Private Equity and Pay Gaps Inside the Firm[*]

Lily Fang[†]   Jim Goldman[‡]   Alexandra Roulet[§]

Abstract

Exploiting a 20-year sample of leveraged buyouts matched to French administrative data, we document that target firms post-buyout experience reduced within-firm inequality together with increased profitability relative to control firms. Wage gaps between men and women, managers and non-managers, and older and younger employees decline by 9%, 4%, and 21%. The P90/P50 and P90/P10 wage ratios also decline. Composition effects drive these results. Post-buyout, target firms separate from and replace expensive employees with cheaper ones. Remaining employees experience small pay increases. The results suggest that, in seeking to improve firm profitability, private equity investors attenuate wage inequalities inside target firms.

1 Introduction

Between 2010 and 2020, private equity funds invested nearly $5 trillion in firms worldwide. Target firms of private equity buyouts generally experience not only substantial improvements in operational efficiency, but also sizable labor reallocation (Davis et al., 2014). Several recent studies examine the net effect of these labor reallocations on employee outcomes (Morris and Phalippou, 2020), focusing, for example, on employment counts at target firms (Boucly et al., 2011 ; Davis et al., 2014), on the medium-term earnings of employees affected by the deal (Antoni et al., 2019), on the career paths of these employees (Agrawal and Tambe, 2016), and on work injuries (Cohn et al., 2021).

We contribute to the literature on the labor effects of private equity buyouts by examining the interplay between the efficiency-increasing labor reallocations at target firms and possible changes to the wage distribution inside these firms. An active body of research in labor economics documents an important role of firms in pay inequalities, highlighting that within-firm inequality explains a sizable part of overall earnings inequality (Song et al., 2018). In fostering target firms' profitability, private equity investors, typically owning a controlling stake in the companies they invest in, may affect pay differences either by adjusting the earnings of individual workers or by changing the composition of workers at the firm. As private equity remains a pervasive – and controversial – form of corporate ownership, and as within-firm pay inequality has become a salient labor outcome (Mueller et al., 2017), understanding the role that private equity buyouts play in within-firm inequalities calls for thorough empirical examination.

In this paper, we exploit matched employee-employer administrative data on the universe of French firms and employees to examine the evolution of the wage distribution and pay differences between men and women, managers and non-managers, and younger and older employees at target firms around private equity buyouts. France presents some advantages for such a study. The French administrative data enable us to construct, for each year, a comprehensive view of the wage distribution inside each firm which we can link to the demographic and occupational characteristics of the workers.

The data also allow us to follow a random sample of workers over time, which is helpful in disentangling possible composition effects from across-the-board changes in compensation. France is the second largest private equity market by investment in Europe after the U.K., ahead of the larger economy Germany.[1] Therefore private equity plays an important role in its economy. Furthermore, the French labor market is rigid, which raises the bar for finding evidence of any reallocation effect.

We find that, relative to a control group of firms, target firms after the buyouts experience increased profitability, increased employment and reduced pay gaps. In particular, the difference between the 90th and the 10th percentile of the wage distribution declines by 3%, the gender pay gap by 9%, that between managers and non-managers by 4%, and that between older (50 years old or above) and younger employees by 21% relative to the mean at control firms post-buyout. These declines materialize one year after the buyout closing and remain significant three years after. The decline in each of the gender, occupation, and age pay gaps is also robust to controlling for changes in the other two pay gaps, suggesting that the three results do not merely reflect overlapping sources of variation.

Next, we examine what drives these reductions in pay gaps. At the firm level we find that the declines in pay gaps are entirely driven by post-buyout relative reductions in the average wages of employees in the "high-pay" categories, that is, men, managers, and older employees. Relative to control firms, average wages at target firms for these employees decline by 2.4%, 5.2% and 6.0%. In contrast, the average wages of women, non-managers, and younger employees relative to their control groups are little affected by the buyout.

The reduction in the relative wages of the high-pay categories can in turn be the result of a relative decline in the wages of employees in these categories who stay at the target firms after the buyout, or of changes in the composition of employees within these categories. To disentangle these explanations, we leverage data on a random sample of employees that we can track over time. We find little support for the idea that a reduction

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[1]Source: Statista. https://www.statista.com/statistics/324047/uk-equity-market-private-equity-investments-europe/, accessed June 6, 2021.

in the wages of men, managers, and older employees who stay at the firm explain the reductions in firm-level gender and occupation pay gaps that we find. In fact, men staying at the target firm experience small pay *increases* of 1.7% after the buyout relative to the control group, while the wages of staying managers and older employees do not change significantly relative to their control groups.

Instead, consistent with composition effects, we find particularly high separation likelihoods for men, managers, and older employees at buyout firms relative to the control group, in the year of the buyout and the three following years. To understand how these separations affect pay gaps, we then compare the pre-buyout wages of employees who end up leaving the target firm (whom we will call leavers), to those who end up staying (whom we will call stayers). We find that in the year before the buyout, men, managers and older leavers are paid 10.3%, 9.6% and 16.2% more than stayers in the same categories. In contrast, women, non-managers, and younger leavers are not paid significantly differently than stayers in the same categories. In addition, expensive employees in the high-pay categories are replaced with cheaper employees. Newly hired men and managers are on average paid about 13% less than previous employees in these categories (i.e., both leavers and stayers). Finally, firms hire few older employees to replace older leavers; relative to control firms, the share of older employees at target firms after the buyout declines by 5%.

As our wage measure includes performance-related pay and bonuses but may miss some stock-based incentives, we also consider the possibility that private equity backed firms start implementing stock-based compensation after a buyout, and that the change in compensation structure drives our main findings. Collecting data from a separate administrative survey on stock-based compensation, we find that only about 1% of surveyed firms and about 1% of LBO firms use such form of compensation between 2006 and 2012. Stock-based compensation is therefore unlikely to negate our results.

Taken together, our results are consistent with the notion that, in seeking to improve target firms' profitability, private equity investors can reduce pay inequalities inside tar-

get firms by fostering highly-paid employees' separation from the firm.

This paper contributes to the literature on the effect of private equity on firm and employee outcomes. As an increasingly prevalent form of ownership, private equity has attracted intense academic and media debate. It is well documented that private equity buyouts tend to increase the efficiency and profitability of target firms (Kaplan, 1989 ; Kaplan and Stromberg, 2009 ; Guo et al., 2011 ; Boucly et al., 2011 ; Bernstein et al., 2017 ; Cohn et al., 2020), and our results are consistent with these findings. Recent research has also examined the consequences of private equity buyouts for consumer outcomes (Eaton et al., 2019) and management practices (Bloom et al., 2015). With respect to employee outcomes, at the firm and establishment levels Davis et al. (2014) show that buyouts are associated with substantial increases in both hiring and firm-employee separations. Olsson and Tåg (2017) focus on separations, and document that private equity buyouts accelerate offshoring and job polarization. Agrawal and Tambe (2016) report positive effects on employability and wages for workers in the information technology industry, while Antoni et al. (2019) document adverse effects on employment and wages for managers and older employees separating from target firms. In contrast to these studies, we focus on the effect of buyouts on changes to the within-firm wage distribution. We show that buyouts materially affect within-firm income inequality, which extant literature has shown to have a significant bearing on the wider income inequality in the economy. Overall, our study offers uniquely granular evidence on pay gaps in private equity controlled firms, indicating the possibility that private equity can improve efficiency and simultaneously reduce income inequality.

2 Data and Empirical Strategy

2.1 Data Sources

We obtain information on leverage buyouts of French target firms between 1997 and 2014 from Capital IQ.[2] To extract these transactions from the database, we apply the same selection criteria as Davis et al. (2014). We select deals featured as "going private", "leveraged buyout", "management buyout", or "platform", and for which the buyer is an investment firm. As Davis et al. (2014) note, this approach excludes growth investments of minority stakes with little or no leverage.

Capital IQ does not provide national firm identifiers. We thus hand-match buyout targets to the French corporate registry by name and address. We then use firms' national identifier (called SIREN) to link target firms to the tax and labor files described below. For 66% of the transactions, we find at least one suitable SIREN with a non-zero number of employees and sales over 1 million euros. This match rate is comparable to the match rates obtained by Davis et al. (2014) in US data. When a buyout occurs in a holding company, our analysis treats the operating subsidiaries as the target firms.[3] For each firm, we record the year of the first private equity transaction that we observe as the buyout year.

Information on firms and their employees comes from INSEE, France's national statistics bureau. Firm financial information is from the tax files (called FICUS until 2007 and FARE from 2008), as in, for example, the study of Garicano et al. (2016). We merge these data with matched employer-employee registers (DADS Postes data). These data give us information at the firm-level on each job (wages, number of days of work, number of hours, type of contract, gender, age and occupation of the worker). The wages variable

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[2]The sample period is driven by data availability. Consistent administrative firm and employee data are available from 1994 to 2017, and we study outcomes from three years before to three years after a buyout.

[3]We can identify holding companies by their industry code, their name, and their small number of employees. To ensure that we capture the main operating entities, we exclude the 71 buyouts for which we identify more than three subsidiaries recorded under the target name at the target's address. As we show in the appendix, the results are little affected by removing this restriction.

captures all compensation subject to social security contributions, including performance-based pay. We define "older" employees as those over 50 years of age and the managers as workers with occupation codes starting with either "23" or "3" (e.g., *"chefs d'entreprises"*, *"cadres et professions intellectuelles supérieures"*). FICUS/FARE and DADS Postes data cover the universe of firms every year.

One limitation of the DADS Postes data is that they are a panel only for firms, and repeated cross-sections for employees: the employees IDs are randomized every year and it is not possible to track them over time. But for a random subset of employees, a separate DADS Panel tracks the details of workers' employment history (including the firm in which they work, wages and other administrative data on their employment, and unemployment) from 1994 to 2017 continuously. The sampling rate is 1/24th of the total population until 2001, and 1/12th afterwards.

To account for the fact that different categories of employees work different hours, we base our outcome variables on the hourly wage (computed as gross wage over number of hours worked), but our results are robust to using gross wages. We calculate the (percentage) wage gap between two categories of employees as the average (hourly) wage of employees in the high-pay minus that of the low-pay category, divided by the average (hourly) wage of employees in the high-pay category.[4]

2.2 Forming the Control Group

Our main empirical strategy compares outcomes at target firms from three years before a buyout to three years after, relative to a set of matched control firms. We restrict the analysis to firms with more than 1 million euros in sales and at least 1 employee in each of the categories we study (men, women, managers, non-managers, older, and younger). We then sort target firms - our treated sample - into cells defined by the cross-product of characteristics that heavily determine buyout probability: 2-digit industry, industry-specific terciles of size (total assets), profitability (return on assets, measured, following

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[4]For example, the gender pay gap is calculated as $(\overline{\text{Wage}}_{men} - \overline{\text{Wage}}_{women})/\overline{\text{Wage}}_{men}$.

Boucly et al., 2011, as earnings before interest and taxes over tangible assets and working capital), and employment growth. For each target firm, we take as controls those firms that were not bought by a private equity investor and that fall into the same cell as the target in the year before the buyout. We focus the analysis on target and control firms that exist in the three years before and after the transaction year.[5] To associate each target firm to a given control firm, our regressions weight each control firm by the number of treated firms in the matching cell divided by the number of control firms in the cell.

We adopt a similar approach in our employee-level analyses based on the DADS Panel. We identify individuals working at a private equity target firm one year before a buyout, and match them to control individuals of the same gender, in the same 5-year age bracket, hourly wage bracket (10 brackets), 2-digit occupation code (36 categories), full-time/part-time status, and who are working at a firm in the same 2-digit industry and in the same size category (3 categories). We focus on individuals aged between 20 and 70 and exclude individuals who work at more than four jobs in a given year. In instances where an individual works in more than one job in a given year, we aggregate their wages and hours at the firm level and keep the employee-firm record with the highest wage in the year. To associate each treated employee to a given control employee and ensure that large firms are not over-represented in the analysis, we weight each treated employee by $1/N$, and each control employee by $n_t/(n_c \times N)$ where n_t is the number of treated employees in the matching cell, n_c is the number of control employees in the matching cell, and N the number of employees at the firm.

The final main sample contains 813 target firms. Figure 1 plots the number of target firms in the sample in each year. Consistent with aggregate worldwide buyout activity, the number of deals in the sample increases between 2004 and 2008, and stabilizes at the 2005-06 level after 2009.

[Figure 1 here]

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[5]Imposing such restriction increases the precision of some estimates but does not qualitatively affect our results. We present the main results without this restriction in the appendix Table A.2.

Table 1 presents summary statistics for the characteristics of sample firms. We report the means of characteristics for target firms and their matched control firms. The means of sales, leverage, return on assets (RoA) and employment growth are remarkably similar in target and control firms, as are wages and wage gaps across all employee categories. The distribution of employees across the categories (men, managers, old) is also very similar in target and control firms. Target firms tend to be somewhat larger than control firms in terms of total employment, but, as shown in Figure 2 panel (b), the two groups of firms follow similar trends in employment before the buyout, which is what matters for the validity our empirical methodology. Table A.1 in the Appendix presents summary statistics for the characteristics of the private equity investors in the sample.

At the employee-level, our panel of individuals contains 14,403 unique employees working at a target firm in the year before a buyout. As shown in Table 2, 63% are men, 18% managers, and 17% aged over 50. The observable characteristics of individuals are again very similar across target and control groups.

[Tables 1 and 2 here]

2.3 Empirical Strategy

Our main empirical analysis uses a difference-in-differences approach to compare outcomes in the years around a buyout at target units (firms or employees) relative to their control group. Specifically, we estimate the following regression:

$$Y_{ijt} = \beta(LBO_i \times Post_j) + \eta_j + \alpha_i + \tau_t + \epsilon_{ijt} \tag{1}$$

where i indexes a firm or an employee, j indexes time in event-years and t time in calendar year. Y_{ijt} represents the firm or employee outcomes that we study (e.g., a firm-level pay gap or the logarithm of an employee's wage). LBO is a dummy variable that equals one if unit i is affected by a leveraged buyout. $Post$ is a dummy variable that equals one for the years after the buyout. α_i are firm or employee fixed effects that control for time-

invariant characteristics, η_j are the event-time fixed effects, and τ_t are the calendar year fixed effects to control for aggregate shocks. The coefficient of interest is β, the difference-in-differences estimator. Since the events that we study occur at the firm level, we cluster standard errors at that level in both the firm and the employee analyses.

We also examine the dynamics of the outcomes we study, paying particular attention to potential trends before the event. We do this by estimating the following event-study specification:

$$Y_{ij} = \sum_{k=-3}^{3} \beta_k D_{ij}^k \times LBO_i + \sum_{k=-3}^{3} \gamma_k D_{ij}^k + \alpha_i + \epsilon_{ij} \tag{2}$$

In our dynamic analyses, we then collect and plot the β_k coefficients. D^k are dummy variables indicating that unit i underwent a buyout (or placebo buyout if it is a control) k periods ago. We take the year before the buyout as benchmark year and omit the associated dummy variable in the regression ($D^{-1} = 0$). Thus, the β_k coefficients estimate the difference in outcome between buyout and control units, in event-year k, relative to the year before the buyout.[6]

3 Results

3.1 Firm Leverage, Profitability and Employment

We start by examining the effect of the buyouts on target firms' leverage, profitability, employment counts, and labor costs. Table 3 reports the results from estimating equation

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[6]Recent work in econometrics suggests that difference-in-differences estimates can be biased when the OLS estimator does not aggregate the treatment effects with appropriate weights. The concern particularly arises without never-treated observations, a case in which all the identification relies on the difference in timing across treated observations. In our case, this concern is mitigated because we do have never-treated observations (the control group) and we carefully match, year-by-year, treated observations to control observations. Nevertheless, we checked the robustness of our results to using new estimators, in particular that proposed by Borusyak et al. (2021) which is robust to the weighting concern and also performs the test for parallel trends separately from the estimation of the treatment effect. Our event studies results are robust to using this estimator as presented in Appendix Figure A.2.

[1](#) on these outcomes.

We first validate our empirical setting by examining firms' financial leverage, measured as the ratio of debt over assets, as we expect leveraged buyouts to increase targets' access to debt financing (Boucly et al., 2011).[7] The coefficient in column 1 is significant and suggests that, relative to control firms, target firms' leverage ratio increases in the three years after the transaction. This increase in debt financing is economically meaningful as it represents a 13% increase from the average control firm leverage after the transaction.

The buyouts in this sample are also associated with improvements in firms' operational profitability. The estimate in column 2 indicates that, on average, target firms' return on assets increases significantly by 3.1 percentage points after the transaction, which represents a 17% increase relative to the post-buyout mean of the control firms. In column 3, we examine the evolution of target firms' employment count. We find a 3.7% post-LBO net increase in employment relative to control firms. The coefficient in column 4 indicates that the firm's labor cost decreases as a fraction of sales by 3% from the post-buyout control mean. Overall, these findings are consistent with the previously documented profitability focus of private equity investors, and with their success at restructuring firms.

[Table 3 and Figure 2 here]

Figure 2 displays the year-by-year effects for leverage and employment, estimated from equation 2. The coefficients on the years before the buyout are generally close to zero and insignificant, suggesting that the parallel trend assumption on which our estimation strategy is based is likely met. Leverage starts increasing the year after the buyout and continues to increase steadily in the following two years. Three years after the buyout, target firms' leverage is approximately 3 percentage points higher than control firms'. Employment starts expanding significantly two years after the deal closing.

Overall, the buyouts are associated with measurable increases in debt financing, in-

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[7]Boucly et al. (2011) note that the debt raised for the LBO itself is typically borne by a holding company and therefore does not appear in the unconsolidated accounts on which the tax files are based. Thus, the leverage ratio that we calculate indicates the target firm's ability to raise debt beyond what has been raised by the private equity firm to finance the LBO.

creases in operating profitability and employment. The patterns are generally consistent with those documented by Boucly et al. (2011) in their earlier sample of French transactions, though the magnitude of the effect on employment count in our sample is smaller than in theirs.

3.2 Average Pay Gaps Inside the Firm After a Buyout

In this section, we present our main results on measures of pay gaps at target firms in the years around a buyout.

We start by looking at how, on average, firms' wage at each decile of the wage distribution changes after a buyout relative to the control group, using our main specification of equation 1. Table 4 shows that post-buyout, the 99th percentile of the within-firm wage distribution declines by 9.6% at treated firms relative to control firms. The decrease at the 90th percentile is 1.7% while the decrease at the 95th percentile is 4.0% (untabulated). In contrast, the wage at the 10th percentile, the median and the 80th percentile are little affected by the buyout. We find that this is also the case for all other intermediate deciles not reported in the table. As a result, post-buyout, at target firms the ratio between the 90th and the 50th percentile and that between the 90th and the 10th percentile decline respectively by 0.04 and 0.07 relative to the post-buyout control means of 1.85 and 2.61. They thus represent relative declines of approximately 2% and 3% (0.04/1.85 and 0.07/2.61).

[Table 4 here]

Next, we assess pay gaps along three dimensions: gender, occupation, and age. Specifically, we compare men and women, managers and non-managers, and older and younger employees. For each dimension, we first estimate log wage regressions for the two relevant employee categories (e.g., men and women). These regressions separately assess the evolution of the average wage of employees in that category at target firms relative to control firms. We then estimate a wage gap regression with the wage difference between the two categories in each dimension (i.e., the gender wage gap, the occupation wage

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gap, and the age wage gap) as the dependent variable. The coefficient on the LBO X Post variable is the difference-in-differences estimator. Table 5 presents these results.

Starting with gender, the estimate in column 1 indicates that the average wage for men at a target firm declines by a statistically significant 2.4% after a buyout, relative to the average wage for men at control firms. In contrast, the estimate in column 2 suggests that the average wage for women at target firms remains broadly unchanged. In column 3, we find that the average relative pay gap between men and women declines by 1.5 percentage points at treated firms relative to control firms, which represents a 9% reduction relative to the average gender pay gap at control firms post-buyout.

We find similar results for the occupation and age pay gaps. Columns 4 and 7 indicate that the average wage for managers and older employees decline by 5.6% and 6.0% after the buyout relative to their control groups, while columns 5 and 8 show that the relative wages of their non-managers and younger counterparts are little affected: The point estimate for the wage of non-managers and younger employees is small (-0.2%) and statistically insignificant. In terms of wage gaps, target firms' occupation and age wage gaps decline, respectively, by 1.9 and 4.0 percentage points, which translate to a 4% and 21% decline relative to the post-buyout mean of control firms.

[Table 5 and Figure 3 here]

Figure 3 illustrates these results and plots the year-by-year coefficients in event-time from the log wage regressions for the six employee categories that we study. Sub-figures (a), (c) and (e) in the first column display the event study coefficients for the high-pay categories (men, managers, older employees). In the years before the buyout, these difference-in-differences estimates are all insignificant and almost exactly zero. Starting the year after the buyout, they exhibit a sharp decline that reaches about 6 percentage points for managers and older employees and 2.5 percentage points for men. In contrast, sub-figures (b), (d), and (f) in the second column plot the coefficients for the low-pay categories (women, non-managers, younger employees). There, the small and statistically insignificant coefficients oscillate around zero throughout the plots. Figure A.1 shows the

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evolution of wages for all categories separately for treatment and control firms. At control firms, for each category the average wage is on a linearly increasing trend while at treated firms, we see a break in the trend for the high-pay groups right after the buyout. Figure A.2 shows the robustness of the results of Figure 3 to using the estimator proposed by Borusyak et al. (2021).

To the extent that men are more likely to be managers and managers are likely to be older, the three pay gaps that we study are positively correlated. To assess each pay gap independently of the other two, in Table 6, we re-estimate each pay gap regression while controlling for contemporaneous changes in the other two pay gaps. We present these variations in three panels, one for each pay gap as the dependent variable. Controlling for contemporaneous changes in the other pay gaps tends to reduce slightly the point estimates on the LBO X Post variable (to 1.1, 1.5 and 3.5 percentage points), but these coefficients all remain statistically significant at conventional levels, suggesting that the significant declines in these three pay gaps also capture non-overlapping sources of variation.

[Table 6 here]

Appendix Table A.3 also checks that our results on within-firm pay gaps are not driven by changes in the CEO's wage or in wages of employees in the top 1%. The decrease in all three pay gaps of interest at target firms post-buyout relative to control firms is still significant when we exclude the highest earner or the top 1%. The magnitude of the coefficients drops a little but so do average pay gaps. Target firms' gender, occupation and age pay gaps decline by 5%, 2.5% and 10% post-buyout relative to the control means, respectively.

Overall, the results point to measurable reductions in pay gaps at target firms after a buyout relative to control firms. The relative reductions in pay gaps could reflect the implementation of broad individual pay cuts affecting employees in high-pay categories or substantial labor reallocation and changes in employee composition in these categories. We examine these alternative explanations in the next section.

3.3 What Drives the Reduction in Pay Gaps? Employee-Level Analyses

In this section, we analyze the drivers of the post-buyout reductions in within-firm pay gaps. To do so, we take advantage of the DADS Panel, which contains information on a random sample of employees tracked over time.

3.3.1 Stayers' Wages

We start by analyzing the wages of employees who stay at the firm during the three years after the buyout. If across-the-board pay cuts for existing employees drive the reduction in the average wages of men, managers and older employees at target firms, we would expect to find, at the individual level, large wage reductions for individuals in these employee categories.

Table 7 presents the results of log wage regressions estimated using the sample of employees who remain at the firm from three years before a transaction to three years after. Starting with gender, the results in column 1 indicate that men staying at the firm experience a small but statistically significant wage *increase* of 1.7% after a buyout relative to men at control firms. In column 2, we find a smaller and insignificant positive effect for staying women. These results suggest that the relative reduction in the gender pay gap at target firms is unlikely to be due to wage adjustments of staying men and women. Similarly, the reduction in the occupation pay gap is not driven by widespread reduction of the wages of managers staying at the target firm. The estimate in column 3 suggests that, if anything, these managers tend to experience a small pay *increase* relative to their counterparts in control firms, with a 3.1% positive but statistically insignificant estimate. The wages of staying non-managers relative to their control group are little affected by the buyout: we find a small and insignificant 0.8% increase in wages in column 4.

In sum, while the firm-level results of Table 5 show clear relative declines in the average wages of men and managers in the buyout target firms compared to their control firms, we do not find corroborating wage drops at the individual level for incumbent employees staying at the target firms after the deal.

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Moving to the age pay gap, in columns 5 and 6 we do find evidence of an adjustment of stayers' wages that is in line with a reduction in the age pay gap. While the relative wage of staying older employees is little affected by the buyout (the point estimate in column 5 is an insignificant 0.4%), younger employee wages *increase*, on average, by a statistically significant 1.5% relative to the control group. However, this pattern does not fully explain the reduction in within-firm age-related wage gap, as Table 5 indicated a substantial reduction (6.0%) in the average wage for older employees at target firms relative to control firms.

Overall, we do not find evidence supporting the notion that broad pay cuts among high-pay categories (men, managers, and older employees) at buyout targets drive the reduction in wage gaps at these firms. If anything, staying employees of these categories experience slight, if statistically weak pay increases. Next, we examine composition effects and the reallocation of labor - which include employee separations and hiring - within the different categories of employees.[8]

[Table 7 here]

3.3.2 Firm-Employee Separations

Table 8 displays the results of regressions comparing the probability that individuals employed at the target firm in the year before the buyout leave the firm in the year of the transaction or in the following three years, relative to their control group.

Across gender, occupation, and age dimensions, target firm employees in high-pay categories (that is, men, managers, and older employees) display a higher probability of separating from the firm after the buyout, relative to control firm employees in the same categories. The estimate in column 1 indicates that men at target firms are 5.6 percentage points more likely than their control group to separate from the firm after the transaction, which is 15% higher than the mean separation rate for men at control firms. For

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[8]When we pool stayers across categories, we find that, on average, stayers at target firm experience a 1.3% wage increase relative to the control group (p=0.050).

women the effect is a 4.0 percentage point higher likelihood (column 2); 9% higher than the mean in the control group. The effect for men and women is not statistically significantly different. Turning to the occupation and age related pay gaps, managers and older employees at target firms are 9.9 and 8.9 percentage points more likely than their control groups to separate from the firm after the buyout (columns 3 and 5), while non-managers and younger employees are only 3.7 and 4.3 percentage points more likely to separate. These reflect a 25% and 23% increase in the separation rate for managers and older employees from the control group means, against a 9% and 11% increase for non-managers and younger employees. In contrast to the gender difference in separate rates, the differences in separation rates between managers and non-managers and between older and younger employees are statistically significant at the 1% and 10% level (columns 2 and 3 of Appendix Table A.4).

In Appendix Table A.4, we show the robustness of these results to controlling for the two other characteristics and the differential probability of separation of employees with these characteristics at target and control firms. We see, for instance, in column 7 of Table A.4 that, conditional on the other two characteristics (and these characteristics' differential probability of separation at target and control firms), managers at target firms are still more likely to separate from their firms than are managers at control firms, and the same holds for older workers.

[Table 8 here]

Thus, overall, separations at target firms disproportionally affect the high-pay categories of employees (especially managers and older employees). To understand how these labor reallocations affect the pay gaps, we next examine the wages of employees who leave as well as the wages of employees who join the firm after the buyout.

3.3.3 Leavers' and Joiners' Wages

Table 9 compares, one year before the buyout, the wage of employees who will leave the target firm after the buyout (leavers) to those who will stay at the firm (stayers). Since

this analysis focuses on within-firm wage differences between leavers and stayers, in this analysis we drop the control group and estimate regressions on workers at the target firms only. We find that, one year before the transaction, target firm leavers in high-pay categories (men, managers, older employees) are on average paid significantly more than the target firm's stayers in the same category. Indeed, in the three years before the buyout, men, managers and older employees who end up being the leavers are on average paid 10.3%, 9.6%, and 16.2% more than stayers. In contrast, women, non-manager, and younger leavers are not paid statistically differently from their stayer counterparts (the 1.2%, -1.8% and 3.0% point estimates in columns 2, 4 and 6 are all statistically insignificant).[9]

[Table 9 here]

In Appendix Table A.5, we assess the interactions between our three dimensions of interest, i.e., gender, occupation, and age among the leavers. We find that among leavers, the wage difference between managers and non-managers and that between older and younger employees survive the controlling for the other two dimensions, while the wage difference between men and women leavers becomes insignificant (column 7). This suggests that the wage difference between leaver men and leaver women is driven by the former being more likely to be managers and older. These results in turn indicate that private equity investors can reduce the gender pay gap inside target firms by separating from particularly expensive employees who are more likely to be men.

Private equity investors can also affect the within-firm pay gaps by setting the wages of newly hired employees, whom we will call "joiners". In Table 10, we compare the wages of joiners to that of employees working at the firm the year before the buyout (including both leavers and stayers). In each employee category other than older employees, we find that private equity target firms replace expensive separated workers with

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[9]Consistent with previous work (Antoni et al., 2019), individuals who separate from the target firm after a buyout fare worse than those who do not. In this sample, they are 50 percentage points less likely to be employed three years after the buyout, and this effect reaches 80 percentage points when it comes to older employees.

cheaper joiners. Joiners who are men and managers are paid about 13% less than incumbents. Thus, for men and managers, the magnitude of the "discount" on new hire wages is slightly larger than the existing wage "premium" associated with the leavers, which we estimated to be about 10% in Table 9. We do not find older joiners to be paid statistically differently from older incumbent employees.

In sum, in the post-buyout labor reallocation, target firms are more likely to separate from expensive employees in the high-pay categories (highly paid men, managers, and older employees) than their control firms, and hire more cost-effective new employees in the men and managers categories. In the older employee category, our next sub-section shows that hiring is muted. These adjustments reduce the observed gender-, occupation-, and age-related pay gaps in the target firms relative to the control firms.

[Table 10 here]

3.4 Workforce Composition

Our analysis so far has focused on pay gaps within employee groupings by gender, occupation, and age. We have shown that target firms of private equity buyouts reduce pay gaps across these dimensions by separating from expensive members of high-paid categories and replace them with cheaper employees. We now examine whether the workforce composition across categories changes after the buyout. Specifically, we re-estimate equation 1 at the firm-level to compare the evolution of the percentage of men, managers and older employees at target and control firms around the buyout.

Table 11 reports these results. In columns 1 and 2, we find that the labor reallocation that we document in the gender and occupation dimensions mostly occur within employee categories (i.e., within men, women, managers, or non-managers). The share of men (and women) and the share of managers (and non-managers) at target firms do not change significantly after the buyout. The point estimates of -0.001 and 0.003 are small relative to the sample averages for the percentage of men (66%) and the percentage of managers (22%) at control firms post-buyout and are statistically insignificant. In con-

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trast, the estimate in column 3 indicates that the percentage of older employees at buyout target firms declines significantly by 1.1 percentage points after a buyout, which represents a 5% reduction from the percentage of old employees at control firms post-buyout (22%).

Together, these results suggest that when target firms separate from expensive men and managers, they replace them at the same rate as their women and non-manager counterparts; but when target firms separate from expensive older employees, they tend to replace them with younger (and also less expensive) employees.

[Table 11 here]

3.5 Stock-Based Compensation

In our final analysis, we examine the possibility that our results are driven by changes in highly-paid employees' compensation structure that may not be captured by our main compensation data. This could be the case, for example, if private equity owners substitute a portion of the compensation that we observe with a type of compensation that we do not observe.

The wage variable that we use throughout the analysis captures all forms of compensation that are subject to social contributions (called Generalized Social Contribution – CSG – in France); this includes, but is not limited to, bonuses and performance-based compensation. However, it may not capture possible stock-based compensation such as stock grants or stock option grants. Therefore, to assess whether a substitution of benefits between the wage that we observe and stock-based compensation for men, managers and older employees may be a plausible driver of our results, we collect additional data from an administrative survey, called ACEMO-PIPA, which records consistent information on firms' use of stock-based compensation between 2006-2012.[10]

Using this survey, we assess the use of stock-based compensation among French pri-

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[10]The survey is sent to a sample of firms with between 10 and 250 employees and to all firms with more than 250 employees by the French ministry of Labor (DARES). By law, response to the survey is mandatory.

vate firms in general, and at LBO firms included in the survey. In column 1 of Table 12, we report the fraction of survey respondents using stock-based compensation or stock-based incentives. To make the survey sample comparable to our sample firms we restrict the survey respondents to private firms with between 20 and 800 employees, which corresponds to the 5th and 95th percentiles of the number of employees at treated firms. The percentages in column 1 indicate that the use of stock-based compensation is very rare among these firms. Only 0.4% of respondents allow employees to purchase stocks at a preferential price, 0.6% grant free stocks directly, 0.8% grant stock options, and 0.6% grant stocks through a company savings plan. We then match survey respondents to our sample of target firms and assess their use of stock-based compensation after the buyout. We find similarly low use of stock-based compensation or incentive at LBO targets after a buyout: 0.3% allow employees to purchase stocks at a preferential price, 1.1% grant free stocks directly, 1.3% grant stock options, 0.3% grant stocks through a company savings plan. Overall, these low numbers, together with the absence of wage declines for employees of high wage groups staying at the firm after the buyout, suggest that the granting of stock-based compensation is unlikely to be driving the decline in post-buyout pay gaps that we find.

[Table 12 here]

4 Conclusion

Over the past thirty years, private equity has become an increasingly mainstream ownership type globally. Much academic research and popular debate have focused on the efficiency gains and the shareholder value creation or destruction associated with these transactions. Our study focuses on the labor outcomes of workers in private-equity controlled firms, with a particular focus on the income distribution effects of private equity ownership. We provide granular evidence about the evolution of gender, occupation, and age-related pay gaps.

We find that relative to a carefully constructed control group of firms, firms under private equity ownership experience significant and sustained reductions in pay inequality between the 90th and 10th percentile of the wage distribution, as well as in all three demographic dimension (gender, occupation, age) that we study. The declines in these wage gaps come primarily from the job separations of the most expensive employees in the high-paid category (men, managers, and older workers). Instead of across-the-board wage reductions, men and young employees who stay at the target firms experience moderate wage increases relative to their peers in control firms, while the wages of staying older employees are not materially affected by the buyout. Expensive men and managers are replaced with new employees of the same category but who are cheaper. Expensive older employees are replaced by younger (and hence cheaper) employees. Overall these composition effects explain the relative reduction in wage gaps in buyout target firms relative to control firms.

The fact that the relative reduction in pay gaps at buyout targets is largely driven by the separations of expensive employees and their replacement with cheaper employees is consistent with the notion that private equity owners reduce the rents of expensive workers. At the same time, our evidence points to small wage increases for the employees staying at target firms relative to employees staying at control firms. These effects in combination suggest that private equity owners use changes in the workforce composition, more than wage cuts, as tools that increase the cost-effectiveness of the target firms. Meanwhile, these measures lead to reductions of pay gaps *inside* the target firms relative to other firms. These findings contribute to our understanding of the role of private equity in the labor market and highlight that cost-effectiveness and reductions in wage inequality can be achieved at the same time. How in the longer term these changes may affect the productivity of employees in different categories both inside and outside the firms and the attractiveness of private-equity owned firms as employers are interesting avenues for further research.

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Tables and Figures

Figure 1: Number of Target Firms in the Sample by Buyout Year

The figure shows the number of target firms in the final sample, by buyout closing year.
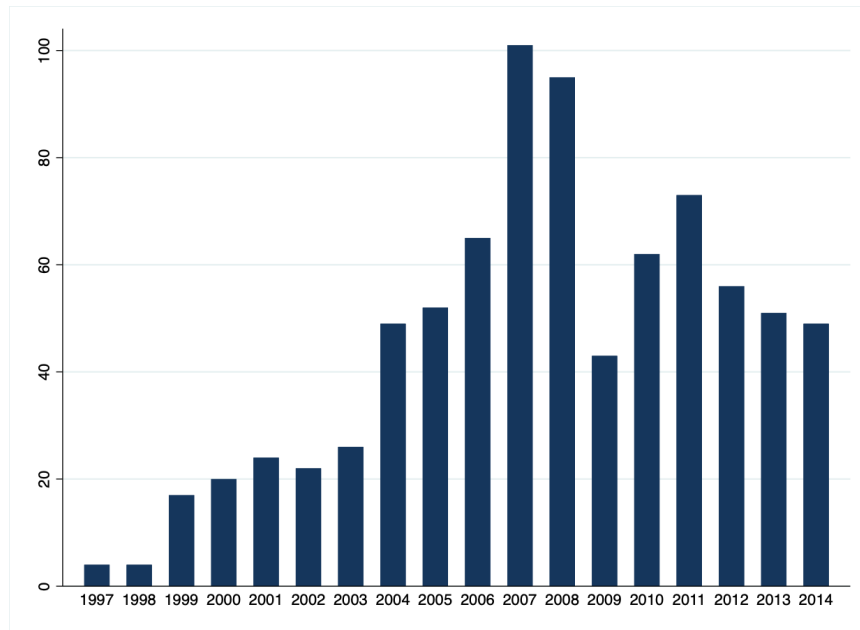
Figure 2: Leverage and Growth around LBOs

This figure shows the event study plot for firm leverage and the logarithm of employment around a buyout for target firms, relative to the matched control group. The figure is constructed by plotting the coefficients on event-time fixed effects interacted with the buyout indicator in a regression estimating equation 2. The X-axis displays the years relative to the buyout closing year. The dotted bars indicate the 95% confidence intervals, based on standard errors clustered at the firm level.

(a) Leverage

(b) ln(Employment)
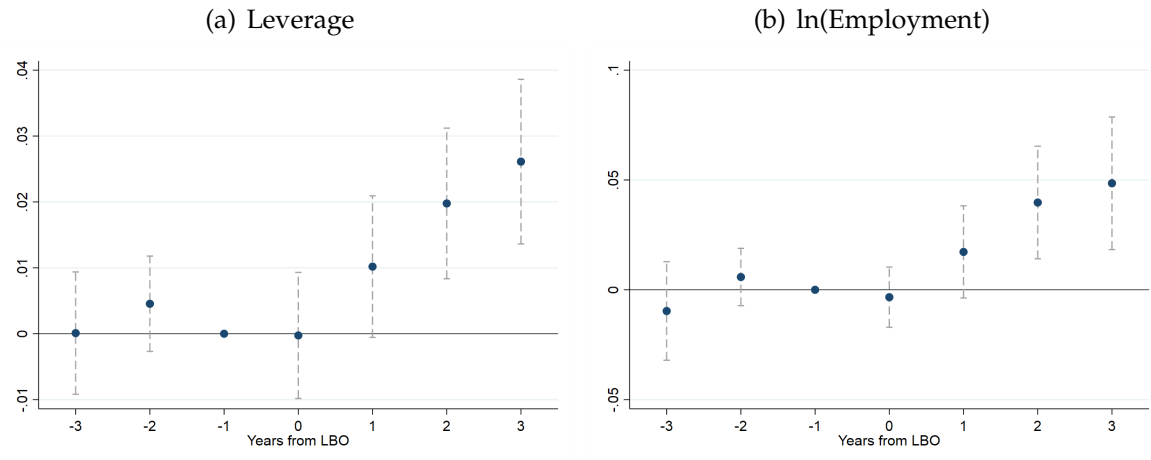
Figure 3: Firm-Level Average Wage by Employee Category

This figure shows the event study plot for the logarithm of average wage of men, women, managers, non-managers, older, and younger employees around a buyout at target firms, relative to the matched control group. The figure is constructed by plotting the coefficients on event-time fixed effects interacted with the buyout indicator in a regression estimating equation 2. The X-axis displays the years relative to the buyout closing year. The dotted bars indicate the 95% confidence intervals, based on standard errors clustered at the firm level.

(a) Men

(b) Women

(c) Managers

(d) Non-Managers

(e) Employees Aged over 50 (Older)

(f) Employees Aged less than 50 (Younger)
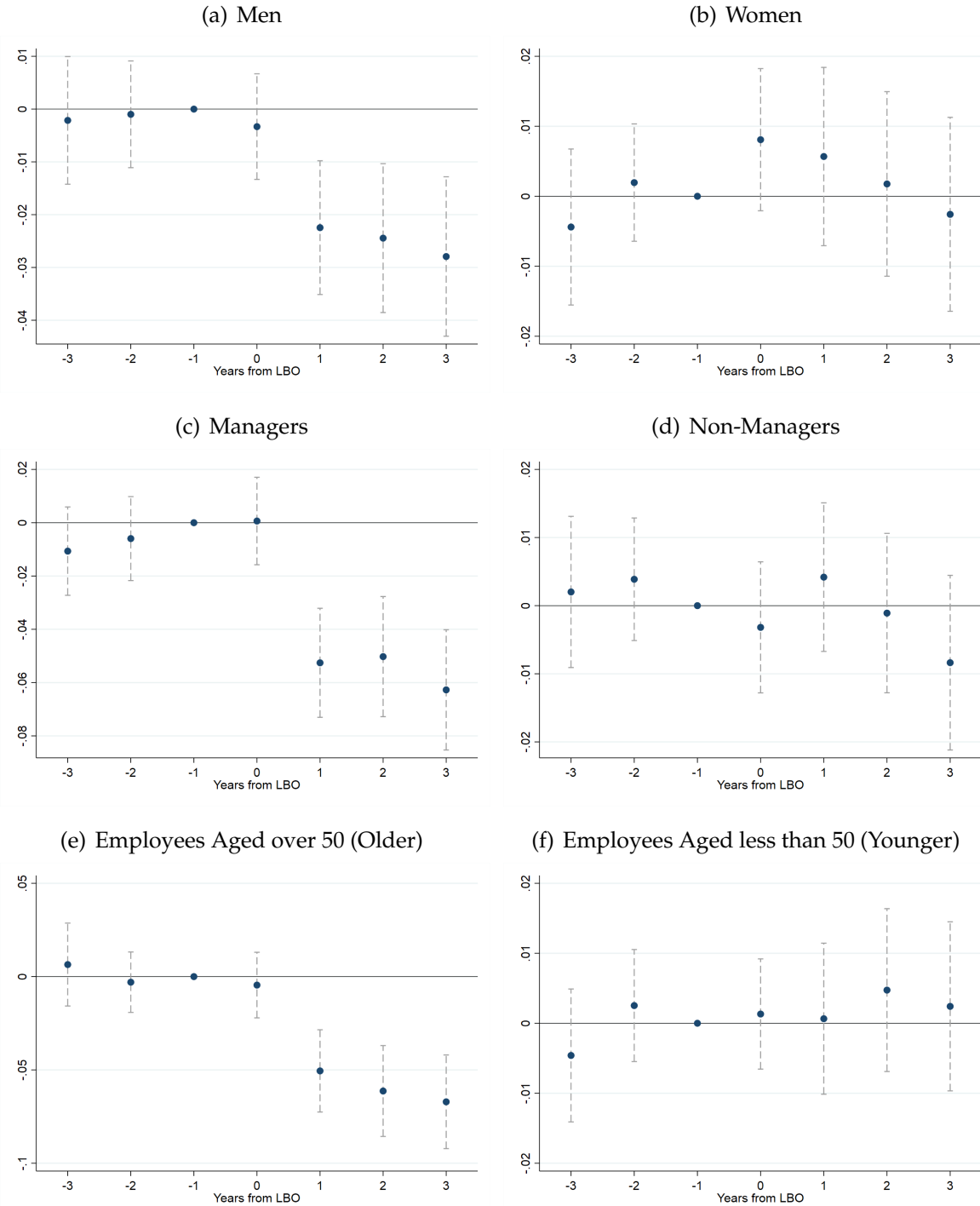
Table 1: Firm-Level Descriptive Statistics

Descriptive statistics for the sample of LBO targets and their control firms. Firms are matched on 2-digit industry, and industry-specific terciles of employment growth, total assets, and RoA before the deal. For each firm in the sample, each variable in the table is measured as the average over the pre-buyout period. Hourly wages by employee category (men, women, managers, non-managers, older, and younger employees) are measured as the average across the firm's employees between the ages of 20 and 70 who belong to that category. Monetary variables are expressed in euros.

|	LBO Targets (N = 813)			Control Firms (N = 76334)		
	Mean	Median	SD	Mean	Median	SD
Sales (thousands)	39610	16303	68609	37627	13652	76375
RoA	0.29	0.23	0.36	0.28	0.21	0.35
Leverage	0.14	0.10	0.14	0.14	0.09	0.16
Salary/sales	0.19	0.17	0.11	0.18	0.16	0.11
Employment growth	0.06	0.03	0.13	0.04	0.02	0.12
Employment	177.24	75.83	245.93	132.33	59.33	193.47
Wage p90	29.03	27.07	11.82	29.07	25.90	13.56
Wage p50	14.97	13.80	4.72	15.35	14.02	5.35
Wage p10	10.65	10.22	2.33	10.90	10.40	2.62
Wage gap p90-p50	1.93	1.85	0.44	1.87	1.78	0.46
Wage gap p90-p10	2.71	2.51	0.85	2.63	2.44	0.89
Wage men	20.48	18.89	7.54	20.61	18.24	8.59
Wage women	15.62	14.60	4.74	16.01	14.69	5.50
Gender wage gap	0.20	0.20	0.19	0.19	0.20	0.19
Wage managers	34.29	31.96	10.94	34.90	32.50	11.88
Wage non-managers	14.54	13.96	3.44	14.77	14.20	3.81
Occupation wage gap	0.55	0.55	0.13	0.55	0.55	0.13
Wage above 50 years old	24.75	21.09	12.56	24.07	20.56	12.41
Wage below 50 years old	17.22	16.24	5.22	17.48	16.06	6.02
Age wage gap	0.22	0.21	0.22	0.20	0.20	0.21
Percentage men	0.65	0.70	0.22	0.67	0.72	0.22
Percentage managers	0.22	0.15	0.21	0.21	0.13	0.21
Percentage above 50	0.18	0.17	0.11	0.19	0.18	0.11

Table 2: Employee-Level Descriptive Statistics

Descriptive statistics for the sample of employees working at a buyout target one year before the buyout and their control employees. For each employee in the sample, each variable is measured in the year before the buyout. Employees are matched the year before the buyout on gender, 5-year age bracket, 2-digit occupation code, full-time status, wage bracket (10 categories), firm size bracket (3 categories), and 2-digit industry. Monetary variables are expressed in euros.

	Employees at LBO Targets (N = 14403)			Control Employees (N = 457780)		
	Mean	Median	SD	Mean	Median	SD
Age	37	37	11	37	37	11
Annual Hours	1522	1814	466	1532	1818	465
Days worked / year	317	360	83	321	360	81
Male	0.63	1.00	0.48	0.63	1.00	0.48
Managers	0.18	0.00	0.38	0.18	0.00	0.38
Above 50 years old	0.17	0.00	0.38	0.17	0.38	0.00
Hourly Wage	18.05	14.66	12.73	18.12	14.69	13.02
Annual Earnings	28182	24157	23655	28488	24453	22590

Table 3: Firm Financials and Employment

This table displays the results of firm-level difference-in-differences regressions comparing firm outcomes at buyout target firms relative to the matched control group. Firms are matched on 2-digit industry, and industry-specific terciles of employment growth, total assets, and RoA before the deal. The coefficients come from estimating equation 1. Standard errors are clustered at the firm level. *, **, and *** denote significance at 10%, 5%, and 1% level.

	Leverage	RoA	ln(Empl.)	Salaries/Sales
	(1)	(2)	(3)	(4)
LBO X Post	0.018***	0.031**	0.037***	-0.006***
	(0.004)	(0.013)	(0.012)	(0.002)
Firm FE	Yes	Yes	Yes	Yes
Event-time FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
N	476981	476311	523213	526444

Table 4: Firm-Level Change in Wage Distribution

This table displays the results of firm-level difference-in-differences regressions comparing the evolution of firm wages at various percentiles at buyout targets relative to the control group. Firms are matched on 2-digit industry, and industry-specific terciles of employment growth, total assets, and RoA before the deal. The coefficients come from estimating equation 1. The dependent variable is the logarithm of wages at the focal decile in columns 1-5 and the ratio between the wage at some of these deciles in columns 6-7. Standard errors are clustered at the firm level. *, **, and *** denote significance at 10%, 5%, and 1% levels.

	ln(Wage)					Differences	
	p99	p90	p80	p50	p10	p90/p50	p90/p10
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
LBO X Post	-0.096***	-0.017***	-0.005	0.002	0.002	-0.044***	-0.070***
	(0.013)	(0.006)	(0.005)	(0.004)	(0.003)	(0.010)	(0.018)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Event-time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	526444	526444	526444	526444	526444	526444	526444

Table 5: Firm-Level Average Wage and Wage Gap across Categories of Employees

This table displays the results of firm-level difference-in-differences regressions comparing the evolution of firm average wages and wage gaps across employee categories at buyout targets relative to the control group. Firms are matched on 2-digit industry, and industry-specific terciles of employment growth, total assets, and RoA before the deal. The coefficients come from estimating equation 1. The dependent variable is the logarithm of wage for the focal employee category in columns 1-2, 4-5, 6-7 and the relative wage gap (difference in average wage scaled by the average wage of the high-wage category) in columns 3, 6, and 9. "Mgrs" denotes "Managers". Standard errors are clustered at the firm level. *, **, and *** denote significance at 10%, 5%, and 1% level.

	Gender			Occupation			Age		
	ln(Wage)		Wage gap	ln(Wage)		Wage gap	ln(Wage)		Wage gap
	Men	Women		Mgrs	Non-mgrs		Older	Younger	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
LBO X Post	-0.024***	-0.000	-0.015***	-0.052***	-0.002	-0.019***	-0.060***	0.002	-0.040***
	(0.005)	(0.005)	(0.004)	(0.008)	(0.004)	(0.004)	(0.009)	(0.004)	(0.006)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Event-time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	526444	526444	526444	526444	526444	526444	526444	526444	526444

Table 6: Firm-Level Wage Gaps Controlling for Changes in Other Wage Gaps

This table displays the results of firm-level difference-in-differences regressions comparing the evolution of firm wage gaps across employee categories at buyout targets relative to the control group. Firms are matched on 2-digit industry, and industry-specific terciles of employment growth, total assets, and RoA before the deal. The coefficients come from estimating equation 1. For each wage gap (gender, occupation, age), the regression controls for contemporaneous changes in the other types of wage gaps. The dependent variable is the relative wage gap (difference in average wage scaled by the average wage of the high-wage category). Standard errors are clustered at the firm level. *, **, and *** denote significance at 10%, 5%, and 1% level.

	(1)	(2)	(3)	(4)
Panel A: Gender wage gap				
LBO X Post	-0.015***	-0.012***	-0.013***	-0.011**
	(0.004)	(0.004)	(0.004)	(0.004)
Occupation wage gap		0.169***		0.162***
		(0.020)		(0.020)
Age wage gap			0.044***	0.033***
			(0.011)	(0.011)
Panel B: Occupation wage gap				
LBO X Post	-0.019***	-0.017***	-0.016***	-0.015***
	(0.004)	(0.003)	(0.003)	(0.003)
Gender wage gap		0.107***		0.101***
		(0.013)		(0.013)
Age wage gap			0.071***	0.066***
			(0.008)	(0.008)
Panel C: Age wage gap				
LBO X Post	-0.040***	-0.039***	-0.036***	-0.035***
	(0.006)	(0.006)	(0.006)	(0.006)
Gender wage gap		0.085***		0.063***
		(0.021)		(0.021)
Occupation wage gap			0.214***	0.204***
			(0.025)	(0.024)
Firm FE	Yes	Yes	Yes	Yes
Event-time FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
N	526444	526444	526444	526444

Table 7: Employee-Level Analysis: Wage of Employees Staying at the Firm

This table displays the results of employee-level difference-in-differences regressions comparing the evolution of the logarithm of wage of buyout target employees who are employed at the firm in the three years before and after a buyout relative to their matched control group. Employees are matched the year before the buyout on gender, 5-year age bracket, 2-digit occupation code, full-time status, wage bracket (10 categories), firm size bracket (3 categories), and 2-digit industry. The coefficients come from estimating equation 1. The dependent variable is the logarithm of wages. "Mgrs" denotes "Managers". Standard errors are clustered at the firm level. *, **, and *** denote significance at 10%, 5%, and 1% levels.

	ln(Wage)					
	Gender		Occupation		Age	
	Men	Women	Mgrs	Non-mgrs	Older	Younger
	(1)	(2)	(3)	(4)	(5)	(6)
LBO X Post	0.017**	0.004	0.031	0.008	0.004	0.015**
	(0.007)	(0.013)	(0.020)	(0.006)	(0.018)	(0.007)
Employee FE	Yes	Yes	Yes	Yes	Yes	Yes
Event-time FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
N	241479	86681	38759	289401	56007	272153

Table 8: Probability of Employee Separation by Employee Category

This table displays the results of employee-level regressions comparing the probability of separation of employees at a target firm relative to their matched control group. Employees are matched the year before the buyout on gender, 5-year age bracket, 2-digit occupation code, full-time status, wage bracket (10 categories), firm size bracket (3 categories), and 2-digit industry. The dependent variable is a dummy variable equal to one if the employee leaves the firm in the buyout year or in the three years after. "Mgrs" denotes "Managers". Standard errors are clustered at the firm level. *, **, and *** denote significance at 10%, 5%, and 1% levels.

	Leaver					
	Gender		Occupation		Age	
	Men	Women	Mgrs	Non-mgrs	Older	Younger
	(1)	(2)	(3)	(4)	(5)	(6)
LBO	0.056***	0.040**	0.099***	0.037***	0.089***	0.043***
	(0.012)	(0.017)	(0.021)	(0.012)	(0.023)	(0.011)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
N	258646	213537	47799	424384	39097	433086

Table 9: Leavers' Pay

This table displays the results of employee-level regressions comparing the wage of employees who leave the target firm after the buyout to those who stay at the target firm. The dependent variable is the logarithm of wages. Wages are measured in the year before the buyout. "Mgrs" denotes "Managers". Standard errors are clustered at the firm level. *, **, and *** denote significance at 10%, 5%, and 1% levels.

	ln(Wage)					
	Gender		Occupation		Age	
	Men	Women	Mgrs	Non-mgrs	Older	Younger
	(1)	(2)	(3)	(4)	(5)	(6)
Leaver	0.103***	0.012	0.096***	-0.018	0.162***	0.030
	(0.025)	(0.021)	(0.033)	(0.013)	(0.049)	(0.019)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
N	10013	5921	2945	12989	3082	12852

Table 10: Joiners' Pay

This table displays the results of employee-level regressions comparing the wage of employees who join the target firm after the buyout (joiners) to employees who were already employed at the target firm in the year before the buyout (incumbents). The dependent variable is the logarithm of wages. The regression is estimated on employees of the target firm. For incumbents, wages are measured in the year before the buyout. For joiners, wages are measured in the year they join. Standard errors are clustered at the firm level. *, **, and *** denote significance at 10%, 5%, and 1% level.

	ln(Wage)					
	Gender		Occupation		Age	
	Men	Women	Mgrs	Non-mgrs	Older	Younger
	(1)	(2)	(3)	(4)	(5)	(6)
Joiner	-0.133***	-0.125***	-0.123***	-0.116***	0.030	-0.140***
	(0.021)	(0.018)	(0.035)	(0.010)	(0.071)	(0.014)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
N	23389	17865	7251	34502	6201	35552

Table 11: Workforce Composition

This table displays the results of firm-level difference-in-differences regressions comparing the share of men, managers and older employees at buyout target firms relative to the matched control group. Standard errors are clustered at the firm level. *, **, and *** denote significance at 10%, 5%, and 1% levels.

	Fraction of all employees		
	Men	Managers	Older
	(1)	(2)	(3)
LBO X post	-0.001	0.003	-0.011***
	(0.002)	(0.003)	(0.002)
Firm FE	Yes	Yes	Yes
Event-time FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
N	526444	526444	526444

Table 12: Usage of Stock Based Compensation

This table displays statistics on firms' use of stock-based compensation in the ACEMO-PIPA survey, between 2006 and 2012. Column 1 presents the mean across observations for all unlisted respondents with between 20 and 800 employees. Column 2 presents the mean across observations for respondents that have experienced a leveraged buyouts in the last 3 years.

	Mean	
Instrument	Survey	LBO
Purchase of stocks at a preferential price	0.4%	0.3%
Free grant of stocks	0.6%	1.1%
Grant of stock options	0.8%	1.3%
Grant of stocks through company savings plan	0.6%	0.3%

ONLINE APPENDIX
for
"Private Equity and Pay Gaps Inside the Firm"

Table A.1: Investors' Descriptive Statistics

Descriptive statistics of the private equity investors in the sample of LBO transactions.

	Mean	S.D.	p25	p50	p75	Count
Year founded	1992	23	1988	1997	2002	318
No. of professionals in Capital IQ	58.61	148.10	9.00	21.00	55.00	335
Largest fund size (USDm)	873.83	2686.20	36.31	172.37	448.96	161
Typical investment ("bite") size (USDm)	135.57	324.63	9.36	33.00	113.56	235
No. of deals in sample	3.41	4.57	1.00	1.00	3.00	360
Is headquartered outside France	0.33	0.47	0.00	0.00	1.00	358
Is headquartered in UK or US	0.21	0.41	0.00	0.00	0.00	358

Table A.2: Robustness: Firm-Level Wage Gaps

This table displays the results of robustness tests around the main results of Table 5. Panel A does not impose the existence of firms from three years before to three years after the buyout. Panel B includes the buyouts for which we find more than three firms recorded in the administrative data under the target's name at the target's address. The dependent variable is the logarithm of wages in columns 1-2,4-5,7-8 and the relative pay gap between the two preceding categories in columns 3, 6, and 9. "Mgrs" denotes "Managers". Standard errors are clustered at the firm level. *, **, and *** denote significance at 10%, 5%, and 1% levels.

	Gender			Occupation			Age		
	ln(Wage)		Wage gap	ln(Wage)		Wage gap	ln(Wage)		Wage gap
	Men	Women		Mgrs	Non-mgrs		Old	Young	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: No Restriction on Firm Existence before and after the Buyout									
LBO X Post	-0.021***	0.001	-0.015***	-0.050***	-0.002	-0.019***	-0.060***	0.003	-0.042***
	(0.005)	(0.005)	(0.004)	(0.008)	(0.004)	(0.003)	(0.009)	(0.004)	(0.006)
N	692936	692936	692936	692936	692936	692936	692936	692936	692936
Panel B: No Filter on Number of Subsidiaries Matches to Administrative Data									
LBO X Post	-0.022***	-0.000	-0.014***	-0.047***	-0.001	-0.017***	-0.057***	0.003	-0.039***
	(0.005)	(0.005)	(0.004)	(0.008)	(0.004)	(0.003)	(0.009)	(0.004)	(0.006)
N	571206	571206	571206	571206	571206	571206	571206	571206	571206
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Event-time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table A.3: Excluding Top Earners

This table displays the results of firm-level difference-in-differences regressions comparing the evolution of firm wage gaps across employee categories at buyout targets relative to the control group. The coefficients come from estimating equation 1. The dependent variable is the relative wage gap (difference in average wage scaled by the average wage of the high-wage category). Panel A excludes the top earner in the firm. Panel B excludes the top 1% of earners at the firm. Standard errors are clustered at the firm level. *, **, and *** denote significance at 10%, 5%, and 1% levels.

	Wage gap					
	Gender		Occupation		Age	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Excluding highest earner						
LBO X Post	-0.008**	-0.008**	-0.013***	-0.011***	-0.019***	-0.018***
	(0.004)	(0.004)	(0.003)	(0.003)	(0.005)	(0.005)
Occupation wage gap		0.113***				0.126***
		(0.018)				(0.021)
Age wage gap		0.035***		0.051***		
		(0.011)		(0.008)		
Gender wage gap				0.081***		0.061***
				(0.013)		(0.018)
N	525879	493790	495984	493790	523351	493790
Panel B: Excluding the top 1%						
LBO X Post	-0.007**	-0.007**	-0.011***	-0.010***	-0.014***	-0.013***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.005)	(0.005)
Occupation wage gap		0.110***				0.121***
		(0.016)				(0.019)
Age wage gap		0.015		0.050***		
		(0.009)		(0.008)		
Gender wage gap				0.086***		0.028
				(0.013)		(0.017)
N	525805	488403	490827	488403	522991	488403
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Event-time FE	Yes	Yes	Yes	Yes	Yes	Yes

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Table A.4: Robustness: Employee Separation Controlling for Other Categories

This table displays the results of employee-level regressions comparing the probability of separation of employees at a buyout target relative to their control group. The dependent variable is a dummy variable equal to one if the employee leaves the firm in the buyout year or in the three years after. Standard errors are clustered at the firm level. *, **, and *** denote significance at 10%, 5%, and 1% levels.

	Leaver						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
LBO	0.041**	0.037***	0.043***	0.031*	0.033*	0.029**	0.024
	(0.017)	(0.012)	(0.011)	(0.018)	(0.018)	(0.013)	(0.018)
LBO X Men	0.016			0.009	0.015		0.009
	(0.021)			(0.021)	(0.021)		(0.021)
LBO X Managers		0.063***		0.060**		0.061***	0.059**
		(0.024)		(0.023)		(0.023)	(0.023)
LBO X Older			0.046*		0.046*	0.045*	0.045*
			(0.025)		(0.025)	(0.025)	(0.025)
Year X Men	Yes	No	No	Yes	Yes	No	Yes
Year X Managers	No	Yes	No	Yes	No	Yes	Yes
Year X Older	No	No	Yes	No	Yes	Yes	Yes
N	472183	472183	472183	472183	472183	472183	472183

Table A.5: Robustness: Leavers' Wage Controlling for Other Categories

This table displays the results of employee-level regressions comparing the wage of employees who leave the target firm after the buyout to those who stay at the target firm. The dependent variable is the logarithm of wages. Wages are measured in the year before the buyout. Standard errors are clustered at the firm level. *, **, and *** denote significance at 10%, 5%, and 1% levels.

	ln(Wage)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Leaver	0.012	-0.018	0.030	-0.032*	-0.004	-0.041***	-0.039**
	(0.021)	(0.013)	(0.019)	(0.017)	(0.023)	(0.015)	(0.019)
Leaver X Men	0.091***			0.031	0.068**		0.013
	(0.032)			(0.023)	(0.032)		(0.023)
Leaver X Managers		0.114***		0.101***		0.103***	0.093***
		(0.036)		(0.034)		(0.034)	(0.033)
Leaver X Older			0.131**		0.097*	0.086**	0.066*
			(0.052)		(0.050)	(0.034)	(0.034)
Year X Men	Yes	No	No	Yes	Yes	No	Yes
Year X Managers	No	Yes	No	Yes	No	Yes	Yes
Year X Older	No	No	Yes	No	Yes	Yes	Yes
N	15934	15934	15934	15934	15934	15934	15934

Figure A.1: Firm-Level Average Wage by Employee Category at Target and Control Firms

This figure shows the event study plot for the logarithm of average hourly wage of men, women, managers, non-managers, older, and younger employees around a buyout separately for target firms and control firms. The figure is constructed by plotting the coefficients on event-time dummies, in a regression of the dependent variables on these event-time dummies and firm fixed effects. The dotted bars indicate the 95% confidence intervals, based on standard errors clustered at the firm level.

(a) Men

(b) Women

(c) Managers

(d) Non-Managers

(e) Employees Aged over 50 (Older)

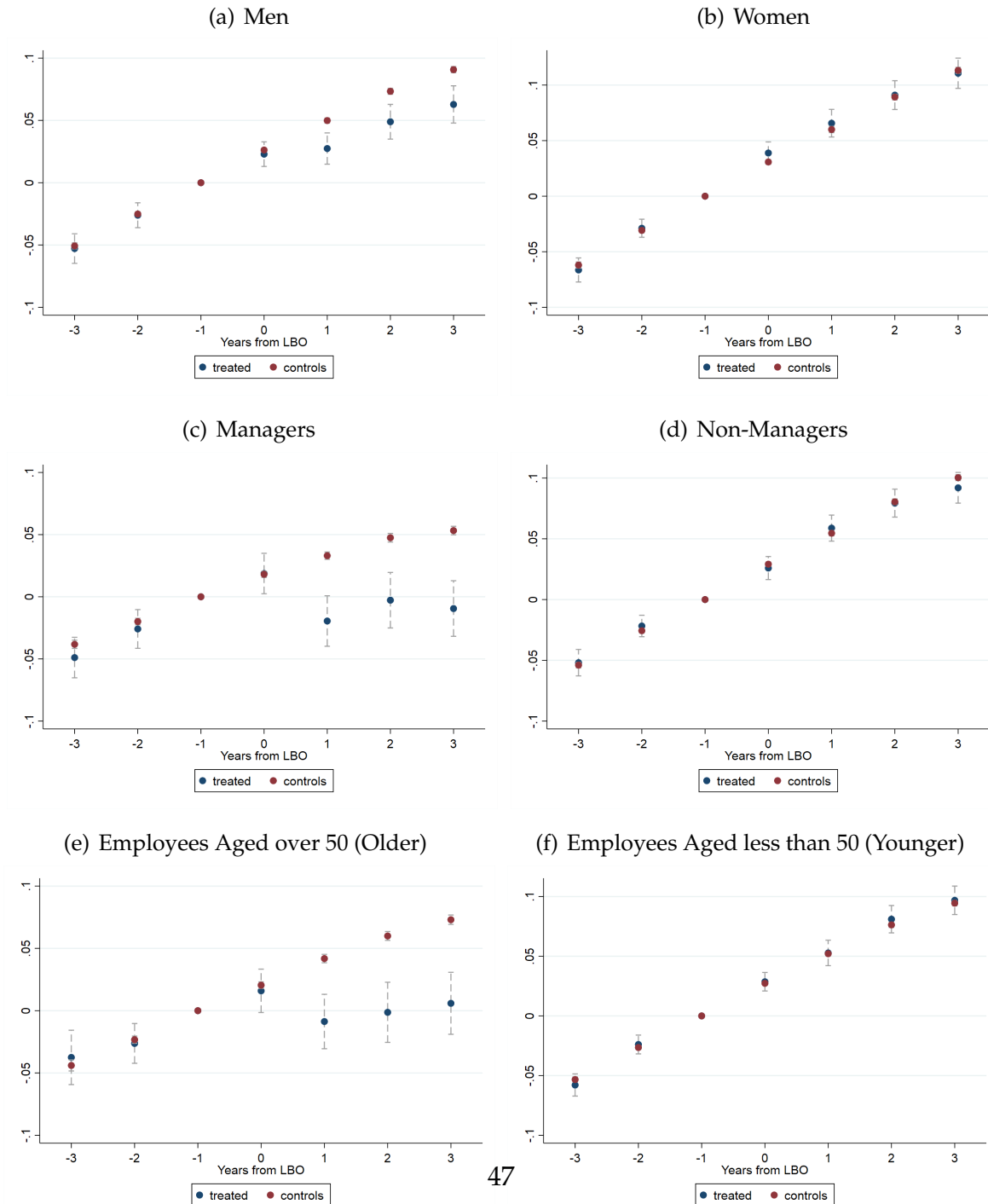(f) Employees Aged less than 50 (Younger)

Figure A.2: Robustness: Firm-Level Average Wage by Employee Category

This figure shows the event study plot for the logarithm of the average wage of men, women, managers, non-managers, older, and younger employees around a buyout at target firms, relative to the control group using the estimator of Borusyak et al. (2021).

(a) Men

(b) Women

(c) Managers

(d) Non-Managers

(e) Employees Aged over 50 (Older)

(f) Employees Aged less than 50 (Younger)