Private Equity and Human Capital Risk

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Abstract

We study the human-capital effects of private equity buyouts in Germany. We conduct matched-sample difference-in-differences estimations at the establishment and at the individual employee level with more than 152,000 buyout employees and a carefully matched control group. Buyouts are followed by a reduction in overall employment and an increase in employee turnover. Employees of buyout targets experience earnings declines equivalent to 2.8% of median earnings in the fifth year after the buyout. Managers and older employees fare far worse after buyouts compared to the average target employee, even though they are not more likely to lose their jobs at the target compared to other employees. We argue that the employees most negatively affected after buyouts are those who are less likely to find new employment, not those who are most likely to lose their jobs. There is evidence for a reduction in administrative staff and more hiring into jobs that require IT skills.

Keywords: Private Equity, Restructuring, Human Capital Risk, Buyouts, Wages

JEL Classifications: G30, G34, J24, J31, M51

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1 Introduction

In this paper, we analyze the human capital risk associated with private equity buyouts in Germany.[1] The social costs associated with private equity restructuring have been the subject of emotional debates. The head of the German Social Democratic Party once compared buyout firms to "swarms of locusts" who "descend on companies, graze, and then move on," suggesting that private equity firms make short-term profits by imposing large costs on employees. Public discussions in other European countries and the US reflect similar concerns.[2]

The literature in finance and economics has conventionally regarded private equity buyouts as vehicles for improving firms' governance and operating performance, facilitating growth and creative destruction, and, more recently, modernizing firms' technology.[3] From this *modernization* perspective, private equity buyouts create value by creating leaner firms and enhancing growth through organizational, operational, and technological improvements. By contrast, critics argue that shareholders gain in private equity buyouts at the expense of other stakeholders, in particular, the government through lower taxes, and employees. This *transfer-of-wealth* view echoes the critical stance articulated in the public debate. Shleifer and Summers (1988) provide a theoretical foundation for this view and suggest investor-led restructurings may not create value, but simply transfer wealth from employees and other stakeholders to shareholders by reneging on implicit contracts.

We contribute to this debate by analyzing 511 private equity buyouts in Germany be-

[1] The literature conventionally refers to LBOs, whereas our study is on private equity buyouts, abbreviated as PE buyouts in the main body of the paper. We discuss this distinction in Section 2.1.

[2] Bild am Sonntag, April 17, 2005 (see also http://de.wikipedia.org/wiki/Heuschreckendebatte). Discussions in other countries created similar sentiments. Davis et al. (2014) cite a closely related argument by then Prime Minister of Denmark Poul Rasmussen (see Grace Wong, "Private Equity and the Jobs Cut Myth", CNNMoney.com, May 2, 2007 at http://money.cnn.com/2007/05/02/markets/pe_jobs/index.htm). The same arguments about private equity firms were rehearsed again in the 2012 US presidential campaign when Democratic politicians chastised Republican candidate Mitt Romney for his career at Bain Capital, blaming him for socially irresponsible restructuring methods. See Jacob Weisberg, "The Pain in Bain," Slate, July 17, 2012. The International Trade Union Confederation made similar statements (ITUC, 2007).

[3] The following papers articulate these views. Operating performance: Jensen (1989), Kaplan and Stromberg (2009); facilitating growth: Boucly et al. (2011); catalyzing creative destruction: Davis et al. (2014); modernizing technology: Agrawal and Tambe (2016), Olsson and Tåg (2017).

tween 2002 and 2008. Germany is fairly representative for the OECD regarding employment protection legislation, making it a well-suited laboratory for studying this question.[4] We perform matched-sample difference-in-differences analyses at the establishment level and the individual level. We first match each target establishment to multiple control establishments and then we match each target employee to another employee from one of the matching control establishments. Matching at both levels is performed based on a rich set of establishment, job, and employee characteristics. We conduct analyses at the establishment level and the individual level over a five-year period after the buyout.

We ask two questions. First, we ask how job growth, separations, and hiring at the establishment level develop after buyouts. Second, we investigate if buyouts are associated with human capital risk for the employees of target firms by conducting matched-sample difference-in-differences analyses of total earnings, wages, and employment of individual employees. We ask both questions for all employees in our sample and for groups of employees who may be either particularly vulnerable to or who may benefit from restructuring. The two questions we ask are related but distinct. PE firms may increase employee turnover without reducing overall establishment-level employment, and some of the employees who are replaced and lose their jobs with the target may not find new employment. We find this to be the case for older workers, who lose their jobs at target establishments at almost exactly the same rate as younger workers, but experience significantly larger losses of long-term employment and wages. Hence, it is important to distinguish firm-level decisions and individual outcomes, because some groups, e.g., low-paid workers, seem to find new employment easily, whereas others, such as older workers, often remain unemployed.

Buyout establishments reduce their employment by 8.96% more compared to the control group in the period up to five years after the buyout. This effect can be decomposed into an

[4] Our assessment is based on the EPL (employment protection legislation) index published by Allard (2005) and constructed by the OECD, which was also used by Simintzi et al. (2014). In 2003, the last year reported by Allard (2005) and the second year of our sample, Germany ranks 12th in terms of the strictness of employment protection among 21 OECD countries with an index value of 2.1, which is also the mean. Other countries with studies on the employment implications of buyouts include the US (index value: 0.6; rank 21), the UK (index value: 1.4; rank: 15) and Sweden (index value: 2.7; rank: 5).

increase in the separation rate of 18.75% and an increase in the hiring rate of 9.79%. Hence, about half of the increase in departures from buyout targets results in replacements and the other half in job destruction. The investigation of deal-level growth, separation, and hiring rates shows that there is a strong and positive correlation between hiring rates and separation rates and almost half of the buyouts are followed by a period of increased employee turnover. Moreover, we often find higher separation rates *and* higher hiring rates for the same groups of employees. Hence, private equity firms restructure firms by reducing employment and by replacing employees; in our sample, they employ both strategies at about the same rate. The increase in hiring is largely concentrated in the first years after the buyout, whereas most of the separations happen in later years. We may observe separations later because buyout firms want to increase profitability towards the end of their investment horizon to achieve better sales prices. Alternatively, the evaluation of targets' operations and the implementation of restructuring strategies may simply take time. At the individual level, we find a downward trend in employee earnings after private equity buyouts. The average buyout target employee loses €980 in annual earnings after five years compared to the matched control group, which is 2.8% of median earnings in our sample.

The individual-level analyses identify three groups of employees whose post-buyout losses are significantly larger than those of the average buyout employee: White-collar workers, managers, and older employees. Our discussion of employee groups is guided by three sets of explanations of buyout-related changes in employment and wages: (1) organizational streamlining; (2) technological modernization; and (3) transfers of wealth. We begin with organizational streamlining, i.e. the notion that buyout investors reduce administrative staff and layers of management. White-collar workers experience higher separation rates with less replacement in the short-term and significantly higher losses of employment and earnings compared to other employees, consistent with the notion that buyout investors streamline firms by reducing administrative staff. For managers, we find very strong results at the individual level, but not at the establishment level, which suggests that buyout firms do not systematically reduce layers of middle management. Hence, we attribute the adverse development for managers' to their difficulties in finding new employment rather than the human-resource policies of buyout investors.

Next, we turn to the argument that buyouts foster technological modernization. Private equity firms may implement new technologies, either because target managers resist change, or because private-equity investors have additional technological expertise. As a result, buyout targets may undergo faster technological modernization than control firms. We are careful to distinguish different notions of technological change, each of which has specific and sometimes different implications for employees. Proponents of the skill-biased technological change hypothesis (Katz and Autor, 1999, Autor et al., 2003) argue that technological change is biased against lower-skilled jobs and increases wage inequality. Separation rates for low-wage workers are indeed almost twice as high as those for the sample as a whole. However, they are not displaced by those with higher wage levels, but by other low-wage employees. The net rate of job growth for low-wage workers is not unusually low, whereas turnover is unusually high. Individual-level results even suggest that low-wage employees lose less after buyouts than other employees, suggesting that skill-biased technological change does not determine individual outcomes.

According to a more recent version of the technological-modernization argument, it is medium-skilled workers who may lose out towards either high-skilled or low-skilled workers through the displacement of routine jobs as a result of investments in information technology and robots ("routinization"), or through the reorganization of supply chains and trade ("offshoring"). We investigate these hypotheses at the individual and at the establishment level by looking at a range of technology-related job and employee classifications and find no evidence to support these hypotheses in our sample. Closely related is the argument that technological trends favor groups who have skills complementary to new technologies, such as IT skills; we find some evidence that employment in jobs that require stronger IT skills increases in the first two years after buyouts.[5]

Finally, we investigate if buyouts involve a transfer of wealth in which the new owners gain at the expense of buyout target employees. We distinguish two versions of the transfer-of-wealth argument, both of which rely on implicit-contract theory. The first version holds that optimal risk-sharing between employees and firms involves that firms offer employees employment insurance (e.g., Azariadis, 1975) and that dynamic wage profiles rise over time, providing quasi-rents for older workers (Harris and Holmstrom, 1982), which buyout investors may appropriate. The second version of the transfer-of-wealth argument holds that new owners benefit at the expense of employees by taking advantage of employees' lock-in from firm-specific human capital. As remarked above, the separation rates between older and younger employees do not differ, and the separation rates for employees with higher tenure, our measure of firm-specific human capital, are in fact lower than those for employees with lower tenure. Hence, we find no support for either version of the transfer-of-wealth argument from these as well as other analyses reported in the main part of the paper. The finding on tenure is better explained by insider-outsider theories that postulate the entrenchment of insiders and more job security for employees with a longer tenure on their jobs (Lindbeck and Snower, 1986, 1988). Nonetheless, we document a large long-term decline in earnings for older employees, the only group for which we observe a significantly negative effect on daily wages. These observations suggest that older employees suffer from the increase in employee turnover because they are less successful in finding new employment, and sometimes have to accept employment for lower pay.

Several theories we investigate, in particular explanations related to organizational streamlining and technological change, build on the notion that buyout investors change the composition of the workforce of buyout targets. However, apart from the observations on white-collar workers and jobs with IT requirements mentioned above, there is no support for explanations related to the composition of the workforce. Instead, we document declining

[5]See Autor et al. (2003) and Autor and Dorn (2013); on buyouts see Agrawal and Tambe (2016) and Olsson and Tåg (2017).

employment and increased employee turnover for most groups of employees, which is broadly consistent with the modernization perspective on private equity buyouts. Increased turnover has long-term negative consequences for those employees who find it more difficult to find new employment, probably because the new owners after the buyout identify lower-ability employees and their departures from the buyout target provide a negative signal to the labor market.

Prior work on the human capital consequences of buyouts studies employment and wage effects mostly at the firm level or at the establishment level, and we discuss these papers more thoroughly in the main part of the paper.[6] Three recent contributions are close to ours in terms of data and methodology. Davis et al. (2014) are unique in combining firm-level and establishment-level analyses, whereas all other papers focus on only one level of analysis. Our analysis complements theirs by combining individual and establishment-level analyses. Two contributions to the buyout literature are based on individual-level data. Olsson and Tåg (2017) analyze individual-level employment data for private-equity buyouts in Sweden. They find strong evidence for labor-market polarization, which contrasts with our results, most likely because the economic environment and labor market regulation in Sweden are different from that in Germany. Agrawal and Tambe (2016) use an individual-level data set obtained from an online job-search platform in the US. They argue that buyouts increase IT-related investments, which enhance workers' human capital and increase firms' likelihood of survival. We differ from Agrawal and Tambe (2016) in terms of methodology, data sources, and results. Our analysis includes a broader set of variables and covers aspects of modernization other than IT-related investments. By relying on an online job-search platform, their analysis may not reflect the negative impact of buyouts on workers who do not use such platforms.

Our paper also contributes to the larger literature on finance and labor, which is too large to present and discuss here. In particular, we contribute to the part of the finance and

[6] A non-exhaustive list of papers on the employment consequences of buyouts is: Kaplan (1989), Lichtenberg and Siegel (1990), Wright et al. (1992), Amess and Wright (2007), and Boucly et al. (2011). The surveys by Kaplan and Stromberg (2009), Wright et al. (2009), and Eckbo and Thorburn (2013) list additional contributions.

labor literature that investigates how corporate finance decisions and events affect employees. Other parts of this literature investigate the implications of mergers and acquisitions (Tate and Yang, 2016, Lee et al., 2017), bankruptcies (Brown and Matsa, 2016; Graham et al., 2013), and capital-structure choices (Matsa, 2010; Agrawal and Matsa, 2013). The buyout context differs from mergers and acquisitions, since it does not involve a reallocation of employees between acquirer and target, and from bankruptcies, since the buyouts in our sample do not seem to be in financial difficulties. We do not analyze leverage; the two studies on capital structure mentioned above analyze questions entirely different from ours.

2 Data and methodology

In this section, we describe the construction of the sample (Section 2.1), the matching process (Section 2.2), and descriptive statistics (Section 2.3).

2.1 Sample construction

The analysis requires linking three separate data sets: A data set containing private-equity backed majority acquisitions, a data set on establishments (Establishment History Panel, BHP), and a data set containing the employment history of individuals (Integrated Employment Biographies, IEB). The administrative establishment and employment history data are provided by the Institute for Employment Research (IAB) in Nuremberg. The data of the IAB are not organized in terms of legal units such as companies, but in terms of establishments, defined by their physical location.

We collect data on 891 German private equity buyouts for the period 2002 to 2008 by integrating the transactions reported in Thomson One, Capital IQ, and a proprietary data set of the Bundesverband für Kapitalanlagegeselleschaften (BVK) into one data set. We include all deals in which a private equity investor acquires a majority stake in a firm. In the following, we use the term “private equity buyout,” and abbreviate it as “PE buyout” and sometimes just as "buyout" to avoid repetition. The data set starts in 2002, because coverage of PE buyouts for earlier years is very low in all three databases. We exclude secondary buyouts as well as transactions after 2008, because we want to observe the performance over the subsequent five years, and individual employment history data are only available until 2013. This leaves us with a list of 798 transactions. Table A-1 in the Online Appendix provides an overview of the steps involved in constructing the sample.

We hand-collect the subsidiary structure of buyout targets provided by *Hoppenstedt's Firmendatenbank*. The IAB then employs record linkage techniques (for details see the Appendix) to link parent companies and majority-owned subsidiaries to their establishments in the Establishment History Panel (BHP, see Schmucker et al., 2016). After this step, we are left with 544 transactions and 2,652 establishments. For those 544 transactions, we select all employees for whom we have sufficient information on all control or matching variables on both the employee and the establishment level over the eleven-year period we require. Our employee data come from the Integrated Employment Biographies (IEB) of the IAB.[7] Next, we delete all transactions for which we find less than 10 employees, since companies with fewer than 10 employees enjoy privileges in terms of labor protection laws; excluding these deals is inconsequential for our results. These steps leave us with 513 transactions, 2,563 establishments, and 208,449 employees. In the final step, we construct matched samples on both the establishment level and the individual level. We eventually end up with 511 transactions and 2,420 target establishments, and 152,057 target employees.

We collect some additional information on target firms. This information is limited, since disclosure regulation for private firms was not enforced before 2007 and standard financial data are not available for most of our target firms for most of our sample period. Therefore, we match the target firms to data that were collected by *Creditreform*, a company specialized on debt collection, and provided to us by the *Centre for European Economic Research* (ZEW).

[7] For an overview on all control and matching variables, see Table 1 and Table A-2 in the Online Appendix. The IEB contain detailed longitudinal data on almost the entire German workforce. We provide details on the sources of the IEB and our data preparation in Section A.2 of the Appendix.

We can match close to half of our sample and provide the results in Table A-3 in the Online Appendix. Creditreform provides credit scores in four levels from “very good” to “very critical,” and 216, or 93% percent of firms for which credit scores are available, have a credit score of “good” or “very good.” *Creditreform* asks companies about their business outlook and rates business outlooks on a scale with twelve verbal descriptions, which we aggregate into five scores from best (“expanding”) to worst (“declining”). Only 15 or 6.4% of the 233 companies for which data were available in the event year described their business outlook as “declining” or “stagnating” in the event year; 42 (18%) did not respond to this question. Hence, based on their credit ratings and descriptions of their business outlook, most target companies appear to be financially healthy; only about 6%-7% of the firms for which we have data seem to be declining or in a critical situation.

German labor regulation provides employees with significant representation on the supervisory boards of corporations. Specifically, corporations with more than 500 employees in Germany are required to have at least one-third of the members of the supervisory board elected by employees, whereas for firms with more than 2,000 employees, half of the seats of the supervisory board are reserved for employee representatives. The firms in our sample are mostly below these thresholds. Hiring and separation rates of establishments do not differ depending on the level of employee representation on the board and we do not follow up on this categorization. (See Table A-4 in the Online Appendix.)

2.2 Constructing matched samples

We perform a two-stage matching process in which we first match target establishments to control establishment and then draw control employees from a set of control establishments.

2.2.1 Matching establishments

For each target establishments, we identify 50 potential control establishments using the BHP and the following criteria. First, we remove all establishments from the BHP that have been targets themselves at any time during the sample period.[8] Second, we build matching cells based on two-digit industry affiliation (60 categories), establishment size deciles, establishment age classes (10 classes: 0-2, 3-5, 6-10, 11-15, 16-20, 21-25, and more than 25 years), and the buyout year (7 calendar years). This step is closely modeled on the process used by Davis et al. (2014) and results in 29,400 cells, of which 1,185 are filled after matching. Next, we pick the 50 nearest neighbors in terms of the Euclidean distance based on establishment size, establishment age, median establishment daily wage; and the shares of, respectively, medium-qualified, highly-qualified, full-time, and female employees; and the average age of all employees.

For each target establishment, we identify the ten closest establishments out of the 50 potential control establishments based on the normalized Euclidean distance computed over establishment size, establishment age, mean establishment *Daily Wage*, the shares of, respectively, medium-qualified, highly-qualified, full-time employees, and female employees, and the average age of all employees. We match with replacement, i.e., a control establishment may be matched to more than one target establishment. Our final establishment data set includes 2,420 target establishments and 24,147 control establishments. We find at least six matches for each target establishment.

2.2.2 Matching employees

In the final step, we form a control group of matching employees. For each employee from the buyout group, we select a matching employee from one of the matched control establishments identified in the previous step. To base our matching on characteristics that have not been affected by the buyout, we match on characteristics recorded in the year before the buyout announcement: We match individuals exactly in terms of education, employment status, experience, gender, industry, nationality, occupation, qualification, and geographic location

[8] We explored an alternative matching algorithm, in which we allow establishments to become controls as long as they have not been part of a buyout transaction in the five years before the matching year. The changes for the control sample would be negligible and affect at most 0.4% of the control establishments. We did not pursue this line of analysis further since the scope for look-ahead bias seems to be negligible.

(region) (cf. Table A-2 in the Online Appendix for a detailed overview). Next, we remove individuals for whom the absolute deviation from the target employee in terms of *Earnings*, *Age*, or *Tenure* is larger than 25%, the absolute deviation in *Establishment Size* from the target employee is larger than 50%, and the absolute deviation of *Days Employed* from the target employee is larger than 45 days.[9] Finally, we pick the nearest neighbor based on the normalized Euclidean distance of the numerical variables mentioned above.

We match with replacement, i.e., we allow for a control employee to be matched to more than one target employee. The final individual-level data set includes 152,057 target employees; hence, we can match 74% of all target employees based on our criteria. The number of control employees is equal to 130,553, which is smaller than the number of target employees because of matching with replacement.

2.2.3 Matching success

We match individuals exactly on the nine categorical variables listed in the previous section. For the five numerical variables, the relative differences between the target group and the control group are low or very low.[10] We use the normalized differences proposed by Imbens and Wooldridge (2009) and used by Imbens and Rubin (2015) to examine significant differences between two groups of observations. Imbens and Wooldridge (2009) recommend that normalized differences be below 0.25 in absolute value. We record a test statistic of 0.13 for the fraction of full-time employees in Table A-6. For all other matching variables on both the establishment level and the individual level, the test statistic is never higher than 0.06, and we conclude that our control groups match target establishments and target employees very closely on all relevant criteria.

The differences between matched and unmatched buyout employees are substantial and

[9] We chose 45 days or one-eighth of the fraction of the year in which an employee is employed, i.e. $(Days\ Employed/number\ of\ days\ in\ calendar\ year) \times 0.125$.

[10] See Tables A-5 and A-6 in the Online Appendix for matching statistics for the numerical variables for the target establishments, the matched buyout employees, the unmatched buyout employees, and the control employees.

largely the result of industry clustering of transactions, on which we comment further in the next section. We have greater difficulty with matching part-time employees and those without vocational training. Hence, our analysis does not include these, arguably more vulnerable, groups of employees. Consequently, annual income and tenure are both substantially lower in the unmatched employee sample than in the matched employee sample.

2.3 Descriptive statistics

Table 2 presents descriptive statistics on the numerical variables. Our data set consists of 511 deals with 425 employees on average. Two-year pre-buyout employment growth is 13.36% on average, which shows that our sample is not dominated by restructuring buyouts. We observe each target establishment and each target individual over time from five years before the buyout to five years after the buyout. Our final data sets are panels of 185,969 establishment years and about 3.35 million individual years. The average employee is 42 years old and has held his or her current job for almost 9.5 years. A very small number of individuals enter our data set when they are still below working age because we track individuals starting five years before the buyout.

Table 3 describes the composition of the individual-level sample with respect to qualification, gender, nationality, occupation, and education, separately for control employees and employees of PE buyout targets. The fourth column shows the composition of the whole labor force based on IAB data.[11] PE buyouts target mostly manufacturing companies, which are overrepresented in the buyout sample (66.3% of employees) relative to the economy in general (25.7%). This bias reflects PE investors' tendency to acquire firms in manufacturing and the larger size of manufacturing targets; see also Table A-3 in the Online Appendix on the industry composition of the deals in our sample. The higher weight of manufacturing in the sample characterizes all differences between the composition of the buyout sample and the German labor force. About a quarter of employees are grouped into the lowest occupational

[11] The composition of the labor force is based on 2004 data, which is half way between the first year (2001) and the last year (2007) of the sample we use for matching.

group of *simple manual occupations*, whereas only 17% of the general labor force belongs to this group. Managers constitute only 3.1% of the whole sample, in line with the general labor force. Women have a share of 24.4% in the sample, much less than the proportion of women in the labor force (46.1%). The PE buyout sample is biased towards employees with an intermediate school leaving certificate with vocational training (69%) compared to the German labor force (59%); towards the south of Germany (49% vs. 38%); and towards full-time workers (89% vs 59%), a consequence of comparatively poor matching of part-time employees.

3 Employment and wages after private equity buyouts

This section analyzes the development of employment and wages after PE buyouts at the establishment level (Section 3.1) and at the individual level (Section 3.2).

3.1 Establishment-level analysis

We define the growth rate of employment from time t to time $t+k$ as $g_{j,t,t+k} = \frac{E_{j,t+k} - E_{jt}}{0.5\left(E_{j,t+k} + E_{jt}\right)}$, where E_{jt} denotes the level of employment in establishment j at time t.[12] Subscript t refers to points in time for stock measures (employment) and to periods for flow measures (e.g., separations). Precise definitions of all variables can be found in Table 1. We regress one-year and multi-year growth rates of employment on a buyout-target indicator, the two-year pre-buyout growth rate, and a set of fixed effects:

$$g_{j,t-1+k,t+k} = \alpha_t + \sum_c D_{cj}\delta_c + \lambda g_{j,t-3,t-1} + \theta_k \times Target_j + \varepsilon_{j,t+k},\ k = 0, ..., 5, \qquad (1)$$

where D_{cj} is a set of dummy variables for cell c for establishment j, in which cells are defined by the full cross product of buyout year, industry, firm size category, and firm age category (see Section 2). In (1), $Target_{jt}$ is a dummy variable equal to 1 for target establishments in

[12] This definition and our regression approach are modeled on Davis et al. (2014).

all sample years. We follow Davis et al. (2014) and control for past employment growth using $g_{j,t-3,t-1}$, even in regressions in which the dependent variable is not employment growth.

All establishment-level regressions are weighted, with weights proportional to employment to give larger establishments a higher weight.[13] Throughout the paper, we report t-statistics and significance levels based on standard errors clustered at the firm level (see Petersen, 2009; Abadie et al., 2017). We discuss some of the issues related to firm-level clustering and deal-level clustering in Appendix A.1, where we also discuss further robustness checks.

We are interested in decomposing establishment-level employment growth after PE buyouts into separations and new hires. Hence, let H_{jt} (S_{jt}) be the number of employees who enter (leave) establishment j in period t, and denote the normalized flow of newly-hired employees by $h_{jt} = \frac{H_{jt}}{0.5(E_{jt}+E_{j,t-1})}$, and analogously for the separation rate s_{jt}. With these definitions, $g_{j,t-1,t} = h_{jt} - s_{jt}$. (See Appendix A.3 for further details.) We estimate equation (1) with, respectively, $g_{j,t-1,t}$, h_{jt}, and s_{jt} as dependent variables, but with the same set of controls and independent variables as in equation (1). The coefficients of interest are the difference-in-differences estimates of $\theta_k(g)$ ($\theta_k(h)$, $\theta_k(s)$), which measure by how much the employment growth rate (hiring rate, separation rate) for buyout establishments exceeds that of matching control establishments. With the definitions and provisions above, the coefficients have to add up such that $\theta_k(g) = \theta_k(h) - \theta_k(s)$, i.e., the establishment growth rate equals the difference between hiring rate and separation rate.

We caution the reader to be careful with causal interpretations of these coefficients. While we take great care with our matching algorithm (see Section 2.2.3), matching relies on observables. We cannot measure output, labor productivity, the quality of management, or other characteristics of the workforce that may be relevant for buyers in private equity buyouts, and may give rise to selection effects.

The results of this analysis can be found in Table 4. The top part of the table shows the

[13] We divide our sample into three subsamples based on deal size and repeat the analysis in Table A-4 in the Online Appendix, which shows that our results are not driven by a small number of deals with very large establishments.

results for regressions with one-year employment growth rates, annual separation rates, and annual hiring rates for the event year t and each of the subsequent five years after the buyout. We observe a long-term, cumulative establishment-level employment decline between periods t and $t+5$ of 8.96%.[14] Kaplan (1989) finds industry-adjusted employment losses at buyout targets of 6.2% to 12.0% for an earlier sample. For the UK, Wright et al. (1992) report employment losses for buyouts of 6.3% with a subsequent recovery. Lichtenberg and Siegel (1990) find an 8.5% decline over three years, whereas Davis et al. (2014) find only 2.6% for a comparable period. Overall, the large literature on this topic - Wright et al. (2009) review 17 papers on employment effects - tends to find comparable long-term effects of LBOs, albeit with significant variation across studies.

From Table 4, the net cumulative employment decline of 8.96% can be decomposed into an increase in separations associated with PE buyouts of 18.75% and a 9.79% increase in the hiring rate. Hence, similar to Davis et al. (2014), we observe that PE buyouts are associated with a simultaneous increase in the lay-off rate *and* the hiring rate, a process they describe as creative destruction. Table 4 shows the ratio of the coefficients $\theta(h)/\theta(s)$. Over the five-year period after the buyout, about half of the buyout-related separations are replaced by new hires, whereas the other half of the jobs are lost permanently.

To further explore the pattern of separations and hiring we re-run equation (1) for rates from the event year to year $t+5$ separately for each deal in our sample and obtain 511 estimates of $\theta_f(h)$ and $\theta_f(s)$. We plot $\theta_f(s)$ against $\theta_f(h)$ in Figure 1. The cross-sectional correlation between $\theta_f(h)$ and $\theta_f(s)$ is 48.7%. Hence, post-buyout separation rates and hiring rates, each calculated relative to a control group, tend to be strongly positively correlated, and about half of the deals are followed by increased separation *and* hiring rates.

It is instructive to look at the time-series patterns of hiring, job losses, and employment decline, which reveals a phase-shift in this development. In the event year and the

[14] Our results correspond to what they describe as a semi-parametric regression with homogeneous treatment effects across the cells defined in the matching process (see Section 2.2.1). In unreported results we also reproduce their non-parametric specification, but the results are not much different from the semi-parametric results, neither in their case nor in ours and are, therefore, not reported.

subsequent two years, the buyout-related cumulative separation rate is low at 5.94%, and the corresponding cumulative hiring rate is high (4.07%). Hence, 68% of buyout-related separations are replaced, as measured by the ratio $\theta(h)/\theta(s)$, resulting in a small cumulative employment decline of 1.86%. If we cumulate the rates for years 3 to 5 after the transaction, we can observe how this pattern changes: the buyout-related cumulative separation rate increases to 10.61% (=4.22%+3.41%+2.98%) and the cumulative hiring rate decreases to 3.35% (=0.69%+1.54%+1.12%); the replacement ratio $\theta(h)/\theta(s)$ drops to about 0.3 (=3.35%/10.61%), resulting in a more pronounced employment decline of about 1.81%. Hence, the years in the immediate aftermath of the transaction could be characterized as years of creative destruction, with comparable increases in the separation rate and the hiring rate, whereas later years seem to be characterized more by streamlining, associated with more job losses and less replacement. We conjecture that PE investors emphasize streamlining and cost-cutting in later years since their investments have a finite time horizon. As they approach the time when they want to resell target companies or take them public, cost-cutting may become more important. Alternatively, PE investors may take time to evaluate operations in a newly-acquired firm and to implement reorganization measures, which may also explain why separations do not happen immediately after buyouts.

Finally, column (5) of Table 4 reports the same regression results with the growth of total earnings as the dependent variable. Total earnings of an establishment are defined as the sum of income earned in this establishment for all employees who have been employed at that establishment at the end of the calendar year. The post-buyout development for earnings growth and employment growth would differ between target and control establishments if PE firms would systematically replace high-earning employees with employees who earn less to cut costs, or if they would do the opposite, e.g., to attract more qualified employees. However, the development of establishment-level earnings growth mirrors that of employment growth, suggesting that there is no systematic bias towards hiring or laying off better-paid employees.

Panel B of Table 4 reports a sample split of the establishments into public versus private targets. The split according to public status shows that the coefficients $\theta(s)$ on separation rates in the two subsamples are virtually identical, but those on hiring rates $\theta(h)$ are much higher for private targets than for public targets. Consequently, employment growth is lower for public targets than for private targets, but on average it is negative for both groups and the difference is statistically not significant.[15] These results stand in contrast to Davis et al. (2014), who observe positive growth for private targets. Note that their results are at the firm level and include the employment effects of starting new establishments.[16]

3.2 Individual-level analyses

Our approach for the individual-level analysis builds on Jacobson et al. (1993) and Couch and Placzek (2010), who use panel regressions with fixed effects and matching estimators in a program evaluation context. We define three main outcome variables Y_{it}:

- **Earnings:** The employee's earnings summed up over all employment spells in a given year.
- **Daily Wage:** *Earnings* of employee i in year t, divided by the number of days employed during that year. *Daily Wage* is set to missing if the employee or the employee's match was unemployed during the whole year t.[17]
- **Days Employed:** The number of days in year t during which employee i was employed.

Our analysis relies on matched-sample difference-in-differences regressions:

$$Y_{it} = \alpha_i + \gamma_t + \sum_{k=-5}^{k=+5} \delta_k D_{ik} + Target_i \times \sum_{k=-5}^{k=+5} \theta_k D_{ik} + \varepsilon_{ik}. \tag{2}$$

[15]Note also that the aggregate effect is now positive and that these data are available only for about 40'% of the sample.

[16]In Table A-7 in the Online Appendix, we perform an individual-level analysis to investigate if employees of public targets fare differently after buyouts, but cannot find any significant differences.

[17]We cannot calculate hourly wages, because our data does not report the number of hours worked per day or per week. According to Table 3, 6% of our sample are part-time employees for whom *Daily Wage* will be lower than a full-time equivalent daily wage.

In (2), Y_{it} denotes the outcome variable in levels (*Earnings, Daily Wage*, *Days Employed* or their logarithms), X_{it} is a vector of control variables, α_i and γ_t are, respectively, individual and calendar-year fixed effects, i indexes individuals, t indexes calendar time, and k indexes event time. In all cases, when we refer to the logarithm of a variable Y, we use the transformation $ln\,(1+Y)$.[18] The event time dummy variables D_{ik} begin five years before the buyout ($k = -5$) and end five years after the buyout ($k = +5$). Our data cover all individuals from five years before to five years after the event and the dummies for the year before the event ($k = -1$) are omitted; hence, all event-time effects are measured relative to the year before the buyout. The dummy variable $Target_i$ distinguishes employees of PE buyout targets from employees in the matched sample ("controls") and equals one for target employees in all sample years. Clustering of standard errors is again at the firm level (see Appendix A.1 for further details).

The approach in (2) generalizes standard difference-in-differences estimators by adding a temporal dimension to the standard dummy variable $POST$, which would assume a value of one in the post-buyout period. Specification 2 differs from Jacobson et al. (1993) and Couch and Placzek (2010) by entering the event time dummies D_{ik} in addition to the calendar time effects γ_t. PE buyouts happen at different dates in calendar time, so the event-year dummies are not collinear with calendar-year effects (see Boucly et al., 2011). The parameters of interest are the coefficients θ_k on the interactions $D_{ik} \times Target_i$, which measure the average difference between target employees and control employees for the outcome variable Y_{it} in event-year k. By contrast, the coefficients δ_k measure the average differences in event time, after controlling for calendar time effects. As in the case of the establishment-level analysis, we are careful with causal interpretations, because we cannot exclude selection effects and that unobservable factors influence wages, employment, and also buyout decisions.

We demonstrate that our data do not violate the parallel-trends condition and show the

[18]This transformation is commonly applied, but not necessarily without problems if Y is small relative to one. See Burbidge et al. (1988) and Pence (2006) for further discussion. Since the values of all our variables Y are orders of magnitude larger than one, the resulting approximation error is very small.

trends before the event graphically for *Earnings*, *Daily Wage,* and *Days Employed* in Figures 2 to 4. The figures provide us with a first look at the individual-level data by showing the post-event trends as well. For all three variables, we can see almost-perfect parallel developments from $t-5$ to $t-1$; *Daily Wage* for both groups grows at a rate of about 2.4% per year. *Days Employed* trends upward for employees in the target and control groups from $t-5$ to $t-1$, peaking at 357. The inverted-V pattern is a mechanical consequence of the requirement that employees in both groups have to be employed in the event year, but not necessarily before or after the event year.[19]

Earnings. We begin with an analysis of the impact of PE buyouts on *Earnings*, defined as labor income summed across all employment spells of an employee in a given calendar year. Figure 5 plots the coefficients θ_k on the interaction $D_{ik} \times Target_i$ from equation (2) without controls except for person and calendar-year fixed effects. We tabulate the coefficients on $D_k \times Target$ in Table A-8 in the Online Appendix. Panel A of Figure 5 reports results in Euros and the number of days and Panel B reports results in log points. *Earnings* decline steadily by 24 log points over the six-year period from the beginning of the event year to the end of the fifth year after the event, to which we refer as the long term. The decline is €979 of annual income, which corresponds to 2.8% of the median wage for all target employees in the sample. The change in *Earnings* is very skewed, giving rise to more extreme estimates in terms of log points.

Lichtenberg and Siegel (1990) found that average annual compensation per production worker increases by 3.6% in the second year after the buyout, whereas non-production worker compensation falls by 5.2%. Amess and Wright (2007) show that in their sample of MBOs and MBIs, buyouts have a 0.53% lower growth of income per worker compared to other firms. Davis et al. (2014) also find reductions in annual earnings per worker. These studies look at the development of employees' total annual earnings for up to two years after the buyout.

[19] See Figure 3A in Davis et al., 2014 for a similar effect.

Wages and employment. We decompose the development of *Earnings* into a wage component and an employment component by studying the effects on *Daily Wage* (dashed line) and *Days Employed* (broken line). There is no measurable association of PE buyouts with *Daily Wage*, with a long-term decline of €0.32 per day (0.66 log points) relative to a median of €99.29. Earlier studies on the employment effects of LBOs either do not analyze wages, or look at annual earnings per worker, which corresponds to our definition of *Earnings* (see Wright et al., 2009 for a more comprehensive survey and the Introduction for a discussion of this literature). Employment of target employees declines by 8.83 days per year (13.6 log points) over the long term, which corresponds to 2.4% of the median of *Days Employed*.

3.3 Job loss, unemployment, and career paths

Comparing the results from establishment-level analyses and individual-level analyses allows us to make some tentative inferences about employees' post-buyout career paths. The long-term post-buyout separation rate of 18.75% reported in Table 4 translates into an employment decline of about 8.96%. We caution the reader that the establishment sample covers more employees, since it includes also unmatched employees and the separations reported above include employees who were hired after the buyout, both of which do not appear in the individual-employee sample. However, if we assume that these two groups are not large or different enough to materially distort the picture, we can conclude that about half of the employees who are separated from buyout target firms find new employment.

To further analyze the importance of employees' post-buyout career paths, we repeat the analysis in Figure 5, but now add additional control variables for career events. In particular, we add three dummy variables to regression (2): for switches to another establishment within the same 3-digit industry; for switches to another establishment outside the employee's 3-digit industry; and if the employee becomes unemployed. The variables always capture the status of the employee five years after the buyout. Figure 6 plots the estimates of θ_k, which capture the interactions of the event-time dummies with the target indicator, $D_{ik} \times Target_i$.

Figures 5, Panel B and 6 are drawn to the same scale to make them comparable. After controlling for career-path events, the PE buyouts are not associated with individual-level declines of income, wages and employment. Hence, career path events can account for the long-term post-buyout decline of income and employment.

4 Who benefits and who loses after buyouts?

In this section we analyze how different groups of employees fare after buyouts. Our discussion is guided by three groups of theories that have been proposed in the literature on buyouts, each of which identifies employees with certain characteristics as likely losers or beneficiaries from buyouts. Section 4.2 analyzes organizational streamlining and Section 4.3 investigates different variants of the technological-modernization argument. Finally, in Section 4.4 we address the question whether private equity buyouts are primarily a mechanism to transfer wealth from employees to shareholders, and to what extent they may breach implicit employment guarantees.

4.1 Methodology

All hypotheses we investigate in this section make contrasting predictions on the development of wages and employment for specific subgroups of employees relative to each other and identify several factors, such as workers' age, skill level, and the specificity of their human capital. Therefore, we extend the methodologies of Sections 3.1 and 3.2 and incorporate employee characteristics into analyses at the establishment and the individual level. Each hypothesis identifies groups of employees who are more likely to suffer impairments of their human capital after buyouts and we refer to these characteristics as risk factors. For each group, we repeat the establishment-level analysis in Table 4 and test whether separation (hiring) rates after buyouts are particularly high (low) for groups of employees considered to be at risk in buyouts. All establishment-level analyses for groups of employees are presented in

Table 6 for the period from the event year to five years after the buyout. Table 5 provides the same analysis for rates calculated until two years after the buyout. Columns (1) to (3) report the θ-coefficients from equation (1) for growth, separation, and hiring rates, respectively. Columns (4) to (6) of Panel A show tests for the differences between the group shown and all other employees, e.g., all managers and all non-managers. Columns (4) to (6) of Panel B show tests for the differences between the highest and lowest group. E.g., the coefficient in column (4) for *Low wage* shows the difference between the estimates for *High wage* - *Low wage* and a t-test for whether this difference is significantly different from zero. Note that for twelve of the 15 subgroups in Table 6, above-sample average (below average) separation rates go along with above-average (below average) hiring rates, i.e., departures by a certain category of employees is associated with increased hiring in the same category.

Similarly, we perform individual-level triple-difference analyses, in which we interact the target indicator and event-time dummies with risk factors that identify the respective subgroups of employees. We build on equation (2) and estimate the triple-difference equation

$$\begin{aligned} Y_{it} =& \alpha_i + \gamma_t + \sum_{k=-5}^{k=+5} \delta_k D_{ik} + Target_i \times \sum_{k=-5}^{k=+5} \theta_k D_{ik} \\ &+ RF_i^f \times \sum_{k=-5}^{k=+5} \lambda_k D_{ik} + Target_i \times RF_i^f \times \sum_{k=-5}^{k=+5} \eta_k D_{ik} + \varepsilon_{ik}. \end{aligned} \tag{3}$$

The coefficients of interest in (3) are the η_k's on the triple interaction of $Target$, the event dummies, and the risk factor, which measure by how much target employees characterized by risk factor RF^f differ from control employees with the same risk factor, and by how much target employees characterized by risk factor RF^f differ from target employees not characterized by this risk factor. We run each regression for all three dependent variables, once in Euros (*Earnings*, *Daily Wage*, columns 1, 3) or days (*Days Employed*, column 5) and once in logarithms (columns 2, 4, 6). All individual-level analyses based on equation (3) are presented in Table 7. We only report the estimates of η_2 and η_5 to be consistent with the establishment analysis and to conserve space. In Table A-9 of the Internet Appendix, we report all coefficient estimates for period t to period $t+5$ for all risk factors yielding significant results. As before, we cluster standard errors at the firm level.

4.2 Organizational streamlining

One strand of the LBO literature sees PE buyouts as an organizational form that rivals the public corporation with dispersed shareholders (Jensen, 1989, Kaplan, 1989). This literature characterizes private equity firms as lean, decentralized organizations and argues that buyouts replace governance by direct monitoring with governance through high-powered incentives (Jensen, 1989; Lichtenberg and Siegel, 1990). For example, if top executives prefer a "quiet life" (Bertrand and Mullainathan, 2003), they may avoid confrontations with employees, pay higher wages, and favor middle management to avoid conflicts in their immediate work environment (Cronqvist et al., 2009). If buyout firms address these agency problems, buyout targets should reverse these trends. Based on these notions, we expect buyout targets to streamline their organizational structure by reducing the layers of management and creating a leaner organization. We hypothesize that these measures will fall disproportionately on white-collar workers and managers and expect a general decline in employment for these groups. Our data set includes two variables that allow us to analyze organizational restructuring. The first one is *Manager,* an indicator variable for those employees who have an executive or middle management position (occupational group 10 in Table 3). The second one is *White Collar*, an indicator variable for white-collar workers (occupational groups 8, 9, and 10).[20]

Table 5 shows that the short-term growth rate for white-collar workers is lower, reflecting a separation rate that is significantly higher (at the 10%-level) than that for non-white collar workers; the long-term effects in Table 6 point in the same direction, but are not statistically significant. However, we observe economically and statistically significant increases in the long-term hiring and separation rates, and a low replacement rate for white-collar workers, which results in a substantial long-term decline of 11.82% in white-collar employment. Hence,

[20]White-collar employees form 28.5% of our sample (Table 3). Managers are a subset of white-collar employees and account for 3.1% of the sample and 11% of white-collar employees.

there is evidence that buyout investors streamline the administration of the firm after the buyout, especially during the first two years. The short-term results for managers in Table 5 point in the same direction, but are less precisely estimated and statistically not significantly different from those for other employees. The long-term point estimates for managers for all rates reported in Table 6 are low, about half of those for the whole sample, and statistically insignificant. Hence, there is some evidence for an increase in short-term turnover of middle managers, but no evidence for a reduction in the layers of management.

Table 7 shows the individual-level results for managers (regression (1)) and white-collar employees (regression (2)). We observe an economically dramatic decline in long-term *Earnings* for managers by 29.4 log points (€2,019), which is entirely driven by a decline in employment (-14.4 log points or 8.36 days). These effects are statistically significant. The post-buyout experience of white-collar workers is also negative, with a long-term employment decline of 8.0 log points (5.4 days) and a weakly significant long-term decline in earnings by €702. We conclude that the adverse implications for managers and white-collar workers after buyouts are severe, and significantly worse than those for other target employees. The short-term establishment-level results provide some evidence for a reduction in administrative staff, but not for a reduction in the layers of management. We suggest that the negative individual-level results for managers should be attributed to the greater difficulties they experience in finding new employment, probably from a stigma associated with losing their jobs at the target.

4.3 Technological modernization

This section investigates the technological-modernization argument. Technological change can be beneficial for employees if their skills are complementary to the new technology; then employees become more productive. However, a new technology may also have negative effects if it substitutes for employees' skills and depreciates their human capital. The technological-change argument relies on this notion of technology-skills complementarity and, therefore, does not make general predictions on how employees should be affected by technological change, and how PE buyouts that foster technological change should affect employees. Rather, different types of technological change may affect employees differently. The recent literature sees PE buyouts as vehicles that raise investment in information technology (Agrawal and Tambe, 2016), or overcome firms' resistance to adapt to trends, such as skill-biased technological change, offshoring and routinization of jobs, which in turn lead to job polarization (Olsson and Tåg, 2017). In this section, we analyze whether these trends can explain the overall results we show in Section 3 for employment and wages.

Skill-biased technological change. The skill-biased technological change (SBTC) hypothesis attributes the rising wage inequality in industrialized countries to technological progress, which has benefited high-skilled workers, whose skills are complementary to new technologies, but caused a relative reduction in wages for low-skilled workers (see Katz and Autor, 1999, for a survey). If private-equity firms overcome resistance to this trend, then the costs of PE buyouts may fall disproportionately on employees with lower education and skill levels, whose wages and employment fall relative to employees with higher education and skill levels.

We use pre-buyout wage levels to stratify employees in Tables 5, 6, and 7. We divide all employees who are employed for the full year evenly into terciles according to *Daily Wage*: the lowest (medium, highest) income tercile is labeled as low (medium, high) wage. With this classification, we have two risk factors in equation (3), one for *Low Wage* and one for *Medium Wage.*

The skill-biased technological change (SBTC) hypothesis predicts employment and income should decline for the lowest group. The establishment-level results in Tables 5 and 6 are partially consistent with the SBTC hypothesis. The short-term and long-term separation rates for low-wage target employees are both higher than those for control employees, and statistically and economically much higher compared to high-wage employees; these results support the SBTC hypothesis. However, most of the increased separations are compensated by more hiring of low-wage employees by buyout targets, and the long-term buyout effect on hiring is higher by 14.68% and significant at the 10%-level. As a result, the post-buyout employment decline of low-wage employees is only moderately larger, and statistically not distinguishable from that of high-wage employees. Since the SBTC hypothesis predicts that high-wage employees displace low-wage employees, our results offer at best modest support to the hypothesis that buyouts in our sample foster SBTC; rather than observing the displacement of low-wage employees by high-wage employees, we only find higher turnover within the group of low-wage employees, for which there is no obvious explanation. To explore this question further, we investigate alternative stratifications of employees based on education and a classification of occupational skill levels (see Table A-10, Panel A), but cannot find any support for the prediction that the separation (employment growth) rates for low-education, respectively, low-skill employees are lower (higher) than those for highly-educated or high-skill employees.

At the individual employee level, we find that the triple interactions for the short-term and the long-term ($D_{i2} \times Target \times Low\ Wage$ and $D_{i5} \times Target \times Low\ Wage$) both show a significant and positive employment change for low-wage target employees if we measure *Earnings* in Euros; all other results in Table 7 are statistically insignificant, except for one coefficient for *Daily Wage,* which has the wrong sign. Hence, we cannot find support for the SBTC hypothesis at the individual level, probably because low-wage employees find it easy to find new employment after separations from the target.

Offshoring, routinization, and job polarization. More recent research notes inconsistencies between the SBTC hypothesis and developments in labor markets (e.g., Card, 2002; Goos et al., 2014; Mishel et al., 2013). Instead, some studies observe that employment shares rise in the highest-wage and lowest-wage occupations, at the expense of mid-level occupations (e.g., Goos and Manning, 2007), a pattern described as job polarization. The reason for this development is seen in the fact that low-wage manual jobs (e.g., health workers, janitors, security guards) are more difficult to replace with computerized technologies, or cannot be outsourced to countries with lower labor costs ("offshorable" jobs). By contrast, medium-skilled workers who perform routine tasks may see their jobs replaced by technology or outsourced to low-wage countries (e.g., Blinder, 2009; Blinder and Krueger, 2009).

Following Olsson and Tåg (2017), we hypothesize that PE buyouts overcome resistance to offshoring and routinization and ask whether employees with routine or offshorable jobs fare worse after buyouts.[21] We apply the definitions of Goos et al. (2014) to categorize jobs as routine, respectively, as offshorable.[22] At the individual level, we find positive effects, and, hence, the opposite of the predicted signs, which are statistically significant for the association of routinization with *Earnings* and *Days Employed*, whereas all other effects are statistically insignificant (Table 7). The establishment-level analyses corroborate these findings (Tables 5, 6). The long-term separation and growth rates are numerically higher, but statistically indistinguishable for employees with less routinized and offshorable jobs. Thus, neither the analyses at the individual nor at the establishment level offer supporting evidence for the prediction that buyouts foster offshoring and routinization, and there is even some evidence to the contrary.

Job polarization predicts negative developments for the medium stratum of the labor market relative to the other groups. Hence, we can check if the results for the medium-wage employees conform to the predictions of the job-polarization hypothesis. However, the individual-level analysis shows that the triple interactions for these groups have mostly positive signs, although only two are statistically significant at the 10% level (Table 7, Panel B). Establishment-level results are consistent with this result as separation and hiring rates are higher for low-wage employees than for medium-wage employees, although the differences between medium-wage and either high-wage or low-wage employees are never statistically significant. Hence, our results offer no support for the job-polarization hypothesis.

[21]Offshoring is, strictly speaking, not a technology, but the literature on job polarization relates the offshorability of jobs to their technological aspects and skill requirements, so we discuss offshorability in this context.

[22]We follow the description in the Online Appendix of Goos et al. (2014), which is available under http://dx.doi.org/10.1257/aer.

IT-related skills. Agrawal and Tambe (2016) argue that PE buyouts foster the investments in IT and the implementation of IT-based technologies. Based on their analysis we expect that target employees with more IT exposure become more valuable after the buyout and that their wages and employment increases compared to the control group. To investigate this question, we obtain access to IAB data about job classifications based on the tools each job requires (Genz et al. (2018)). Tools are categorized into three categories as *IT-integrated tools*, defined as tools "that are electronically based or supported and that are explicitly dedicated to an industry 4.0 or services 4.0 feature, such as 3D printers, machine learning software or mobile robots," *IT-aided tools*, which "are electronically based or supported, such as computers, printers, electronic machines," but not classified as IT-integrated, and *Non-IT tools* (Genz et al. (2018), p. 5). Genz et al. (2018) then establishes the proportion of tools that can be classified as IT-integrated or IT-aided used in each occupation. Higher scores on the IT-aided tools index describe jobs with a broad skill set, whereas higher scores on the IT-integrated tools index describe jobs with higher skill requirements. We use these proportions and say that all employees who have a job with an above-median score of the sum of IT-aided and IT-integrated tools as having an *IT-related Job.* Far fewer employees use IT-integrated tools, so the median of this score is zero. Hence, we say an employee has an IT-integrated job if at least one of the tools used in that job is IT integrated. Both variables describe jobs with significant exposure to computerized technologies.

We use the classification of jobs as IT-related and non-IT related, respectively, as IT-integrated and not IT-integrated, and repeat the analyses for this sample split. The point estimates for long-term hiring rates and net employment growth in Table 6 suggest that buyout firms hire more IT-integrated and IT-related workers, and that net employment growth is larger for the former group compared to all other employees, but the estimates are too imprecise to be statistically significant. Short-term net employment growth is statistically significantly higher (10%-level) for IT-integrated jobs. Panel D of Table 7 reports the individual-level results, all of which are insignificant. Target employees who use IT tools, defined narrowly or more broadly, do not have different wages or employment levels after buyouts compared to a matched control group and compared to non-IT employees. Hence, we observe additional hiring and job growth for IT-integrated jobs, consistent with prior literature, but no effect on the human capital of the affected employees.

4.4 Transfers of wealth

A long-standing debate on buyouts and the activity of private-equity firms is whether they create shareholder value primarily through transfers of wealth − see the discussion at the beginning of the Introduction − or whether the adverse consequences for employees should be seen as a side effect of a process of modernization and creative destruction (Kaplan, 1989; Davis et al., 2014). In this section we try to shed some light on this discussion. Empirically, it is impossible to distinguish between intended effects and side effects. Hence, we analyze more specific processes associated with the transfer-of-wealth mechanism.

The transfer-of-wealth argument was articulated most clearly by Shleifer and Summers (1988), who argue that firms offer long-term employment insurance to employees (Shleifer and Summers, 1988). Employees rely on managers and owners to honor these unwritten agreements, which are credible, e.g., because managers and owners pass through "loyalty filters" (Akerlof, 1983) in their career that align their preferences with those of the employees. A change in ownership may undermine the commitment to such implicit contracts if the new owners do not feel bound by agreements the previous owners entered into with the employees.

There are two different arguments in the literature on transfers of wealth, which both build on the notion of implicit contracts. The first relies on risk-sharing within firms and the second relies on firm-specific human capital. Since these arguments have different implications for employment and wages, we develop and test them separately.

4.4.1 Risk-sharing and dynamic wage contracts

According to the risk-sharing argument, firms provide employment and wage insurance to employees in exchange for lower wages (Azariadis, 1975; Baily 1974). The dynamic version of this argument implies that wage profiles are rising with employees' age: Insurance implies that wages cannot be cut when productivity falls, but voluntary employment implies that wages increase when productivity rises, resulting in a ratchet effect (Harris and Holmstrom, 1982). Wages can then rise in excess of employees' marginal productivity towards the end of their careers, and they may rise above employees' productivity. Firms extract expected rents, e.g., through lower wages, at the beginning of employment relationships (Ray, 2002). A similar argument follows from Lazear (1979), who develops a model in which firms elicit unobservable effort in exchange for rising wage profiles. If the new owners after PE buyouts would renege on these implicit agreements, they would lay off or cut the wages of older employees.

In Regression (1) of of Table 7, Panel E, we use *Age* as a risk factor and split the sample at the median age of all employees (42 years); we find statistically and economically strong effects. The long-term decline of *Earnings* of older buyout employees is larger by 18.4 log points, or €807 (2.3% of the median wage), compared to older control employees, or compared to younger target employees. Unlike for other groups of employees, we observe a significantly negative wage effect for older employees, which compounds the negative employment effect.

Next, we investigate whether older employees of PE targets experience higher separation rates after buyouts. The theoretical argument implies that PE buyers replace older employees who earn above their productivity with younger employees, who earn less. Such a policy would result in higher post-buyout separation rates for older target employees compared to control employees, and higher post-buyout hiring rates for younger employees. Tables 5 and 6 show that this is not the case. The long-term post-buyout separation rate is even slightly higher for younger target employees compared to older target employees, although this difference is almost zero. The difference for hiring rates has the predicted sign (young:

9.48%; old: 7.61%), but is small and statistically insignificant. Hence, the human-resource policies of private-equity investors do not appear to be biased against older employees. In all likelihood, the negative individual-level results for older employees should be attributed to older employees not finding new employment after losing employment at the target firm. We could cast the discussion of dynamic wage profiles in terms of tenure with the firm rather than age, but the discussion in the next section shows that this would not affect our conclusions.

4.4.2 Firm-specific human capital

Shleifer and Summers (1988) formulate a different version of the transfer-of-wealth argument. They hypothesize that firms offer long-term employment protection to employees to provide incentives for investments in firm-specific human capital. New owners may abrogate these contracts and take advantage of employees with firm-specific human capital by forcing them to accept lower wages. The testable implications of this hypothesis are that (1) PE buyouts lead to a reduction in wages for employees who continue to be employed by the target firm (see Rosett, 1990 and Gokhale et al., 1995 for tests of a related argument on takeovers); (2) these wage cuts fall disproportionately on employees with more firm-specific human capital.

We test the first implication, i.e., the reduction of wages for continuing employees, by performing a triple-difference analysis at the individual level. We add interaction effects with the dummy variable "*Leaver,*" which equals one for employees who leave their establishment between the end of year $t-1$ and the end of year $t+5$. We provide the results for the overall sample and for some of the pertinent subgroups of employees in Table 8.[23] Baseline growth rates of *Earnings* and *Daily Wage* are consistently positive, whereas *Days Employed* declines by 2.7 days per year. Most importantly, changes for all three variables for target employees (interaction $Target_i \times D_{i5}$) in the whole sample, and for almost all subgroups, are economically and statistically indistinguishable from zero. The exception are employees in the lowest wage tercile, who experience an increase in *Earnings* (€440). Hence, employees who stay with

[23]Table A-11 in the Online Appendix provides the same results for the shorter period from $t-1$ to $t+2$.

their establishments after PE buyouts do not lose, neither in absolute terms nor in relative terms, compared to employees of non-PE targets, which is inconsistent with the specific-human capital argument. By contrast, employees who leave the firm (interaction $D_{i5} \times Leaver_i$) lose substantially in terms of *Earnings.* This negative effect on leaving employees is exacerbated for target employees, and the triple interaction effect is significant for all seven groups of employees included in Panel B of Table 8. The results are quantitatively strongest for employees in the highest wage tercile and for managers.

To address the specific-human capital argument more directly, we follow the literature (e.g., Poletaev and Robinson, 2008) and measure the specificity of human capital by individuals' tenure in their current job. We use the median tenure in our sample to distinguish between high and low tenure. Regression (2) in Table 7, Panel E presents the individual-level results. None of the coefficients are significant, suggesting that the specificity of human capital cannot explain the differences for employment and earnings of buyout employees compared to control employees. It is also instructive to look at the establishment-level results in Table 6. The long-term separation rate for low-tenure employees is 20.69%% compared to only 1.19% for high-tenure employees, and the difference is significant at the 1%-level. These results are consistent with a special protection of high-tenure employees, which is implied by insider-outsider theories (see Lindbeck and Snower, 1986, 1988) that build on the notion that insiders are more entrenched than outsiders, an arrangement that is apparently immune to private-equity interventions.

To summarize, we cannot find support for any of the implications of the transfer-of-wealth view we can test. There is no evidence that PE targets extract quasi-rents from older employees by laying them off at a higher rate than younger employees. There is also no evidence that PE buyouts benefit from the lock-in of employees with more firm-specific human capital through lower wages, or at least through lower wage growth. We are careful to add that we test specific versions of the transfer-of-wealth argument. The losses of employment and earnings we document above may still involve some breach of implicit employment contracts, which are not observable. We can only test specific implications and not the broader questions whether PE buyers sever *any* implicit long-term employment guarantees.

5 Discussion and conclusion

We study the development of employment and wages of a large sample of German employees whose firms were acquired by private equity firms. Buyouts are followed by a decline in employment and an increase in employee turnover. Increases in separation and hiring rates are strongly correlated across transactions and across the groups of employees we study. Individual-level earnings of buyout employees fall by €980 five years after the buyout, which amounts to about 2.8% of median earnings.

When we analyze groups of employees with particular characteristics, establishment-level results and individual-level results often point in different directions. For example, employees in the lowest wage tercile and those with below-median tenure at the firm experience a higher incidence of separations from the target after buyouts. However, these characteristics do not predict individual-level unemployment and losses to employees' long-term earnings. By contrast, managers and older employees experience large losses after buyouts, even though they do not experience higher separations from the target compared to other employees. Hence, the employees who lose most after buyouts are those who seem to find it harder to find new employment, not those who experience a higher incidence of job loss after buyouts. We infer that buyout investors replace employees based on characteristics such as ability, which are observable to managers after buyouts, but not reflected in the employee characteristics in our data.

We find only limited evidence for theories that predict changes in the composition of the workforce after buyouts. There is some evidence that a disproportionate part of the decline in employment falls on white-collar workers, pointing to the creation of leaner firms through laying off administrative staff. Similarly, there is some evidence that there is net employment growth in jobs that require more IT skills. Other theories that propose changes in the composition of the workforce, e.g. those related to particular forms of technological change, have little or no explanatory power. We conclude that the first-order effects after buyouts are changes in the size and quality of the workforce rather than a change in its overall composition.

A Appendix

This appendix provides more detailed information about the record-linkage process (Section A.1), the construction of some of the more complex variables (Section A.2), and the computation of growth rates, hiring rates, and separation rates (Section A.3).

A.1 Record linkage and clustering

We link establishments to transactions based on company names, because there are no common company identifiers that would easily link our PE buyout sample to the Establishment History Panel (BHP). The BHP contains all establishments in Germany with at least one dependent employee at the reference date, June 30th, of the respective year. The Institute for Employment Research (IAB) has developed record linkage techniques described in Herzog et al. (2007) for the purpose of name-based matching using establishment names.[24] Establishment names consist of the company name, the legal form, and additional information. In principle, the linkage techniques create two standardized variables containing the company name and the firm's legal form of incorporation for all data sets that need to be linked. Based on these variables, the IAB performs a record linkage that includes the handling of exceptional cases such as very common firm names or obviously stand-alone establishments. In-sample tests suggest that this procedure is very accurate.[25]

The BHP represents the highest level of aggregation of IAB data. The IAB does not provide data on the firm level and does not track ownership or relations among establishments. To cluster standard errors at the firm level, we apply the record linkage techniques by Herzog et al. (2007) to generate a synthetic firm identifier from the universe of all establishments existing in IAB's data in our observation period. For the target establishments, we furthermore use the initial record linkage of company names to establishments, i.e., we group the 2,420 establishments into exactly 682 firm-level clusters. This approach yields more clusters than deals because some deals involve several legally distinct companies (e.g., multiple subsidiaries). Subsidiaries often have distinct human resource policies and should, therefore, be treated separately.[26] Nevertheless, we alternatively build clusters based on deals, i.e., we group the 2,420 establishments into exactly 511 deal-level clusters. We re-run our key analyses in Tables 4, 5, and 7 using deal-level clustering (cf. Table A-12 in the Online Appendix).

[24] The record linkage was performed using methods developed by the German Record Linkage Center (GRLC, see `http://www.record-linkage.de`).

[25] See Schild (2014) and Schild (2016) for a detailed description of the methods and data sources used to perform very similar linkages with IAB's administrative establishment data. They demonstrate that the methods we use are effective in linking IAB's establishment data with external company-level data.

[26] A prominent example is Eurowings, a budget airline and wholly-owned subsidiary of Lufthansa, which offers lower pay to pilots and flight attendants.

It turns out that standard errors never increase by more than 5% for any relevant coefficient and statistical significance remains unchanged.

A.2 Variable construction

Most variables in our analyses are derived from the Integrated Employment Biographies (IEB) database. The IEB contains every dependent employee in Germany, i.e. all regular employees since 1975 in West Germany and since 1992 in East Germany as well as all marginally employed workers since 1999.[27] The data are structured in terms of spells, i.e. employment relationships, and the data source reports starting and ending dates of these spells on a daily basis. If employment relationships continue into the following calendar year, a notification is given by the employer at the end of each year. The continued employment relationship is represented by a new spell in the following calendar year. For categorical variables such as education, qualification, and establishment affiliation, we use the information from the latest spell in a calendar year. All variables except nationality and gender are time-varying and can change for the same individual during the observation period. Numerical variables such as *Earnings*, *Daily Wage*, and *Days Employed* are computed over both the full calendar year and all spells in the respective calendar year, regardless of whether the spells refer to different employers or the same employer. *Earnings* are top-coded, because wages above a threshold ranging from 51,000 in 1998 to 70,000 in 2013 Euros are exempt from certain social-security contributions. Maximum *Earnings* reported in the data can nevertheless be higher because some individuals have more than one job in a given year and social security contributions are calculated for each job, even if the income of all jobs combined exceeds the threshold. Numerical variables such as *Age* and *Tenure* are determined on the last day of the calendar year.

The qualification variables presented in Table 3 and used in subsequent analyses are derived from Blossfeld (1987). The author classified jobs that are coded according to the German Classification of Occupations 1988 (KldB 1988) into 12 distinct major occupations. Table 1 on page 99 in Blossfeld (1987) provides a detailed overview on those 12 occupations and related ISCO codes. In Table 3, we leave out agricultural occupations because our data set does not include individuals from this group and we merge technicians and engineers into one group.

For our establishment analysis, we aggregate the annualized employment information of individuals at the establishment level. Every calendar year, we sum up or build averages

[27] The IEB does not cover civil servants and the self-employed. These groups are irrelevant for the companies in our sample. For more details on the sources and structure of IAB's administrative data, see Antoni et al. (2016).

over all employees that were employed at an establishment at the end of the calendar year (i.e., December 31st.). Therefore, changes in establishment-level employment are based on changes from December 31st of the previous year to December 31st of the current year.

A.3 Growth rates, separation rates, and hiring rates

We use the following definitions:

Symbol	**Definition**
E_{jt}	Number of all employees employed in establishment j at the end of year t.
H_{jt}	Number of employees who enter establishment j in period t, i.e. between the end of year $t-1$ and the end of year t.
S_{jt}	Number of employees who are separated from establishment j in period t, i.e. between the end of year $t-1$ and the end of year t.

We then define employment growth between period $t-1$ and period t as

$$g_{j,t-1,t} = \frac{E_{jt} - E_{j,t-1}}{0.5\left(E_{jt} + E_{j,t-1}\right)} \tag{4}$$

and observe that

$$E_{jt} - E_{j,t-1} = H_{jt} - S_{jt}. \tag{5}$$

We define one-year hiring rates and separation rates as

$$h_{jt} = \frac{H_{jt}}{0.5\left(E_{jt} + E_{j,t-1}\right)}, \ s_{jt} = \frac{S_{jt}}{0.5\left(E_{jt} + E_{j,t-1}\right)}. \tag{6}$$

From (4) and (5), we have $g_{j,t-1,t} = h_{jt} - s_{jt}$. We also compute multi-period employment flows as

$$E_{j,t+k} - E_{j,t-1} = \sum_{\tau=0}^{\tau=k} \left(E_{j,t+\tau} - E_{j,t+\tau-1}\right) = \sum_{\tau=0}^{\tau=k} \left(H_{j,t+\tau} - S_{j,t+\tau}\right) = H_{j,t-1,t+\tau} - S_{j,t-1,t+\tau}. \tag{7}$$

Multi-period growth rates between periods $t-1$ and $t+k$ are defined as

$$g_{j,t,t+k} = \frac{E_{j,t+k} - E_{j,t-1}}{0.5\left(E_{j,t+k} + E_{j,t-1}\right)}. \tag{8}$$

Multi-period hiring rates and separation rates are defined analogously to (8). Note that, generally, $g_{j,t-1,t+k} \neq \sum_{\tau=0}^{\tau=k} g_{j,t+\tau-1,t+\tau}$ and analogously for separation and hiring rates.

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Figures

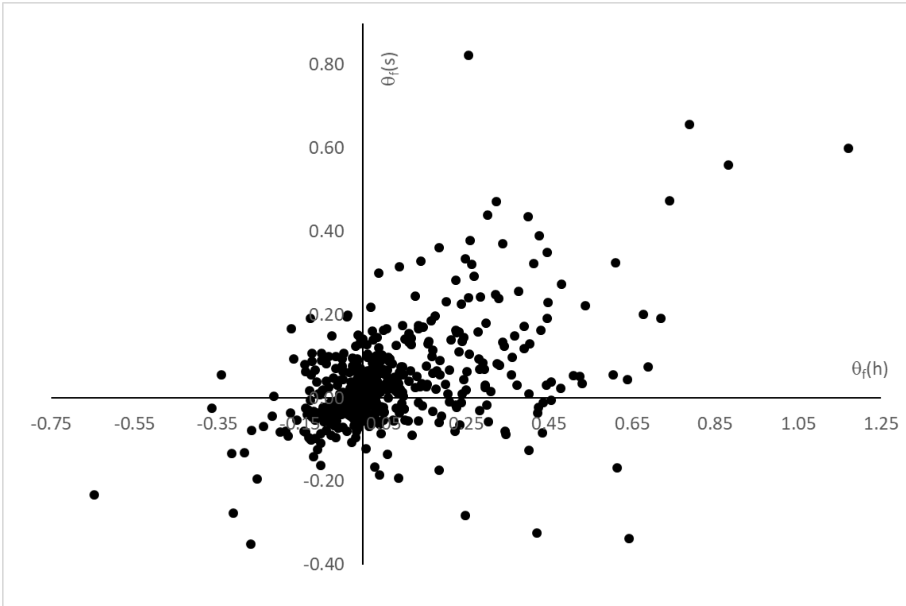

Figure 1: Deal-level hiring and separation rates. The figure plots the coefficients $\theta_f(s)$ against $\theta_f(h)$ from estimating equation (1) for rates from the event year to year $t+5$ separately for each deal in the sample. The cross-sectional correlation between $\theta_f(h)$ and $\theta_f(s)$ is 48.7%. Of the 511 deals, 234 (46%) have positive estimates for both, $\theta_f(h)$ and $\theta_f(s)$, whereas 122 (24%) have negative values for both; 74 deals (14.5%) have $\theta_f(h) < 0$ and $\theta_f(s) > 0$; 81 deals (15.9%) have $\theta_f(h) > 0$ and $\theta_f(s) < 0$.

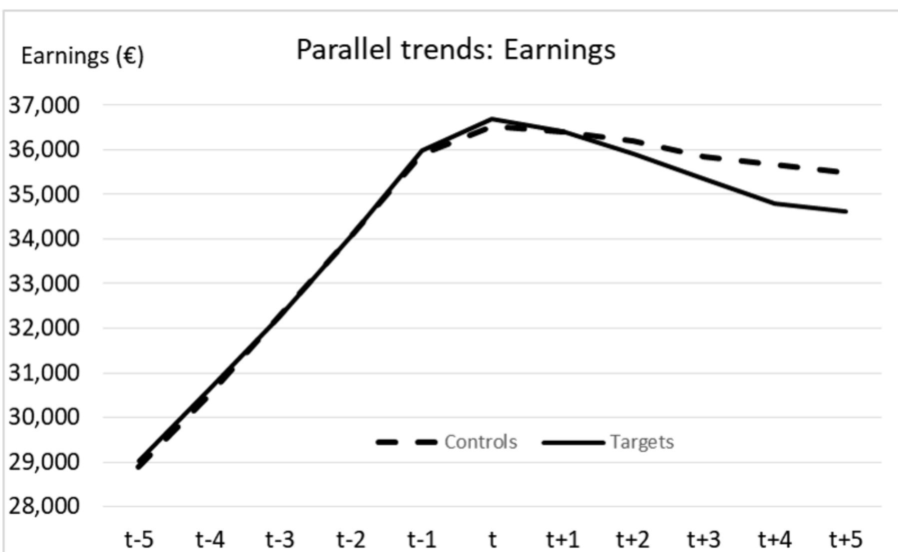

Figure 2: Parallel trends analysis: Earnings. This figure presents the development of *Earnings* in event time. For every event year, we compute the mean of *Earnings* for target employees and control employees separately. *Earnings* is defined in Table 1.

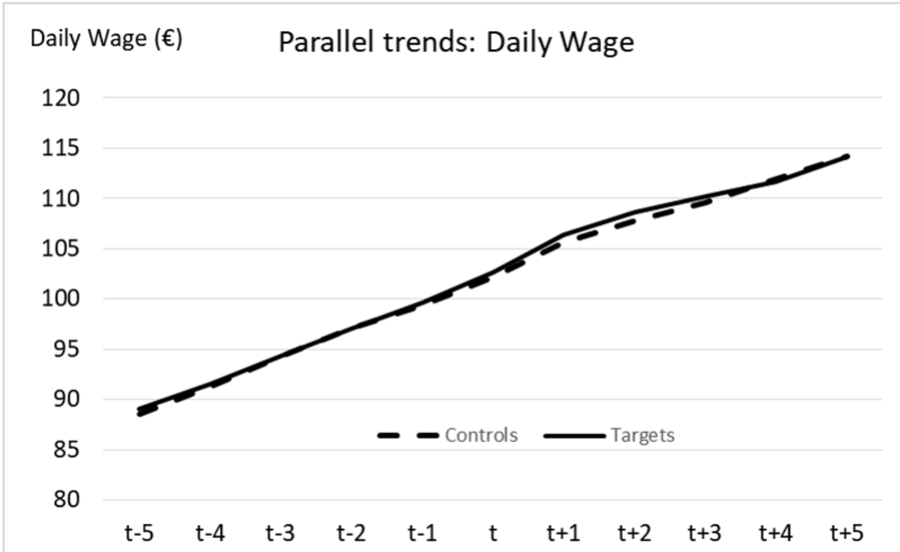

Figure 3: Parallel trends analysis: Daily Wage. This figure presents the mean of *Daily Wage* for target employees and control employees separately. *Daily Wage* is defined in Table 1. *Daily Wage* is set to missing if *Daily Wage* of matched pair is missing in a given year.

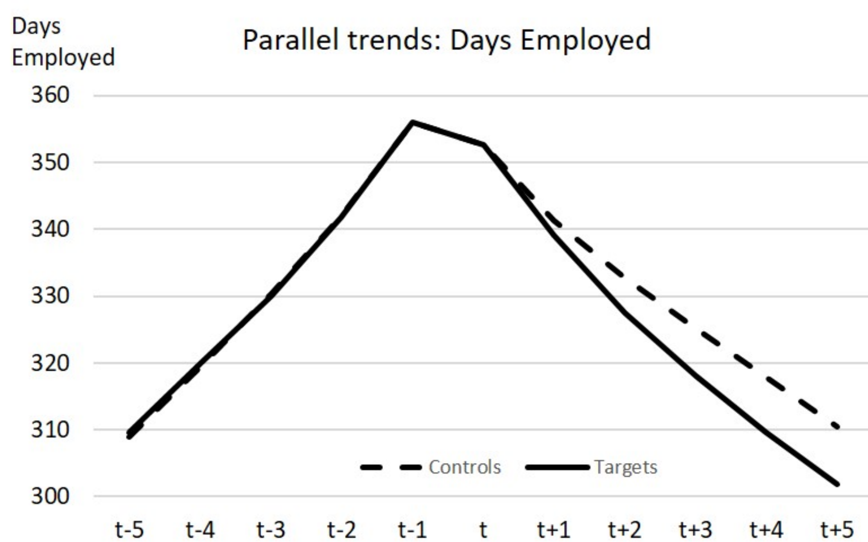

Figure 4: Parallel trends analysis: Days Employed. This figure presents the mean of *Days Employed* for target employees and control employees separately. *Days Employed* is defined in Table 1. The inverted-V pattern is a mechanical consequence of the requirement that employees in both groups have to be employed in the event year, but not before or after the event year.

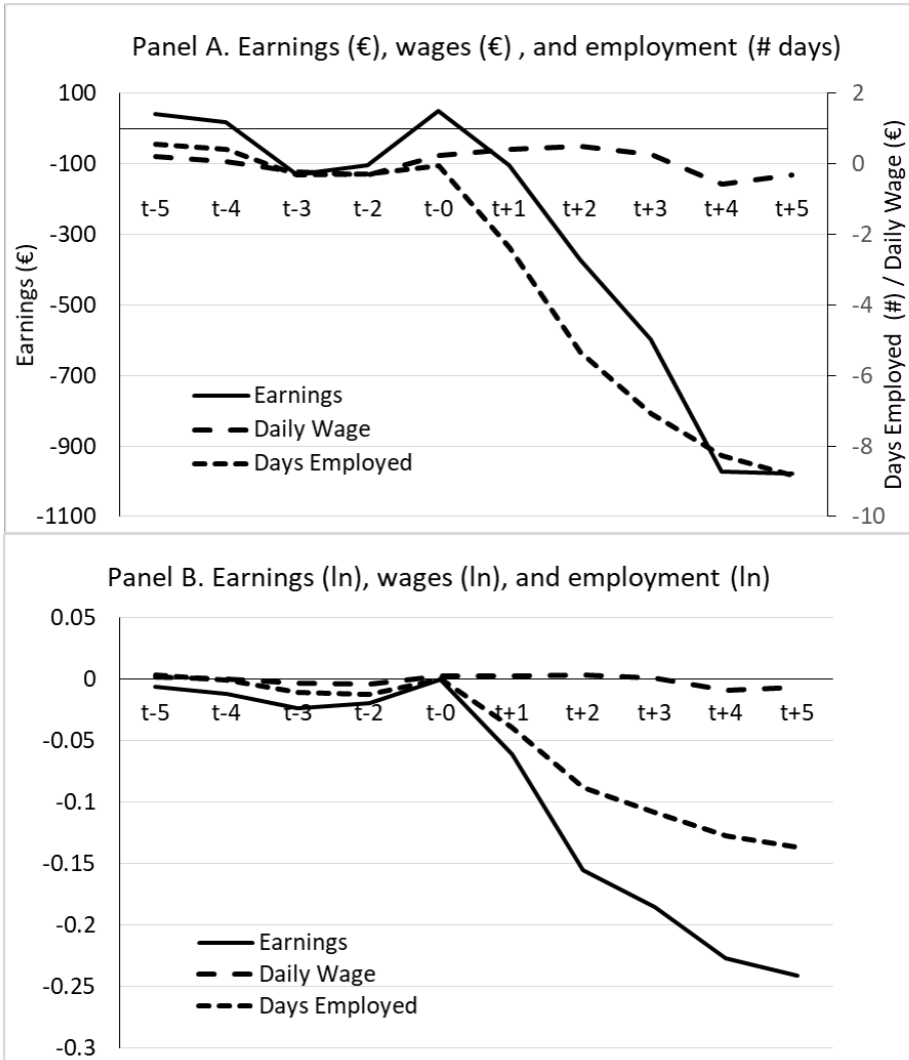

Figure 5: The impact of PE buyouts on wages and employment. The figure plots the coefficients θ_k on the interaction terms $Target \times D_{ik}$ from OLS-regressions of *Earnings*, *Daily Wage,* and *Days Employed* on a difference-in-differences setup and control variables as in equation (2). In Panel B, we use logarithmic transformations of the dependent variable. D_{ik} is a dummy variable which is one for observations k years after the event year, where k and runs from five years before the buyout $(t-5)$ to five years after the buyout $(t+5)$. The dependent variables are in logs and defined in Table 1. Regressions control for person fixed effects and calendar-year fixed effects.

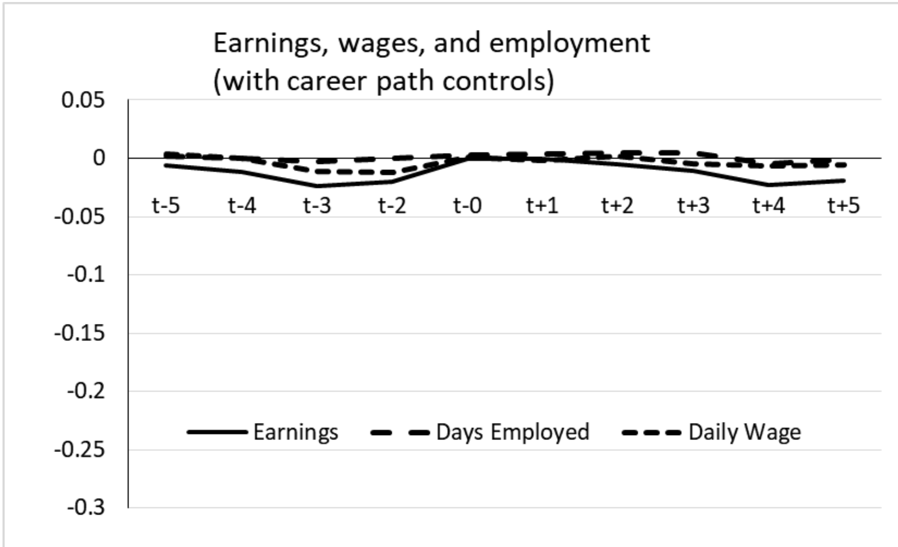

Figure 6: PE buyouts and career path events. The figure plots the coefficients θ_k on the interaction terms $Target \times D_{ik}$ from OLS-regressions of *Earnings*, *Daily Wage,* and *Days Employed* on a difference-in-differences setup and control variables as in (2). The regressions include dummy variables *Establishment Change*, *Industry Change*, and *Unemployment*. D_{ik} is a dummy variable which is one for observations k years after the event year t and runs from five years before the buyout $(t-5)$ to five years after the buyout $(t+5)$. The dependent variables are in logs and defined in Table 1. Regressions control for person fixed effects and calendar-year fixed effects.

Tables

Table 1: Description of variables. The table describes all numerical variables. For each variable, the table reports the definition and the value range.

Variable Name	Definition	Range
Age	Age of the individual in years	$[0;\infty]$
Daily Wage	Wagesum divided by Days Employed	$]0;\infty]$
Days Employed	Sum of days in Employment over all spells in one calender year	[0;366]
Earnings	Sum of income across all spells in one calendar year	$[0;\infty]$
Employed	1 unless unemployed or in vocational training	0 or 1
Establishment Age	Years since first record of establishment in database	$[0;\infty]$
Establishm. Size (E)	Number of employees in establishment	$[0;\infty]$
Establishment Wage	Average *Daily Wage* of employees in establishment	$[0;\infty]$
Firm Tenure	Days in employment in current spell	$[0;\infty]$
Fraction Employed	Days Employed divided by 366	[0;1]
Employment Growth Rate (g)	Employment growth rate of establishment j from time t to time t+k, see Appendix A.3 for a precise definition	[-0.5;0.5]
Hiring Rate (h)	Flow of newly-hired employees of establishment j from time t to time t+k, see Appendix A.3 for a precise definition	[0;0.5]
IT-integrated Job	1 if the job description includes the use of at least one IT-integrated tool as defined in Genz et al. (2018)	0 or 1
IT-related Job	1 if the job description is associated with an above median use of IT related tools as defined in Genz et al. (2017)	0 or 1
Manager	1 if occupational group is equal to "Managers" (cf. Table 3)	0 or 1
Offshorable Job	1 if high offshorability risk job as defined in Goos et al. (2014)	0 or 1
Routine Job	1 if high routine intensity job as defined in Goos et al. (2014)	0 or 1
Separation Rate (s)	Flow of leaving employees of establishment j from time t to time t+k, see Appendix A.3 for a precise definition	[0;0.5]
Target	1 if employee is in target company	0 or 1
Total Earnings	Sum of *Earnings* of all employees employed in an establishment	$[0;\infty]$
Total Earnings Growth	*Total Earnings* growth rate, computed analogously to g	[-0.5;0.5]
White Collar	1 if employee is associated with occupational groups (8), (9), or (10) as defined in Table 3	0 or 1

Table 2: Descriptive statistics. This table provides descriptive statistics for all numerical variables. The establishment level data set consists of 2,420 target establishments, 24,147 control establishments, and eleven years of observations: (2,420 + 24,147) x 7 = 185,969 establishment-year observations. The individual level data set consists of 152,057 target employees, the same number of control employees, and eleven years of observations: 152,057 x 2 x 11 = 3,345,254 individual-year observations. "Pre-buyout growth rate" denotes the growth of deal-level employment from the end of t-3 to the end of $t-1$. All other variables are defined in Table 1.

	N	Mean	Median	Minimum	Maximum	Standard Deviation
Panel A. Deal statistics						
Employees	511	425	182	10	8,902	825
Pre-buyout growth rate	511	13.36%	4.14%	-178.64%	200.00%	49.34%
Panel B. Establishment-level data set						
Establishment Size (E)	185,969	77	19	0	8,257	205
Growth Rate (g)	185,969	-3.62%	0.00%	-200.00%	200.00%	54.83%
Hiring Rate (h)	185,969	25.62%	15.38%	0.00%	200.00%	34.13%
Separation Rate (s)	185,969	29.23%	16.00%	0.00%	200.00%	41.26%
Panel C. Individual-level data set						
Age	3,345,254	42	42	10	81	11
Daily Wage	3,071,118	102	99	0	1,663	42
Days Employed	3,345,254	329	365	0	366	98
Earnings	3,345,254	34,251	34,474	0	207,583	17,505
Firm Tenure	3,345,254	3,291	2,374	0	14,245	3,069
Fraction Employed	3,345,254	1	1	0	1	0
IT-integrated Job	3,345,254	26%	0%	0%	100%	44%
IT-related Job	3,345,254	46%	0%	0%	100%	50%
Offshorable Job	3,210,327	62%	100%	0%	100%	49%
Routine Job	3,287,989	48%	0%	0%	100%	50%

Table 3: Sample description. This table provides an overview of our sample with respect to our categorical variables. Occupational groups are based on the job classification scheme provided on page 99 in Blossfeld (1987). "Semi-Professions" comprises service-oriented jobs with a high degree of scientific education, comprising, e.g., nurses, social workers, secondary school teachers. "Professions" covers service-oriented jobs with a very high degree of scientific education, e.g., physicians, judges, pharmacists. "Managers" includes both, executives and mid-level managers. Each occupational group is assigned a level of qualification (low, medium, high). "Immigrant population" covers employees who are citizens of Italy or Turkey or who are from a former Yugoslavian country. Our sample includes 152,057 PE buyout employees, the same number of control employees, and 56,392 unmatched PE buyout employees. The statistics are based on the year prior to the transaction.

Occupational group (Qualification)	Target Employees	Control Employees	Unmatched Employees	Total Labor force
(1) Simple manual occupations (low)	24.8%	24.8%	20.8%	17.1%
(2) Skilled manual occupations (medium)	20.0%	20.0%	15.5%	14.3%
(3) Technicians/engineers (high)	16.7%	16.7%	7.6%	6.3%
(4) Simple service (low)	8.3%	8.3%	14.1%	19.7%
(5) Qualified service (medium)	0.7%	0.7%	2.2%	3.3%
(6) Semi-professions (medium)	0.5%	0.5%	1.8%	4.6%
(7) Professions (high)	0.6%	0.6%	2.1%	1.9%
(8) Simple commercial and administr. occupations (low)	6.4%	6.4%	12.3%	11.4%
(9) Qualified commercial and administr. occupations (medium)	19.0%	19.0%	20.8%	19.0%
(10) Managers (high)	3.1%	3.1%	2.6%	2.3%
Females	24.4%	24.4%	42.0%	46.1%
Nationality				
(1) German	93.6%	93.6%	86.5%	89.8%
(2) Immigrant population	4.3%	4.3%	6.1%	5.1%
(3) Rest of the world	2.1%	2.1%	7.4%	5.1%
Occupational status				
(1) Vocational training	1.5%	1.5%	19.8%	7.8%
(2) Full-time employees	88.7%	88.7%	51.4%	58.7%
(6) Home worker	0.1%	0.1%	0.2%	0.1%
(7) Part-time employees	9.7%	9.7%	28.5%	33.8%
Education				
Intermediate school leaving certificate				
(1) without vocational training (low)	10.9%	10.8%	32.5%	23.3%
(2) with vocational training (medium)	69.2%	69.2%	47.5%	58.7%
Upper secondary school leaving certificate				
(3) without vocational training (medium)	0.9%	1.0%	3,0%	3.4%
(4) with vocational training (high)	5.5%	5.5%	6.5%	5.8%
(5) College or university degree (high)	13.5%	13.6%	10.5%	8.9%

Table 3: Sample description (continued).

Industries	LBO Employees	Control Employees	Unmatched Employees	Total Labor force
Manufacturing	66.3%	66.3%	27.4%	25.7%
Retail, maintenance and repair services	13.2%	13.2%	14.6%	17.3%
Real estate	13.6%	13.6%	20.4%	15.1%
Telecommunications	3.9%	3.9%	5.6%	5.5%
Construction	1.4%	1.4%	3.1%	6.3%
All other	1.6%	1.6%	28.9%	30.1%
Region				
(1) North (Schleswig-Holstein, Hamburg, Niedersachsen, Bremen)	14.0%	14.0%	17.6%	15.6%
(2) East (Berlin, Brandenburg, Meckl.-Vorp., Sachsen, Sachsen-Anhalt, Thueringen)	11.4%	11.4%	15.6%	18.2%
(3) South (Hessen, Baden-Wuertt., Bayern)	49.2%	49.2%	43.4%	38.4%
(4) West (Nordr.-Westf., Rheinl.-Pfalz, Saarl.)	25.5%	25.5%	23.4%	27.7%

Table 4: Establishment-level aggregate employee flows. The table reports estimated employment growth rates and coefficients θ between targets and controls in the buyout year (t=0) and subsequent years from regression (1). In every regression, we control for each of our matching cells based on two-digit industry, buyout year, *Establishment Age*, and *Establishment Size*. See Section 2.2 for further details. In addition, we control for pre-buyout growth $g_{j,t-3,t-1}$. The coefficient θ denotes the coefficient $\theta(g)$ if the dependent variable in equation (1) is employment growth, $\theta(s)$ if the dependent variable is the separation rate, and $\theta(h)$ when the dependent variable is the hiring rate (see Appendix A.3 for definitions of growth rates, separation rates, and hiring rates). The variables are defined in Table 1. Each reported coefficient is for a different semi-parametric, employment-weighted regression. For example, in "$t+2$", we report $\theta(g)$, which is calculated for the one-year growth rate $g_{j,t+1,t+2}$ from $t+1$ to $t+2$ following the buyout. In "t to $t+2$", we report the estimated differences from the beginning of the event year until the end of the second year after the event year. The number of observations is 26,567 (2,420 target establishments and 24,147 control establishments). $\theta(h)/\theta(s)$ denotes the ratio of the coefficients. In Panel B, we perform a sample split into public targets and private targets. Standard errors are clustered at the firm level and we present the corresponding t-statistics below the coefficient estimates. *, **, *** indicate significance at the 10%, 5% and 1% level respectively.

Dep. Variable:	(1) Empl. Growth Rate ($\theta(g)$)	(2) Separation Rate ($\theta(s)$)	(3) Hiring Rate ($\theta(h)$)	(4) $\theta(h)$ / $\theta(s)$	(5) Total Earnings Growth
Panel A. Growth rates and worker flows					
t	-0.0092	0.0229**	0.0137	60%	0.0018
	-0.65	2.25	1.48		0.12
t+1	-0.0053	0.0230**	0.0177**	77%	-0.0027
	-0.39	2.21	1.98		-0.21
t+2	-0.0050	0.0189***	0.0138**	73%	-0.0019
	-0.55	2.59	2.23		-0.21
t+3	-0.0353***	0.0422***	0.0069	16%	-0.0295**
	-3.02	3.83	1.23		-2.46
t+4	-0.0187	0.0341*	0.0154**	45%	-0.0237
	-0.94	1.85	2.17		-1.24
t+5	-0.0186	0.0298**	0.0112	38%	-0.0176
	-1.21	2.31	1.34		-1.17
t to t+2	-0.0186	0.0594***	0.0407**	68%	-0.0088
	-0.89	3.11	2.47		-0.41
t to t+5	-0.0896***	0.1875***	0.0979***	52%	-0.0787**
	-2.61	4.45	2.83		-2.14

Table 4: Establishment-level aggregate employee flows. (continued)

Period	(1) Empl. Growth Rate ($\theta(g)$)	(2) Separation Rate ($\theta(s)$)	(3) Hiring Rate ($\theta(h)$)	(4) $\theta(h)$ / $\theta(s)$	(5) Total Earnings Growth
Panel B. Sample split into pulic targets and private targets					
Public Targets (N=87)					
t to t+2	-0.0282	0.0581*	0.0299	52%	-0.0154
	-0.67	1.74	0.71		-0.37
t to t+5	-0.1376**	0.1605***	0.0229	14%	-0.1322**
	-2.26	2.98	0.34		-2.02
Private Targets (N=424)					
t to t+2	-0.0173	0.0599***	0.0426***	71%	-0.0081
	-0.73	2.69	2.61		-0.34
t to t+5	-0.0773*	0.1921***	0.1148***	60%	-0.0648
	-1.93	3.74	3.02		-1.52

Table 5: Establishment-level and group-specific employee flows from t to $t+2$. This table replicates the analysis of Table 4 for specific groups. Rates are calculated over period t to period $t+2$, i.e., growth rates are computed over a three-year period from the end of $t-1$ to the end of $t+2$. All variables are defined in Table 1. Wage terciles (low, medium, high) are based on *Daily Wage*. High/low splits are based on the median. Columns (1) to (3) report the coefficients θ from regression (1) with growth rates (column (1)), separation rates (column (2)) and hiring rates (column (3)) as the dependent variable. Columns (4) to (6) provide tests for differences between groups of employees. In Panel A, the test is for whether these groups are different from their complement, e.g., *White Collar* minus all non white-collar employees. In Panel B, the comparison is always for the difference between the highest and lowest quantile, e.g., *High wage* minus *Low wage*. The number of observations varies per group because observations are missing when the establishment does not have at least one employee of the respective group and one point in time. The maximum number of observations is 24,700 (*White Collar*) and the minimum number is 11,364 (*Manager*). Standard errors are clustered at the firm level. t-statistics are provided below the coefficient estimates. *, **, *** indicate significance at the 10%, 5% and 1% level, respectively.

	(1) Empl. Growth Rate ($\theta(g)$)	(2) Separation Rate ($\theta(s)$)	(3) Hiring Rate ($\theta(h)$)	(4) Empl. Growth Rate ($\theta(g)$)	(5) Separation Rate ($\theta(s)$)	(6) Hiring Rate ($\theta(h)$)
Panel A.		**from t to t+2**			**Group - all other employees**	
White Collar	-0.0468**	0.0930***	0.0462**	-0.0452	0.0534*	0.0078
	-2.03	3.92	2.47	-1.29	1.67	0.29
Manager	-0.0248	0.0725*	0.0477*	-0.0068	0.0137	0.0065
	-0.65	1.95	1.77	-0.16	0.33	0.21
IT-related Job	-0.0313	0.0636***	0.0323**	-0.0303	0.0100	-0.0202
	-1.52	3.03	2.12	-0.93	0.31	-0.73
IT-integrated job	0.0213	0.0453	0.0666**	0.0537*	-0.0163	0.0374
	0.99	1.58	2.37	1.72	-0.47	1.14
Panel B.		**from t to t+2**			**High - Low**	
Low wage	-0.0440	0.1160***	0.0720**	0.0424	-0.0800*	-0.0376
	-1.15	3.06	2.51	0.88	-1.74	-1.06
Medium wage	-0.0146	0.0604*	0.0458***			
	-0.40	1.91	2.70			
High wage	-0.0016	0.0360	0.0344			
	-0.06	1.38	1.63			
Low routine job	-0.0295	0.0768***	0.0473**	0.0152	-0.0310	-0.0157
	-1.39	3.57	2.43	0.48	-1.02	-0.62
High routine job	-0.0143	0.0458**	0.0316*			
	-0.61	2.15	1.92			
Low offshorable job	-0.0111	0.0576***	0.0465**	-0.0204	0.0119	-0.0084
	-0.54	2.62	2.42	-0.59	0.39	-0.30
High offshorable job	-0.0315	0.0695***	0.0381*			
	-1.14	3.25	1.92			
Young	-0.0090	0.0466**	0.0376**	-0.0147	0.0106	-0.0041
	-0.41	2.35	2.09	-0.46	0.37	-0.18
Old	-0.0237	0.0572***	0.0335**			
	-1.03	2.74	2.25			
Low tenure	-0.0220	0.0449*	0.0229	0.0353	-0.0461	-0.0108
	-0.73	1.78	1.12	0.85	-1.24	-0.47
High tenure	0.0133	-0.0012	0.0121			
	0.46	-0.04	1.14			

Table 6: Establishment-level and group-specific employee flows from t to $t+5$. This table replicates the analysis of Table 4 for specific groups. Rates are calculated over period t to period $t+5$, i.e., growth rates are computed over a six-year period from the end of $t-1$ to the end of $t+5$. All variables are defined in Table 1. Wage terciles (low, medium, high) are based on *Daily Wage*. High/low splits are based on the median. Columns (1) to (3) report the coefficients θ from regression (1) with growth rates (column (1)), separation rates (column (2)) and hiring rates (column (3)) as the dependent variable. Columns (4) to (6) provide tests for differences between groups of employees. In Panel A, the test is for whether these groups are different from their complement, e.g., *White Collar* minus all non white-collar employees. In Panel B, the comparison is always for the difference between the highest and lowest quantile, e.g., *High wage* minus *Low wage*. The number of observations varies per group because observations are missing when the establishment does not have at least one employee of the respective group and one point in time. The maximum number of observations is 24,700 (*White Collar*) and the minimum number is 11,364 (*Manager*). Standard errors are clustered at the firm level. t-statistics are provided below the coefficient estimates. *, **, *** indicate significance at the 10%, 5% and 1% level, respectively.

	(1) Empl. Growth Rate ($\theta(g)$)	(2) Separation Rate ($\theta(s)$)	(3) Hiring Rate ($\theta(h)$)	(4) Empl. Growth Rate ($\theta(g)$)	(5) Separation Rate ($\theta(s)$)	(6) Hiring Rate ($\theta(h)$)
Panel A.		**from t to t+5**			**Group - all other employees**	
White Collar	-0.1182**	0.2013***	0.0831**	-0.0586	0.0477	-0.0109
	-2.38	3.57	2.17	-0.94	0.64	-0.20
Manager	-0.0266	0.0809	0.0543	0.0590	-0.1023	-0.0433
	-0.32	0.69	0.97	0.66	-0.81	-0.65
IT-related Job	-0.0819**	0.1521***	0.0702**	0.0002	-0.0701	-0.0698
	-1.96	2.92	2.28	0.00	-0.82	-1.15
IT-integrated Job	-0.0341	0.1877*	0.1536**	0.0674	0.0119	0.0793
	-0.70	1.94	2.29	1.08	0.11	1.03
Panel B.		**from t to t+5**			**High - Low**	
Low wage	-0.1276***	0.3393***	0.2117***	0.0660	-0.2128**	-0.1468*
	-2.95	4.57	2.63	1.00	-2.43	-1.73
Medium wage	-0.0949*	0.0449***	0.0211***			
	-1.92	3.49	2.89			
High wage	-0.0616	0.1265***	0.0649**			
	-1.23	2.73	2.38			
Low routine job	-0.0977**	0.1979***	0.1002***	0.0335	-0.0651	-0.0316
	-2.24	3.96	2.79	0.56	-0.99	-0.68
High routine job	-0.0642	0.1328***	0.0686**			
	-1.59	3.09	2.31			
Low offshorable job	-0.0934**	0.2079***	0.1145***	0.0342	-0.0900	-0.0558
	-2.26	3.89	2.86	0.55	-1.30	-1.11
High offshorable job	-0.0592	0.1179***	0.0587*			
	-1.29	2.68	1.94			
Young	-0.0757**	0.1705***	0.0948**	-0.0157	-0.0030	-0.0187
	-2.06	3.60	2.17	-0.29	-0.05	-0.38
Old	-0.0914**	0.1675***	0.0761***			
	-2.36	4.00	3.33			
Low tenure	-0.1140***	0.2069***	0.0929*	0.1195*	-0.1950***	-0.0755
	-2.62	3.74	1.74	1.94	-2.82	-1.38
High tenure	0.0055	0.0119	0.0174			
	0.13	0.29	1.41			

Table 7: Individual-level analyses of employee characteristics. The table presents OLS-regressions of *Earnings*, *Daily Wage*, and *Days Employed* in a triple-difference setup from equation (3). The dependent variables are in logarithms in columns (2), (4), and (6). Each specification includes a risk factor, which is measured in the year prior to the buyout announcement. We only report the estimates of η_2 and η_5. In Panel A, the risk factors are *Manager* (regression (1)) and *White Collar* (regression (2)). In Panel B, we analyzes wages by entering two risk factors in (3), *Low Wage* and *Medium Wage,* which denote the first and second tercile of *Daily Wage*, respectively. In Panel C, the risk factors are *Routine Job* (regression (1)) and *Offshorable Job* (regression (2)). In Panel D, the risk factors are *IT-related Job* and *IT-integrated Job.* In Panel E the risk factors are *Old* (regression (1)), an indicator set equal to 1 if an employee's age is above the median sample age, and *High Tenure* (regression (2)), an indicator set equal to 1 if an employee's *Firm Tenure* is above the sample median. The numerical variables are defined in Table 1 and the categorical variables are defined in Table 3. Each specification contains individual and year fixed effects. The number of observations for *Earnings* and *Days Employed* is 2,128,798 = 152,057 Target Employees x 2 (Control Employees) x 7 (event years). The number of observations for *Daily Wage* is 1,929,354. Standard errors are clustered at the firm level. t-statistics are provided below the coefficient estimates. *, **, *** indicate significance at the 10%, 5% and 1% level, respectively.

Dependent Variable:	(1) Earnings (Euro)	(2) Earnings (ln)	(3) Daily Wage (Euro)	(4) Daily Wage (ln)	(5) Days Empl. (days)	(6) Days Empl. (ln)
Panel A. Organizational streamlining						
Regression (1): Managers versus all others						
D_{i2} x Target x Manager	-965.67*	-0.071	-0.81	-0.007	-2.38	-0.028
	-1.76	-0.97	-0.80	-0.82	-0.88	-0.70
D_{i5} x Target x Manager	-2019.40***	-0.294**	-0.32	-0.006	-8.36*	-0.144**
	-2.59	-2.36	-0.36	-0.61	-1.91	-2.12
Regression (2): White collar employees versus all others						
D_{i2} x Target x White Collar	-455.01*	-0.123***	-0.05	-0.007	-4.35**	-0.065*
	-1.67	-3.11	-0.09	-0.94	-2.26	-1.90
D_{i5} x Target x White Collar	-701.89*	-0.151**	0.07	-0.010	-5.40**	-0.080*
	-1.95	-2.22	0.10	-0.99	-2.16	-1.73
Panel B. Technological change						
Regression (1): Wage terciles						
D_{i2} x Target x Low wage	975.53***	0.005	0.92	0.010	1.01	-0.007
	2.78	0.09	1.37	1.17	0.39	-0.16
D_{i5} x Target x Low wage	1258.87**	0.018	2.04*	0.016	-0.52	-0.009
	2.01	0.15	1.77	1.35	-0.13	-0.13
D_{i2} x Target x Medium wage	698.65***	0.075**	0.56	0.005	1.71	0.030
	2.97	2.18	0.93	0.92	1.41	1.59
D_{i5} x Target x Medium wage	490.58	0.021	0.62	0.005	-0.34	0.002
	1.36	0.32	0.74	0.75	-0.15	0.06

Table 7: Individual-level analyses of employee characteristics (continued).

Dependent Variable:	(1) Earnings (Euro)	(2) Earnings (ln)	(3) Daily Wage (Euro)	(4) Daily Wage (ln)	(5) Days Empl. (days)	(6) Days Empl. (ln)
Panel C. Routinization and offshorability						
Regression (1): Employees with a routine job versus all others						
D_{i2} x Target x Routine Job	459.79**	0.053*	-0.08	-0.002	2.57*	0.033
	2.27	1.93	-0.15	-0.30	1.93	1.52
D_{i5} x Target x Routine Job	586.16**	0.067	-0.12	-0.003	2.30	0.030
	2.09	1.47	-0.20	-0.32	1.33	1.00
Regression (2): Employees with an offshorable job versus all others						
D_{i2} x Target x Offshorable Job	-229.39	-0.020	-0.15	0.002	-0.57	-0.009
	-0.86	-0.52	-0.28	0.23	-0.37	-0.33
D_{i5} x Target x Offshorable Job	-50.28	0.010	0.14	0.001	0.37	0.014
	-0.15	0.14	0.20	0.06	0.16	0.34
Panel D. IT Expertise						
Regression (1): Employees in jobs with above median use of digital tools						
D_{i2} x Target x IT-related Job	-145.44	-0.045	0.25	-0.004	-1.95	-0.023
	-0.75	-1.56	0.54	-0.64	-1.28	-0.97
D_{i5} x Target x IT-related Job	-280.20	-0.018	-0.06	-0.008	-1.07	-0.006
	-1.03	-0.36	-0.10	-1.13	-0.56	-0.19
Regression (2): Employees in jobs with above median use of IT-integrated tools						
D_{i2} x Target x IT-integrated Job	286.75	0.034	0.26	0.005	1.16	0.009
	1.30	1.09	0.55	0.78	0.89	0.45
D_{i5} x Target x IT-integrated Job	175.55	0.031	-0.10	0.005	1.35	0.018
	0.54	0.53	-0.18	0.66	0.64	0.54
Panel E. Transfer of wealth						
Regression (1): Employees with above median age versus all others						
D_{i2} x Target x Old	-696.40***	-0.098***	-0.73**	-0.008	-4.00***	-0.050**
	-3.92	-3.21	-2.50	-1.44	-3.17	-2.53
D_{i5} x Target x Old	-807.04**	-0.184***	-1.15***	-0.018**	-6.19**	-0.106***
	-2.44	-2.61	-2.61	-2.10	-2.54	-2.71
Regression (2): Employees with above median firm tenure versus all others						
D_{i2} x Target x High Tenure	-127.26	0.031	-0.30	-0.002	0.35	0.019
	-0.50	0.79	-0.57	-0.38	0.20	0.68
D_{i5} x Target x High Tenure	-413.22	-0.019	-1.05	-0.009	-1.14	-0.014
	-0.94	-0.20	-1.34	-1.02	-0.35	-0.26

Table 8: Individual-level analyses of stayers and leavers. The table presents OLS-regressions of *Earnings*, *Daily Wage*, and *Days Employed* in a triple-difference setup from equation (3). Each specification includes an indicator variable *Leaver*, which is one if the employee leaves the target establishment at some point between t and $t+5$. We only report the estimates of γ_5, θ_5, λ_5 and η_5. In Panel A, we report the results for regressions of *Earnings*, *Daily Wage*, and *Days Employed* and their logarithmic transformations for the whole sample. In Panel B, we report the results for regressions of *Earnings* for subsamples of employees. The numerical variables are defined in Table 1 and the categorical variables are defined in Table 3. *Low Wage* and *High Wage* denote the first and third tercile of *Daily Wage*, respectively. Each specification includes individual and year fixed effects. Standard errors are clustered at the firm level. t-statistics are provided below the coefficient estimates. *, **, *** indicate significance at the 10%, 5% and 1% level, respectively.

Panel A. Full sample						
	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable:	Earnings (Euro)	Earnings (ln)	Daily Wage (Euro)	Daily Wage (ln)	Days Empl. (days)	Days Empl. (ln)
D_{i5}	5,730.03***	0.176***	16.41***	0.149***	-2.67**	0.024
	32.15	6.37	43.12	41.23	-2.46	1.54
D_{i5} x Target	75.17	0.000	0.33	0.003	-0.63	-0.003
	0.24	-0.01	0.39	0.52	-0.99	-0.47
D_{i5} x Leaver	-12,786.66***	-2.921***	-6.13***	-0.077***	-100.63***	-1.642***
	-41.28	-62.43	-18.70	-20.19	-61.11	-65.48
D_{i5} x Target x Leaver	-1,659.08**	-0.380***	-1.67*	-0.0260**	-12.84***	-0.207***
	-2.26	-3.26	-1.73	-2.40	-3.21	-3.24

Panel B. Subsamples, dependent variable: Earnings (Euro)						
	(1)	(2)	(3)	(4)	(5)	(6)
Subsample:	White Collar	Manager	Young	Old	Low wage	High wage
D_{i5}	6,137.76***	7,301.13***	4,106.66***	6,944.50***	4,468.62***	6,653.27***
	34.19	13.58	21.10	42.35	32.19	24.27
D_{i5} x Target	150.56	-186.21	11.77	145.27	440.38**	-156.32
	0.48	-0.37	0.04	0.39	1.97	-0.32
D_{i5} x Leaver	-11,829.58***	-21,631.17***	-20,512.65***	-6,348.54***	-5,724.72***	-20,094.56***
	-44.49	-26.96	-46.26	-34.13	-43.89	-31.74
D_{i5} x Target x Leaver	-2,120.96**	-3,715.67***	-1,566.16*	-1,302.55**	-1,213.44***	-2,764.20**
	-2.54	-2.82	-1.80	-2.54	-2.93	-2.44
Number of obs.	609,504	57,190	1,024,373	1,104,425	709,499	709,597

Online Appendix for "Private Equity and Human Capital Risk"

November 9, 2018

This Internet Appendix provides additional information on the construction of the data set for our paper "Private Equity and Human Capital Risk". The discussion can be found in the main text of the paper and the tables in the Online Appendix are referred to as A-#, where # is the table number in the appendix.

Table A-1: Construction of data set. The table describes the steps from the initial list of private equity buyouts to the individual-level matched data set, which we consider in our analyses.

Step	Number of observations
1. Select Transactions from Thomson One, Capital IQ, and Bundesverband der Kapitalanlagegesellschaften	891 Transactions
2. Exclude secondary buyouts	798 Transactions
3. Record linkage of transactions to establishments at transaction announcement date	544 Transactions 2,652 Establishments
4. Selection of employees, for which we have all key variables	541 Transactions 2,597 Establishments 209,345 Target Employees
5. Keep deal if deal has at least 10 employees	513 Transactions 2,563 Establishments 208,449 Target Employees
6. Keep matched employees	511 Transactions 2,420 Establishments 152,057 Target Employees

Table A-2: Matching algorithm. The table presents the categories and dates on which we match target employees to control employees. t denotes event time where $t = 0$ indicates the announcement year.

Dimension	Matching Date	Categories
Education	t=-1	(1) Secondary school leaving certificate without completed vocational training, (2) Secondary school leaving certificate with vocational training or upper secondary school leaving certificate without vocational training, (3) Upper secondary school leaving certificate with vocational training, (4) College or university degree
Empl. status	t=-1, t=0	(1) Employed all year, (2) employed parts of the year, (3) not employed at all
Experience	t=-1	Number of days in employment during last 10 years (quintiles)
Gender	t=-1	(1) male (2) female
Industry	t=-1	1 Agriculture/forestry, (2) Fishing/aquaculture, (3) Mining/quarrying, (4) Manufacturing, (5) Energy and water supply, (6) Construction, (7) Wholesale and retail trade; repair of motor vehicles and durable goods, (8) Accommodation and food service activities, (9) Transportation/communication, (10) Financial and insurance activities, (11) Real estate activities, (12) Public administration/defence/compulsory social security, (13) Education, (14) Health and social work activities, (15) Other service activities, (16) Activities
Nationality	t=-1	(1) German, (2) Immigrant (Greece, Italy, Turkey, former Yugoslavia countries), (3) Rest of the World
Occupation	t=-1	(1) Vocational training, (2) Full-time employment, (3) Part-time employment, (4) Home worker of households as employers, (17) Activities of extraterritorial organisations and bodies
Qualification	t=-1	(1) Simple manual occupations, (2) Skilled manual occupations, (3) Technicians/Engineers, (4) Simple service, (5) Qualified service, (6) Semi-professions, (7) Professions, (8) Simple commercial and administrative occupations, (9) Qualified commercial and administrative occupations, (10) Managers
Region	t=-1	(1) South (Hessen, Baden-Wuerttemberg, Bayern), (2) West (Nordrhein-Westfalen, Rheinland-Pfalz, Saarland), (3) East (Berlin, Brandenburg, Mecklenburg-Vorpommern, Sachsen, Sachsen-Anhalt, Thueringen), (4) North (Schleswig-Holstein, Hamburg, Niedersachsen, Bremen)

Table A-3: Deal characteristics. This table presents descriptive statistics on the 511 deals that we consider in our empirical analyses. "Credit score" and "Business assessment" are derived from data compiled by Creditreform and provided by the Mannheim Company Panel (MCP). Creditreform provides companies with credit advice about their suppliers, dealers, and competitors. Creditreform maintains a credit-worthiness index with values from 0 to 600. We categorize credit scores into "very good" (0-199), "good" (200-299), "moderate" (300-399), "critical" (400-499), and "very critical" (above 500). "Business assessment" is based on survey data and integrates firm financials, feedback from suppliers and customers, as well as other information obtained by Creditreform. Note that credits scores and business assessments are available for only about half of our sample. "Industry" is based on the 1-digit industry category scheme used by the Institute for Employment Research in Nuremberg.

Category	**N**	**in %**
Credit score	231	100.0%
Very good	80	34.6%
Good	136	58.9%
Moderate	12	5.2%
Very critical	3	1.3%
Business assessment	233	100.0%
Expanding	5	2.1%
Positive	53	22.7%
Constant	118	50.6%
Stagnating	12	5.2%
Declining	3	1.3%
Uncertain	42	18.0%
Industry	511	100.0%
Manufacturing	276	54.0%
Real estate	93	18.2%
Trade, maintenance and repairs	75	14.7%
Communication and transport	23	4.5%
Construction	15	2.9%
Other services	9	1.8%
Financial services	8	1.6%
Other industries	12	2.3%

Table A-4: Decomposition of establishment growth and worker flows - Worker influence subsamples. This table replicates the analysis of Table 4 from period t to period $t+5$. The three subsamples are based on the total number of employees associated with a deal at t.

Dependent Variable:	Growth Rate ($\theta(g)$)	Separation Rate ($\theta(s)$)	Hiring Rate ($\theta(h)$)	$\theta(h)$ / $\theta(s)$
PE Buyouts (N=22) with more than 2000 employees				
t to t+5	-0.1004	0.2184**	0.1180*	54%
	-1.50	2.51	1.56	
PE Buyouts (N=82) with more than 500 employees and less than 2000 employees				
t to t+5	-0.0652	0.1360**	0.0708*	51%
	-1.22	2.30	1.71	
PE Buyouts (N=407) with less than 500 employees				
t to t+5	-0.1008**	0.1956***	0.0944***	48%
	-2.31	4.88	3.60	

Table A-5: Establishment matching success. This table presents descriptive statistics on target establishments and control establishments. To each target establishment, we match up to 10 control establishments. On average, we match 9.98 control establishments to one target establishment. All variables are measured in the year of the private equity buyout announcement. The Imbens-Wooldridge statistic measures the normalized difference between two variables. The test divides the difference between two variables by the square root of the sum of their variances. As a rule of thumb, a test statistic exceeding 0.25 indicates that the analysis tends to be sensitive to the specification.

	Establishment Size	Establishment Age	Establishment Wage	Highly skilled	Medium Skilled	Full-time employees	Female employees	Employee age
Panel A. Matched buyout establishments, N = 2,420								
Mean	91.0	10.8	85.5	73.2%	11.0%	68.5%	42.2%	37.6
Median	23.0	8.0	77.8	76.8%	3.5%	85.2%	30.0%	37.7
Variance	232	9.6	32	22.1%	18.6%	33.1%	34.9%	6.4
Panel B. Matched control establishments, N = 24,147								
Mean	79.5	10.9	83.1	73.9%	9.6%	62.4%	42.9%	38.1
Median	22.0	8.0	78.2	78.2%	2.8%	80.0%	30.3%	38.3
Variance	196	9.6	30	21.0%	17.6%	36.0%	33.8%	6.1
Comparison to matched buyout establishments:								
Relative difference	13.5%	-1.6%	2.8%	-0.9%	13.9%	9.4%	-1.8%	-1.1%
Imbens-Wooldridge ststistic	0.04	0.01	0.05	0.02	0.06	0.13	0.02	0.05

Table A-6: Individual matching success. This table presents descriptive statistics on target employees, control employees, and matched target employees. All variables are measured in the year prior to the private equity buyout announcement. The Imbens-Wooldridge statistic measures the normalized difference between two variables. The test divides the difference between two variables by the square root of the sum of their variances. As a rule of thumb, a test statistic exceeding 0.25 indicates that the analysis tends to be sensitive to the specification.

	Earnings	Daily Wage	Fraction Employed	Tenure	Age	Establishment Size
Panel A. Matched target employees, N = 152,057						
Mean	35,986	99.58	0.98	3,351	41.12	778.34
Median	35,168	96.74	1.00	2,375	41.00	354.00
Variance	2.E+08	1606.61	0.01	9.E+06	110.70	2.E+06
Panel B. Matched control employees, N = 152,057						
Mean	35,884	99.29	0.97	3,338	40.94	736.89
Median	35,095	96.57	1.00	2,374	41.00	326.00
Variance	2.E+08	1605.29	0.01	9.E+06	108.36	1.E+06
Comparison to Matched target employees:						
Relative difference	0.28%	0.29%	0.02%	0.39%	0.42%	5.47%
Imbens-Wooldridge statistic	0.00	0.01	0.00	0.00	0.01	0.02
Panel C. Unmatched target employees, N = 56,392						
Mean	13,283	54.35	0.50	901	32.37	499.22
Median	8,012	41.51	0.50	365	29.00	182.00
Variance	2.E+08	1884.73	0.19	2.E+06	166.06	1.E+06
Comparison to Matched target employees:						
Imbens-Wooldridge statistic	1.04	0.77	1.05	0.74	0.53	0.16
Panel D. Unmatched part-time target employees, N = 16,063						
Mean	7,595	33.28	0.58	900	34.69	391.92
Median	2,799	22.51	0.70	275	31.00	130.00
Variance	1.E+08	1106.89	0.17	3.E+06	204.53	9.E+05
Comparison to Matched target employees:						
Imbens-Wooldridge statistic	1.50	1.27	0.92	0.73	0.36	0.25

Table A-7: Public vs. private targets. The table presents OLS-regressions of *Earnings*, *Daily Wage*, and *Days Employed* in a triple-difference setup as in equation (3) in the paper. The dependent variables are in logarithms in columns (2), (4), and (6). In the specification shown the risk factor is "Public Target" and denotes all target employees who work in a publicly listed target. The numerical variables are defined in Table 2 and the categorical variables are defined in Table 3 in the paper. Each specification contains individual and year fixed effects. The number of observations for *Earnings* and *Days Employed* is 912,342 = 152,057 Target Employees x 2 (Control Employees) x 3 (event years). The number of observations for *Daily Wage* is 821,608. Standard errors are clustered at the firm level. t-statistics are provided below the coefficient estimates. *, **, *** indicate significance at the 10%, 5% and 1% level, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable:	Earnings (Euro)	Earnings (ln)	Daily Wage (Euro)	Daily Wage (ln)	Days Empl. (days)	Days Empl. (ln)
Regression (1): Employees associated with public targets						
D_{i2} x Target x Public target	511.38	0.023	1.54	0.012	0.06	0.007
	0.83	0.25	1.22	1.13	0.02	0.13
D_{i5} x Target x Public target	-540.41	0.022	-1.64	-0.012	0.51	0.019
	-0.51	0.12	-0.66	-0.64	0.08	0.18

Table A-8: Private equity and human capital - baseline. The table presents OLS-regressions of *Earnings*, *Daily Wage*, and *Days Employed* in a difference-in-difference setup as in equation (2). Standard errors are clustered at the firm level. t-statistics are provided in parentheses. *, **, *** indicate significance at the 10%, 5% and 1% level respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable:	Earnings	Earnings	Daily Wage	Daily Wage	Days Empl.	Days Empl.
	(Euro)	(ln)	(Euro)	(ln)	(days)	(ln)
D_{i-5} x Target	41.33	-0.006	0.21	0.002	0.55	0.004
	0.21	-0.08	0.54	0.30	0.17	0.07
D_{i-4} x Target	19.27	-0.012	0.06	0.000	0.42	0.000
	0.11	-0.22	0.15	-0.03	0.16	-0.01
D_{i-3} x Target	-130.48	-0.023	-0.23	-0.003	-0.32	-0.011
	-0.98	-0.59	-0.69	-0.80	-0.17	-0.35
D_{i-2} x Target	-105.00	-0.020	-0.32	-0.0039*	-0.29	-0.012
	-1.20	-0.89	-1.43	-1.69	-0.24	-0.67
D_{i0} x Target	49.77	0.000	0.23	0.003	-0.05	0.001
	0.49	-0.02	0.87	0.95	-0.11	0.10
D_{i1} x Target	-102.74	-0.0612**	0.41	0.003	-2.3804**	-0.0386**
	-0.52	-2.16	0.94	0.67	-2.17	-2.07
D_{i2} x Target	-372.43	-0.1551***	0.49	0.004	-5.3387***	-0.0879***
	-1.38	-3.08	1.03	0.74	-2.90	-2.83
D_{i3} x Target	-597.1128*	-0.1855***	0.26	0.001	-7.0898***	-0.1077***
	-1.79	-2.72	0.55	0.18	-2.93	-2.67
D_{i4} x Target	-971.1415**	-0.2272***	-0.58	-0.009	-8.2623***	-0.1269***
	-2.51	-2.69	-1.04	-1.41	-2.85	-2.61
D_{i5} x Target	-979.3816**	-0.2414**	-0.32	-0.007	-8.8330***	-0.1360**
	-2.14	-2.47	-0.50	-1.03	-2.76	-2.47
N	3,345,254	3,345,254	3,071,118	3,071,118	3,345,254	3,345,254
R^2	0.07	0.04	0.28	0.13	0.05	0.05

Table A-9: Individual-level analyses of employee characteristics. The table presents OLS-regressions of *Earnings*, *Daily Wage*, and *Days Employed* in a triple-difference setup from equation 2. The dependent variables are in logarithms in columns (2), (4), and (6). Each specification includes a risk factor, which is measured in the year prior to the buyout announcement. The numerical variables are defined in Table 3 and the categorical variables are defined in Table 2. Each specification contains individual and year fixed effects. The number of observations for *Earnings* and *Days Employed* is 2,128,798 = 152,057 Target Employees x 2 (Control Employees) x 7 (event years). The number of observations for *Daily Wage* is 1,929,354. Standard errors are clustered at the firm level. t-statistics are provided below the coefficient estimates. *, **, *** indicate significance at the 10%, 5% and 1% level, respectively.

Table A-9: Individual-level analyses of employee characteristics. (continued)

Dependent Variable:	(1) Earnings (Euro)	(2) Earnings (ln)	(3) Daily Wage (Euro)	(4) Daily Wage (ln)	(5) Days Empl. (days)	(6) Days Empl. (ln)
Panel B.						
Regression (1): Managers versus all others						
D_{i0} x Target x Manager	-115.64	-0.006	-0.01	-0.001	-0.62	-0.005
	-0.36	-0.25	-0.01	-0.17	-0.45	-0.33
D_{i1} x Target x Manager	-529.23	-0.023	-0.01	-0.001	-2.33	-0.011
	-1.14	-0.40	-0.01	-0.12	-1.04	-0.36
D_{i2} x Target x Manager	-965.67*	-0.071	-0.81	-0.007	-2.38	-0.028
	-1.76	-0.97	-0.80	-0.82	-0.88	-0.70
D_{i3} x Target x Manager	-1344.87**	-0.140	-0.61	-0.009	-3.86	-0.061
	-2.15	-1.55	-0.65	-1.08	-1.21	-1.27
D_{i4} x Target x Manager	-1477.50**	-0.104	0.02	0.001	-5.56	-0.060
	-2.16	-0.97	0.02	0.06	-1.51	-1.03
D_{i5} x Target x Manager	-2019.40***	-0.294**	-0.32	-0.006	-8.36*	-0.144**
	-2.59	-2.36	-0.36	-0.61	-1.91	-2.12
Regression (2): White collar employees versus all others						
D_{i0} x Target x White Collar	-29.10	-0.009	0.01	-0.005	-0.20	-0.001
	-0.19	-0.59	0.02	-1.21	-0.24	-0.06
D_{i1} x Target x White Collar	-210.08	-0.050	0.14	-0.007	-3.10**	-0.030
	-0.91	-1.55	0.27	-1.20	-2.22	-1.09
D_{i2} x Target x White Collar	-455.01*	-0.123***	-0.05	-0.007	-4.35**	-0.065*
	-1.67	-3.11	-0.09	-0.94	-2.26	-1.90
D_{i3} x Target x White Collar	-509.30*	-0.123**	0.17	-0.009	-4.93**	-0.064*
	-1.74	-2.45	0.31	-0.99	-2.34	-1.69
D_{i4} x Target x White Collar	-434.44	-0.117*	0.52	-0.005	-4.46*	-0.062
	-1.39	-1.96	0.81	-0.54	-1.86	-1.45
D_{i5} x Target x White Collar	-701.89*	-0.151**	0.07	-0.010	-5.40**	-0.080*
	-1.95	-2.22	0.10	-0.99	-2.16	-1.73
Regression (3): Employees with above median age versus all others						
D_{i0} x Target x Old	-15.54	-0.004	0.03	0.000	-0.25	-0.003
	-0.17	-0.29	0.15	0.06	-0.34	-0.33
D_{i1} x Target x Old	-522.18***	-0.036*	-0.67***	-0.007	-2.69***	-0.018
	-3.86	-1.82	-2.82	-1.56	-2.76	-1.16
D_{i2} x Target x Old	-696.40***	-0.098***	-0.73**	-0.008	-4.00***	-0.050**
	-3.92	-3.21	-2.50	-1.44	-3.17	-2.53
D_{i3} x Target x Old	-723.87***	-0.133***	-0.74**	-0.012*	-5.68***	-0.079***
	-2.85	-2.72	-2.21	-1.73	-3.17	-2.81
D_{i4} x Target x Old	-804.55**	-0.194***	-0.84**	-0.012	-6.24***	-0.109***
	-2.5538	-2.81	-2.13	-1.56	-2.61	-2.80
D_{i5} x Target x Old	-807.04**	-0.184**	-1.15**	-0.018**	-6.19**	-0.106**
	-2.1006	-2.12	-2.46	-2.02	-2.10	-2.17

Table A-10: Alternative measures of skill. This table replicates the analyses of Panel B of Table 5 (in Panel A) and of Panel B of Table 7 (in Panel B) for alternative measures of skill. Skill is based on on the ten qualification categories presented in Table 3. "Low skill" is based on categories 1, 4, 8 and and "medium skill" is based on categories 2, 5, 6, 9. The categorization of education into low, medium, and high is defined in Table 3. Standard errors are clustered at the firm level. t-statistics are provided below the coefficient estimates. *, **, *** indicate significance at the 10%, 5% and 1% level, respectively.

Panel A.	(1) Empl. Growth Rate ($\theta(g)$)	(2) Separation Rate ($\theta(s)$)	(3) Hiring Rate ($\theta(h)$)	(4) Empl. Growth Rate ($\theta(g)$)	(5) Separation Rate ($\theta(s)$)	(6) Hiring Rate ($\theta(h)$)
	from t=-1 to t=+2			**High-Low**		
Low skill	-0.0148	0.076***	0.0612**	0.0040	-0.0304	-0.0260
	-0.55	2.70	2.44	0.10	-0.78	-0.75
Medium skill	-0.0268	0.0488***	0.0220			
	-1.43	2.78	1.58			
High skill	-0.0108	0.0456*	0.0352			
	-0.34	1.70	1.45			
Low education	-0.0328	0.0828***	0.0496*	-0.0072	-0.0020	-0.0088
	-1.15	2.74	1.86	-0.18	-0.05	-0.26
Medium education	-0.0128	0.0520***	0.0392**			
	-0.57	2.62	2.32			
High education	-0.0400	0.0808***	0.0408**			
	-1.48	3.33	1.99			
	from t=-1 to t=+5			**High-Low**		
Low skill	-0.0944**	0.242***	0.1476**	-0.0236	-0.0412	-0.0644
	-2.08	3.83	2.55	-0.32	-0.46	-0.92
Medium skill	-0.0764**	0.1328***	0.056**			
	-2.15	3.07	2.04			
High skill	-0.1180**	0.2008***	0.0832**			
	-2.01	3.11	2.09			
Low education	-0.1136***	0.2628***	0.1492**	-0.0056	-0.0564	-0.0624
	-2.86	3.70	2.28	-0.09	-0.67	-0.84
Medium education	-0.0856***	0.1808***	0.0952**			
	-2.43	4.38	2.80			
High education	-0.1192***	0.2064***	0.0868**			
	-2.63	4.50	2.53			

Table A-10: Alternative measures of skill (continued).

Panel B. Triple Diff analysis

Dependent Variable:	(1) Earnings (Euro)	(2) Earnings (ln)	(3) Daily Wage (Euro)	(4) Daily Wage (ln)	(5) Days Empl. (days)	(6) Days Empl. (ln)
Regression (1): Education groups						
D_{i2} x Target x Low education	459.77	-0.003	-0.38	-0.003	0.73	-0.004
	1.26	-0.05	-0.52	-0.12	0.27	-0.07
D_{i5} x Target x Low education	396.23	-0.052	-0.29	-0.004	-2.46	-0.036
	0.89	-0.64	-0.32	-0.13	-0.79	-0.59
D_{i2} x Target x Medium education	634.08**	0.036	0.23	0.002	2.01	0.018
	2.09	1.07	0.30	0.36	1.53	1.02
D_{i5} x Target x Medium education	426.76	0.032	-0.63	-0.007	0.54	0.010
	1.24	0.55	-0.90	-1.15	0.26	0.33
Regression (2): Skill terciles						
D_{i2} x Target x Low skill	224.22	-0.018	-0.34	-0.004	-0.18	-0.011
	0.69	-0.43	-0.53	-0.53	-0.11	-0.37
D_{i5} x Target x Low skill	253.92	-0.056	0.23	-0.004	-3.03	-0.042
	0.54	-0.75	0.24	-0.38	-1.11	-0.91
D_{i2} x Target x Medium skill	234.75	-0.001	0.28	0.003	0.06	0.000
	0.76	-0.04	0.42	0.59	0.05	-0.02
D_{i5} x Target x Medium skill	130.93	-0.025	0.44	-0.001	-2.01	-0.019
	0.31	-0.44	0.58	-0.08	-1.01	-0.65

Table A-11: Individual-level analyses of stayers and leavers. The table presents OLS-regressions of *Earnings*, *Daily Wage*, and *Days Employed* in a triple-difference setup from equation (3). Each specification includes an indicator variable *Leaver*, which is one if the employee leaves the target establishment at some point between t and $t+2$. We only report the estimates of γ_2, θ_2, λ_2 and η_2. In Panel A, we report the results for regressions of *Earnings*, *Daily Wage*, and *Days Employed* and their logarithmic transformations for the whole sample. In Panel B, we report thre results for regressions of *Earnings* forsubsamples of employees. The numerical variables are defined in Table 1 and the categorical variables are defined in Table 3. *Low Wage* and *High Wage* denote the first and third tercile of *Daily Wage*, respectively. Each specification includes individual and year fixed effects. Standard errors are clustered at the firm level. t-statistics are provided below the coefficient estimates. *, **, *** indicate significance at the 10%, 5% and 1% level, respectively.

Panel A. Full sample						
	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable:	Earnings (Euro)	Earnings (ln)	Daily Wage (Euro)	Daily Wage (ln)	Days Empl. (days)	Days Empl. (ln)
D_{i2}	3297.25***	0.066***	8.98***	0.083***	-0.782*	-0.016***
	40.07	6.21	43.07	41.04	-1.80	-2.68
D_{i2} x Target	206.58	-0.008	0.70	0.007*	-0.53	-0.010**
	0.99	-0.97	1.29	1.66	-1.58	-2.54
D_{i2} x Leaver	-6596.68***	-1.271***	-4.27***	-0.057***	-56.116***	-0.751***
	-41.65	-56.86	-23.18	-22.88	-60.50	-58.86
D_{i2} x Target x Leaver	-939.02*	-0.266***	-0.35	-0.006	-7.713**	-0.136***
	-1.94	-3.30	-0.64	-0.90	-2.46	-2.85

Panel B. Subsamples, dependent variable: Earnings (Euro)						
	(1)	(2)	(3)	(4)	(5)	(6)
Subsample:	White Collar	Manager	Young	Old	Low wage	High wage
D_{i2}	3314.30***	4343.06***	2814.77***	3931.10***	2911.86***	3687.90***
	32.26	14.06	27.62	44.65	35.19	27.36
D_{i2} x Target	189.56	-189.08	157.29	263.41	395.32**	82.76
	0.88	-0.71	0.76	1.23	2.26	0.31
D_{i2} x Leaver	-5989.46***	-11259.52***	-9873.58***	-3845.92***	-3832.75***	-9199.47***
	-37.77	-19.26	-46.85	-34.22	-41.03	-31.61
D_{i2} x Target x Leaver	-1192.90**	-1353.47	-1275.92**	-468.92	-531.25*	-2229.27***
	-2.00	-1.44	-2.16	-1.53	-1.69	-3.12
Number of obs.	609,504	57,190	1,024,373	1,104,425	709,499	709,597

Table A-12: Deal level clustering. This table replicates the analyses of Panel A of Table 4 (Panel A), and of Panel A and of Regression (1) of Panel E of Table 7 (Panel B). For details on deal-level clustering, see Appendix A.3 in the paper. Standard errors are clustered at the firm level. t-statistics are provided below the coefficient estimates. *, **, *** indicate significance at the 10%, 5% and 1% level, respectively.

	(1)	(2)	(3)	(4)	(5)
Dep. Variable:	Growth Rate ($\theta(g)$)	Separation Rate ($\theta(s)$)	Hiring Rate ($\theta(h)$)	$\theta(h)$ / $\theta(s)$	Total Earnings Growth
Panel A. Growth rates and worker flows					
t	-0.0092	0.0229**	0.0137	60%	0.0018
	-0.65	2.24	1.48		0.12
t+1	-0.0053	0.0230**	0.0177**	77%	-0.0027
	-0.39	2.17	1.96		-0.21
t+2	-0.0050	0.0189***	0.0138**	73%	-0.0019
	-0.54	2.60	2.13		-0.21
t+3	-0.0353***	0.0422***	0.0069	16%	-0.0295**
	-2.98	3.76	1.23		-2.42
t+4	-0.0187	0.0341*	0.0154**	45%	-0.0237
	-0.86	1.67	2.17		-1.12
t+5	-0.0186	0.0298**	0.0112	38%	-0.0176
	-1.18	2.21	1.32		-1.14
t to t+2	-0.0186	0.0594***	0.0407**	68%	-0.0088
	-0.88	3.10	2.44		-0.42
t to t+5	-0.0896***	0.1875***	0.0979***	52%	-0.0787**
	-2.45	4.17	2.77		-2.03

Table A-12: Deal level clustering (continued).

Dependent Variable:	(1) Earnings (Euro)	(2) Earnings (ln)	(3) Daily Wage (Euro)	(4) Daily Wage (ln)	(5) Days Empl. (days)	(6) Days Empl. (ln)
Panel B.						
Regression (1): Managers versus all others						
D_{i2} x Target x Manager	-965.67*	-0.071	-0.81	-0.007	-2.38	-0.028
	-1.73	-0.96	-0.83	-0.85	-0.86	-0.69
D_{i5} x Target x Manager	-2019.40**	-0.294**	-0.32	-0.006	-8.36*	-0.1440**
	-2.42	-2.27	-0.35	-0.61	-1.84	-2.05
Regression (2): White collar employees versus all others						
D_{i2} x Target x White Collar	-455.01*	-0.123***	-0.05	-0.007	-4.35**	-0.0649**
	-1.65	-3.32	-0.09	-0.88	-2.39	-1.99
D_{i5} x Target x White Collar	-701.89*	-0.151**	0.07	-0.010	-5.40**	-0.0802*
	-1.86	-2.23	0.10	-0.92	-2.20	-1.81
Regression (3): Employees with above median age versus all others						
D_{i2} x Target x Old	-696.40***	-0.098***	-0.73***	-0.008	-4.00***	-0.050***
	-4.00	-3.48	-2.59	-1.51	-3.26	-2.68
D_{i5} x Target x Old	-807.04**	-0.184***	-1.15***	-0.018**	-6.19**	-0.106***
	-2.44	-2.61	-2.61	-2.10	-2.54	-2.71

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