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Private Equity and Human Capital Risk

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August 2017

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Abstract

We study the impact of leveraged buyouts in Germany on employees' wages, employment, and career paths. We contrast different views of buyouts, which see LBOs as facilitators of organizational and technological modernization, or as vehicles for transferring wealth from employees to shareholders. We conduct matched-sample difference-in-differences estimations with more than 147,000 LBO employees and a carefully matched control group. LBOs increase annual income in the short term, but reduce income by about 11% in the long term. White-collar workers and middle management lose most, probably because of organizational streamlining. LBOs in our sample do not foster trends related to technological modernization such as skill-biased technological change or job polarization. We find a strong negative impact of LBOs for older employees, but no support for the notion that shareholders benefit from LBOs by forcing employees to accept lower wages. All human capital impairments are borne by employees who leave the firm and become unemployed or employed in a different industry.

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

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

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Abstract

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1 Introduction

In this paper, we analyze the human capital risk associated with leveraged buyouts (LBOs) in Germany. 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.[1]

The literature in finance and economics has conventionally regarded LBOs as vehicles for improving firms' governance and operating performance, facilitating growth and creative destruction, and, more recently, modernizing firms' technology.[2] From this *modernization* perspective, LBOs create value by supporting economic efficiency and economic growth through organizational, operational, and technological improvements. LBOs benefit employees if they foster the adoption of new technologies and management practices that complement their human capital, but may have costs for some groups of employees whose human capital is substituted by new technologies. However, given the benefits of LBOs, these costs are considered moderate (Kaplan, 1989, Davis, Haltiwanger, Handley, Jarmin, Lerner, and Miranda, 2014). By contrast, critics argue that shareholders gain in LBOs 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

[1]Bild am Sonntag, April 17, 2005 (see also http://de.wikipedia.org/wiki/Heuschreckendebatte). Discussions in other countries created similar sentiments. Davis, Haltiwanger, Handley, Jarmin, Lerner, and Miranda (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 PE 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).

[2]The following papers articulate these views. Operating performance: Jensen (1989), Kaplan and Stromberg (2009); facilitating growth: Boucly, Sraer, and Thesmar (2011); catalyzing creative destruction: Davis, Haltiwanger, Handley, Jarmin, Lerner, and Miranda (2014); modernizing technology: Agrawal and Tambe (2016), Olsson and Tag (2016).

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. The new owners do not honor employment guarantees and may even take advantage of employees' inability to switch to other firms by forcing them to accept lower wages.

We ask three interrelated questions in the context of this debate about whether LBOs create wealth or transfer wealth. First, we ask whether private-equity buyouts create or mitigate human capital risk for the employees of target firms. Second, we wish to shed some light on the question whether the influence of private equity is more consistent with the transfer-of-wealth view or the modernization view, where we differentiate between organizational and technological modernization. Third, and finally, we are interested in the channels through which LBOs affect employees' human capital and investigate how LBOs affect employees' career paths.

We analyze data on 479 LBOs between 2002 and 2008 in Germany, which involve 705 companies and 147,116 employees. We match each employee of an LBO target to another employee with otherwise similar characteristics, whose firm did not become the target of an LBO. We match employees on a range of variables, including age, earnings, education, qualification, occupation, industry, region, nationality, gender, and employment status. Then we follow individuals over a five-year period after the LBO and perform matched-sample difference-in-differences analyses on employees' total income, wages, and employment. Next, we analyze employees' career paths and identify career events, such as unemployment, job changes to another establishment, or changes to another industry. We ask whether such career events are more or less likely for employees of LBO targets compared to matching non-target employees. Finally, we perform triple-difference analyses to uncover effects for specific subgroups of employees identified by a number of employee characteristics, including occupational status, education, tenure, age, and job characteristics.

We find a short-term positive effect of LBOs on employment, wages, and income, which

reverses about two years after the LBO. In the long term, LBOs cause a decline relative to the control group of 11% in annual earnings, of more than 5% in employment, and a marginal decline in daily wages of about 1%. The long-term decline in wages is concentrated in those LBO employees who become unemployed or have to accept jobs in other industries. However, the wages of employees who stay with the LBO target increase at a rate of about 3% per year after the transaction in absolute terms, in line with the wages of the control group. Hence, LBO firms may not honor implicit long-term employment guarantees. However, there is no evidence of a more narrowly defined transfer of wealth through wage cuts from employees who stay with the target to shareholders.

The long-term decline in income and employment falls largely on older LBO target employees, who receive lower wages. By contrast, the firm-specificity of human capital does not seem to matter nearly as much as age. Hence, employees who are presumably less able to adapt to a new organizational and technological environment (or, perceived to adapt less well), suffer larger losses. These losses are associated with adverse career changes, i.e. unemployment and job changes to another industry; job changes within the same industry result on average in higher wages, but the increase is lower for LBO employees. Our interpretation is that LBO employees experience more involuntary career changes compared to the control group. We compare the incidence of unemployment and job changes, within and across industries, to the base rates of these events in the control group and find a moderate effect: LBOs increase the incidence of career changes by about 8% to 15% compared to the frequency of the same events in the control sample.

We find some support for organizational modernization. Most of the losses of employment and wages fall on middle management and white-collar employees, in line with the notion that LBOs reduce layers of management, trim the administration of the firm, and potentially also cut wage premiums that resulted from entrenched management (Cronqvist, Heyman, Nilsson, Svaleryd, and Vlachos, 2009). By contrast, we find no evidence consistent with technological modernization. Proponents of the skill-biased technological change hypothesis

(Katz and Autor, 1999, Autor, Levy, and Murnane, 2003) observe that technological change in recent decades was biased towards higher-skilled jobs and against lower-skilled jobs, giving rise to increased wage inequality. LBOs may overcome resistance to the introduction of new technologies and thereby foster skill-biased technological change. However, we cannot observe such a decline in employment and wages for any of the definitions of low-skilled workers we analyze. We do not find evidence for skill-biased technological change in the control group either. Hence this trend does not seem to be relevant for our sample period and the segments of the labor market targeted by private equity firms.

Another version of the technological-modernization hypothesis emphasizes the bias against medium-skilled workers towards either high-skilled or low-skilled workers through the displacement of routine jobs as a result of investments in information technology ("routinization"), or through the reorganization of supply chains and trade ("offshoring"). This development is seen to foster job polarization (Goos and Manning, 2007, Autor and Dorn, 2013). Prior work finds that LBOs increase employment polarization (Olsson and Tag, 2016) and promote the adoption of computerized technologies (Agrawal and Tambe, 2016). However, we find that employment, wages and income of employees with a medium education benefit from LBOs relative to control employees. Our results are consistent with LBOs fostering outsourcing, but not with the routinization argument. Again, we show that these effects work through a reduction in the likelihood of adverse career changes.

Overall, we see LBOs in our sample period as facilitators of organizational change, a streamlining of firms' administration and management, and a reorganization of supply chains. Thus, our results complement those of Bernstein and Sheen (2016), who show that LBOs cause operational improvements in the restaurant industry. We find no evidence for technology-related explanations. LBOs have a negative impact on older employees, because these employees experience adverse career events. However, employees who stay with the firm benefit, most likely because their human capital is complementary to the new organizational and technological environment.

The contributions closest to ours are Olsson and Tag (2016) and Agrawal and Tambe (2016), since they both provide individual-level analyses of how LBOs affect employees. Olsson and Tag (2016) analyze individual-level employment data for private-equity transactions in Sweden. They find strong evidence for labor-market polarization, which contrasts with our results, most likely because the economic environment in Sweden is different from that in Germany. Moreover, Olsson and Tag (2016) do not analyze wages or career effects, which are crucial for our analysis to distinguish between different theories of LBOs. Agrawal and Tambe (2016) use an individual-level data set obtained from an online job-search platform in the US. They argue that LBOs increase IT-related investments, which enhance workers' human capital and increase firms' likelihood of survival. Thus, Agrawal and Tambe (2016) emphasize a particular aspect of modernization. However, they do not discuss wages or job changes and cover only those employees who use online job-search platforms, potentially the more skilled, educated and younger employees; their analysis may not reflect the negative impact of LBOs on workers who do not use online job-search platforms.

Other previous work on the human capital consequences of LBOs studies these questions either at the firm level or the establishment level.[3] Our setup based on individual-level data has several advantages. First, it allows us to investigate whether the human capital impairments in LBOs are a side effect of modernization or whether they are better understood as transfers of wealth; we can also differentiate technological and organizational modernization. Second, individual-level analyses provide a different methodological angle. While PE firms may select LBO targets based on characteristics related to the target's workforce, it is unlikely that they select LBO targets based on the characteristics of individual employees other than management. In particular, matching on pre-LBO wages and controlling for person fixed effects should also control for unobserved differences in productivity. These features make matching at the individual level more attractive than matching at the establishment

[3]A non-exhaustive list of papers is: Kaplan (1989), Lichtenberg and Siegel (1990), Wright, Thompson, and Robbie (1992), Amess and Wright (2007), Boucly, Sraer, and Thesmar (2011), and Davis, Haltiwanger, Handley, Jarmin, Lerner, and Miranda (2014). The surveys by Kaplan and Stromberg (2009) and Wright, Bacon, and Amess (2009) list additional contributions.

or firm level and avoid selection bias. Third, and finally, aggregating at the establishment level or firm level may hide effects on individual employees. Even if one employee gains what another employee loses, the losing employee will suffer a reduction in the value of her human capital, which is probably not insurable, thus creating uninsurable risk.

2 Hypothesis development

In this section, we develop our hypotheses in more detail and relate them to the relevant literature. Section 2.1 discusses organizational and technological modernization, and Section 2.2 develops the transfer-of-wealth argument.

2.1 Modernization

A large part of the literature sees LBOs - and transfer-of-control transactions more generally - as a means to overcome resistance to change, either technological change or organizational change.[4] These changes can be beneficial for employees if their skills are complementary to the new organizational structure or 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.

Technological modernization. The recent literature on LBOs emphasizes technological change and sees LBOs as vehicles that foster a process of creative destruction (Davis, Haltiwanger, Handley, Jarmin, Lerner, and Miranda, 2014), raise investment in information technology (Agrawal and Tambe (2016)), or overcome firms' resistance to adapt to technological trends, such as skill-biased technological change and job polarization (Olsson and Tag, 2016).

[4]Kaplan and Stromberg (2009) emphasize operational changes and changes in governance as one key activity by which private equity firms create value through buyouts. Olsson and Tag (2016) and Agrawal and Tambe (2016) argue that LBOs promote 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 LBOs 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.

More recent research notes inconsistencies between the SBTC hypothesis and developments in labor markets (e.g., Card, 2002; Goos, Manning, and Salomons, 2014; Mishel, Schmitt, and Shierholz, 2013). Instead, some studies have identified a pattern of labor-market polarization, such 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). The reason is that low-wage manual jobs (e.g., health workers, janitors, security guards) are more difficult to replace with computerized technologies and cannot be outsourced to countries with lower labor costs. 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). If employment polarization represents a macroeconomic trend, then it will also affect wages and result in higher wage increases for low-skilled and high-skilled workers compared to medium-skilled workers (Autor and Dorn, 2013). If LBOs overcome resistance to computerization and outsourcing, they should reinforce job polarization and benefit employees with low or high skills and education at the expense of those with intermediate skill and education.

Organizational modernization. One strand of the LBO literature emphasizes the improvement in management and sees LBOs 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 LBOs replace governance by direct monitoring with governance through high-powered incentives (Jensen,

1989; Lichtenberg and Siegel, 1990). Based on these notions, we expect LBO targets to streamline their organizational structure by reducing the layers of management and creating a leaner organization. We hypothesize these measures will fall disproportionately on white-collar workers and middle management and expect a general decline in employment for these groups.

Managers may delay organizational and technological modernization, because their skills are complementary to a particular technology or organizational structure and private equity may be a mechanism to address this agency problem. Similarly, 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, Heyman, Nilsson, Svaleryd, and Vlachos, 2009). If buyout firms address these agency problems, LBO targets should reverse these trends, and the wages of middle management should decline.

Specific human capital. LBOs should benefit employees whose human capital is less tied to the particular technology or organizational model of their firm (Becker, 1962; Hashimoto, 1981) or industry (see Neal, 1995 on industry-specific human capital). By contrast, we expect LBOs to hurt those with firm-specific or industry-specific human capital, who have fewer opportunities to find other employment at their current wage. Following the literature, we measure firm-specific (industry-specific) human capital by employees' tenure in their firm (e.g., Hashimoto, 1981; Topel, 1991) or industry (Neal, 1995). Hence, we hypothesize that LBOs reduce the wages and employment of workers with longer tenure in their firm (industry).

2.2 Transfers of wealth

The transfer-of-wealth argument holds that firms provide long-term employment insurance to employees to provide them with incentives to invest in firm-specific human capital (Shleifer and Summers, 1988). Employees rely on managers and owners to honor these unwritten

agreements, e.g., because managers and owners pass through "loyalty filters" (Akerlof, 1983) in their career, which align their preferences with those of other 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 with the employees of the firm. The new owners may cut wages and employment. Shleifer and Summers (1988) see implicit contracts primarily in the context of employees' incentives to invest in firm-specific human capital, but believe their argument can be easily extended. Implicit contracts may also provide employment and wage insurance to employees in exchange for lower wages (Azariadis, 1975; Harris and Holmstrom, 1982) or may elicit unobservable effort in exchange for rising wage profiles (Lazear, 1979). The empirical implications of these alternative interpretations of implicit contracts seem to be similar and are, therefore, not discussed here.

In our context, the implications of the transfer-of-wealth view for employment are largely congruent with those from the modernization view; both imply increased layoffs. Modernization processes imply that employment guarantees will not be honored, or will be honored only to a limited extent. Gamber (1988) and Guiso, Pistaferri, and Schivardi (2005) recognize this limitation when they argue that firms optimally provide employment insurance only against temporary, firm-specific shocks, but not against permanent, industry-wide changes, such as changes in technology. Firms that provide more generous insurance to workers would become uncompetitive and risk bankruptcy. We can neither observe implicit contracts nor can we distinguish between temporary and permanent shocks. Accordingly, we cannot establish whether employment losses after LBOs constitute a "breach of trust" in the sense of Shleifer and Summers (1988).

However, the transfer-of-wealth view and the modernization view do have distinguishable implications for wages. According to Shleifer and Summers (1988), new owners may simply transfer wealth from employees to shareholders by forcing employees to continue working for lower wages (see Rosett, 1990 and Gokhale, Groshen, and Neumark, 1995 for tests of a related argument on takeovers). If workers have made firm-specific investments in the

past, their wages will be higher than their outside employment options, and they may have to accept lower wages after the buyout. Then wage reductions should be larger for those workers who continue to work for the same firm, but have lower outside options, i.e., those with more firm-specific human capital and less able to adapt to a new work environment. The modernization view associates wage reductions with career changes forced on those workers whose skills become obsolete after organizational or technological changes. Workers whose skills are complementary to the old technology are forced to leave, whereas those with general skills or skills suited to the new technology stay. Hence, under the modernization view, the wages of those workers who leave the firm are more negatively affected than those who stay with the firm after an LBO. Under the transfer-of-wealth view, the opposite prediction holds. In both cases, the negative effect is stronger if human capital is more firm-specific.

3 Data and methodology

In this section, we describe the construction of the data set and the methodology.

3.1 Sample construction

The analysis requires linking three separate data sets: A data set containing LBO transactions, 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 transactions 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 coherent data

set. We include all deals where a private equity investor acquires a majority stake in a firm. In the following, we use the term "private equity transaction" and "LBO" interchangeably, although we do not formally require these deals to be categorized as LBOs in Thomson One or Capital IQ. Such a restriction would exclude many restructuring deals for which no debt financing is required because of, for example, a very low purchase price. Furthermore, relying on the LBO category in Thomson One and Capital IQ would exclude all deals for which data providers establish whether the deal was leveraged have not been able to identify LBOs as such. Eventually, the descriptions of deals flagged as LBOs do not reveal the degree in which debt was used to finance the deal and, therefore, such a restriction would seem arbitrary. The data set starts in 2002, because coverage of LBOs for earlier years is very low in all three databases. We exclude secondary buyouts and transactions after 2008, because we want to observe the performance over the subsequent five years, and individual employment history data is only available until 2013.

We hand-collect the subsidiary structure of every transaction in *Hoppenstedt's Firmendatenbank* and collect data on all companies involved in any of the transactions as acquirers, targets, or their parents. We then link companies to their establishments in the Establishment History Panel (BHP, see Schmucker, Seth, Ludsteck, Eberle, and Ganzer, 2016). The BHP contains all establishments in Germany with at least one dependent employee at the reference date, June 30th, of the respective year. To link establishments to transactions, we match based on company names, since there are no common company identifiers that link Hoppenstedt with the BHP. We use record linkage techniques described by Herzog, Scheuren, and Winkler (2007) to link company names to establishment names.[5] We are left with 530 transactions, 815 companies, and 3,330 establishments.

Finally, we use the list of establishments compiled in the previous step to identify all individuals employed with any of the target companies on the day of the announcement of

[5]The record linkage was performed by the German Record Linkage Center (GRLC, see `http://www.record-linkage.de`). See Schild (2016) for a detailed description of the methods and data sources used to perform a very similar linkage with IAB's administrative establishment data.

the LBOs. We remove employees for whom we do not have sufficient information on all control or matching variables over the eleven-year period we require (for an overview on all control and matching variables, see Table 1 and Table A1 in the Online Appendix). This step leaves us with 190,192 employees of 488 transactions with 722 companies and 2,429 establishments, which we record over an eleven-year period between 1997 (five years before the first transaction) and 2013 (five years after the last transaction). Table A1 in the Online Appendix provides an overview of these steps.

3.2 Wage and employment data

The employment history data are derived from the Integrated Employment Biographies (IEB), which contains the employment histories of individuals. The IEB contains every dependent employee in Germany, i.e. all regular employees since 1975 (West Germany) and all marginally employed workers since 1999.[6] The data are structured in terms of spells, i.e. employment relationships, and the data source reports starting and end 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. To construct consistent annual earnings and employment variables, earnings and employment data for all spells recorded for a calendar year are aggregated into annual earnings and an annual number of days employed, regardless of whether the spells refer to different employers or the same employer. For categorical variables such as education, qualification, and establishment affiliation, we pick 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.

[6]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, Ganzer, and vom Berge (2016).

3.3 Matching employees

In the final step, we form a control group. To base our matching on characteristics that have not been affected by the LBO, we match on characteristics recorded in the year before the LBO announcement. As a first step, we identify establishments that have been active in the same sectors as the target establishment during 2001 to 2007 without being targets themselves at any time during the observation period. We use the resulting list of establishments to compile our initial data set of potential control individuals. For each employee from the LBO group, we identify one matching employee from establishments in the same industry who was not affected by an LBO within the sample period according to the following algorithm:

1. Match individuals exactly in terms of nationality, gender, region, education, qualification, occupational group, occupational status, employment status, and industry. Table A2 in the Online Appendix provides details on these categories.

2. Remove individuals for whom the absolute deviation of *Days Employed* from the treated employee is larger than 45 days.[7]

3. Remove individuals for whom the absolute deviation from the treated employee in terms of *Earnings*, *Age*, or *Firm Tenure* is larger than 25%.

4. Remove individuals for whom the absolute deviation in *Establishment Size* from the treated employee is larger than 50%.

5. From the remaining set of candidate control employees, pick the nearest neighbor based on the normalized Euclidean distance of the numerical variables mentioned in steps two to four. If the set remaining after steps one to four is empty, the target employee is removed from the data set.

We match with replacement, i.e., we allow for a control employee to be in the matched data set more than once. We end up with 147,116 treated employees who have been involved

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

in 479 transactions, involving 705 companies and 2,264 establishments, which amounts to a matching rate of 77%. The number of control employees is equal to 105,405. The number of control employees is smaller than the number of treated employees because we allow for matching with replacement.

3.4 Matching success

Table A3 in the Online Appendix presents matching statistics on our numeric variables for the matched LBO employees, the unmatched LBO employees, and the control employees. The relative differences between the matched LBO employees and the control employees are below 1% and of low economic significance. We use the normalized differences proposed by Imbens and Wooldridge (2009) to examine significant differences between two groups of observations. Normalized differences relate the absolute difference to the square root of the the sum of the variances of both groups. This statistic does not inflate the test statistics if the number of observations is very large, which makes t-tests statistically significant even when differences are not meaningful for matching. According to Imbens and Wooldridge (2009), the resulting statistic should be lower in absolute value than 0.25. In our sample, the test statistic is never higher than 0.01, and we conclude that our control group is representative of our LBO sample and matches the treatment group very closely. The differences between matched and unmatched LBO employees are substantial. In particular, individuals who have not been employed the full year are less likely to be matched. Therefore, annual income and tenure are both substantially lower in the unmatched LBO sample than in the matched LBO sample.

3.5 Descriptive statistics

Table 2 describes the composition of the sample with respect to qualification, gender, nationality, occupation, and education, separately for control employees and employees of LBO targets. The third column shows the composition of the whole labor force based on IAB

data.[8] LBOs target mostly manufacturing companies. Manufacturing is overrepresented in the LBO sample (59.3% of employees) relative to the economy in general (26.3%), and this bias characterizes all differences between the composition of the LBO sample and the German labor force. About a quarter of employees are grouped into the lowest occupational group "simple manual occupations" (general labor force: 17%) and about one-fifth of employees are grouped into the second-lowest category "skilled manual occupations" (general labor force: 14%). Managers constitute only 2.4% of the whole sample, in line with the general labor force. Women have a share of 21.6%, much less than the proportion of women in the labor force (46.1%). More LBO employees have an intermediate school leaving certificate (67%) compared to the German labor force (59%). Similarly, more LBO employees (47%) have been employed in the south of Germany, i.e., in Baden-Wuerttemberg, Bavaria or Hesse compared to the general labor force (38%). The LBO sample contains more full-time workers (94%) compared to the general labor force (67%) as a result of our matching algorithm.

Table 3 presents descriptive statistics on the numerical variables. Note that we observe each individual over time from five years before the LBO to five years after the LBO. Therefore, our final data set is a panel of about 3.2 million observations, spanning eleven years for 147,116 LBO employees and their matching employees. Average *Earnings* in the sample are equal to 34,905 Euro, which is only marginally lower than the median of *Earnings*. Because earnings are derived from contributions to social security, earnings are censored at the social security maximum that ranges from about 51,000 Euro in 1998 to about 70,000 Euro in 2013. The maximum *Earnings* reported in the data can nevertheless be higher because some individuals have more than one job in a given year and social contributions are calculated for each job irrespective of any other jobs in the same year. The average employee is 41 years old and has held his or her current job for 3,358 days, or almost 9.5 years. A very small number of individuals enters our data set when they are still below the legal age to work because we start to track individuals five years before the LBO. For example, an individual that is 14

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

years old at the time of the LBO will enter our data set at the age of nine.

3.6 Empirical approach

Our approach builds on Jacobson, LaLonde, and Sullivan (1993) and Couch and Placzek (2010), who use panel regressions with fixed effects and matching estimators in a program evaluation context. In our setting, we regard an LBO as an exogenous treatment, which may depend on the composition of the workforce of the firm, on which we match, but not on the characteristics of an individual worker. We define three main outcome variables Y_{it}:

- **Earnings:** The logarithm of the employee's earnings summed up over all employment spells in a given year.
- **Daily Wage:** The logarithm of *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 was unemployed during the whole year t.[9]
- **Days Employed:** The logarithm of the number of days in year t during which employee i was employed.

In all cases, when we refer to the logarithm of a variable X, we use the transformation $ln\,(1+X)$. Since the values of all out variables X are orders of magnitude larger than one, the approximation error is very small.[10] Our analysis relies on matched-sample difference-in-differences regressions for these two dependent variables. It is therefore essential to show that the parallel-trends assumptions is satisfied. We demonstrate the trends before the event for *Earnings*, *Daily Wage* and *Days Employed* graphically in Figures 2 to 4. The figures provide us with a first look at the data by showing the post-event trends as well. For all three variables, we can see almost-perfect parallel developments from $t-5$ to $t-1$, with

[9] We cannot calculate hourly wages, because our data does not report the number of hours worked per day or per week. According to Table 2, 6% of our sample are part-time employees for whom *Daily Wage* will be lower than a full-time equivalent daily wage.

[10] This transformation is commonly applied, but not necessarily unproblematic if X is small relative to one. See Burbidge, Magee, and Robb (1988) and Pence Karen (2006) for further discussion.

targets' wages being consistently above the wages of the control group*; Daily Wage* for both groups grows at a rate of about 2.4% per year. *Earnings* increases temporarily in the event year and the year after, and then falls, resulting in slightly lower income in $t+4$ and $t+5$. For the development of employment, we observe the same upward trend for employees in the target and control groups from $t-5$ to $t-1$. The trend is steeply upward sloping during this period, leading to a peak of 94% in the event year, following by a decline for both groups. 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.

4 Analysis

The following Section 4.1 analyzes the overall impact of LBOs on wages, annual income, and employment. The subsequent Section 4.2 tests the transfer-of-wealth hypothesis. Section 4.3.3 investigates the specificity of human capital. Section 4.3 distinguishes between organizational and technological modernization.

4.1 The impact of LBOs on employment and wages

Methodology. In the first step we estimate difference-in-differences regressions to evaluate the overall impact of LBOs on income, wages, and employment:

$$Y_{is} = \alpha_i + \gamma_s + X_{is}\beta + \sum_{t=-5}^{t=+5} \delta_t D_{is}^t + Target_i \times \sum_{t=-5}^{t=+5} \theta_t D_{is}^t + \varepsilon_{is}. \quad (1)$$

In (1) Y_{is} denotes the outcome variable in levels (*Earnings, Daily Wage*, *Days Employed*), X_{is} is a vector of control variables, α_i and γ_s are, respectively, individual and time fixed effects, i indexes individuals, s indexes calendar time, and t indexes event time. The event time dummy variables D_{is}^t begin five years before the LBO ($t=-5$) and end five years after the LBO ($t=+5$). Our data cover all individuals from five years before to five years after the event and the dummies for event year $t=-1$ are omitted; hence, all event time effects

are measured relative to the year before the LBO. The dummy variable $Target_i$ distinguishes employees of LBO targets ("treated") from employees in the matching sample ("controls"). Note that $Target_i$ equals one for target employees in all sample years.

The approach in (1) generalizes standard difference-in-differences estimators by adding a temporal dimension to the standard dummy variable $POST_{is}$, which would assume a value of one in the post-LBO period. Note that our specification differs from Jacobson, LaLonde, and Sullivan (1993) and Couch and Placzek (2010), because we enter the event time dummies D_{is}^t in addition to the calendar time effects γ_s. LBOs happen at different dates in calendar time, so the event time dummies are not collinear with calendar time effects (see Boucly, Sraer, and Thesmar, 2011). The parameters of interest are the coefficients θ_k on the interactions $D_{is}^t \times Target_i$, which measure the average influence of the LBO on the outcome variable Y_{is} in event-year t. By contrast, the coefficients δ_t measure the average influence of event time on the outcome, after controlling for calendar time effects.

We report t-statistics and significance levels based on clustered standard errors calculated at the establishment level. This specification generates correct standard errors for the case of time-invariant as well as time-varying unobserved establishment effects (Petersen, 2009). Since individuals change establishments, the same individual can be assigned to different clusters at different points in time.

To better interpret the event time dummies, consider a scenario in which LBOs systematically target firms with employees whose skills are in short supply, so that the employees of LBO firms experience increasing incomes and wages. Then matching will pair the employees of LBO firms with similar employees at non-LBO firms, who experience the same increase. This trend would then be picked up as an event time effect by δ_t, which measures how the wages of employees of LBO firms would have developed without the LBO, only based on their matching characteristics, such as the assumed scarcity of skills in this example. The coefficient θ_t measures how employees of LBO firms deviate from the non-LBO control group.

Earnings. We begin with an analysis of the impact of LBOs on *Earnings*, defined as labor income summed across all employment spells of an employee in a given calendar year. Figure 4 plots the coefficients θ_t on the interaction $D^t \times Target$ from regression (1) without controls except for person and calendar-year fixed effects. We tabulate the coefficients on $D^t \times Target$ in Table 5 in the Online Appendix. *Earnings* increases temporarily by about 5.5% in the event year and the subsequent year. The effect declines and becomes negative as of year $t+3$, with a highly significant long-term effect slightly below 11% in year $t+5$. Before we discuss interpretations of these effects, we decompose them into a wage component and an employment component.

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, Haltiwanger, Handley, Jarmin, Lerner, and Miranda (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. Their data do not allow them to control for changes in the workforce at the establishment level and may not be directly comparable to ours.

Wages. We decompose the effect on *Earnings* into a wage component and an employment component by studying the effects on *Daily Wage* (dashed line) and *Days Employed* (broken line). The effect on *Daily Wage* shows a positive influence of LBOs on wages of about 1% in the event year, which remains positive until year $t+3$ and then becomes negative, with a decline of about 1% in $t+5$. 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, Bacon, and Amess, 2009 for a more comprehensive survey and the Introduction for a discussion of this literature).

Employment. The short-term impact of LBOs on *Days Employed* is positive in the event year (1% increase), but declines steadily in subsequent years with a long-term effect around -5% in years $t+4$ and $t+5$. Davis, Haltiwanger, Handley, Jarmin, Lerner, and Miranda (2014) measure employment losses at the firm level of more than 1% per year (see their Table 3). Kaplan (1989) finds industry-adjusted employment losses at buyout targets of 6.2% to 12.0% for an earlier sample. For the UK, Wright, Thompson, and Robbie (1992) report employment losses for buyouts of 6.3% with a subsequent recovery. In the context of this large literature - Wright, Bacon, and Amess (2009) review 17 papers on employment effects - we find an aggregate long-term effect of LBOs on employment consistent with prior estimates.

Long-term effects. The long-term negative effects on income, employment and wages are plausible and consistent with human capital impairment from modernization, but also with a transfer of wealth. The long-term effect on *Earnings* is largely driven by employment and only to a lesser extent by declines in wages. The short-term positive effects and their subsequent reversal are somewhat puzzling. For employment, this pattern may be attributed to standstill agreements associated with mandatory social compensation plans, which private equity investors enter with firms' employees. These agreements may temporarily provide better protection to LBO employees compared to employees in the control group. Unfortunately, no public information is available on standstill agreements. Standstill agreements can also not explain the development of *Daily Wage*. It seems plausible that LBO targets provide assurance and pay a premium to workers considered critical for the operation of the firm. These conjectures are speculative, and our subsequent discussion focuses on the long-term consequences of LBOs on employees' human capital.

4.2 Transfers of wealth

One implication of the transfer-of-wealth argument is the negative impact of a transfer of ownership of the firm on employees who stay with the firm. According to this view, a

transfer of control may result in a breach of implicit contracts and reduces the (quasi-)rents of employees by reducing their wages. By contrast, all versions of the modernization hypothesis imply that negative effects of LBOs on wages should affect those workers more strongly whose human capital becomes outdated and who leave the firm. To test these contrasting hypotheses of LBOs on wages, we repeat our previous analysis for the sample of target employees who stay with the establishment they were associated with in the event year.

Figure 5 reports the results for regression (1) for all employees who stay with the same establishment from period $t-1$ until $t+5$ with *Daily Wage* as the dependent variable. The solid line shows the difference between the wages of target employees and control-group employees, both of whom stay with their respective firm. For this analysis, a matched target-control pair is removed from the sample if either the target employee or the control-group employee change their establishment between $t-1$ and $t+5$. Figure 4 suggests employees who stay with their establishments and those who leave their establishments do not differ regarding the short-term gains documented above; they enjoy a temporary wage gain of about 1.2%. However, employees who stay with their establishments do not share in the long-term losses in periods $t+4$ and $t+5$ observed in Figure 4 for all employees. For the sample restricted to continuing employees in Figure 5, all coefficients in these later periods are zero or positive, though statistically insignificant. By contrast, target employees who change careers after the LBO receive lower wages compared to those employees who stay with their establishment, which motivates our later analysis of career paths. The broken line represents a single-difference analysis and restricts the sample to target employees who stay with the target until $t+5$. As in Figure 4, we include person and calendar-year fixed effects as the only control variables. The wages of target employees grow before and after the LBO. In the five years after the LBO, *Daily Wage* increases by 2.8% per year.

Figure 5 shows employees who stay with their establishment after LBOs do not lose, neither in absolute terms (broken line) nor in relative terms compared to employees of non-LBO targets (solid line). These observations support the modernization hypothesis, but not

the wage implications of the transfer-of-wealth hypothesis, which predicts the opposite of what we find.

4.3 Modernization

In this section, we analyze the different versions of the modernization hypothesis (see Section 2). These hypotheses make predictions on the development of wages and employment for subgroups of employees relative to each other and identify several factors, such as workers' age, skill level or their educational attainments, which influence whether employees are more or less likely to be adversely affected by LBOs.

Methodology. We extend the regression approach used in Section 4.1 and interact the dummy variables D_{is}^t with risk factors and estimate the triple-difference equation

$$\begin{aligned} Y_{is} =& \alpha_i + \gamma_s + X_{is}\beta + \sum_{t=0}^{t=+5} \delta_t D_{is}^t + \sum_{t=0}^{t=+5} \theta_t D_{is}^t \times Target_i \\ &+ \sum_{t=0}^{t=+5} \lambda_t D_{is}^t \times RF_i^f + \sum_{t=0}^{t=+5} \eta_t D_{is}^t \times Target_i \times RF_i^f + \varepsilon_{is}, \end{aligned} \tag{2}$$

where RF_i^f is risk-factor f. Risk factors are measured in the year before the LBO announcement. Here the coefficients of interest are η_t on the triple interaction of $Target$, the event dummies, and the risk factor. We exclude the period from $t-5$ to $t-2$, because we set $t-1$ as the benchmark year and the preceding years do not add to the identification of the triple-difference effects.[11] For simplicity, we aggregate the post-event dummies for years $t+1$ to $t+4$ with the event-year dummy into one variable $D_{is}^{[0;4]} = \sum_{t=0}^{t=4} D_{is}^{\tau}$. This specification is more parsimonious and avoids a proliferation of interaction terms. We we report the effects for year $t+5$ separately to report long-term effects. The reason for this separation is the

[11] Including the period from $t-5$ to $t-2$ would complicate our econometric setup, because we would have to interact all level variables with each other. Meanwhile, after correctly controlling for all of the interaction effects before $t-1$, all of the triple-difference results would be identical, while standard errors would be lower because of sample size. Thus, excluding these years, if anything, just raises the hurdle for finding significant effects. Sensitivity analyses including periods $t-5$ to $t-2$ show that our results are robust.

frequent reversal of short-term effects in periods $t+2$ or $t+3$. As before, we cluster standard errors at the establishment level.

Table 4, reports the results of triple-difference estimations based on equation (2) for different risk factors. Panel A includes measures of the specificity of human capital, Panel B includes measures of skills, Panel C includes variables identified as drivers of job polarization in the literature, and Panel D includes variables related to organizational change.

4.3.1 Technological modernization

In Panel B of Table 4, we begin with risk factors that stratify workers according to their education and occupational status. We analyze three different levels of the risk factors (low/medium/high) to address job polarization and skill-biased technological change:

1. **Education (columns 1 to 3).** Educational attainment is high if an individual has completed a degree at an institute of higher education ("college graduates"). Education is classified as medium if the individual completed either upper secondary school or vocational training, or both. Educational attainment is low otherwise, with at most intermediate secondary schooling and no vocational training.

2. **Occupational status (columns 4 to 6).** We group employees into skilled blue-collar employees, unskilled blue-collar employees, and white-collar employees.[12] While we can easily rank skilled and unskilled blue-collar workers in terms of skills, there is some overlap in the skill levels of skilled blue-collar workers white-collar workers. The latter group includes not only high-skilled employees, but also low-level office staff, which often requires only a medium education.

3. **Wages (columns 7 to 9).** We divide all employees who are employed for the full year evenly into terciles according to *Earnings*: the lowest (medium, highest) income tercile is labeled as low (medium, high) wage.

[12]These three categories cover 87% of target employees, see Table 2. Employees who do not belong to any of these three categories (e.g., trainees, master craftsmen, part-time employees) are removed from the sample, which is why column (2) has fewer observations.

Equation (2) is extended by a second risk factor. For all three measures, we assign *Risk Factor 1* to a person in the lowest category (low education, unskilled, low wage) and *Risk Factor 2* to a person in the middle category. The highest category always forms the reference group and is left out.

The skill-biased technological change (SBTC) hypothesis predicts income and wages should decline for the lowest group. Looking at the triple interactions for the short term ($D_{is}^{[0;4]} \times Target \times RF1$) and the long term ($D_{is}^{5} \times Target \times RF1$) shows that LBOs either have no significant impact on target employees in the lowest group according to any of the three definitions. The only exception is the short-term impact of LBOs on Earnings for employees in the low-education group, which is economically strong (+13.08 log points) and has, therefore, the opposite sign as predicted by the SBTC hypothesis; it is statistically highly significant. We cannot find evidence consistent with SBTC for the control group either. The coefficients on $D_{is}^{[0;4]} \times RF1$ and $D_{is}^{5} \times RF1$ are large and positive for education. For occupational status and wages, we observe inconsistent effects. Hence, we find no evidence for SBTC in the control group and LBOs do not create or reinforce trends that could be interpreted as SBTC.

Job-polarization predicts negative developments for the medium stratum of the labor market relative to the other groups. However, the triple interactions for these groups ($D_{is}^{[0;4]} \times Target \times RF2$ and $D_{is}^{5} \times Target \times RF2$) *all* have a positive sign, and some of them are even statistically significant. Hence, LBOs sometimes have a significant positive influence in the short term as well as the long term for the middle group, in particular, skilled blue-collar workers (columns (4) to (6)), which contradicts the job-polarization hypothesis. In these regressions, comparisons are with respect to the top group, which forms the reference group.[13] Findings for the control group of non-target employees (coefficients on $D_{is}^{5} \times RF2$) are mixed. Effects are strong and negative for employees with a medium education, and

[13] In Table A4 in the Online Appendix we provide an alternative specification in which the middle group is compared to the bottom and top groups, which together form the reference group in that specification. Results are robust.

positive for skilled blue-collar workers and workers in the medium wage tercile. Thus, the labor market for the control group was not characterized by a consistent trend towards job polarization, and our results soundly reject the notion that LBOs foster job polarization.

To investigate wage polarization further, we analyze whether employees with routine jobs and jobs that can be more easily outsourced to low-wage countries ("offshorability") are more strongly affected. The results are presented in Panel C of Table 4. In columns (1) to (3), we categorize jobs as routine as defined by Goos, Manning, and Salomons (2014).[14] We find positive short-term and long-term effects on *Earnings* for LBO employees with routine jobs compared to those with non-routine jobs. These results seem to be driven entirely by gains in employment; the coefficients for *Daily Wage* are almost zero and do not support the predictions of the wage polarization argument. Thus, LBOs do not foster the displacement of employees with routine jobs.

In columns (4) to (6) of Panel C, we look at the offshorability of jobs, which we again categorize following Goos, Manning, and Salomons (2014). In this case, we find support for some of the predictions of the job-polarization hypothesis: Target employees with offshorable jobs suffer short-term reductions in earnings of 3.4 log points, which reinforces a similar effect for the control group (−4.7 log points short-term, −11.1 log points long-term).

4.3.2 Organizational modernization

The second aspect of modernization emphasized in the LBO literature focuses on organizational modernization, which includes streamlining of the administration of the firm and reducing the layers of management. Our data set includes two variables that allow us to analyze these issues. The first one is *Middle Management*, an indicator variable for those employees who belong to the middle layers of management (occupational group 10 in Table 2). The second one is *White Collar*, an indicator variable for the white collar workers that were used as the reference group in Table 4. The quiet-life hypothesis (Cronqvist, Heyman,

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

Nilsson, Svaleryd, and Vlachos, 2009) implies top executives are excessively generous towards those in their immediate work environment, which we refer to as middle management. Hence, if LBOs address this agency problem, middle management should do worse in LBOs.

Table 4, Panel D shows the results for organizational change. In columns (1) to (3), we analyze if middle management is adversely affected by LBOs. We observe an economically dramatic decline in *Earnings* for middle management by 21.22 log points after five years, which is entirely driven by a decline in employment (−11.49 log points). These effects are statistically marginally significant (p-value: 0.08). In columns (4) to (6) of Panel D, we analyze how white-collar workers are affected by LBOs. The difference between white-collar workers and other workers is consistently negative, but statistically never significant. However, Table 5 in the Online Appendix shows that the long-term effects shown in Figure 4 are concentrated entirely in the group of white-collar workers, for whom the decline in *Earnings*, *Daily Wage*, and *Days Employed* in $t + 5$ is about 50% larger than for blue-collar workers. The table shows also that the results are often insignificant for non-white collar workers, but always highly significant for white-collar workers.[15]

4.3.3 Human capital specificity

We follow the literature (e.g., Poletaev and Robinson, 2008) and measure the specificity of human capital by the individual's tenure in their current job (firm-specific human capital), respectively, by the individual's tenure in the same 3-digit industry (industry-specific human capital). We use the median tenure in the same firm (industry) to distinguish between high and low tenure. Columns (1) to (3) (*Firm Tenure*) and columns (4) to (6) (*Industry Tenure*) present the results. While the point-estimates on *Earnings* and *Days Employed* have the predicted sign, the effects of human capital specificity on income, wages, and employment for LBO-employees are statistically not distinguishable from the effects for non-target employees. Interestingly, the baseline effects for the control group (coefficients $D_{it}^5 \times Tenure(H)$ and

[15]The difference obtains independently of whether we compare white-collar workers only to blue-collar workers, or also to other non-white collar workers, such as part-time workers, which form a separate category.

$D_{it}^{5} \times Ind.\,Tenure(H)$) are large and highly significant. Human-capital specificity has a significant and large, negative impact, especially on *Days Employed*, but target employees do not fare differently from non-target employees in this respect.

In columns (7) to (9), we use *Age* as a risk factor and find much stronger effects for *Age* (long-term effect: -21.26 log points for *Earnings*) compared to those for the tenure variables. We conjecture that older employees have a more dated education and are less adaptable to the technological and organizational changes introduced by private equity investors. This interpretation is consistent with Gathmann and Schönberg (2010), who find that more experienced employees are less likely to adjust their skills portfolio, and move to new jobs only if the required task portfolio is not too different from the previous jobs. However, these age-related effects seem to arise independently of whether employees gathered their experience in a number of different jobs or whether their experience is closely related to their current establishment or their current industry. The age effect is also consistent with the view that LBO targets incentivize workers by offering them wage profiles that increase over their careers, resulting in wages in excess of employees' marginal productivity towards the end of their careers. These employees become unemployed after LBOs instead of earning lower wages.

5 Employees' career paths

In this section we investigate the channels through which LBOs affect employees' human capital. Based on our hypothesis development we expect that LBOs affect employees' human capital through the way in which they change employees' career paths.

5.1 Career path effects on income, wages, and employment

As a first step, we repeat the analysis in Figures 4 and 5, but now add additional control variables, which indicate changes in employees' careers. In particular, we control for three

career indicators:

1. *Establishment Change*, a dummy variable for switches to another establishment within the same 3-digit industry within five years of the LBO.

2. *Industry Change*, a dummy variable for switches to another establishment outside the employee's 3-digit industry within five years.

3. *Unemployment*, a dummy variable, which assumes a value of one if the employee is unemployed at the end of the respective year.

We interact all three career indicators with the treatment indicator $Target$ and the indicator $Post = \sum_{t=0}^{t=5} D_{is}^{t}$, which aggregates the event dummy with the post-event dummies. We add these triple interactions to the baseline specification plotted in Figure 4. We plot the interactions of the event time dummies with the target indicator, $D_{is}^{t} \times Target$, from these extended specifications in Figure 6 and report the results for the three career indicators and their interactions, $CI \times Post$ and $CI \times Post \times Target$, in Table 5. Figures 4 and 6 are drawn to the same scale, so they are directly comparable. The regression specifications for both figures are identical except for the inclusion of the additional career indicators and their interactions. Given the results in Section 4.3, we show the results separately for non-white collar workers and white-collar workers.

After controlling for career-path events, the long-term influence (periods $t+4$ and $t+5$) of LBOs on income, wages and employment vanishes or changes signs, whereas the short-term dynamics (periods t to $t+2$) are not much affected. With the additional controls, there is no long-term negative effect anymore. Hence, career path events can account for the negative long-term influence of LBOs on employees' income, wages and employment, but they cannot account for short-run dynamics.

To analyze the effect of career events on the income, wages and employment of LBO employees, we need to analyze the effects for the control group together with the corresponding effects on LBO employees. We discuss non-white collar employees first. The effects of estab-

lishment changes and industry changes on *Earnings* and *Days Employed* on control employees are positive and economically as well as statistically highly significant (positive coefficients on $CI \times Post$ for both events), whereas long-term effects on *Daily Wage* are negative. However, the positive impact and income and employment is reduced for LBO employees (negative coefficients on $CI \times Post \times Target$). For example, the increase in *Earnings* for control employees of 40.64 log points is reduced by 16.86 log points to just under 24 log points and therefore positive, but lower for LBO employees compared to control employees (column (1)). The effects for industry changes and the effects on *Days Employed* are qualitatively similar (column (2)). The positive effect of within-industry establishment changes on *Daily Wage* for the control group (+2.79 log points) is reversed for LBO employees (−2.74 log points), who therefore experience a net wage effect of zero (column (3)). Industry changes have a substantial negative wage effect for control employees (−6.16 log points), which is even larger for LBO employees by (another −4.23 log points). Naturally, the impact of unemployment on *Earnings* is negative and large.

Our interpretation is that for control employees, job changes within the same industry are mostly voluntary and lead, on average, to increases in wages and employment. Changes to other industries are mostly involuntary and lead to a decline in wages, but control employees accept them, because it significantly increases their income by increasing their employment. By contrast, LBOs increase the incidence of involuntary job changes within the industry and to other industries. The job changes of LBO employees are therefore not associated with increases in *Daily Wage*, and for them, the negative impact of changes to other industries on *Daily Wage* is larger; the employment gains from job changes are lower for LBO employees. Overall, a significant fraction of LBO employees is negatively affected by buyouts, because they are forced to accept lower-paid jobs or become unemployed.

We analyze white-collar employees separately in columns (4) to (6) of Table 5. Most effects are qualitatively similar, but quantitatively weaker, and sometimes insignificant, for white-collar workers compared to nonwhite-collar workers. The most important difference

concerns baseline effects for the control group, which are much less positive or even negative for the control group. Post-LBO establishment changes have practically no influence on white-collar workers.

5.2 How LBOs affect career paths

The results in the previous section suggest employees' human capital outcomes depend critically on how LBOs affect their career paths. Therefore, we are interested in whether employees change the establishment, the industry, or whether they become unemployed. We use the same career events as before and define the career indicator $CI_{i,p}$, which equals one if employee i changes his or her establishment, industry, or employment status within t years of the LBO. We use $t = 1$, $t = 3$, and $t = 5$ and estimate the regression

$$CI_{i,t} = \alpha_i + \gamma_s + \delta Target_{is} + \varepsilon_i. \tag{3}$$

The coefficient δ measures the increase in the frequency of career event CI_t for LBO target employees over a period of t years after the LBO. In some specifications we add additional control variables. Clustering is again at the establishment level. Since the analysis is cross-sectional, individuals can only belong to one cluster, which is assigned based on their establishment in the event year.

We estimate equation (3) for each career indicator and report the results in Table 6. Panel A reports estimation results for the causal impact of LBOs on career path variables. Each cell in the panel corresponds to a different regression. Each row displays results for one of the three career-path indicators as the dependent variable. Each column reports results for one of three different time horizons, all beginning in year t and extending, respectively, to years $t + 1$, $t + 3$, and $t + 5$. Calendar-year dummies are included in all regressions, but their estimates are not reported. Panel B is organized similarly and reports the frequencies of the same career-path events for the control group. Panel C reports the ratios of the effects

of Panel A divided by the corresponding ratio in Panel B. For example, over a three-year horizon, LBOs increase the incidence of within-industry job changes by 1.00%, whereas 4.74% of control employees experience the same event, or 21.08%=0.0100/0.0474 in relative terms. This comparison allows us to assess the importance of LBO-induced career path events.

We begin with establishment changes and focus on long-term effects. From Panel B, establishment changes within the same industry are rare and affect only 6.06% of all employees over a five-year horizon, independently of whether their employer became an LBO target. LBOs increase the frequency of within-industry job changes by an insignificant 0.7% in absolute terms and by 12% in relative terms. Industry changes are more frequent, with a baseline rate of 20.50% over a five-year horizon. LBOs increase the frequency of industry changes by 3.4 percentage points, or roughly 17% of the base rate over the same time horizon. LBOs account for less of the unemployment risk of LBO employees. In the control group, 10.33% of employees become unemployed within five years. This indicator measures whether employees became unemployed, not whether or when they regained employment. LBOs increase the incidence of unemployment by 0.9%, or 8.71% compared to the base rate. The effects of LBOs on industry changes and unemployment are statistically highly significant, whereas those on within-industry job changes are insignificant.

Table 6 helps us to bridge the analysis of Table 5, which relates career path events to human capital risk with that in Figure 4, which documents the adverse effect of LBOs on target employees' human capital. Table 5 shows employees who experience industry changes or unemployment lose a portion of their human capital, whereas Table 6 shows LBOs increase the likelihood of these adverse career events (unemployment, switching into lower-paid jobs). Hence, about 85% to 92% of the job fluctuations experienced by LBO employees can be attributed to normal fluctuations experienced by employees with similar characteristics. Nevertheless, the incremental impact of LBOs on target employees' careers can still explain their losses of human capital.

5.3 Career changes for different groups of employees

In this section we take the analysis of the previous section one step further and investigate whether LBOs have a different impact on the career paths of employees for different segments of the labor market. In Section 4.1, we observe different human capital effects for different subgroups of target employees. We hypothesize that LBOs influence the careers of these employees' differently. To test this hypothesis, we extend equation (3) and add triple interactions between the risk factors analyzed in Table 4, the $POST$ indicator, and the $Target$ dummy. Table 7 presents career path changes from the announcement year to five years after the LBO. (Table A5 in the Online Appendix presents results for the period three years after the LBO, which yields similar results.)

Age and both tenure variables increase the likelihood of becoming unemployed and reduce the likelihood of finding a job in the same industry, although these effects are not always significant. The influence of *Age* on unemployment risk (16.5 log points) is much larger than that of the tenure variables (0.7 and 1.1 log points). Hence, the loss of income and employment of LBO employees related to *Age* and, observed in Table 4, Panel A can be attributed to the fact that *Age* is a strong predictor of adverse career changes, which can account for why these employees suffer larger losses of income.

From Table 7, skilled employees are less likely to experience outside-industry job changes, which are typically associated with declines in income and wages. Medium-wage employees are *more* likely to find jobs within the same industry, which is associated with gains in wages and income. Together with Table 5, these results can now explain why these groups of workers tend to benefit from LBOs, a result we found surprising in Section 4.3 in the light of earlier studies on job polarization. White-collar employees and middle management have higher likelihoods of experiencing adverse career events such as industry changes (white-collar employees) and unemployment, where statistical significance is marginal for middle management (see Section 4.3.2).

Overall, the career-path analysis of Table 7 deepens the analysis in Table 4, which relates

employee characteristics to human capital risk. Table 7 shows that those subgroups of employees that are more likely to experience adverse career changes after an LBO suffer larger losses to their income and employment. Thus, this analysis helps to close the causal chain from employee characteristics and LBOs via career changes to human capital risk.

6 Discussion and Conclusion

We study the impact of LBOs on wages, employment, and the career paths of a large sample of German employees whose firms were acquired by private equity firms. The immediate impact of LBOs is positive, but overall, the long-term impact of LBOs on employment and income is negative.

We find strong evidence for views of LBOs based on organizational change, since middle management and white-collar workers are the main losers of these transactions, consistent with the notion that LBOs trim targets' administration and reduce layers of management. By contrast, we find no evidence that LBOs foster skill-biased technological change and enhance wage inequality. While we do find evidence for job polarization in the control group, this trend is mitigated and not reinforced by LBOs. The negative consequences of LBOs fall mostly on older employees and employees with longer tenure in their firms, most likely because these employees have more firm-specific human capital and are less adaptable to organizational and technological change. While our results are consistent with the view that private equity firms do not honor implicit employment guarantees, the effect is quantitatively small. The likelihood of LBO employees to experience adverse career changes is about 8% to 15% higher compared to employees in the control group. The specific prediction of the transfer-of-wealth view with respect to wages is not supported by our data: the wages of employees who continue to work for the target increases by about 3% per year, in line with wages in the control group.

All losses of human capital can be attributed to adverse career events such as unemploy-

ment or changes into lower-paid jobs. These events affect only those employees who lose their employment with the target firm. In particular, target employees who become unemployed or who are forced to accept jobs outside of the LBO target's industry lose in terms of income and employment. Within-industry job changes increase the wages of LBO employees, but by less than those of their peers in the control group.

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Figures

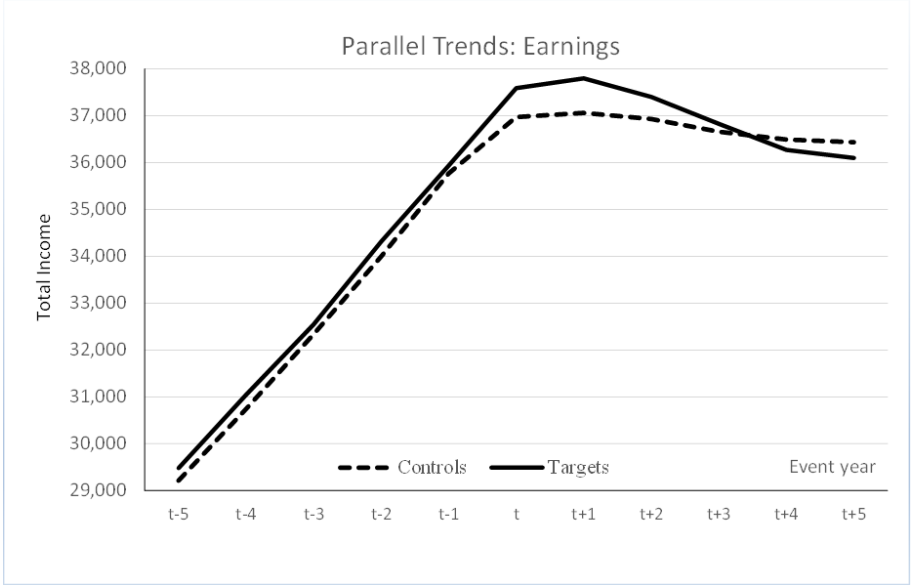


Figure 1: Parallel trends analysis: Earnings. This figure presents the development of *Earnings* in event time. For every event year, we compute the mean of *Total income* for target employees and control employees separately. *Earnings* is defined in Table 1.

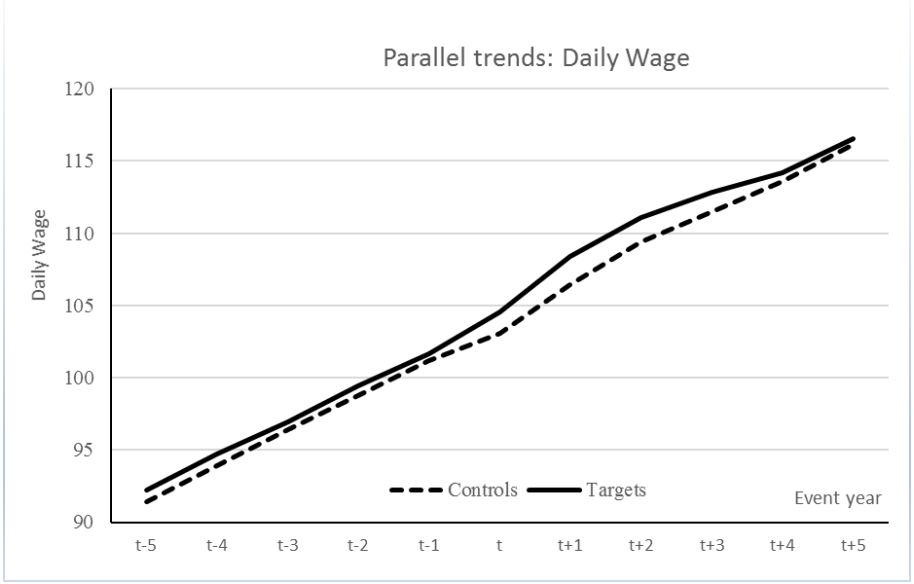


Figure 2: Parallel trends analysis: Daily Wage. This figure presents the mean of *Daily Wage* for target employees and control employees separately. *Daily Wage* is defined in Table 1.

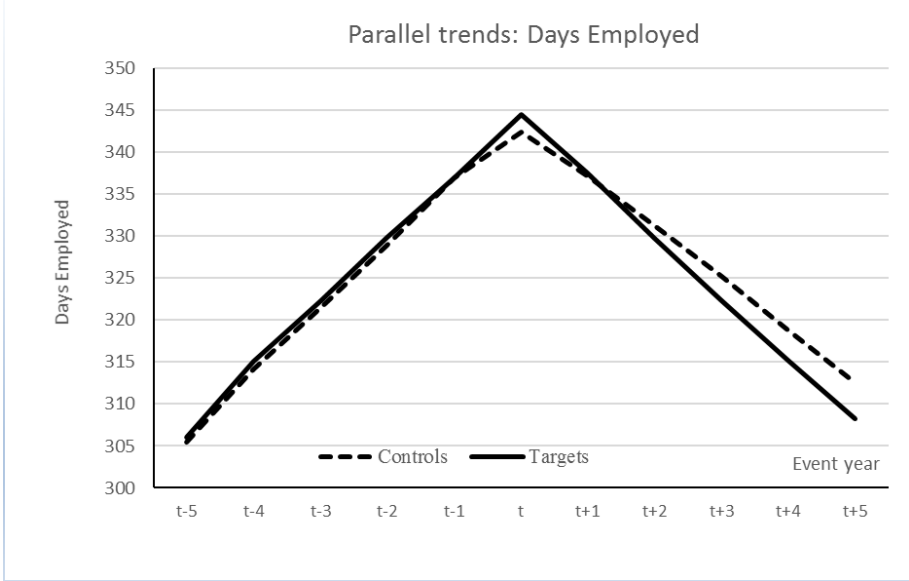


Figure 3: Parallel trends analysis: Days Employed. This figure presents the mean of *Days Employed* for target employees and control employees separately. *Days Employed* is defined in Table 1. The inverted-V pattern is a mechanical consequence of the requirement that employees in both groups have to be employed in the event year, but not before or after the event year.

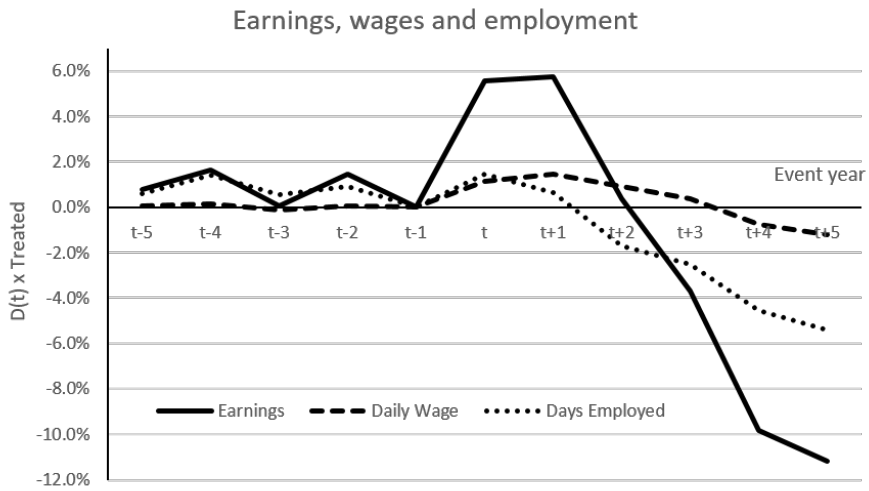


Figure 4: The impact of LBOs on wages and employment. The figure plots the coefficients θ_t on the interaction terms $Target \times D^t$ from OLS-regressions of *Earnings*, *Daily Wage* and *Days Employed* on a difference-in-differences setup and control variables as in (1). D^t_{is} is a dummy variable which is one for observations t years after the event year, where t and runs from five years before the LBO ($t-5$) to five years after the LBO. The dependent variables are in logs and defined in Table 1. Regressions control for person fixed effects and calendar-year fixed effects.

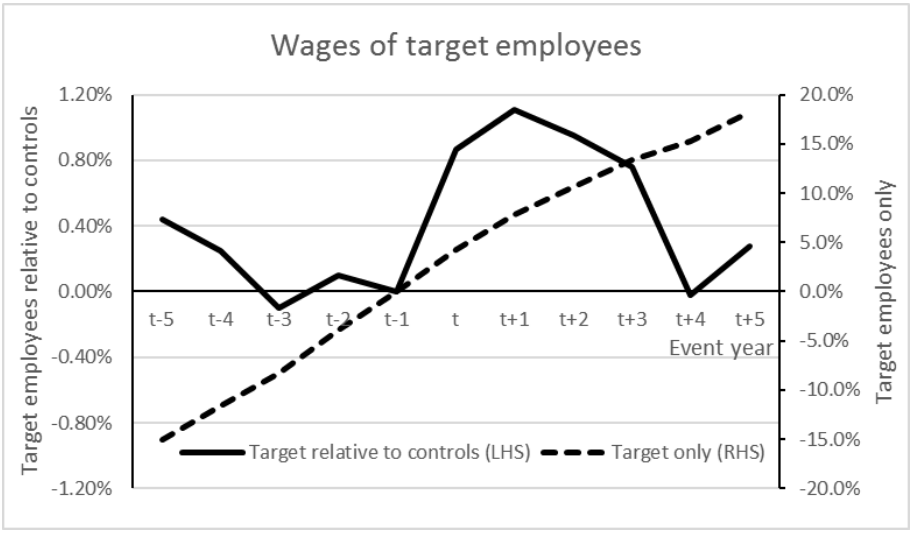
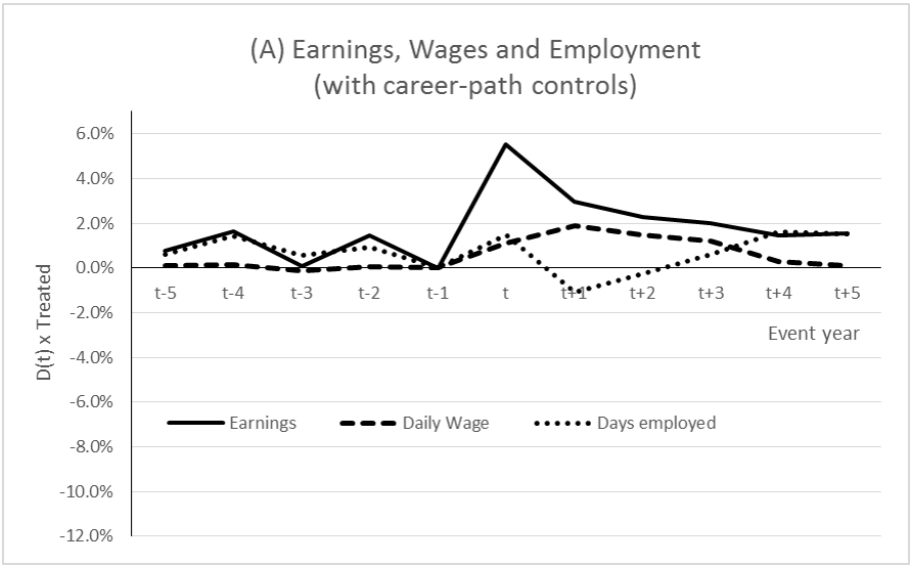


Figure 5: Breach of Trust. The table presents OLS-regressions of *Daily Wage* on event time dummies and control variables. The solid line (left axis) represents the difference in Daily Wage between employees who stay with the target and their controls. Target employees are included if both they and their match stay with their establishment from $t-1$ to $t+5$. The broken line (right axis) shows a single-difference analysis and includes only target employees who stay with their establishment from $t-1$ to $t+5$.



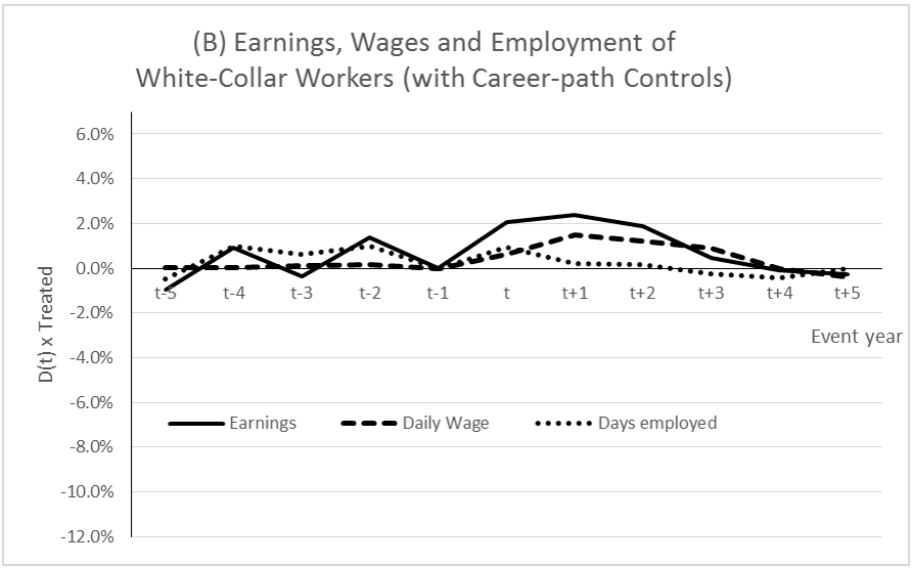


Figure 6: LBOs and career path events. The figure plots the coefficients θ_t on the interaction terms $Target \times D^t$ from OLS-regressions of *Earnings*, *Daily Wage* and *Days Employed* on a difference-in-differences setup and control variables as in (1). The regressions include dummy variables *Establishment Change*, *Industry Change*, and *Unemployment*. D^t_{is} is a dummy variable which is one for observations k years after the event year t and runs from five years before the LBO $(t-5)$ to five years after the LBO $(t+5)$. The dependent variables are in logs and defined in Table 1. Regressions control for person fixed effects and calendar-year fixed effects. Panel A includes all observations in the sample. Panel B includes only observations for white-collar workers.

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;∞]
Daily Wage	Wagesum divided by Days Employed	]0;∞]
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;∞]
Employed	1 unless unemployed or in vocational training	0 or 1
Fraction Employed	Days Employed divided by 366	[0;1]
Target	1 if Person in Target Company	0 or 1
Firm Tenure	Days in employment in current spell	[0;∞]
Industry Tenure	Days in employment in 3-digit industry	[0;∞]
Middle Management	1 if Qualification is equal to "Managers"	0 or 1
Offshorable job	1 if high offshorability risk job as defined in Goos et al. (2014)	0 or 1
Routine-based job	1 if high routine intensity job as defined in Goos et al. (2014)	0 or 1

Table 2: Sample description. This table provides an overview of our sample with respect to our categorical variables. Our sample includes 147,116 LBO employees and the same number of control employees. The results are solely based on the year prior to the transaction, which has been used for matching.

	LBO	Control	Total
Occupational groups	Employees	Employees	Labor force
(1) Simple manual occupations	26.1%	26.1%	17.1%
(2) Skilled manual occupations	21.4%	21.4%	14.3%
(3) Technicians/engineers	18.8%	18.8%	6.3%
(4) Simple service	6.5%	6.5%	19.7%
(5) Qualified service	0.4%	0.4%	3.3%
(6) Semi-professions	0.2%	0.2%	4.6%
(7) Professions	0.4%	0.4%	1.9%
(8) Simple commercial and administrative occupations	5.1%	5.1%	11.4%
(9) Qualified commercial and administrative occupations	18.7%	18.7%	19.0%
(10) Managers	2.4%	2.4%	2.3%
Females	21.6%	21.6%	46.1%
Nationality			
(1) German	94.0%	94.0%	89.8%
(2) Immigrant population	4.3%	4.3%	5.1%
(3) Rest of the world	1.7%	1.7%	5.1%
Occupational status			
(1) Vocational training	4.8%	4.8%	7.8%
(2) Blue collar (unskilled)	21.0%	22.5%	16.2%
(3) Blue collar (skilled)	25.0%	23.7%	13.5%
(4) Master craftsmen	1.9%	1.9%	0.9%
(5) White collar	41.3%	41.0%	28.0%
(6) Home worker	0.0%	0.0%	0.1%
(7) Part-time employees	6.0%	6.0%	33.4%
Education			
Intermediate school leaving certificate			
– (1) without vocational training (low education)	12.9%	12.9%	23.3%
– (2) with vocational training (medium education)	67.2%	67.3%	58.7%
Upper secondary school leaving certificate			
– (3) without vocational training (medium education)	1.4%	1.3%	3.4%
– (4) with vocational training (high education)	4.9%	4.9%	5.8%
(5) College or university degree (high education)	13.6%	13.6%	8.9%

Table 2: Sample description (continued).

Industries	LBO Employees	Control Employees	Total Labor force
Manufacturing	71.3%	71.3%	25.7%
Retail, maintenance and repair services	10.4%	10.4%	17.3%
Real estate	10.9%	10.9%	15.1%
Telecommunications	2.8%	2.8%	5.5%
Construction	1.2%	1.2%	6.3%
All other	3.4%	3.4%	30.1%
Region			
(1) North (Schleswig-Holstein, Hamburg, Niedersachsen, Bremen)	11.9%	11.9%	15.6%
(2) East (Berlin, Brandenburg, Mecklenburg-Vorpommern, Sachsen, Sachsen-Anhalt, Thueringen)	10.7%	10.7%	18.2%
(3) South (Hessen, Baden-Wuerttemberg, Bayern)	47.0%	47.0%	38.4%
(4) West (Nordrhein-Westfalen, Rheinland-Pfalz, Saarland)	23.2%	23.2%	27.7%

Table 3: Descriptive Statistics. This table provides descriptive statistics for all numerical variables. The variables are defined in Table 1.

	N	Mean	Median	Minimum	Maximum	Standard Deviation
Age	3,236,552	41	42	9	91	11
Daily Wage	2,959,570	104	101	0	1,663	42
Days Employed	3,236,552	325	365	0	366	106
Earnings	3,236,552	34,905	35,154	0	207,583	17,820
Fraction Employed	3,236,552	0.89	1.00	0.00	1.00	0.29
Firm Tenure	3,236,552	3,358	2,421	0	14,245	3,113
Industry Tenure	3,236,552	3,894	3,075	0	14,245	3,296
Offshorable job	3,195,483	0.48	0.00	0	1	0.50
Routine-based job	3,195,483	0.53	1.00	0	1	0.50

Table 4: Modernization and human capital. The table presents OLS-regressions of *Earnings*, *Daily Wage*, and *Days Employed* in a triple-difference setup as in equation (2). All dependent variables are in logarithms. Each specification includes a risk factor, which is measured in the year prior to the LBO announcement and is described at the top of the table. In Panel A, we analyze education, qualification, and wages. *Unskilled* and *Skilled* both refer to the respective groups of blue collar workers. *Low Wage* and *Med Wage* denote the first and second tercile of *Earnings* of all employees employed over the full year. The categorization of education into low, medium, and high is defined in Table 2. In Panel B, we analyze offshorability and routinization. In Panel C, we analyze two groups of the labor force that are most likely to be affected by organizational change: white collar workers and *Middle Management* ($Mid.Man.$). In Panel D, we analyze tenure and age and use superscript H (L) for sample splits into high (low) values. $F.Tenure^H$ ($F.Tenure^L$) is *Firm Tenure* above (below) the sample median. $Ind.Tenure^H$ ($Ind.Tenure^L$) is *Industry Tenure* above (below) the sample median. Age^H (Age^L) is *Age* above (below) the sample median. The numerical variables are defined in Table 1 and the categorical variables are defined in Table 2. Standard errors are clustered at the establishment level. t-statistics are provided in parentheses. *, **, *** indicate significance at the 10%, 5% and 1% level respectively.

Table 4: Modernization and human capital: Panel A - technological modernization.

Dependent Variable:	(1) Earnings	(2) Daily Wage	(3) Days Employed	(4) Earnings	(5) Daily Wage	(6) Days Employed	(7) Earnings	(8) Daily Wage	(9) Days Employed
Risk Factor (RF) 1	Low edu.	Low edu.	Low edu.	Unskilled	Unskilled	Unskilled	Low wage	Low wage	Low wage
Risk Factor (RF) 2	Med edu.	Med edu.	Med edu.	Skilled	Skilled	Skilled	Med wage	Med wage	Med wage
Reference group	High edu	High edu	High edu	White collar	White collar	White collar	High wage	High wage	High wage
$D_{is}^{[0;4]}$	0.5191***	0.0040*	0.1686***	0.3886***	0.0210***	0.2214***	0.2977***	0.0093***	0.1684***
	31.33	1.88	17.24	27.25	11.69	27.82	19.20	5.06	20.07
D_{is}^{5}	0.5869***	-0.0037	0.0905***	0.3918***	0.0307***	0.2343***	0.2587***	0.0112***	0.1716***
	18.67	-0.97	4.82	14.80	9.47	15.80	7.63	3.35	9.36
$D_{is}^{[0;4]}$ x Target	-0.0467***	0.0032	-0.0289***	-0.0227	0.0037	-0.0151**	-0.0523**	0.0007	-0.0291**
	-2.65	1.18	-3.01	-1.59	1.57	-2.02	-2.00	0.23	-2.16
D_{is}^{5} x Target	-0.1425***	-0.0127***	-0.0703***	-0.1377***	-0.0144***	-0.0672***	-0.1738**	-0.0169**	-0.0857**
	-3.30	-2.91	-3.06	-3.52	-3.17	-3.28	-2.48	-2.57	-2.36
$D_{is}^{[0;4]}$ x RF 1	0.3212***	0.1606***	0.9279***	-0.0199	-0.0165***	-0.0117	-0.0761***	0.0100***	-0.0811***
	13.76	27.19	34.24	-1.52	-8.98	-1.61	-5.22	5.84	-10.17
D_{is}^{5} x RF 1	0.3115***	0.2737***	1.3526***	0.0092	-0.0264***	-0.0031	0.0026	0.0241***	-0.0811***
	7.49	30.69	35.12	0.33	-8.33	-0.20	0.08	8.19	-4.36
$D_{is}^{[0;4]}$ x RF 2	-0.1077***	-0.0082***	-0.0292***	0.01	0.0007	0.0028	0.0585***	0.0126***	0.0181**
	-8.17	-4.70	-3.84	0.84	0.44	0.43	4.40	8.30	2.42
D_{is}^{5} x RF 2	-0.1242***	-0.0107***	-0.0329**	0.0809***	0.0144***	0.0290**	0.1868***	0.0262***	0.0728***
	-4.60	-3.95	-2.22	2.94	5.31	1.97	5.80	9.61	4.19
$D_{is}^{[0;4]}$ x Target x RF 1	0.1308***	0.0025	0.0245	-0.0077	-0.0027	-0.0036	0.0049	0.0058	-0.0035
	2.75	0.19	0.41	-0.33	-0.69	-0.27	0.17	1.55	-0.21
D_{is}^{5} x Target x RF 1	0.0035	-0.0048	0.02	-0.0397	0.0027	-0.0308	0.025	0.0107	0.0037
	0.05	-0.25	0.25	-0.72	0.39	-0.99	0.34	1.45	0.09
$D_{is}^{[0;4]}$ x Target x RF 2	0.0379*	0.0037	0.0182	0.0172	0.0090**	0.0038	0.0359*	0.0074**	0.0158
	1.89	1.05	1.63	0.86	2.45	0.36	1.68	2.16	1.35
D_{is}^{5} x Target x RF 2	0.0441	0.0012	0.0199	0.1095**	0.0092	0.0502**	0.0842	0.0064	0.0378
	1.11	0.22	0.94	2.40	1.36	2.08	1.60	1.12	1.33
Observations	2,059,624	1,877,366	2,059,624	1,836,506	1,695,780	1,836,506	1,841,364	1,702,490	1,841,364
R^2	0.05	0.05	0.03	0.05	0.02	0.05	0.06	0.02	0.07
Person FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Calendar-Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 4: Modernization and human capital: Panel B - routinization and offshorability.

Dependent Variable:	(1) Earnings	(2) Daily Wage	(3) Days Employed	(4) Earnings	(5) Daily Wage	(6) Days Employed
Risk Factor (RF) 1	Routine job	Routine job	Routine job	Offshor-able job	Offshor-able job	Offshor-able job
Reference group	All others	All others	All others	All others	All others	All others
$D_{is}^{[0;4]}$	0.4900***	0.0118***	0.2397***	0.4957***	0.0273***	0.2852***
	33.93	5.89	26.27	33.54	12.51	27.46
D_{is}^{5}	0.5127***	0.0135***	0.1990***	0.5800***	0.0429***	0.3038***
	19.49	3.61	12.32	21.94	11.09	18.07
$D_{is}^{[0;4]}$ x Target	-0.0486***	0.0053*	-0.0305***	0.0078	0.0088*	-0.0063
	-3.67	1.84	-3.39	0.53	1.87	-0.43
D_{is}^{5} x Target	-0.1682***	-0.0136***	-0.0835***	-0.1101**	-0.0089	-0.0506*
	-4.51	-2.69	-4.30	-2.52	-1.03	-1.75
$D_{is}^{[0;4]}$ x RF 1	-0.0339***	0.0109***	0.0389***	-0.0471***	-0.0195***	-0.0511***
	-3.14	5.58	4.16	-4.38	-9.80	-5.35
D_{is}^{5} x RF 1	0.0212	0.0193***	0.0914***	-0.1108***	-0.0382***	-0.1126***
	0.96	6.34	6.09	-4.97	-12.26	-7.41
$D_{is}^{[0;4]}$ x Target x RF 1	0.0773***	0.0003	0.0260*	-0.0341**	-0.0076*	-0.0238
	4.32	0.08	1.65	-1.99	-1.77	-1.50
D_{is}^{5} x Target x RF 1	0.1103***	0.0013	0.0491*	-0.0035	-0.0098	-0.0186
	3.06	0.20	1.91	-0.09	-1.29	-0.66
R^2	2,034,417	1,856,440	2,034,417	2,034,417	1,856,440	2,034,417
Observations	0.05	0.03	0.02	0.05	0.04	0.02
Person FE	Yes	Yes	Yes	Yes	Yes	Yes
Calendar-Time FE	Yes	Yes	Yes	Yes	Yes	Yes

Table 4: Modernization and human capital: Panel C - organizational change.

Dependent Variable:	(1) Earnings	(2) Daily Wage	(3) Days Employed	(4) Earnings	(5) Daily Wage	(6) Days Employed
Risk Factor (RF)	Middle Mgmt.	Middle Mgmt.	Middle Mgmt.	White Collar	White Collar	White Collar
Reference group	All others	All others	All others	Blue Collar	Blue Collar	Blue Collar
$D_{is}^{[0;4]}$	0.4949***	0.0189***	0.2746***	0.3843***	0.0135***	0.2173***
	36.52	9.91	31.48	27.84	7.05	28.12
D_{is}^{5}	0.5628***	0.0238***	0.2586***	0.4395***	0.0262***	0.2485***
	23.81	6.88	17.49	16.68	7.87	16.77
$D_{is}^{[0;4]}$ x Target	-0.0017	0.0062*	-0.0123	-0.0156	0.0081**	-0.0141
	-0.14	1.77	-1.34	-0.90	2.07	-1.56
D_{is}^{5} x Target	-0.1068***	-0.0123*	-0.0514**	-0.0917*	-0.0067	-0.0516**
	-3.05	-1.95	-2.47	-1.95	-0.98	-2.04
$D_{is}^{[0;4]}$ x RF	-0.2872***	-0.0394***	-0.2747***	0.0041	0.0074***	0.004
	-7.19	-13.43	-12.17	0.39	5.33	0.69
D_{is}^{5} x RF	-0.8300***	-0.0721***	-0.6122***	-0.0471**	0.0044*	-0.0139
	-10.30	-13.75	-14.07	-2.07	1.89	-1.12
$D_{is}^{[0;4]}$ x Target x RF	-0.0897	-0.0056	-0.0434	-0.0071	-0.0044	-0.001
	-1.58	-1.05	-1.38	-0.41	-1.36	-0.10
D_{is}^{5} x Target x RF	-0.2122*	-0.0094	-0.1149*	-0.046	-0.0076	-0.0155
	-1.76	-1.04	-1.78	-1.19	-1.44	-0.74
Observations	2,059,624	1,877,366	2,059,624	1,836,506	1,695,780	1,836,506
R^2	0.05	0.04	0.02	0.05	0.02	0.05
Person FE	Yes	Yes	Yes	Yes	Yes	Yes
Calendar-Time FE	Yes	Yes	Yes	Yes	Yes	Yes

Table 4: Modernization and human capital: Panel D - specificity of human capital.

Dependent Variable:	(1) Earnings	(2) Daily Wage	(3) Days Employed	(4) Earnings	(5) Daily Wage	(6) Days Employed	(7) Earnings	(8) Daily Wage	(9) Days Employed
Risk Factor (RF)	F. Tenure^H	F. Tenure^H	F. Tenure^H	Ind. Tenure^H	Ind. Tenure^H	Ind. Tenure^H	Age^H	Age^H	Age^H
Reference group	F. Tenure^L	F. Tenure^L	F. Tenure^L	Ind. Tenure^L	Ind. Tenure^L	Ind. Tenure^L	Age^L	Age^L	Age^L
$D_{is}^{[0;4]}$	0.7539***	0.0599***	0.5342***	0.7760***	0.0633***	0.5498***	0.8091***	0.0672***	0.5553***
	44.58	23.41	42.97	50.34	26.39	49.56	48.97	28.63	48.34
D_{is}^{5}	0.9942***	0.0950***	0.6717***	1.0313***	0.0989***	0.6927***	1.2291***	0.1036***	0.7834***
	37.36	22.92	37.47	40.06	24.65	40.43	47.26	26.79	45.87
$D_{is}^{[0;4]}$ x Target	0.0356*	0.0067	0.0027	0.0184	0.0050	-0.0131	0.0575***	0.0098**	0.0172
	1.90	1.23	0.13	1.02	1.02	-0.71	3.78	2.27	1.20
D_{is}^{5} x Target	-0.0688***	-0.0143*	-0.0217	-0.1052***	-0.0145*	-0.0538*	-0.0016	-0.0042	0.0120
	-2.58	-1.73	-0.76	-3.76	-1.91	-1.93	-0.07	-0.58	0.57
$D_{is}^{[0;4]}$ x RF	-0.5376***	-0.0824***	-0.5388***	-0.5666***	-0.0867***	-0.5557***	-0.6442***	-0.0973***	-0.5795***
	-37.39	-37.36	-50.25	-39.58	-40.22	-53.32	-46.43	-48.14	-54.75
D_{is}^{5} x RF	-0.9030***	-0.1440***	-0.8563***	-0.9683***	-0.1507***	-0.8899***	-1.4393***	-0.1742***	-1.1294***
	-29.51	-41.99	-46.72	-31.78	-43.74	-48.57	-48.95	-51.97	-59.33
$D_{is}^{[0;4]}$ x Target x RF	-0.0789**	-0.0009	-0.0304	-0.0605*	0.0000	-0.0138	-0.1203***	-0.0066**	-0.0565***
	-2.33	-0.19	-1.44	-1.79	-0.01	-0.68	-3.83	-1.99	-2.79
D_{is}^{5} x Target x RF	-0.0846	0.0046	-0.0628*	-0.0374	0.0009	-0.0221	-0.2126***	-0.0159***	-0.1242***
	-1.18	0.66	-1.76	-0.53	0.13	-0.62	-3.32	-2.83	-3.27
R^2	2,059,624	1,877,366	2,059,624	2,059,624	1,877,366	2,059,624	2,059,624	1,877,366	2,059,624
Observations	0.05	0.04	0.03	0.05	0.05	0.03	0.06	0.05	0.04
Person FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Calendar-Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 5: The impact of LBOs on wages and employment controlling for career paths. The table presents OLS-regressions of *Earnings*, *Daily Wage* and *Days Employed* on a difference-in-differences setup. All dependent variables are in logarithms. We distinguish between changes within the industry (*Establishment Change*) and changes to another industry (*Industry Change*) and control for establishment changes after the LBO year by interacting the career indicators with *Post* which is a binary variable set to 1 from t+1 onward. *Unemployed* is set to 1 if the individual is unemployed at the end of the respective calendar year. D_{it}^{k} is a dummy variable which is 1 if the difference between t and the announcement year is equal to k. In columns (4) to (6) run the same analyses as in columns (1) to (3) but we restrict the sample to employees that had a white collar job one year prior to the transaction. The numerical variables are defined in Table 1 and the categorical variables are defined in Table 2. Standard errors are clustered at the establishment 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	Daily Wage	Days Employed	Earnings	Daily Wage	Days Employed
Sample	— All non-white collar workers —			— All white collar workers —		
Establishment Change x Post	0.4064***	0.0279***	0.4680***	0.0772***	-0.0130***	0.0600***
	10.02	2.93	13.58	3.80	-2.83	4.59
Industry Change x Post	0.4768***	-0.0616***	0.6560***	0.0039	-0.0818***	0.0518***
	21.39	-12.20	39.21	0.34	-19.46	7.06
Establishment Change x Post x Target	-0.1686***	-0.0274**	-0.2011***	-0.0092	0.0152**	-0.0073
	-2.60	-2.05	-3.46	-0.34	2.33	-0.41
Industry Change x Post x Target	-0.0687*	-0.0423***	-0.0385	-0.0451***	-0.0255***	-0.0095
	-1.72	-5.01	-1.11	-2.60	-3.97	-0.84
Unemployed	-9.1485***		-5.0801***	-10.0686***		-5.5498***
	-303.35		-242.16	-340.55		-353.81
Unemployed x Target	-0.2748***		-0.2081***	-0.1494***		-0.0688***
	-6.84		-7.45	-3.75		-3.29
R^2	0.49	0.12	0.39	0.64	0.17	0.57
Observations	3,236,552	2,959,570	3,236,552	1,332,023	1,245,317	1,332,023
Person FE	Yes	Yes	Yes	Yes	Yes	Yes
Calendar-Time FE	Yes	Yes	Yes	Yes	Yes	Yes

Table 6: The impact of LBOs on employees' career paths. Panel A presents the coefficient on *Target* of cross-sectional regressions of a change in employment on *Target* and control variables. The dependent variable denotes a change in the respective category relative to the announcement year (s). We report the coefficients obtained for *Target*. Panel B reports the number of career path events for the control group divided by the number of control employees (N=147,116). The numerical variables are defined in Table 1 and the categorical variables are defined in Table 2. Standard errors are clustered at the establishment level. t-statistics are provided below the coefficient estimates. *, **, *** indicate significance at the 10%, 5% and 1% level respectively.

Panel A.			
Dependent variable / Time Horizon	(t+1)	(t+3)	(t+5)
Establishment Change within 3 digit industry	0.002	0.01	0.007
	0.37	1.38	0.98
Industry Change to other 3 digit industry	0.017*	0.021*	0.034***
	1.73	1.95	2.71
Change into unemployment	-0.004***	0.003*	0.009***
	-3.65	1.7	3.68

Panel B. Baseline effects in the control group			
	(t+1)	(t+3)	(t+5)
	N (in %)	N (in %)	N (in %)
Establishment Change within 3 digit industry	3192 (2.17%)	6979 (4.74%)	8913 (6.06%)
Industry Change to other 3 digit industry	10126 (6.88%)	22132 (15.04%)	30158 (20.50%)
Change into unemployment	3047 (2.07%)	9567 (6.50%)	15199 (10.33%)

Panel C. LBO / Base rates			
	(t+1)	(t+3)	(t+5)
Establishment Change within 3 digit industry	9.22%	21.08%	11.55%
Industry Change to other 3 digit industry	24.70%	13.96%	16.59%
Change into unemployment	-19.31%	4.61%	8.71%

Table 7: LBOs and employees' career paths - subgroup analysis. The table presents cross-sectional regressions of a change in employment from t to $t+5$ on *Target,* a risk factor and the interaction of *Target* and the risk factor, where t denotes the announcement year. The table solely presents the coefficients on the interaction terms. The analysis is an extension of the analysis in Panel A of Table 6. Each specification includes a set of control variables and a risk factor, which is specified at the top of the table. All variables are measured in the year of the LBO announcement. In addition to the risk factor, we control for gender, qualification, middle management, $F.\,Tenure^H$, and Age^H in all specifications. The categorization of education into low, medium, and high is defined in Table 2. *Unskilled* and *Skilled* both refer to the group of blue collar workers. *Low Wage* denotes the bottom tercile of the annual wage of all employees who are employed the full year. $F.\,Tenure^H$ ($F.\,Tenure^L$) denotes *Firm Tenure* above (below) the sample median. $Ind.\,Tenure^H$ ($Ind.\,Tenure^L$) denotes *Industry Tenure* above (below) the sample median. Age^H (Age^L) denotes *Age* above (below) the sample median. Mid. Man. denotes *Middle Management*. The numerical variables are defined in Table 1 and the categorical variables are defined in Table 2. Standard errors are clustered at the establishment level. T-statistics are provided below the coefficient estimates. *, **, *** indicate significance at the 10%, 5% and 1% level respectively.

Table 7: Risk factors of LBOs: career path analysis (continued).

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Risk factor (RF) 1	Low edu.	Unskilled	Low Wage	Routine-based job	Offshor-able job	F. Tenure^H	Ind. Tenure^H	Age^H	Mid. man.	W. Collar
Risk factor (RF) 2	Med edu.	Skilled	Med Wage							
Reference group	High edu	White collar	High Wage	All others	All others	F. Tenure^L	Ind. Tenure^L	Age^L	All others	B. Collar
Dependent Variable: Establishment Change within 3 digit industry										
Target x RF 1	0.006	0.017	0.021**	0.001	0.007	-0.005	-0.024***	-0.01	-0.018*	-0.002
	0.49	1.23	2.13	0.14	0.95	-0.75	-2.74	-1.49	-1.69	-0.23
Target x RF 2	0.006	-0.009	0.011							
	0.77	-1.37	1.33							
Dependent Variable: Industry change to other 3 digit industry										
Target x RF 1	-0.008	-0.028*	-0.003	-0.023**	0.020*	0.007	0.001	-0.104***	0.034*	0.031**
	-0.48	-1.7	-0.18	-2.09	1.8	0.46	0.11	-7.33	1.81	2.52
Target x RF 2	-0.009	-0.027**	-0.004							
	-0.79	-2.03	-0.29							
Dependent Variable: Change into unemployment										
Target x RF 1	0.003	0.007	-0.001	-0.001	-0.002	0.011**	0.007	0.165***	0.020*	0.003
	0.41	1.46	-0.2	-0.22	-0.53	2.51	1.63	17.42	1.66	0.82
Target x RF 2	-0.002	-0.005	-0.007							
	-0.54	-1.28	-1.57							

Online Appendix for "Private Equity and Human Capital Risk"

July 27, 2017

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 Internet Appendix are referred to as A-#, where # is the table number in the appendix.

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

Level / Step	Data Sources	Matching Process	Number of observations
1. Transactions	Thomsone One, Capital IQ, Bundesverband der Kapitalanlagegesellschaften	–	891 Transactions
2. Companies	Hoppenstedt Firmendatenbank	Manually	619 Transactions 1,108 Companies
3. Establishments	IAB: Establishment History Panel	Record Linkage	530 Transactions 815 Companies 3,330 Establishments
4. Employment Histories	IAB: Integrated Employment Biographies	Establishment ID	488 Transactions 722 Companies 2,429 Establishments 190,192 Target Employees
5. Matched Data Set		Algorithm see Table 2	479 Transactions 705 Companies 2,264 Establishments 147,116 Target Employees

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

Dimension	Matching Date	Categories
Nationality	t=-1	(1) German, (2) Immigrant (Greece, Italy, Turkey, former Yugoslavia countries), (3) Rest of the World
Region	t=-1	(1) South (Hesse, Baden-Wuerttemberg, Bavaria), (2) West (North Rhine-Westphalia, Rhineland-Palatinate, Saarland), (3) East (Berlin, Brandenburg, Mecklenburg-West Pomerania, Saxony, Saxony-Anhalt, Thuringia), (4) North (Schleswig-Holstein, Hamburg, Lower Saxony, Bremen)
Employment	t=-1, t=0	(1) Employed all year, (2) employed parts of the year, (3) not employed at all
Experience	t=-1	Quintiles according to tenure in spell (establishment)
Occupational group	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
Occupational status	t=-1	(1) Vocational Training, (2) Full-time employment, (3) Part-time employment, (4) home worker
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
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 of households as employers, (17) Activities of extraterritorial organisations and bodies

Table 3: Matching success. This table presents descriptive statistics on LBO employees, control employees, and unmatched LBO employees. All variables are measured in the year prior to the LBO announcement. The Imbens-Wooldridge test is a test statistic for assessing the matching quality in large samples. 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
Panel A. Matched LBO employees, N = 147,116					
Mean	35,939	98.63	0.92	3,313	40.53
Median	35,517	97.07	1.00	2,313	41.00
Variance	3.E+08	1865.48	0.07	9.E+06	119.99
Panel B. Matched control employees, N = 147,116					
Mean	35,768	98.63	0.92	3,296	40.33
Median	35,347	97.07	1.00	2,284	41.00
Variance	3.E+08	1865.48	0.07	9.E+06	116.64
Comparison to Matched LBO employees:					
Relative difference	0.48%	0.00%	0.00%	0.50%	0.50%
Imbens-Wooldridge test	0.01	0.00	0.00	0.00	0.01
Panel C. Unmatched LBO employees, N = 43,076					
Mean	23,950	77.09	0.82	1,448	37.18
Median	20,870	71.84	1.00	730	36.00
Variance	3.E+08	2193.35	0.09	4.E+06	145.34
Comparison to Matched LBO employees:					
Imbens-Wooldridge test	0.50	0.34	0.26	0.52	0.21

Table 4: Modernization and human capital: technological modernization. The table presents OLS-regressions of *Earnings*, *Daily Wage*, and *Days Employed* in a triple-difference setup as in equation (2). All dependent variables are in logarithms. Each specification includes a risk factor, which is measured in the year prior to the LBO announcement and is described at the top of the table. We analyze education, qualification, and wages as in Table 4 Panel A with the only difference being that we only include the middle terciles and leave out the bottom and top terciles. *Skilled* refer to the group of skilled blue collar workers. *Med Wage* denotes the second tercile of *Earnings* of all employees employed over the full year. The categorization of education into low, medium, and high is defined in Table 2. The numerical variables are defined in Table 1 and the categorical variables are defined in Table 2. Standard errors are clustered at the establishment level. t-statistics are provided in parentheses. *, **, *** indicate significance at the 10%, 5% and 1% level respectively.

Table 4: Modernization and human capital: technological modernization. (continued)

Dependent Variable:	(1) Earnings	(2) Daily Wage	(3) Days Employed	(4) Earnings	(5) Daily Wage	(6) Days Employed	(7) Earnings	(8) Daily Wage	(9) Days Employed
Risk Factor (RF)	Med edu.	Med edu.	Med edu.	Skilled	Skilled	Skilled	Med wage	Med wage	Med wage
Reference group	All others	All others	All others	All others	All others	All others	All others	All others	All others
$D_{is}^{[0;4]}$ x Target x RF	-0.0159	0.0027	0.0081	0.0195	0.0097***	0.0048	0.0324*	0.0044	0.0167
	-0.71	0.58	0.36	1.01	2.77	0.47	1.71	1.21	1.62
D_{is}^{5} x Target x RF	0.0427	0.003	0.0117	0.1231***	0.0078	0.0606**	0.0709	0.001	0.0349
	1.19	0.45	0.37	2.69	1.14	2.51	1.47	0.15	1.37
R^2	2,059,624	1,877,366	2,059,624	1,836,506	1,695,780	1,836,506	1,841,364	1,702,490	1,841,364
Observations	0.05	0.04	0.03	0.05	0.02	0.05	0.06	0.02	0.07
Person FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Calendar-Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 5: The impact of LBOs on wages and employment: White collar vs. blue collar workers. The table presents OLS-regressions of *Earnings*, *Daily Wage* and *Days Employed* on a difference-in-differences setup. D_{it}^{k} is a dummy variable which is 1 if the difference between t and the announcement year is equal to k. *Target* denotes employees that haven been involved in a Private Equity takeover. In columns (2), (5), (7) we restrict the sample to white collar employees (*W. Collar*) and in columns (3), (6), (9), we restrict the sample to blue collar employees (*B. Collar*). We only report the interaction terms to have the table clearly laid out but include the level variables in the analysis. The numerical variables are defined in Table 1. Standard errors are clustered at the establishment level. t-statistics are provided below the coefficient estimates. *, **, *** indicate significance at the 10%, 5%, and 1% level respectively.

Table 5: The impact of LBOs on wages and employment: White collar vs. blue collar workers. (continued)

Sample:	All	W. Collar	B. Collar	All	W. Collar	B. Collar	All	W. Collar	B. Collar
Dependent Variable:	— Earnings (ln) —			— Daily Wage (ln) —			— Days Employed (ln) —		
D_{it}^{-5} x Target	0.0078	-0.0095	0.0005	0.0007	0.0002	0.0019	0.0059	-0.0046	0.0023
	0.23	-0.39	0.02	0.16	0.04	0.35	0.31	-0.27	0.11
D_{it}^{-4} x Target	0.0164	0.0094	0.0098	0.0013	0.0020	0.0009	0.0143	0.0100	0.0100
	0.66	0.54	0.57	0.34	0.52	0.20	1.01	0.83	0.62
D_{it}^{-3} x Target	0.0006	-0.0034	0.0004	-0.0012	0.0014	-0.0021	0.0056	0.0066	0.0013
	0.04	-0.29	0.03	-0.37	0.46	-0.48	0.60	0.76	0.11
D_{it}^{-2} x Target	0.0145*	0.0139**	0.0197**	0.0006	0.0015	0.0011	0.0091*	0.0099**	0.0129*
	1.80	1.97	2.36	0.28	0.73	0.38	1.77	2.00	1.79
D_{it}^{0} x Target	0.0554***	0.0206*	0.0276***	0.0112***	0.0064***	0.0127***	0.0147*	0.0093	0.0137**
	4.57	1.91	2.74	5.78	4.14	4.99	1.88	1.47	2.09
D_{it}^{1} x Target	0.0573***	0.0364***	0.0319***	0.0144***	0.0112***	0.0167***	0.0063	0.0116**	0.0064
	5.83	3.64	2.65	4.57	4.91	4.33	0.84	2.04	0.98
D_{it}^{2} x Target	0.0036	-0.0135	-0.0024	0.0093**	0.0081***	0.0097**	-0.0169	-0.0126	-0.0099
	0.23	-0.74	-0.12	2.32	2.71	2.08	-1.59	-1.34	-0.95
D_{it}^{3} x Target	-0.0371	-0.0470*	-0.0335	0.0038	0.0024	0.0069	-0.0254*	-0.0259*	-0.0237
	-1.52	-1.75	-1.09	0.84	0.72	1.33	-1.70	-1.84	-1.44
D_{it}^{4} x Target	-0.0984***	-0.1106***	-0.1002**	-0.0078	-0.0093**	-0.0047	-0.0453**	-0.0581***	-0.0561**
	-2.96	-3.01	-2.39	-1.36	-2.26	-0.70	-2.29	-2.99	-2.47
D_{it}^{5} x Target	-0.1118***	-0.1381***	-0.0916*	-0.0120*	-0.0144***	-0.0059	-0.0541**	-0.0673***	-0.0515*
	-2.65	-3.03	-1.72	-1.92	-3.14	-0.82	-2.28	-2.83	-1.83
Observations	3236552	1332023	1553915	2959570	1245317	1455507	3236552	1332023	1553915
R^2	0.03	0.03	0.03	0.13	0.16	0.12	0.02	0.04	0.03
Person FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Calendar-Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 6: Risk factors of LBOs: career path analysis. The table presents cross-sectional regressions of a change in employment from t to t+3 on *Target,* a risk factor and the interaction of *Target* and the risk factor, where t denotes the announcement year. The table solely presents the coefficients on the interaction terms. The analysis is an extension of the analysis depicted in Table 5. Each specification includes a risk factor, which is measured in the year of the LBO announcement and which is described at the top of the table. The categorization of education into low, medium, and high is defined in Table 2. *Unskilled* and *Skilled* both refer to the group of blue collar workers. $F.Tenure^H$ ($F.Tenure^L$) denotes *Firm Tenure* above (below) the sample median. $Ind.Tenure^H$ ($Ind.Tenure^L$) denotes *Industry Tenure* above (below) the sample median. Age^H (Age^L) denotes *Age* above (below) the sample median. Mid. Man. denotes *Middle Management*. The numerical variables are defined in Table 1 and the categorical variables are defined in Table 2. *, **, *** indicate significance at the 10%, 5% and 1% level respectively.

Table 6: Risk factors of LBOs: career path analysis (continued).

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Risk factor (RF) 1	Low edu.	Unskilled	Low Wage	Routine-based job	Offshor-able job	F. Tenure^H	Ind. Tenure^H	Age^H	Mid. man.	W. Collar
Risk factor (RF) 2	Med edu.	Skilled	Med Wage							
Reference group	High edu	White collar	High Wage	All others	All others	F. Tenure^L	Ind. Tenure^L	Age^L	All others	B. Collar
Dependent Variable: Establishment Change within 3 digit industry										
Target x RF 1	0	0.009	0.011	-0.001	0.006	0.005	-0.011	-0.006	-0.006	-0.003
	-0.02	0.77	1.18	-0.11	0.89	0.64	-1.16	-0.86	-1.01	-0.37
Target x RF 2	0.004	-0.001	0.012							
	0.56	-0.11	1.45							
Dependent Variable: Industry change to other 3 digit industry										
Target x RF 1	0.003	-0.004	0.009	-0.012	0.025***	0.017	0.006	-0.071***	0.027	0.018*
	0.18	-0.32	0.7	-1.43	2.78	1.41	0.47	-5.53	1.6	1.95
Target x RF 2	-0.007	-0.024**	0.001							
	-0.68	-2.39	0.12							
Dependent Variable: Change into unemployment										
Target x RF 1	-0.008	0.005	0.002	-0.001	0.004	0.010***	0.008**	0.092***	0.011	0.001
	-1.39	1.39	0.39	-0.43	1.52	3.09	2.56	14.1	1.2	0.34
Target x RF 2	-0.004	0.001	-0.002							
	-1.11	0.38	-0.74							

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