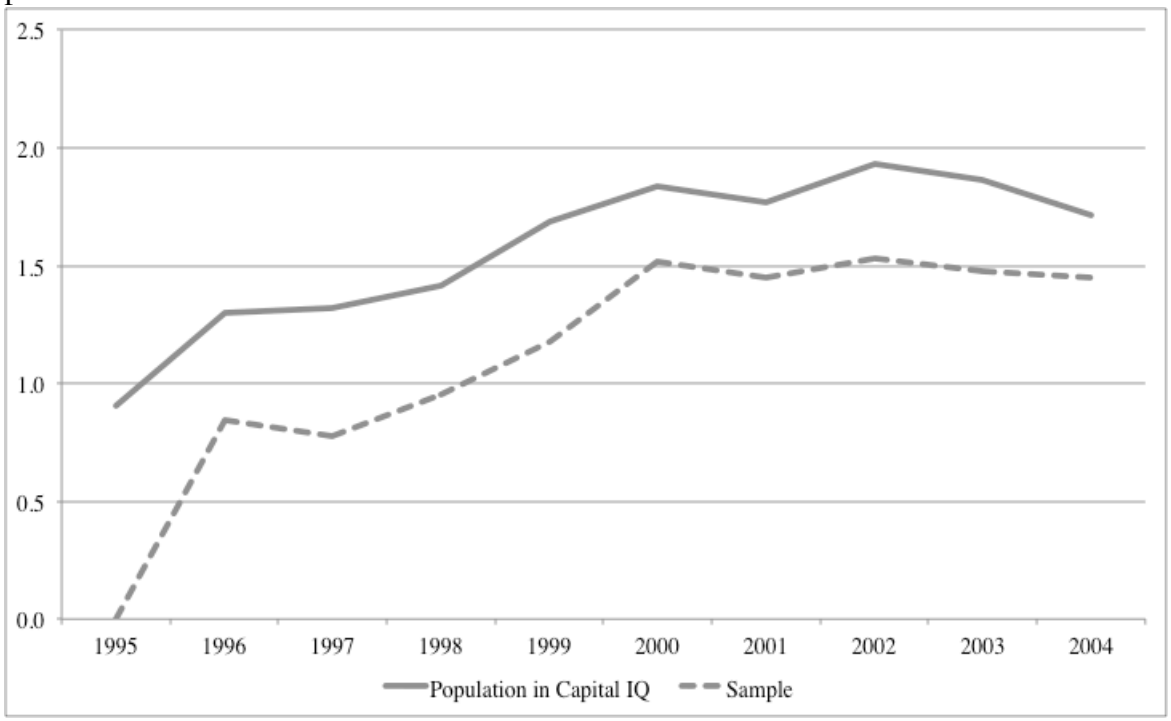


Figure B2: number of LBOs (in logs) listed in Capital IQ and majority deals in our sample over the period 1995-2004.