MANAGERIAL AND DECISION ECONOMICS
Manage. Decis. Econ. **35**: 161–171 (2014)
Published online 23 September 2013 in Wiley Online Library
(wileyonlinelibrary.com) DOI: 10.1002/mde.2650

The Wage and Employment Consequences of Ownership Change

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This paper provides a comparative examination of the consequences of leveraged buyouts (LBOs) and corporate takeovers on employment growth and wage growth. Employing both difference-in-differences combined with propensity score matching and the control function approach, we find evidence that (i) wages remain unchanged after either a private equity (PE)-backed or non-PE-backed LBO, (ii) wages remain unchanged after an unrelated takeover and (iii) related takeovers have negative employment consequences, possibly because of rationalisation. Our evidence does not find strong support for intervention in the market for corporate control on the grounds of protecting employees' welfare. Copyright © 2013 The Authors. *Managerial and Decision Economics* published by John Wiley & Sons, Ltd.

INTRODUCTION

The effect of ownership change on employment and wages has received relatively little research attention compared with its effect on performance. This is surprising given (i) the importance of this issue for policy-makers concerned with the appropriateness of intervention in the market for corporate control, (ii) unions' concern with protecting their members' interests and (iii) practitioners argue that their restructuring activities are unjustly criticised. This issue has become of increasing importance because the market for corporate control has developed beyond traditional takeovers of one corporation by another to include leveraged buyout (LBO) acquisitions involving specially created takeover vehicles often backed by private equity (PE) firms.

Leveraged buyouts, and mergers and acquisitions (henceforth takeovers) have received criticism concerning their impact on employees.[1] The most severe criticism has most recently been directed towards PE-backed LBOs because of union concerns that PE investors gain at employees' expense. Unions argue that employees suffer via layoffs and lower wages (International Trade Union Confederation, 2007). This has led to wider public debate on the role of LBOs and PE within the economy (see, for example, the Treasury Select Committee, 2007).

The impact of takeovers on jobs and wages has been a controversial issue for several decades. The seminal work of Shleifer and Summers (1988) argues that ownership change via acquisition creates the opportunity for management to breach implicit and explicit agreements with employees, leading to job losses and lower wage payments. Proponents of the market for corporate control, however, argue that the takeover is a means of shifting stewardship of corporate assets to those that

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can best utilise them (Manne, 1965). In addition, the takeover is an important discipline of last resort when other corporate governance devices, such as the Board of Directors, have failed to curtail managerial inefficiencies. Such arguments have also been employed to defend PE involvement in the LBO of publicly quoted companies (Jensen, 1986), that is, the LBO wrests control of corporate resources from under-performing management and ownership structures.

The debates outlined earlier raise important questions about the market for corporate control and any policy intervention in this market. Should policymakers intervene to protect employees' interests during ownership change? In order to address this question, we must first address the question: what are the wage and employment consequences for employees? This is an empirical question, and if there are no significant consequences for employees, there is no basis for policy intervention in the market for corporate control in order to protect them.

Studies have sought to provide systematic evidence for the labour consequences of takeovers (e.g. Conyon *et al*., 2001, 2002, 2004) and LBOs (e.g. Lichtenberg and Siegel, 1990; Amess and Wright, 2007: Davis *et al*., 2008). Until now, however, LBOs and takeovers have been analysed separately in distinct strands of the corporate restructuring literature. From an empirical perspective, this is problematic because the identification of the effects of ownership change is not carried out within a unified framework. From a policy perspective, separate analysis of takeovers and LBOs is unhelpful. Policy intervention in the market for corporate control could be misplaced without a comparison of the labour consequences of different types of ownership change within a unified framework.

Davis *et al*. (2008) examine the employment effects of only PE-backed LBOs compared with a control group. The current paper seeks to isolate the labour consequences of PE involvement in LBOs by examining both PE-backed and non-PE-backed LBOs. This is a unique feature of the current paper that allows systematic analysis of the employment and wage consequences of PE and LBOs that hitherto has not been conducted. In addition, we follow the takeover literature by separately analysing the consequences of related and unrelated takeovers (e.g. Conyon *et al*., 2002). Note, as PE investors are financial acquirers running each LBO purchase as a separate entity with its own financial structure, all LBO purchases are unrelated.

The paper is organised as follows. The section on Literature Review and Theoretical Background examines the hypothesised relationships between the different types of ownership change, and employment and wages. The section on Data describes the data and sources. The section on Modelling Frameworks outlines the control function approach, which is the empirical modelling approach employed in the paper. Then we have the Results section and finally, the Conclusions section.

LITERATURE REVIEW AND THEORETICAL BACKGROUND

The motivation for ownership change impacts on theoretical predictions concerning employment and wage consequences. The market for corporate control is typically characterised as a natural selection process whereby under-performing firms are targeted for ownership change. Indeed, takeovers are considered the ultimate sanction against under-performing managers when internal control devices are ineffective. The counter-argument is that an acquisition might be a consequence of acquiring firms' weak corporate governance (Shleifer and Vishny, 1988). LBOs do not suffer from this problem and might therefore be considered a superior disciplinary device to under-performing management compared with takeover by another firm (Weir and Wright, 2006).

Takeovers

Profit-maximising managers conducting related takeovers are more likely to be seeking cost savings, which can potentially be achieved by the elimination of duplicated activities. Such cost savings can manifest in job losses. In addition, such ownership change creates an opportunity for new management to renege on implicit contracts with employees and reduce their extra-marginal wage payments (Shleifer and Summers, 1988). Dutz (1989) argues that horizontally related takeovers might also occur in mature and declining industries in order to remove excess capacity in the industry. This can result in plant closures, which in turn leads to job losses. Horn and Wolinsky (1988) show that horizontally related takeovers can change industry structure that strengthens bargaining power post-acquisition, due to a change in industry structure, which improves profits. Depending on their bargaining power, workers may share in the additional revenues generated post-acquisition.

If unrelated takeovers arise because of weak corporate governance that does not motivate managers to disgorge free cash flow (Jensen, 1986), there is no prediction of job losses. Indeed, the acquisition might be

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a strategy to realise managerial objectives of expanding the size of the firm and the number of employees under their control (Williamson, 1964). Such under-performing management are not likely to be seeking to cut costs via wage reductions. In contrast, unrelated takeovers might arise in a market for corporate control that removes under-performing management subsequent to ownership change (Manne, 1965). If pre-acquisition management pursued their own non-profit-maximising objectives and employed sub-optimally high levels of employees in a manner suggested by Williamson (1964), a profit-maximising acquisition would result in job losses. Moreover, ownership change via acquisition would provide management with the opportunity to reduce extra-marginal wage payments (Shleifer and Summers, 1988).

Evidence on the employment consequences of takeovers is mixed. Brown and Medoff (1988) provide early US evidence finding that takeovers are associated with a small increase in employment. More recently, McGuckin and Nguyen (2001) report that acquired plants increase their employment rates by about 3% per year faster than non-acquired plants. They also suggest benefits for employees arising from acquired firms being less likely to close plants than non-acquired firms. In contrast to US evidence, UK studies depict negative employment consequences following an acquisition. Conyon *et al.* (2002) find that related takeovers are associated with 19% lower employment, whereas unrelated takeovers are associated with about 8% lower employment in the year of the transaction. The expected effects of unrelated acquisitions on employment are ambiguous but are expected to at least be less negative than for related acquisitions.

Evidence on the wage consequences of takeovers consistently indicates that takeovers are associated with wage increases. McGuckin and Nguyen (2001) report US evidence indicating that wages increase by an average of about 3% per annum. Conyon *et al.* (2004) find UK evidence of wages being 14% higher 2 years after related takeovers, which contrasts with unrelated takeovers having no significant effect. They suggest that this reflects higher labour productivity after related takeovers. An alternative explanation, however, is that higher wages and labour productivity are due to a 'batting average' effect. This is where low productivity workers lose their jobs after takeovers resulting in higher average productivity and wages of the remaining workers. Hence, we expect related acquisitions to result in wage increases as a result of efficiency gains from synergies, whereas unrelated acquisitions are expected to have no effect on wages.

Leveraged Buyouts

The LBOs install a governance structure that reduces managers' opportunistic behaviour and induces them to increase effort (Elitzur *et al.*, 1998). First, increased debt increases firms' fixed interest obligations, which provides managers with incentives to generate cash to service the debt (Thompson *et al.*, 1992) and to reduce sub-optimal investments, and re-direct this cash towards servicing the debt (Fox and Marcus, 1992). Thus, there is debt bonding. Second, managers are incentivised by the concentration of equity in their possession to reduce organisational slack in the post-buyout firm (Thompson and Wright, 1995). Managers' increased equity stake also induces them to increase their effort (Elitzur *et al.*, 1998). Finally, PE investors typically have Board representation, often nominate the Chair of the Board and have greater access to information regarding firm performance compared with shareholders of public corporations.[2] This places them in a strong position to monitor senior management's decisions and the performance of the firm.

The previous discussion therefore suggests that the LBO governance structure provides managers with incentives to reduce labour costs. If pre-buyout firms indulged in sub-optimally high levels of employment and made extra-marginal wage payments due to weak corporate governance, LBOs provide managers with incentives to reduce levels of employment and wage payments post-buyout. Thus, the change in pre-buyout and post-buyout employment and wages is negative.

The arguments outlined previously have been largely advanced in respect of buyouts of listed corporations (Thompson and Wright, 1995), but these account for only a minority of PE and buyout deals. An alternative argument is that pre-ownership change, managers in buyouts may have been prevented from pursuing growth opportunities because of restrictive control by the parent, in the case of divisional buyouts, or disinterested owners in the case of buyouts of private family firms. Besides monitoring skills and their provision of funds, PE firms may also provide expertise that helps enhance the exploitation of growth opportunities through their strategic knowledge of markets (Meuleman *et al.*, 2009). As such, LBOs and PE backing could result in employment growth. The ownership change still, however, provides an opportunity to reduce extra-marginal wage payments, if they existed prior to the buyout.

Evidence in the LBO literature is mixed with respect to both wage and employment consequences. Kaplan (1989) found a 12% decline in median employment in the year after a management buyout

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(MBO), whereas Smith (1990) found no employment effects, but these studies focused on buyouts of listed corporations. Lichtenberg and Siegel (1990) added further insight by finding that production worker employment was not affected by an LBO; however, they found that non-production worker employment declined by 8.5% 2 years after the transaction. Amess and Wright (2007) provided the first direct comparison of insider-led MBOs and outsider-led management buy-ins (MBIs). They found that MBOs and MBIs had 0.51% faster and 0.81% slower employment growth than the control sample, respectively. However, Amess and Wright (2007) did not distinguish PE and non-PE-backed deals. Cressy *et al*. (2011), also using UK data, find that employment in PE-backed buyouts falls relative to their control group of non-PE-backed companies for the first 4 years post-buyout but rises in the fifth year. They suggest that initial rationalisation creates the basis for more viable job creation.

Davis *et al.* (2008), using a sample of PE-backed buyouts in the US, report that cumulative employment growth in the 2 years prior to a transaction is 4% slower than that for the control. In addition, employment shrinks more rapidly in the first 2 years post-buyout with employment being 7% lower than that for control firms. However, 6% more 'greenfield' job creation occurs in PE-backed buyouts compared with the control group. Overall, the findings suggest PE firms are catalysts for creative destruction. Evidence from France (Boucly *et al*., 2009) demonstrates increases in employment in PE-backed buyouts relative to a control group.

With respect to the wage effects of LBOs, Lichtenberg and Siegel (1990) report US evidence indicating that the principal negative impact is on non-production workers. They report that the hourly compensation of production workers increases by 3.6% and 2.3% in each of the first 2 years post-buyout. For UK MBOs and MBIs, Amess and Wright (2007) find that they both have slightly slower rates of wage growth compared with their control sample.

We seek to contribute to the literature on the consequences of organisational change by determining and quantifying differences in the effects of LBOs and takeovers on employment and wages. We examine four different types of ownership change (PE-backed LBOs, non-PE-backed LBOs, related takeovers and unrelated takeovers) within a unified empirical framework. The discussion in this section suggests that the employment effects of related takeovers are expected to be most negative and PE-backed buyouts most positive, with unrelated takeovers and non-PE-backed takeovers in between these two end-points. The previous discussion also suggests, in contrast, that related acquisitions are likely to be associated with the largest positive effect on wages.

DATA

The sample is constructed from three sources: the Centre for Management Buy-Out Research (CMBOR) database, Zephyr and Financial Analysis Made Easy (FAME). We obtain from the FAME employment, wage, firm age and turnover data covering the period 1996–2006.

The CMBOR database includes the name of all firms that adopted the LBO governance structure without an upper or lower size cut-off, the year in which the LBO governance structure was adopted, whether the LBO was PE-backed and the year in which the firm ceased to have the LBO governance structure. The database enables LBOs to be distinguished into those that are PE-backed and those that do not use PE finance. After matching the CMBOR and FAME data and cleaning the data to remove firms subject to multiple ownership change, we obtain 253 LBOs, 133 of which are PE-backed.

The Zephyr database was used to obtain information on takeovers. We label a deal an acquisition when the acquiring firm attains a minimum 50% ownership stake in the target firm. If the acquirer held a minority stake and subsequently increased its stake to at least 50%, this is also classed as an acquisition. After matching Zephyr and FAME data and cleaning the data to remove firms subject to multiple ownership change, we obtain 274 takeovers. In total, we observe 527 firms subject to ownership change.

We remove firms subject to more than one ownership change in order to obtain clear measures of the impact of ownership change. The control function approach removes selection bias, and therefore, results are interpreted as causal. If a firm is subject to more than one ownership change, the causal relationship between the ownership changes and outcomes becomes problematic to determine. In the construction of our sample of firms treated to ownership change, we are careful to exclude firms that indulge in acquisition and divestment after the ownership change. This is important in distinguishing between organic and non-organic employment growth, which are conflated in many studies. Indeed, in our data, we observe only organic growth.

Our control sample consists of firms that experienced no ownership change during the period under

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study. Starting with the population of firms for which FAME reported the required accounting data, we removed firms where a minority stake had been taken during the sample period, resulting in a control sample that contains 27 029 observations.

Sample characteristics are reported in Table 1. The productivity variable is labour productivity, measured as the ratio of turnover to number of employees. For firms subject to ownership change, firm characteristics in the year prior to ownership change are reported. For the control sample, the characteristics of firms when they are first observed in the data set are reported. Table 1 illustrates that there are differences in characteristics between those firms subject to ownership change and the control sample. There are also differences in characteristics between firms subject to different types of ownership change.

Growth rates for the different types of ownership change over the period $t-1$ to $t+1$ (where t is the year of ownership change) are reported in Table 2.[3] Differences in these are 'raw' growth rates observed in the data for firms subject to different types of ownership change. Employing two techniques, difference-in-differences combined with propensity score matching and the control function approach, a systematic analysis of wage and employment growth is conducted.

Table 2. Growth Rates (from $t-1$ to $t+1$) of the Variables of Interest

Variable	Mean	S. D.	Min	Max
Control				
Employment	0.14	0.48	−6.30	7.17
Wage	0.11	0.35	−7.34	4.67
Productivity	0.07	0.54	−8.48	8.33
PE-backed LBO				
Employment	0.11	0.39	−2.91	1.61
Wage	0.07	0.28	−1.20	1.12
Productivity	0.05	0.32	−0.93	1.91
Non-PE-backed LBO				
Employment	0.04	0.49	−3.08	1.32
Wage	0.10	0.35	−2.74	0.84
Productivity	0.07	0.48	−1.03	3.56
Related acquisition				
Employment	−0.12	1.02	−5.36	3.02
Wage	0.14	0.59	−2.73	2.26
Productivity	0.06	0.80	−2.83	2.38
Unrelated acquisition				
Employment	−0.12	1.05	−4.92	3.47
Wage	0.05	0.49	−2.61	1.94
Productivity	0.10	0.62	−3.09	2.08

PE, private equity; LBO, leveraged buyout.

MODELLING FRAMEWORKS

A fundamental problem when analysing the consequences of ownership change is in establishing the counterfactual (Smart and Waldfogel, 1994). Firms that experience ownership change are not randomly selected from the population of firms. For instance, firms might be subject to ownership change because they overemploy and/or make extra-marginal wage payments that are identified as sources of organisational inefficiency. Therefore, the ownership change decision could be correlated with the levels of employment and wages prior to ownership change, that is, there is self-selection to ownership change.

To address the issue of self-selection, we use two different modelling strategies and compare their estimates of the Average Treatment Effects of ownership change. First, outlined in the section on Propensity Score Matching Combined with Difference-in-Differences is a modelling strategy that combines propensity score matching (to construct the counterfactual) with a difference-in-differences analysis. By using this approach, it is possible to determine and quantify a causal relationship between ownership change, and employment and wages. Second, outlined in the section on The Control Function Approach is a modelling strategy that incorporates a control function into a regression to control for selection bias. By adding sufficient control variables to the control function, we are able to obtain unbiased estimates to determine

Table 1. Sample Characteristics (a Year before Ownership Change)

	Control		PE-backed LBO		Non-PE-backed LBO		Related acquisition		Unrelated acquisition	
Variable	Mean	S. D.	Mean	S. D.	Mean	S. D.	Mean	S. D.	Mean	S. D.
Log number of employees	3.80	1.53	4.95	1.31	4.20	1.04	4.96	1.65	4.51	1.47
Log wage	3.18	0.69	3.10	0.51	3.27	0.53	3.25	0.96	3.39	0.54
Log productivity	4.85	1.21	4.55	0.83	4.93	1.06	4.74	1.16	4.86	0.99
Age	16.06	19.25	22.19	19.47	22.82	20.63	22.91	27.23	17.80	17.70
Obs.	27 029		149		104		110		164	

PE, private equity; LBO, leveraged buyout.

and quantify the relationship between ownership change, and employment and wages.

Propensity Score Matching Combined with Difference-in-Differences

In this paper, each firm can be thought of as being under any of three treatments or schemes, that is, no restructuring, LBO and acquisition, denoted as S^0, S^1 and S^2, respectively. We denote the status of firm i as $S_i^j = 0$ or $S_i^j = 1$, for $j = 0, 1, 2$. Thus, for example, $S_i^1 = 1$ indicates that firm i has undergone a LBO. We denote the potential outcomes associated to each of the three treatments as y_{i0}, y_{i1} and y_{i2}, where y denotes either employment or wages. The problem is estimating the causal effect of one treatment j relative to another treatment k,

$$\delta_{jk} = y_{ij} - {}^{\text{ij}}y_{ik}. \tag{1}$$

As each firm receives only one of the treatments and the remaining two potential outcomes are unobserved, the problem of estimating δ_{jk} is tantamount to estimating missing data. Thus, to make the problem tractable, we concentrate on identifying the average effect of treatment S^j relative to treatment S^k,

$$\begin{aligned}\Delta_{jk} &= E\{y_{ij} - y_{ik} | S_i^j = 1\} \\ &= E\{y_{ij} | S_i^j = 1\} - E\{y_{ik} | S_i^j = 1\}\end{aligned} \tag{2}$$

Causal inference relies on the construction of the counterfactual for the last term in equation (2), which is the outcome participants of treatment S^k would have experienced, on average, if they had participated in treatment S^j. This is estimated by the corresponding average value of the outcome variable for the participants of treatment S^k

$$E\{y_{ik} | \delta_{ik} = 1\}. \tag{3}$$

An important feature in the accurate construction of the counterfactual is the selection of a valid group of firms with which to estimate expression (3). In this respect, any estimation method has to overcome the problem of selection bias. In our case, firms that are targets for LBOs and acquisitions are likely to have different characteristics than firms that experienced no restructuring. The approach we take here is to employ propensity matching techniques originally proposed for the binary treatment case by Rosenbaum and Rubin (1983) and extended to the multiple treatments case by Imbens (2000) and Lechner (2001). The method of matching seeks to control for all those observable variables that are responsible for selection bias, say X. In this paper, X consists of five observable characteristics that are hypothesised to impact on the probability of a firm being subject to an LBO or acquisition. These are the pre-structuring levels of employment, wages, productivity, age and time trend.

The fundamental assumption of the method of matching is that conditional on X, the distribution of the counterfactual outcome y_{ik} in the group receiving treatment S^j is the same as the observed distribution of y_{ik} in the group receiving treatment S^k. In this case, the average outcome of the matched firms in non-treated cases constitutes the correct sample counterpart for the missing information on the outcomes that the treated would have experienced, on average, if they had not been treated. This assumption therefore ensures that the counterfactual is accurately estimated using data from suitable firms that have not received the relevant treatment.

Under this assumption, matching based on the propensity score ensures the balancing of the observable characteristics X in the two groups that are being compared (i.e. j and k). The propensity score for i, P_{ijk}, is defined as the probability of receiving treatment S^j relative to the probability of receiving treatment S^k.

$$P_{ijk} = \frac{P(S_i^j = 1|X)}{P(S_i^k = 1|X)} \tag{4}$$

where the probabilities are predicted from a multinomial probit regression.

In general, the matching estimator of the causal effect of treatment S^j relative to treatment S^k can be written as

$$\hat{\Delta}_{jk} = \sum_{l \in S^j} \left(y_l - \sum_{i \in S^k} g(P_{ijk}) y_i \right) \tag{5}$$

where g(.) is a function assigning the weights to be placed on the comparison firms in treatment group S^k used as matches for participant of treatment S^j.

When there are repeated observations for the same set of participants over time (indexed by t), it is arguably more reliable to base the evaluation analysis on the difference between the variable of interest s year after the treatment period (viz. y_{it+s}) and its pre-treatment value (viz. y_{it-1}), that is, $\Delta y_{it+s} = y_{t+s} - y_{t-1}$ (e.g. Blundell and Costa Dias, 2000). In this case, the combined differences-in-differences and matching estimator is defined as

$$\hat{\Delta}_{jk} = \sum_{l \in S^j} \left(\Delta y_{lt+s} - \sum_{i \in S^k} g(P_{ijk}) \Delta y_{it+s} \right) \tag{6}$$

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Here, we evaluate the employment and wages effects of restructuring at the year of restructuring and the first three post-restructuring years, that is, for $s = 0,1\ldots4$. Throughout, we impose the so-called common support condition in the matching algorithm. This involves dropping firms belonging to treatment group S^j whose propensity score is higher than the maximum or less than the minimum propensity score of firms in the comparison group S^k.

The different matching estimators proposed in the literature (such as the nearest neighbours and kernel estimators) differ from each other in the choice of the weighting function they employ. However, they share the same property of being consistent estimators of the treatment effect under consideration, although they can exhibit substantial small sample differences. In this paper, we focus on the (three) nearest neighbours matching estimators, but we also have experimented with different weighting schemes.

The Control Function Approach

The basic idea behind the control function approach lies in adding a flexible function of the pre-treatment variables $\boldsymbol{x}$, say h($\boldsymbol{x}$), to a regression of the outcome variable ($\boldsymbol{y}$) on the treatment indicators variable ($\boldsymbol{w}$), to control for possible selection bias. As explained by Wooldridge (2002, pp. 612–614), putting enough control variables in $\boldsymbol{x}$ would render the treatment variables and the unobservables affecting the outcome variables uncorrelated. In this case, ordinary least squares regression of $\boldsymbol{y}$ on $\boldsymbol{w}$ and h($\boldsymbol{x}$) would deliver unbiased and consistent estimators of the treatment effects.

In our empirical model, $\boldsymbol{y}$ denotes employment or wage growth; the treatment indicator variable $\boldsymbol{w}$ consists of four dummy variables indicating related and unrelated takeovers, and PE and non-PE-backed LBOs; and $\boldsymbol{x}$ consists of the pre-treatment period values of firm employment, wage, age and productivity. The function h($\boldsymbol{x}$) is formed by interacting $\boldsymbol{w}$ with $x - \overline{x}$, that is, by multiplying each dummy treatment variable by the demeaned value of $\boldsymbol{x}$ using the sample average. In the final analysis, the following equation is estimated:

$$y = \delta + \alpha w + x\beta + \delta(x - \overline{x}) + \varepsilon \tag{7}$$

where ε is a possibly heteroskedastic error term.

In the previous equation, the coefficient α gives the average treatment effect of the various treatment variables under consideration. A major advantage of the control function approach is that it allows one to study how the average treatment effects vary across various levels of the pre-treatment control variables. Such heterogeneous effects can easily be obtained from the regression coefficients as

$$ATE(x) = \alpha + \delta(x - \overline{x}) \tag{8}$$

RESULTS

Propensity Score Matching Combined with Difference-in-Differences

The results from the multinomial probit regression of the determinants of corporate restructuring are reported in Table 3. We find that larger and older firms are likely to be acquisition and LBO targets. Although the analysis of the determinants of restructuring is interesting in its own right, as far as the matching method is concerned, the only crucial issue is ensuring that the propensity score obtained from the regression is successful in controlling for firm-specific differences in the pre-ownership change period. It is therefore important to test whether the covariates in the multinomial probit regression (employment, wages, productivity, age and trend) are balanced in all treatment pairs of interest. Accordingly, for each covariate in the multinomial probit regression, we test for equality of means across treatment pairs by using standard *t*-tests. These tests are reported in Table 4 and show the success of the propensity score matching, providing support for the validity of our approach.

Table 3. Estimates from Multinomial Probit Regressions of the Determinants of Ownership Change

Coefficient	No change vs LBO	No change vs acquisition
Employment	0.185***	0.285***
	(0.018)	(0.027)
Wage	0.187***	0.082
	(0.054)	(0.078)
Productivity	−0.007	0.064
	(0.031)	(0.039)
Age	0.004**	0.013***
	(0.002)	(0.002)
Time trend	−0.012	0.312***
	(0.019)	(0.021)
Log likelihood	2250.586	
Observations	23 914	
p-value from joint test of significance of covariates	0.000	

t-statistics reported in parentheses. LBO, leveraged buyout. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

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Table 4. Balancing Tests of Matched Samples

	No change vs LBO			No change vs acquisition			LBO vs acquisition		
	Mean			Mean			Mean		
Sample	Treated	Control	*p*-value	Treated	Control	*p*-value	Treated	Control	*p*-value
Employment									
Unmatched	4.6273	3.8392	0	4.6273	4.7316	0.44	4.6273	4.7316	0.44
Matched	4.6273	4.5876	0.623	4.6606	4.4812	0.322	4.6606	4.4812	0.322
Wages									
Unmatched	3.1634	3.1604	0.947	3.1634	3.3053	0.022	3.1634	3.3053	0.022
Matched	3.1634	3.1634	0.999	3.1629	3.1334	0.6	3.1629	3.1334	0.6
Productivity									
Unmatched	4.7086	4.8376	0.102	4.7086	4.7977	0.349	4.7086	4.7977	0.349
Matched	4.7086	4.6032	0.114	4.6991	4.5709	0.166	4.6991	4.5709	0.166
Age									
Unmatched	22.448	16.653	0	22.448	19.214	0.098	22.448	19.214	0.098
Matched	22.448	21.448	0.473	22.916	23.04	0.813	22.916	23.04	0.813
Time trend									
Unmatched	1998.5	1999	0.002	1998.5	2002.2	0	1998.5	2002.2	0
Matched	1998.5	1998.4	0.377	1998.6	1998.7	0.617	1998.6	1998.7	0.617

LBO, leveraged buyout.

Table 5. The Impact of LBOs and Takeovers on Employment and Wages

	Employment		Wages		
	Coefficient	*t*-statistics	Coefficient	*t*-statistics	No. of treated firms
LBO					
$t+1$	−0.0450091	(−1.69)*	0.0025622	(0.14)	232
$t+2$	−0.0503943	(−1.43)	0.0021533	(0.08)	222
Acquisition					
$t+1$	−0.1709787	(−2.74)***	0.1128092	(2.41)***	215
$t+2$	−0.235025	(−2.87)***	0.0309588	(0.56)	133

Absolute *t*-statistics are reported in parentheses. $t-1$ is the base year. LBOs, leveraged buyouts. ***$p<0.01$; **$p<0.05$; *$p<0.1$.

Table 6. The Impact of PE-Backed LBOs, Non-Private PE LBOs, Related Takeovers and Unrelated Takeovers on Employment and Wages

	Employment		Wages		
	Coefficient	*t*-statistics	Coefficient	*t*-statistics	No. of treated firms
PE-backed LBO					
$t+1$	0.016	0.592	−0.023	−0.764	133
$t+2$	0.034	0.687	−0.004	−0.08	130
Non-PE-backed LBO					
$t+1$	−0.11***	−2.336	0.01	0.401	99
$t+2$	−0.091	−1.134	−0.002	−0.031	92
Related takeover					
$t+1$	−0.158*	−1.854	0.197***	2.457	70
$t+2$	−0.254	−1.579	0.136	1.506	44
Unrelated takeover					
$t+1$	−0.155***	−2.053	0.062	1.333	139
$t+2$	−0.201	−1.347	−0.083	−1.524	88

Absolute *t*-statistics are reported in parentheses. $t-1$ is the base year. PE, private equity; LBO, leveraged buyout. ***$p<0.01$; **$p<0.05$; *$p<0.1$.

Table 5 reports findings for LBOs and acquisitions *per se*. We find evidence, at the 10% level, of a 4% decline in employment in the first year after the LBO. LBOs have no statistically significant impact on post-

Table 7. Results Using the Control Function Approach

	Employment		Wages	
	$t+1$	$t+2$	$t+1$	$t+2$
Average treatment effects				
PE-backed LBO	0.081	0.093*	−0.011	−0.063
	(0.0428)	(0.0459)	(0.0268)	(0.0332)
Non-PE-backed LBO	−0.028	−0.039	−0.036	−0.020
	(0.0446)	(0.0531)	(0.0492)	(0.0507)
Related takeover	−0.414**	−0.265	0.065	0.156
	(0.1398)	(0.1527)	(0.0850)	(0.1189)
Unrelated takeover	−0.177	−0.093	−0.022	0.048
	(0.1212)	(0.1515)	(0.0754)	(0.0576)
Control variables				
Number of employees	−0.059***	−0.079***	−0.009***	−0.010***
	(0.0028)	(0.0033)	(0.0020)	(0.0022)
Wage	0.029***	0.031***	−0.155***	−0.182***
	(0.0066)	(0.0083)	(0.0078)	(0.0082)
Productivity	−0.004	−0.002	0.013***	0.015***
	(0.0040)	(0.0051)	(0.0030)	(0.0031)
Age	−0.002***	−0.003***	−0.000**	−0.000***
	(0.0002)	(0.0002)	(0.0001)	(0.0001)
PE LBO* employees	−0.060	−0.047	−0.020	−0.010
	(0.0511)	(0.0534)	(0.0183)	(0.0237)
Non-PE LBO* employees	−0.095	−0.071	0.065	0.074
	(0.0735)	(0.0809)	(0.0577)	(0.0592)
Related takeover* employees	0.100	0.156*	−0.008	−0.115
	(0.0833)	(0.0677)	(0.0412)	(0.0720)
Unrelated takeover* employees	−0.186	−0.289*	0.014	0.024
	(0.1041)	(0.1258)	(0.0555)	(0.0527)
PE LBO* wage	−0.023	−0.013	−0.120	−0.161
	(0.1122)	(0.1226)	(0.0884)	(0.0858)
Non-PE LBO* wage	−0.119	−0.174	0.083	0.130
	(0.1146)	(0.1330)	(0.0712)	(0.1140)
Related takeover* wage	−0.028	0.058	0.213	−0.530***
	(0.2504)	(0.0874)	(0.1282)	(0.1464)
Unrelated takeover* wage	−0.002	−0.336	−0.046	−0.218**
	(0.1947)	(0.1994)	(0.1027)	(0.0792)
PE LBO* productivity	−0.042	−0.055	0.009	0.025
	(0.0475)	(0.0505)	(0.0239)	(0.0284)
Non-PE LBO* Productivity	0.043	0.044	−0.061	−0.002
	(0.0559)	(0.0549)	(0.0734)	(0.0626)
Related takeover* Productivity	−0.169	−0.055	0.010	−0.033
	(0.1151)	(0.1014)	(0.0530)	(0.0681)
Unrelated takeover* Productivity	0.059	0.091	−0.002	−0.045
	(0.1550)	(0.1813)	(0.0745)	(0.0793)
PE LBO* Age	0.000	0.000	−0.000	0.001
	(0.0018)	(0.0018)	(0.0009)	(0.0009)
Non-PE LBO* Age	−0.002	−0.002	0.002	0.000
	(0.0026)	(0.0028)	(0.0017)	(0.0016)
Related takeover* Age	0.004	0.001	0.003	0.004
	(0.0032)	(0.0024)	(0.0021)	(0.0030)
Unrelated takeover* Age	0.021**	0.020*	−0.004	−0.004
	(0.0080)	(0.0086)	(0.0029)	(0.0026)
Constant	0.416***	0.564***	0.473***	0.339
	(0.1050)	(0.1097)	(0.0964)	(0.3458)
Observations	22 179	20 466	22 176	20 465
Adjusted R-squared	0.059	0.067	0.075	0.100

All control variables refer to the pre-ownership change period; robust standard errors in parentheses; all specifications include the full set of time dummies. PE, private equity; LBO, leveraged buyout. *Significant at 10%; **Significant at 5%; ***Significant at 1%.

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buyout wages. It is clear from Table 5 that acquisitions have a more pronounced impact on employment and wages than LBOs. Employment is about 17% and 23% lower in the first 2 years post-acquisition, respectively. In contrast, wages are about 11% higher in the first year post-acquisition.

There is increasing concern about the impact of PE firms in the economy (International Trade Union Confederation, 2007; Treasury Select Committee, 2007). In order to isolate the consequences of PE, we disaggregate LBOs into PE-backed and non-PE-backed LBOs. Results of the subsequent analysis are reported in Table 6. We find no evidence that PE-backed LBOs have a significant impact on either employment or wages. In contrast, there is evidence non-PE-backed LBOs have 11% lower employment in the year after the LBO. Non-PE-backed LBOs have no significant impact on wages, however.

Results reported in Table 6 also distinguish between related and unrelated takeovers. We define related takeovers as those occurring in the same three-digit SIC code. The results in Table 6 show that both unrelated and related acquisitions lead to about 16% decline in unemployment in the year after the transaction; however, this is only significant at the 10% level for related takeovers. Wages are about 20% higher in the year following a related acquisition; however, unrelated acquisitions have no significant impact on wages.

The Control Function Approach

Results using the control function approach outlined in the section on The Control Function Approach are reported in Table 7. Coefficient estimates are average treatment effects indicating the change in employment and wages, respectively, compared with the year prior to the transaction. There is evidence that employment is about 9% higher 2 years after a PE-backed LBO, significant at the 10% level. Otherwise, there is no evidence that LBOs, whether PE-backed or not, have any significant effect on employment and wages. Results for the employment effects of related takeovers are supportive of those reported in the section on Propensity Score Matching Combined with Difference-in-Differences; however, the coefficient estimate indicates quite a large decline in employment of about 41% in the year after the transaction. The remaining results indicate related takeovers have no effect on wages and unrelated takeovers have no significant effect on either employment or wages.

CONCLUSIONS

Ownership change via LBO or takeover has received criticism for their negative effect on employment and wages. Indeed, critics have suggested there should be intervention in the market for corporate control in order to protect the welfare of employees. Intervention to protect employees' welfare is likely to affect the efficiency of the market for corporate control. It is therefore important to establish through systematic analysis the consequences for employees of ownership change. This paper provides an empirical analysis of the effects of ownership change via LBO and takeover on wages and employment using two methods: differences-in-differences combined with propensity score matching and the control function approach. We use two methods in order to examine the robustness of findings to the method employed. In order to examine the controversial effects of PE, the empirical analyses distinguish between LBOs that have PE backing and those that are conducted without PE. This is the first study to make such a comparison. The paper also distinguishes between related and unrelated takeovers.

Four key findings emerge irrespective of the method employed. First, LBOs with PE backing have no significant impact on wage levels. Second, non-PE-backed LBOs have no significant impact on wages. Both these findings accord with Lichtenberg and Siegel's (1990) US study that reports that the wages of blue-collar workers remain unchanged. Third, unrelated takeovers have no significant effect on wages, which supports the findings of Conyon *et al*. (2004). Finally, related takeovers have a negative effect on employment, supporting the findings of Conyon *et al*. (2002).

Using difference-in-differences combined with propensity score matching, we also find that non-PE-backed LBOs have negative employment consequences. In addition, in accordance with Conyon *et al*. (2004) and McGuckin and Nguyen (2001), there is evidence that related takeovers have a positive impact on wages. Using the control function approach, we find that there is weak evidence of higher employment after a PE-backed LBO. This contrasts with Davis *et al*. (2008) who find that PE-backed LBOs in the US have a small negative impact on employment levels.

Should policy-makers intervene to protect employees' interests during ownership change? We argue that our results suggest not. Although related takeovers result in negative employment effects, this could be due to rationalisation and a reduction in excess

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capacity. The activities of PE firms have received particular criticism (International Trade Union Confederation, 2007) and have been under scrutiny (Treasury Select Committee, 2007); our results provide no support for government intervention in order to protect the jobs and wages of employees of firms subject to an LBO.

NOTES

1. LBOs are typically characterised by (i) an increased concentration of firms' equity held by managers, (ii) an increase in leverage with the firm taking on a large amount of debt secured against future cash flows and/or secured against firms' assets and (iii) active involvement in monitoring at board level by private equity funds when they finance an LBO.
2. This arises as financiers are able to negotiate contractual conditions in the shareholders' agreement providing for such disclosure.
3. For the control sample of firms, growth rates over the first 3 years for which firms are observed are reported.

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