Private Equity Buyouts and Employee Health

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Keywords: Private Equity, Buyouts, Restructuring, Health, Human Capital Risk, Wages, Unemployment Insurance

JEL Classifications: G30, G34, I12, J65, J24, J31, M51

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May 13, 2020

Abstract

We examine the role of employee health in Private Equity buyouts using employee-level data on employment, wages, medical prescriptions, and health expenditures. We conduct matched-sample difference-in-differences estimations including more than 55,000 buyout employees. Employees with a lower health status before the buyout face the most substantial losses of income and employment from buyouts, and these losses are predicted by health-related estimates of employees' productivity. Buyouts influence employees' career paths, and health outcomes after buyouts are associated with career-path outcomes: Those who become unemployed are in poorer health and the health of those who find new jobs is better. More than half of the negative effect of buyouts on employees' incomes is buffered by social transfers and this insurance effect is strongest for employees in poor health. We conclude that buyout-related restructuring has a stronger negative impact on the careers and human capital of employees with health problems.

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1 Introduction

Health is a critical component of employees' human capital, and human capital plays a major role if firms restructure their operations by laying off workers and reassigning workers to jobs. The relationship between employees' health and the characteristics of the workplace is bidirectional: Poor health makes employees less productive and more vulnerable to wage cuts and job losses, especially during restructuring. Conversely, a more demanding work environment and job insecurity may negatively affect employees' well-being and their health.

This paper uses private equity buyouts (henceforth PE buyouts, or just buyouts) as a setting to study the two-way relationship between health and labor market outcomes. PE buyouts are a suitable setting for studying this relationship because buyouts are often associated with a large turnover of the labor force and the sources of value creation in buyouts have been the topic of much debate.[1] Whereas the academic literature on buyouts attributes value creation to gains in productivity and operational improvements, public commentators have often cited transfers from other stakeholders, notably employees, as major sources of value creation in PE buyouts.[2] Despite the ubiquity of restructuring transactions such as buyouts, mergers and acquisitions, and insolvencies, surprisingly little is known about how they affect employees' well-being and health, and even less is known about how companies identify those employees that are laid off during restructuring.[3] This paper contributes to filling this gap in the literature.

We match buyout transactions to an integrated employer-employee data set and a range

[1]See Davis et al. (2014) for an establishment-level analysis and the earlier literature they cite. See Olsson and Tag (2017), Agrawal and Tambe (2016), and Antoni et al. (2019) for individual-level studies.

[2]There is a long-standing literature on how buyouts create value and the impact of buyouts on operational performance starting with Kaplan (1989), Lichtenberg and Siegel (1990), and Smith (1990); see Kaplan and Stromberg (2009) for a survey. Bloom et al. (2015) argue that PE buyouts change management practices. Davis et al. (2014) and Antoni et al. (2019) both cite leading politicians who accused PE buyout firms for enriching themselves at the expense of employees.

[3]The extant literature on how corporate restructuring transactions affect employees' health consists of case studies of exemplary transactions and is discussed in more detail below: Cartwright and Cooper (1993) study two mergers in the UK, Väänänen et al. (2004) study one merger in Finland, and Haruyama et al. (2008) analyze how a takeover announcement affects the employees of one Japanese firm. Cohn et al. (2019) show that PE buyouts improve workplace safety.

of health outcomes of individual employees. First, we document a significant loss of employment and income for all buyout employees, and that a disproportionately larger part of the losses falls on buyout employees in poor health. Next, we measure the difference between employees' pre-buyout wages and the counterfactual wage they would earn conditional on their health status. We estimate these counterfactual wages from population-level hedonic wage regressions and show that the risk of job losses after buyouts increases with the gap between actual wages and estimated counterfactual wages. Finally, we show how buyouts affect employees' career paths. Many leave the target; either they move to other firms, become unemployed, or exit the labor force. The health of those who switch to new jobs after the buyout improves, whereas the health of those who become unemployed deteriorates.

The data set for this study combines data from Bureau van Dijk's Zephyr database, the Central Bureau for Statistics (CBS) of The Netherlands, and the Dutch Private Equity Association. Our final data set includes 55,752 employees of 274 buyout targets in the period 2007 to 2013. We conduct matched-sample difference-in-differences analyses and match the buyout employees to a control sample based on a range of firm-level and individual-level characteristics, including variables that describe employees' health status before the acquisition. We track employees until the fourth calendar year after the buyout and analyze data on employees' consumption of three types of prescription medicines (antidepressants, cardiovascular, digestive), the total number of medications they take, and their total annual health expenditure. Furthermore, we collect data on employees' employment history after buyouts and record their main source of income (employment, self-employment, disability insurance, retirement income, unemployment insurance), job changes, and the wages of those who are employed.

We begin by asking whether PE buyouts affect target employees' employment, income, and wages, and find that the impact is significant: Buyout employees lose about 1,300 Euros per year by the fourth calendar year after the buyout, or about 3.7% of the median wage of target employees, which is higher than the effect found in similar studies for other countries.[4] In the next step, we investigate whether the losses of jobs and income affect employees in poor health more. Based on prior literature (e.g., Currie and Madrian (1999); Contoyannis and Rice (2001); Flores et al. (2019); Jäckle and Himmler (2010)), we hypothesize that employees in poor health are less productive, and that buyout firms identify, and then lay off less productive workers. (Note that this argument does not assume that buyout firms have access to employees' health records, which are confidential.) We find this to be the case, and the effect is large: In addition to the baseline loss experienced by all buyout employees, employees on cardiovascular medication lose another 2,500 Euros per year and those on antidepressants another 2,000 Euros per year in the fourth year after the buyout relative to matched control employees; the losses for employees on multiple medications are even larger. These losses can be fully attributed to losses of employment; there is no evidence for reductions in wages for those who remain employed.

We relate the reduction in income and employment of employees in poor health to their productivity using a two-step procedure. In the first step, we run hedonic wage regressions on the entire Dutch population, in which we regress daily wages on firm and individual characteristics, including a range of medications.[5] These estimates provide us with measures of how sensitive individual productivity is to certain types of medications. In the second step, we use these regression estimates to predict the health-related wage gap of sample employees and obtain estimates of how much sample employees would earn less compared to healthy employees if their wage would fully reflect their health status. We interact this wage gap with the treatment indicator in triple-difference regressions and show that it predicts a significant reduction in employment and earnings. We extend this analysis to 25 groups of medications for which we have data and find a remarkably strong correlation: If buyout employees are

[4]Antoni et al. (2019) find a reduction of 979 Euros or 2.8% of the median wage for a German sample using a similar methodology. Olsson and Tag (2017) find a statistically and economically insignificant effect on unemployment incidence for a Swedish sample.

[5]We are grateful to Jean-Noel Barrot for suggesting this methodology. See Kniesner and Leeth (2010) for a survey of hedonic wage regressions

prescribed medications that are associated with a negative (positive) impact on the wages of the entire Dutch workforce, then these employees are also significantly more (less) likely to lose employment at the buyout firm.

We conjecture that the impact of buyouts on employees' human capital depends on how buyouts affect their subsequent careers. Employees' career paths are strongly influenced by buyouts: The likelihood of remaining with the target declines by almost five percentage points compared to control employees following the buyout, the likelihood of unemployment increases by about one percentage point, and the likelihood moving to a lower-paid job increases by about two percentage points.

Next, we ask whether buyouts have a negative impact on employees' health. Based on prior literature, we hypothesize that buyout-related restructuring has a negative impact on employees' health by creating a more demanding and stressful work environment and job insecurity associated with layoffs, which should lead to anxiety-related stress for those who remain employed with the target. Our findings are somewhat different. We find a strong association between career paths and health outcomes for buyout employees and for control employees alike: The health of those who find new jobs tends to improve, whereas the health of employees who become unemployed deteriorates. Buyouts affect employees' career paths, and those who move to new jobs benefit from better health, those who become unemployed suffer from poorer health, and those who stay with the target experience no significant impact on their health.

Finally, we analyze the composition of buyout employees' incomes and find that about half of the negative effect of buyouts on earnings is buffered by social transfers, mostly unemployment insurance. Transfers cover even larger proportions, about 60% to 80%, of the negative income effects for employees on antidepressants and cardiovascular medication. Some results suggest that unemployment insurance covers the income shortfall of affected employees in the first years after the buyout, whereas disability insurance and retirement income become more prominent sources of income replacement in the longer term, suggesting that many affected employees first benefit from unemployment insurance, and later opt for early retirement.

2 Survey of related literature

This is the first study that analyzes the relationship between labor and health for PE buyouts. As such the study has no precedent, but it is broadly related to three strands of the literature, which investigate the impact (1) of buyouts on employment; (2) of restructuring transactions, and adverse labor market events more generally, on employees' health; (3) of health on labor market outcomes.

There is a large literature on the labor-market consequences of buyouts, which mostly confines itself to changes in employment without considering wages or earnings.[6] The earlier literature is based on firm-level data and cannot distinguish the effects on individual workers from effects on the composition of the firm's labor force. Later studies based on establishment-level data are subject to the same qualification, even though they can distinguish plant closures from plant sales. Only recently have researchers started to analyze individual-level data sets, and, to the best of our knowledge, only two studies use administrative data: Olsson and Tag (2017) from Sweden and Antoni et al. (2019) from Germany; Agrawal and Tambe (2016) have individual-level data for the U.S. from a job-search platform. We contribute to this literature by documenting the critical role of employees' health status for their human capital impairments, by adding health as an additional outcome that measures how buyouts are associated with employees' well-being, by using more granular data on employees' career paths, and by analyzing the effects of buyouts in a different institutional context in the Netherlands. Three recent contributions explore dimensions other than income and employment. Bloom et al. (2015) show how PE buyouts affect management practices,

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

Cohn et al. (2019) show that they improve workplace safety, and Gupta et al. (2020) show that PE buyouts of nursing homes reduces the quality of patient care. However, none of these studies analyzes how buyouts affect the relationship between health and labor-market outcomes.

The literature on the health consequences of restructuring events is scarce. We are aware of only four studies on the health and psychological consequences of mergers and acquisitions, all of which study small samples: Cartwright and Cooper (1993) on a merger of two building societies mergers in the UK (number of employees N=157); Haruyama et al. (2008) on employees of one Japanese financial firm after a takeover announcement (N=71); Netterstrom et al. (2010) on employees (N=685) affected by mergers between five Danish municipalities; Väänänen et al. (2004) study a sample of employees of a merger of one Finnish company (N=2,225) and find negative effects on subjective health. Currie and Tekin (2011)) report similar results for mortgage foreclosures. Three of these studies document a significant negative influence on health outcomes. All of these contributions study small, non-representative samples of firms. Unlike ours, they mostly find a negative influence of organizational change on health. None of these studies analyzes buyouts and they all focus only on the impact of organizational change on health, whereas we study also the impact of health on labor market outcomes.

A broader and related literature analyzes how job loss, job insecurity, and stress factors in the work environment affect employees' health without relating them to corporate transactions.[7] The standard way to address the endogeneity between job loss and health is to use exogenous variation from plant closures. Using this identification strategy, the findings are inconsistent across studies. Some studies find that job loss due to plant closure worsens employees' health (Michaud et al. (2016); Schröder (2013); Eliason and Storrie (2009); Kuhn et al. (2009); Bloemen et al. (2018); Sullivan and von Wachter (2009)), while others find no

[7] There is also a small literature on how adverse effects spill over to the dependents of employees. Lindo (2011) estimate the effects of parental job loss on children health outcomes, and Eliason (2011), Marcus (2013) and Reichert and Tauchmann (2017) focus on the effects on spouses.

health effects (Salm (2009); Böckerman and Ilmakunnas (2009); Schmitz (2011); Browning et al. (2006)). A related strand of literature focuses on how job insecurity and work stress affect employees' health.[8] The evidence in this area is based on associations given the difficulty of isolating exogenous variation in these dimensions. A comprehensive recent study is Dahl (2011), who investigates a large data set of Danish firms and shows that organizational restructuring leads to an increase in the consumption of stress-related medication. We contribute to this literature by showing that the effects of restructuring transactions on health are heterogeneous and depend on employees' career paths, and by adding a new exogenous shock to the work environment to this literature, which usually uses plant closures as a source of exogenous variation to the work environment.

There is a large literature on the labor-market consequences of health events. The existing evidence suggests that ill health is negatively associated with all labor market outcomes, i.e, hourly wages, earnings, labor force participation, hours worked, retirement, job turnover, and the award of benefit packages (Currie and Madrian (1999)). However, this association can be biased for several reasons. First, employment or working conditions can have an effect on health. Second, individuals may use their health to justify their non-employment status (justification bias).[9] Third, there could be omitted variable bias, since unobserved characteristics, e.g., risk attitudes or early-life investments, influence both health and employment outcomes. Recently, a few studies combine administrative data with exogenous variation from unexpected health changes to analyze the impact of health events on labor-market outcomes.[10] For example, Dano (2005) and Halla and Zweimüller (2013) find that accidents decrease employment and earnings in Denmark and Austria, respectively. Garcia-

[8]Job insecurity is defined as the fear of unemployment generated from other employees in the firm being laid off or from a business cycle downturn in a legal environment with low job protection. A non-exhaustive list includes: Caroli and Godard (2016); de Jong et al. (2016); Ferrie (2001); Ferrie et al. (1995); Knabe and Rätzel (2010) and the literature review by Sverke et al. (2002).

[9]Several studies have focused in dealing with justification bias. A non-exhaustive list of papers is: Bound (1991); Disney et al. (2006); Au et al. (2005); Cai et al. (2014); Garcia Gomez et al. (2010); Zucchelli et al. (2010); Campolieti (2002).

[10]Other studies use arguably exogenous changes in health using self-reported measures from survey data. See, for example, Lindeboom et al. (2016); Garcia Gomez and Lopez Nicolas (2006); Garcia Gomez (2011); Lechner and Vazquez-Alvarez (2011); Riphahn (1999); Datta Gupta et al. (2015).

Gomez et al. (2013) use unscheduled hospitalizations in the Netherlands and confirm that an unexpected health shock decreases employment and earnings. Hullegie and Koning (2018) extend the analysis to a more recent period and conclude that the reforms in the Dutch disability insurance system have substantially lowered the probability of receiving disability insurance and increased the employment of workers after a health shock. Dobkin et al. (2018) find that a hospital admission reduces earnings and hours worked of US workers. Last, a group of studies focus on specific severe illnesses, e.g. cancer, and confirm that severe negative health shocks have long-lasting negative consequences (Jeon (2017); Datta Gupta and Larsen (2007); Heinesen and Kolodziejczyk (2013); Heinesen et al. (2018)). While our paper contributes to this literature, we ask a somewhat different question. The literature on how health affects labor market outcomes asks how health events such as accidents or severe illnesses impact employment and income. By contrast, we ask how a given health condition predisposes labor market outcomes in a restructuring event. We argue that workers in poorer health are expected to be less productive, and therefore more likely to be dismissed after a PE buyout.

3 Data and methodology

In this section, we describe the construction of the sample (Section 3.1), the matching process (Section 3.2), our main variables of interest (Sections 3.3 and 3.4), show descriptive statistics (Section 3.5) and describe the empirical approach (Section 3.6).

3.1 Sample construction

We start the sample construction by downloading all Private Equity Buyouts in the Netherlands for the period 2007-2013 from Zephyr. Our time period is defined by data limitations: Health data is only available for the period 2006-2017 and we require at least one year prior to the buyout for matching and at least four years post buyout for our analysis of health outcomes. We identify PE buyouts in Zephyr selecting all transactions for which "Deal Type" is equal to "Private Equity"or "institutional buy-out,"as well as all transactions for which "Deal Type" is equal to "Acquisition" and "Deal Financing" is equal to either "Leveraged Buyout" or "Private Equity". In addition, we require for all transactions that the buyer is an institutional investor, the initial stake in the company is less than 50%, the final stake in the company is larger than 75%, and that the transaction is not a secondary buyout. These steps leave us with 216 PE buyouts. The Bureau van Dijk identifier contains the identifier used by the Dutch chamber of commerce (Kamer van Koophandel), which we extract and refer to as KvK-ID. In the next step, the Dutch Central Bureau for Statistics (CBS) converts the KvK-ID into an anonymized number and adds the internal firm-level identifier CBS persoon. CBS persoon can be linked to a variety of other CBS-identifiers relating to fiscal units, statistical business units, employees, households, and individuals. For 156 of our initial sample of 216 PE buyouts, we can obtain the identifier necessary to link employers to employees (rbe_identificatie).

In the next step, we obtain access to a CBS-data set on transactions provided by the Dutch Private Equity Association, which contains all transactions of the members of the European Private Equity Association in the Netherlands. From this data set, we select all transactions where variable "investment stage" is equal to "Buyout", "Public-to-Private", "Build-up Acquisition", or "Rescue/Turnaround". This step provides us with an additional 210 transactions, yielding an initial sample of 366 PE buyouts for the Netherlands.

Finally, we identify all individuals who are employed at one of the 366 PE buyouts in the year of the buyout. For 279 PE buyouts, we can find at least one employee in the CBS database and identify 85,448 employees in total. In order to enter the sample of buyout employees, we require that the individual is employed in the buyout firm at the end of the year before the buyout, is between 18 and 62 years old, and works at least 50% of full-time. This step leaves us with 56,188 employees and 275 buyout firms. Using the steps described in the next section, we match PE buyout employees to control employees and assign one control employee to each buyout employee. We match 99.2% of all employees and arrive at a final data set comprising 55,752 employees and 274 buyout targets.

3.2 Constructing matched samples

For each employee from the buyout group, we select a matching employee from a sample of non-treated employees. To identify all non-treated employees, we start with the whole sample of recorded employees, apply the same filters as described above for buyout employees, and remove all buyout employees and all employees that are associated with one of the buyout firms in the year prior to the buyout. We match employees on characteristics recorded in the year before the buyout to ensure that matching is not affected by the buyout. We match individuals exactly in terms of gender, industry, and medication record. Table 1 defines all the variables included in the analysis. For every employee, we record whether the employee is prescribed digestive, antidepressant, or cardiovascular medication and we require that the medication record with respect to these categories is identical for the buyout employee and the respective control. Next, we remove all control employees from the sample if the control employee's firm size deviates by more than 66% from the firm size of the buyout employee. We use the number of employees working at the firm as a measure of firm size. From those control employees who fit the criteria discussed above, we pick the nearest neighbor based on the normalized Euclidean distance of the buyout employee's earnings, age, tenure in the job, and total number of prescribed medication types.

We match with replacement, i.e., we allow for a control employee to be matched to more than one target employee. We successfully match 55,752 target employees (99%). The number of unique control employees is equal to 50,030, which is smaller than the number of target employees because of matching with replacement. Table 2 compares differences between buyout employees and control employees for ten of our most important variables. For all individual level variables, the differences are very low and do not warrant further discussion. We record a larger difference for firm size, where the differences between treated and control is 19% on average. The average firm size for buyout employees of 2,999 is an employee-weighted average because it is computed over all employees, not firms. With 270 employees, the average firm size in the sample is much lower if we weight all firms equally (number not tabulated). We use the normalized differences proposed by Imbens and Wooldridge (2009) and used by Imbens and Rubin (2015) to examine significant differences between two groups of observations. Imbens and Wooldridge (2009) recommend that normalized differences be below 0.25 in absolute value, and the Imbens-Wooldridge statistic is 0.14 for firm size. For all other variables on both the establishment level and the individual level, the test statistic is never higher than 0.02. We conclude that our control groups match buyout employees very closely on all relevant criteria.

3.3 Employment indicators

We link individual administrative information on all the employment spells for the entire Dutch population over the period 2002-2017, which allows us to document parallel trends for up to five years before the buyout. For every employment spell, we observe the starting and ending time of the contract, earnings, and the percentage of hours worked relative to full-time employment, among other job characteristics. We aggregate this information at the annual level to compute three main labor market outcomes:

- **Earnings:** The employee's earnings summed up over all employment spells in a given year.
- **Daily wage:** *Earnings* of employee i in year t, divided by the number of days employed during that year. *Daily wage* is set to missing if either the buyout employee or the employee's match was not employed during the whole year t.[11]
- **Days employed:** The number of days in year t during which employee i was employed.

[11] We cannot calculate hourly wages, because our data provider does not report the number of hours worked per day or per week. *Daily wage* will be lower than a full-time equivalent daily wage for employees who do not work full-time, and 29.9% of our sample work less than full-time and 2.1% work less than 50% of full-time.

In addition, we investigate to what extent PE buyouts affect the career paths of target employees. We take two complementary approaches. First, we categorize job-to-job transitions into three groups: i) employees who remain within the same firm: the dummy variable *Stayer* takes the value 1 if the employee is in the same firm in year k as in year $k = -1$); ii) employees whose first job change is to a lower paid job: the dummy variable *Switcher down* takes the value 1 if the employee's first job change after the event is to a lower-paid job, and the value of *Switcher down* remains constant thereafter, irrespective of subsequent job changes); iii) employees whose first job-change is to a better-paid job: the dummy variable *Switcher up* takes the value 1 if the employee's first job change after the event is to a higher-paid job, and the value of *Switcher up* remains constant thereafter, irrespective of subsequent job changes. Second, we track employees' main source of income after the buyout to analyze the paths into non-employment. We consider the most common pathways. In particular, we classify individuals depending on whether their main source of income comes from employment or self-employment (*Work*), unemployment insurance benefits (*Unemployment insurance*), disability insurance benefits (*Disability insurance*), and (early-)retirement benefits (*Retirement*). We further group all the other individuals, e.g, those on either social assistance or without income, into one category (*Other*). This categorization allows us to abstract from the fact that individuals can obtain income from multiple sources in a given year by assigning them to the one that is most relevant in terms of income.

3.4 Health indicators

We use registry data from CBS on the consumption of prescribed medication to identify changes in the health status of the employees after a buyout. In particular, we observe for each prescribed drug identified at the ATC4 level and covered by the Dutch basic health insurance if it has been dispensed to an individual by a pharmacy at least once a year (see Norwegian Institute of Public Health (2017) for an explanation of the ATC4 classification system). The Dutch basic health insurance provides a comprehensive coverage of drugs.

We are not able to observe drugs provided by hospitals, e.g., drugs used in oncological treatments.[12] We use this information to compute a broad health indicator based on the number of different types of medications consumed in a given year (*Total medication*). In addition, we focus on medications related to health conditions that have been previously found to be related to job loss and stress (Virtanen et al. (2007); Thielen et al. (2011); Kouvonen et al. (2017); Chandola et al. (2008); Everly Jr. and Lating (2019)). These are: i) *Antidepressant* (ATC4 equal to N06A); ii) *Cardiovascular* (ATC4 equal to C01, C02, C03, C07, C08, C09, C10 or B01); and iii) *Digestive* (ATC4 equal to A02 or A03). We complement this information with an indicator of higher co-morbidity by defining *High medication* as a dummy variable, which takes the value 1 if the individual takes three or more different types of medications, defined at the ATC4 level, within one year, and information on total health expenditures (*Health expenditures*). Unfortunately, we only observe information on total health expenditures from 2009 onward. Our main results focus on *Antidepressant*, *Cardiovascular* and *Total medication*, but provide also estimates on the other measures in the Online Appendix. Last, to test some of our claims, we extent the analysis using a broader set of health measures grouping the detailed information on drugs prescribed at the ATC4 level into 25 categories. The first two columns of table 7 define these additional health variables and provides information on the corresponding ATC4 codes.

3.5 Descriptive statistics

Table 3 provides descriptive statistics on the panel data set used in the empirical analysis. The average employee in our sample is 41.7 years old and earns 40,320 Euro from employment and 2,172 Euro from other sources (unemployment insurance, disability insurance or retirement). Hence, the average age in the sample is similar to that of comparable studies, which report 42 years for Germany (Antoni et al. (2019)) and 41.1 years for Sweden (Olsson and Tag (2017)), whereas earnings are higher in the Netherlands compared to Germany (34,251 Euros) and

[12] The annual reports of the Foundation for Pharmaceutical Statistics (https://www.sfk.nl/english) provide an overview of the main drugs included and excluded from our data.

Sweden (275,430 SEK or 29,292 Euros). The share of women in our buyout sample is equal to 35%, which is higher than the number reported for Germany (24%), but lower than the number for the US (51%, reported by Agrawal and Tambe (2016)). Unsurprisingly, our sample is healthier than the average Dutch population. For example, 4.3% of our sample are prescribed antidepressants compared to around 5% of Dutch men and 9% of women aged 35 to 44 in 2016 (Statistics Netherlands (2016)).[13]

3.6 Methodology

This section describes the baseline difference-in-differences methodology, which we use for our main results (Section 3.6.1) and the triple-differences methodology, which we apply to estimate heterogeneous effects across subgroups based on pre-treatment characteristics (Section 3.6.2).

3.6.1 Difference-in-differences regressions

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

$$Y_{ik} = \alpha_i + \gamma_t + \sum_{k=-2}^{k=+4} \delta_k D_{ik} + Target_i \times \sum_{k=-2}^{k=+4} \theta_k D_{ik} + \varepsilon_{ik}. \quad (1)$$

In (1), Y_{ik} denotes the outcome variable in levels (labor market outcomes, social insurance, or health outcomes), α_i and γ_t are, respectively, individual and calendar-year fixed effects, i indexes individuals, t indexes calendar time, and k indexes event time, where $k = 0$ is the year in which the buyout takes place. The event-time dummy variables D_{ik} begin two years before the buyout ($k = -2$) and end four years after the buyout ($k = +4$). Our data cover all individuals from two years before to four years after the event and the dummies for the year before the event ($k = -1$) are omitted; hence, all event-time effects are measured relative to

[13]This is also broadly comparable to prior studies. For example, 4% of the sample studied by Thielen et al. (2011) and 6% of the men and 12% the women in the Finnish sample used in the study of Virtanen et al. (2007) are prescribed antidepressants.

the year before the buyout.[14] The dummy variable $Target_i$ distinguishes employees of PE buyout targets from employees in the matched sample ("controls") and equals one for target employees in all sample years. We cluster standard errors at the firm level.

The parameters of interest are the coefficients θ_k on the interactions $D_{ik} \times Target_i$, which measure the average difference between target employees and control employees for the outcome variable Y_{ik} in event-year k. By contrast, the coefficients δ_k measure the average differences in event time, after controlling for calendar-time effects. We are careful with causal interpretations, because we cannot exclude selection effects and that unobservable factors influence wages, employment, health, and also buyout decisions.

Figures 1 to 3 plot the trends of the outcome variables *Earnings*, *Daily wage* and *Days employed* from $k = -5$ to $k = +4$. They show parallel trends from *k=-5* to *k=-1* for the three outcomes. In addition, the estimate of the coefficient θ_{-2} provides a formal test of differences before the PE buyout. The figures also provide us with a first look of the effects of the buyouts as they show the post-event trends as well. We see that earnings are about 1,000 Euros lower at the end of the period for the treated compared to the control employees. Similarly, treated employees work on average five days less, but trends remain parallel for *Daily wage*. The inverted-V pattern is a consequence of the requirement that employees in both groups have to be employed at the end of $k = -1$, which mechanically increases employment in the year before the event year.[15]

3.6.2 Triple-differences regressions

We extend the baseline analysis and perform individual-level triple-difference analyses to test whether the estimated effects are heterogeneous across subgroups. We build on equation (1) and interact the target indicator and the event-time dummies with risk factors that identify

[14] PE buyouts happen at different dates in calendar time, so the event-year dummies are not collinear with calendar-year effects (see Boucly et al., 2011)

[15] See Figure 3A in Davis et al., 2014 or Figure 3 in Antoni et al. (2019) for similar effects.

the respective subgroups of employees:

$$
\begin{aligned}
Y_{ik} =& \alpha_i + \gamma_t + \sum_{k=-2}^{k=+4} \delta_k D_{ik} + Target_i \times \sum_{k=-2}^{k=+4} \theta_k D_{ik} \\
&+ RF_i^f \times \sum_{k=-2}^{k=+4} \lambda_k D_{ik} + Target_i \times RF_i^f \times \sum_{k=-2}^{k=+4} \eta_k D_{ik} + \varepsilon_{ik}.
\end{aligned} \tag{2}
$$

The coefficients of interest in (2) are the η_k's on the triple interaction of $Target$, the event dummies, and RF^f, the risk factor, which measure by how much the outcome of interest differs between a target employee characterized by risk factor RF^f from control employees with the same risk factor, compared to the difference between other target and control employees who are not characterized by this risk factor.

4 Employment and wages

This section presents the baseline analysis on the relationship between buyouts and employment outcomes. Section 4.1 presents the baseline analysis of the impact of buyouts on income and employment, and Section 4.2 shows how this relationship depends on employees pre-buyout medication status. Section 4.3 uses hedonic wage regressions using health variables to test whether career-path events are related to employees' productivity.

4.1 Baseline analysis: Income and employment

We begin with an analysis of the impact of buyouts on *Earnings*, *Daily wage*, and *Days employed*. Table 4 reports the coefficients θ_k on the interaction $D_{ik} \times Target_i$ from equation (1) without controls[16] except for person and calendar-year fixed effects in columns (1), (2), and (3), respectively.

[16]We do not include controls in our regressions as they are either time-invariant (e.g. gender, ethnicity) and therefore collinear with the individual fixed effect, or collinear with the event and calendar time dummies (age) or endogenous as they can be affected by the treatment (e.g. occupation, marital status).

Earnings. Column (1) of Table 4 reports the results for *Earnings,* which decline in each of the four calendar years after the buyout year; the effect is statistically significant for years $k = 3$ and $k = 4$, where it plateaus at about 1,300 Euros per year. The median income of target employees in the year before the buyout is 35,225 Euros (see Table 2). Hence, the loss of income equals 3.7% of the median pre-treatment wage. Hence, the effect of buyouts on employees' income is slightly higher in our sample than in the sample of German workers studied by Antoni et al. (2019), who find a decline of just under 1,000 Euros or 2.8% of the median wage in their sample. Davis et al. (2014) also find declines in income after buyouts in their US sample, whereas the results from earlier studies are more mixed (Lichtenberg and Siegel (1990), Amess and Wright (2007a)); however, none of these studies use individual-level data.

Employment and wages. Columns (2) and (3) of Table 4 decompose the results for *Earnings* into a wage component (*Daily wage*, column (2)) and an employment component (*Days employed*, column (3)). There is a statistically significant decline in employment of about two to four days in years $k = 1$ and $k = 2$, and about five days, or 1.9% of annual pre-treatment employment, in $k = 3$ and $k = 4$. By contrast, there is no significant decline in *Daily wage*, hence the decline in income should be attributed to reductions in employment, but not to reductions in wages. The findings for wages parallel those of Antoni et al. (2019), whereas the decline in employment is significantly higher in their German sample at almost nine days per year. Hence, we conclude that the decline in employment in the Dutch sample falls disproportionately on the higher-paid workers to reconcile this difference. Earlier studies on buyouts either do not analyze wages, or look at annual earnings per worker, which corresponds to our definition of *Earnings* (see Wright et al., 2009 for a survey).

4.2 Prior medication and income

In the next step, we test the hypothesis that employees in poorer health experience larger reductions in income and employment than healthy employees. A large literature, which we partially review in Section 2, shows that adverse health events, like severe illnesses, strokes, or accidents, have a negative effect on employees' incomes. While buyout firms cannot observe medical health records, which are confidential, they can observe employees' productivity and sickness leave.[17] Hence, if the target firms' new owners restructure the workforce in an effort to raise productivity, they will likely lay off the less productive workers. If productivity and health are sufficiently negatively correlated, we should then observe higher losses of income and employment for workers in poorer health.

To investigate this hypothesis, Table 5 performs a triple-differences analysis according to equation (2). The risk factors RF_i^f in this equation are our three main measures of employee health, namely, a dummy variable for taking anti-depressants, a dummy variable for taking cardiovascular medication, and the total number of medications taken. Each health risk factor is measured in the year before the buyout $k = -1$, i.e., we ask whether the health status of the employee before the buyout predicts labor market outcomes. Table 5 reports the coefficients for the triple interactions of the event-time dummies with the target (treated) indicator and the health risk factors. We also report the coefficients on the interactions of the event dummies with the target indicator (the θ_k's on $Target_i \times D_{ik}$ in equation (2)), since the total impact on a target employee is measured by the combined effect and is given by the sum $\theta_k + \eta_k$. Table 5 reports the results for anti-depressants in columns (1) and (2), those for cardiovascular medication in columns (3) and (4), and those for the total number of medications in columns (5) and (6). For each health outcome, we report the results for *Earnings* and *Days employed*; the results for *Daily wage* are always insignificant (see Table OA1 in the Online Appendix).

We find strong support for the hypothesis that employees who were in poor health before

[17]Sickness leave is a potential channel if employees are tagged as less productive (Hesselius (2007); Markussen (2012)). Unfortunately, we do not observe in our data whether employees are on sickness leave.

the buyout fare far worse than healthy employees. The overall effect of buyouts on target employees in poor health is much larger than that on healthy employees. The effect on *Earnings* is strongest for those on cardiovascular medication (see column (4)): in year $k = +4$, the loss in income is 3,507 Euros ($\theta_4 + \eta_4 = -1,018 - 2,489$). The corresponding effect for antidepressants is slightly smaller, with a combined impact of 3,235 Euros (=1,223+2,012). For both medications, about half of the long-term effect can already be seen in the year after the buyout ($k = +1$) and the full effect is reached in the third year and amounts to about three times the baseline effect for healthy employees. Column (5) shows that taking one additional type of medication results in a long-term ($k = +4$) additional loss of income of 691 Euros in addition to the 991 Euros drop for those without prior medication consumption. Columns (2), (4), and (6) show that the health status of employees before the buyout predicts large losses of employment of thirteen days (antidepressants, column (2), $\theta_4+\eta_4 = -4.9 - 8.1 = -13.0$) and fifteen days (cardiovascular, column (4), $\theta_4 + \eta_4 = -4.0 - 11.2 = -15.2$).

Table OA2 in the Online Appendix repeats the analysis of Table 5 for three additional health measures (digestive medication, high medication intake, health expenditures). We do not find a significantly higher impact of buyouts on the income and employment of buyout employees who were on digestive medication before the buyout, whereas the results are similar to those for our three main measures of employee health if we use a dummy variable for high medication intake and if we use health expenditures as a broader measure of employees' health. We find that the additional loss of earnings amounts to 2,489 Euros for those with high medication intake and 1,652 Euros for those with health expenditures above the median.

4.3 Productivity, health, and labor market outcomes

In this section, we follow up on the discussion in Section 4.2 and further investigate why pre-buyout health - measured by medication - may influence how buyouts affect employees' labor market outcomes. Our hypothesis is that firms can observe their employees' productivity, and that health, which is observable to the employee and to the researcher through the data we analyze here, but not to employers, is an important component of productivity.

To better relate wages to productivity, we employ hedonic wage regressions, which have been widely used in the literature to study the relationship between wages, job characteristics, and employee characteristics.[18] We follow Kniesner and Leeth (2010) and the literature they cite and first perform the following regression of wages on employee and firm characteristics on the entire population of Dutch employees on which we have administrative data for our sample period:

$$w_i = \alpha_0 + X_i\zeta + u_i, \tag{3}$$

where w_i denotes the wages of employee i and X_i is a vector of occupational and employee characteristics that describe the job of employee i. The variables included in X_i control for *Days employed*, gender, *Age*, *Age* (squared), *Tenure*, *Tenure* (squared), percent of full-time employment, *Firm size*, a 2-digit industry code, the first three digits of the Dutch postal code, ethnicity, and education.[19] Our annual regressions include approximately three million employees on average and explain about 65% of the variation in our dependent variable, *Daily wage*. We depart from the literature on hedonic wage regressions cited above (footnote 18) in two ways. First, we use wages in Euros rather than the logs of wages, in line with the remaining part of our analysis. The variables we construct below are more skewed and more leptokurtic if we use log wages rather than just wages, so there seems to be no reason to depart from our baseline wage measure; robustness checks show that this choice is immaterial. Second, we do not use measures of job-related hazards. We do not have access to such measures and they are relevant in the literature that aims at estimating the value of a statistical life, but are less relevant for our purpose.

In the first step, we run regression (3) and save the residuals $\widehat{u}_{ijs}$. In line with the hedonic

[18]Hedonic regressions have a long tradition in economics and their rigorous treatment goes back to Rosen (1974), who uses them to study the relationship between product characteristics and prices in market for differentiated products. Kniesner and Leeth (2010) provide an extensive survey of the methodology and use of hedonic wage regressions.

[19]The first three digits of the postal code identify small cities and districts of large cities. We have have about three million observations if we include controls for education and ten million observations if we do not include education controls.

wage literature, we treat wages as equilibrium outcomes that equate employees' willingness to perform a certain job with employers' willingness to pay for employees' effort and skills for the same job, where jobs have characteristics that are relevant to employers and employees. Hence, the residuals $\widehat{u}_{ijs}$ from regression (3) reflect the unobserved skills of employees and their preferences for unobservable job characteristics.

In the second step, we regress the residuals from regression (3) on the three medication-based health variables (i.e. *Antidepressant*, *Cardiovascular,* and *Digestive*) as well as the total number of medications:

$$\widehat{u}_i = \beta_0 + \sum_h H_{i,h}\gamma_h + v_i, \tag{4}$$

where $H_{i,h}$ refers to health characteristics measured by consumption of medication. E.g., if characteristic h refers to cardiovascular medication and employee i takes such medication, then $H_{i,h} = 1$. We use these estimates to construct *Medication index,* our first measure of health-related productivity.

Medication index. We run the first-stage (3) and the second-stage regression (4) as described above on the entire Dutch population, and then save the coefficients γ_h on the health variables $H_{i,h}$, where h is an index that runs over all health-related variables included in the hedonic wage regression. The Medication index of employee i is then defined as $\textit{Medication index} \equiv \sum_h H_{i,h}\gamma_h / \sum_h \gamma_h$.

Medication index provides a measure of how medication is associated with wages in the Dutch population. If a type of medication H^h has a large negative association with employees' productivity on average, then γ_h will be negative and large in absolute value. We follow a similar approach to compute *Residual wage*, a second measure of health-related productivity (loss).

Residual wage. For every employee in our sample, we first predict wages based on the coefficient estimates obtained from running regression (3) on the entire Dutch population. We exclude the controls for education because we do not have information on education on approximately 70% of our employees and would therefore not be able to predict wages for half of our sample.[20] In the second step, we predict wages in the same way as in the first step but we also include our major medication variables (*Digestive*, *Antidepressant*, *Cardio*vascular, *Total medication*) in regression (3). *Residual wage* is then the difference between the predicted daily wage without accounting for medication status and the predicted daily wage when accounting for medication status.

The construction of *Residual wage* relies on the assumption that equilibrium wages measure productivity. *Residual wage* is the difference between the individual's estimated productivity without conditioning on the individual's health and the individual's estimated productivity when conditioning on health. Hence, *Residual wage* estimates the loss in productivity associated with medication consumption across the Dutch population. We do not report the full estimation results for regressions (3) and (4), which we use only to create the two measures of employee productivity, whereas the regressions themselves add little to our analysis.[21] Hence, we only report some descriptive statistics on the productivity measures in Table 3.

Next, we perform triple-differences analyses based on equation (2) with *Earnings*, *Daily wage*, and *Days employed* as dependent variables, and use the two productivity measures described above as risk factors in regression (2), i.e. we interact the event-time dummies with the target indicator and the two medication-based productivity measures. Table 6 reports the results for the triple interactions. To interpret the results, note that the standard deviation of *Residual wage* is 1.9 and the standard deviation of *Medication index* is 0.063. Both variables predict declines in *Earnings* and in *Days employed*, and they are statistically highly significant in both cases. The $k = 4$ coefficient for the impact of *Residual wage* on *Earnings* is -94.5 (column (1)), hence, a one-standard deviation increase in *Residual wage* results in an additional income loss of 180 Euros. Similarly, a one-standard deviation increase

[20] Our annual regressions include approximately 10 million employees on average and explain about 60% of the variation in our dependent variable, *Daily Wage*.

[21] Note also that we run these regressions separately for each of the seven calendar years in our sample, so we obtain seven sets of estimates.

in *Medication index* results in an additional 702 Euros decline in income (= 0.063 x 11,146, see column (4)). While there is a measurable impact on employment and income, there is no impact on *Daily wage.* Hence, buyout employees whose health-related productivity is low are more likely to be laid off, but wages are downward rigid and not adjusted.

To further investigate the validity of our conclusions we repeat the analysis for an extended set of health indicators. Specifically, we group all medications we have data on into 25 different groups based on the ATC4 system (see Section 3.4). Then we re-run the hedonic wage regressions (equations (3) and (4)) with these 25 medications and repeat the triple-difference analysis from Table 5 separately for each medication. We provide a summary of the results in Table 7 and Figure 5. Table 7 shows how we map the ATC4 classification into 25 groups of medications (columns (1) and (2)), the prevalence of consumption for each type of medication in the Dutch workforce (column (3)), the prevalence among buyout employees (column (4)), and the prevalence among control employees (column (5)) in the pre-buyout year, and weighted by the number of buyout employees in a given year. Column (6) shows the average of the coefficients γ_h on $H_{i,h}$ in the hedonic wage regression (4), since these regressions are run separately for each calendar year from 2006 to 2012; the standard errors are computed as the standard deviation of the seven coefficient estimates. Finally, column (7) reports the coefficient η_{h4} on the interaction $Target_i \times H_{i,h} \times D_{i4}$ in regression (2), where $H_{i,h}$ is an indicator variable for medications, that becomes one if employee i was prescribed medication h in the year before the buyout.

Based on the reasoning above, we hypothesize that there should be a close relationship between the coefficients γ_h in the hedonic wage regressions and the coefficients η_{h4}, which measure long-term employment effects. The correlation is visualized in Figure 5, which plots the triple-difference coefficients η_{h4} (vertical axis) against the hedonic regression coefficients γ_h (horizontal axis). There is a strong positive relationship with a slope coefficient of 1.47 and a coefficient of correlation of 0.67. Hence, on average, across all medications, a one-Euro medication-related reduction (increase) in *Daily wage* across the Dutch workforce translates into an 1.47-day reduction (increase) in the long-term *Days employed* of buyout employees. Note that some medications are associated with positive effects whereas others are associated with negative effects, but the average of the effects across all medications is negative (see bottom of Table 5: mean of $\eta_{h4}$is -0.88; mean of γ_h is -1.51). Note that we observe a significant negative association of medication intake and *Days employed* not only for cardiovascular medication and antidepressants, but also for medication that addresses conditions of the musculo-skeletal system (ACT4 category M).

Overall, these analyses buttress the argument above that buyout firms select employees based on their productivity. The higher the association between the medication intake and the wages of the Dutch population, i.e., the stronger the predicted decrease of employees' productivity because of their health condition, the higher is the likelihood that the employee will lose income and employment. This results holds for the narrow selection of medications we focus on in this paper as well as for a broad set of medications for which we have data.

5 Career paths and health

In this section, we analyze the career paths of buyout employees after the buyout and relate it to their health status and health outcomes. Section 5.1 analyzes the baseline effects of buyouts on employees' career paths. Section 5.2 shows how career paths depend on the pre-buyout medication status, and Section 5.3 analyzes the reverse association between career paths and health outcomes.

5.1 Baseline career path effects

We begin the analysis of employees' career paths by using the definition of the career path events discussed more extensively in Section 3.4. We consider five career path outcomes, which are defined with reference to employees' main source of income in a particular year, i.e. whether they are working (employed or self-employed), retired, on disability insurance, or on unemployment insurance. Employees who are in neither of these categories are put into the category "other." Further, we distinguish another three career path outcomes based on post-buyout employment spells, namely, whether employees stay with the target firm, whether they move to another firm and increase their wage, or whether they move to another firm and reduce their wage.

We estimate linear probability models using the same matched-sample difference-in-difference identification strategy as in equation (1). The dependent variables are dummy variables, which equal one for the respective career path outcome. Table 8 presents the results, such that each column corresponds to a different career path outcome. Columns (1) to (5) show the results for career-path outcomes constructed from the main source of income and columns (6) to (8) show the results for career-path outcomes constructed from post-buyout employment spells. We report only the coefficients θ_k for the double interactions $Target_i \times D_{ik}$ from equation (1).

There is weak evidence that buyout employees are about 4.5 to 6.7 percentage points less likely to stay with the target firm after the buyout (column (6)). They experience a statistically highly significant likelihood of being unemployed, which is around one percentage point in all post-buyout years (column (4)). There is a weakly significant decline by 1.3 percentage points in the probability of working (column (1)). There is no general movement of buyout employees into retirement or disability insurance (columns (2) and (3)), and a significant increase in the likelihood of just under two percentage points of moving into lower-paid jobs (column (7)), whereas the increase in the likelihood of moving to higher-paid jobs is not significant (column (8)).

5.2 Career paths and pre-buyout medication status

Next, we follow the same steps as in Section 4.2 and investigate to what extent career path events depend on employees' pre-buyout health status. Table 5 shows that employees' health influences their post-buyout earnings and employment, and Table 8 shows that buyouts affect employees' career paths. We hypothesize that buyouts influence earnings and employment by affecting the career path events analyzed in the previous section. Hence, Table 9 presents a triple-difference analysis of career path events, which adds interactions with the five variables that measure health as risk factors: the three health outcome variables used in Table 5, and the two composite measures described in Section 4.3 and used in Table 6. We report the results in Table 9, which has five panels, one for each health measure. Our coefficients of interest are again the η_k-coefficients on the triple interactions of event-time dummies, the target indicator, and the three health risk factors ($Target_i \times RF_i^f \times D_{ik}$ in (2)). We do not report the double interactions, which are similar to the corresponding interactions in Table 8. We only report the coefficients for $k = 2$ and $k = 4$ to conserve space.

Overall, we find highly significant results for all five measures. The impact of health on how buyouts affect career outcomes is economically large relative to the baseline effect of buyouts on career outcomes documented in Table 8. A one-unit increase in the medication index (see Panel A) increases the probability of not working (i.e., not being either employed or self-employed) by 10.15 percentage points in $k = 4$ (column (1)), and the likelihood of staying with the target falls by about the same amount (-10.62 percentage points, column (6)). Buyout employees with a one-unit higher value of the medication index are significantly more likely to retire (+3.77 percentage points, column (2)), receive disability insurance (+4.25 percentage points, column (3)) or unemployment insurance (+3.17 percentage points, column (4)). We find short-term effects ($k = 2$) for an increase in the probability to switch to a lower-paid job (+2.3 percentage points, column (7)), but no long-term effects ($k = 4$), and no significant change in the probability of switching to a higher-paid job. We find qualitatively similar but statistically sometimes weaker effects for *Residual wage* (Panel B).

If we focus on particular health conditions, as in Table 5, then we see that employees on anti-depressants (Panel C of Table 9) are significantly more likely to stop working (-4.40 percentage points) and retire (+1.72 percentage points in $k = 4$) or receive disability insurance (+2.78 percentage points), but they are not more likely to receive unemployment insurance.

By comparison, employees on cardiovascular medication (Panel D) are significantly more likely to receive unemployment (+1.25 percentage points) or disability insurance (+1.33 percentage points), but are not more likely to retire; a higher number of medications taken (Panel E) is similar to a higher consumption of cardiovascular medication in this respect.

Recall that the baseline effects of buyouts on the likelihood of receiving retirement income or disability insurance are negligible and statistically insignificant in Table 8. Similarly, we found economically and statistically small associations between buyouts and the probability of staying with the target and, respectively, the probability of not working. Hence, the adverse career outcomes for employees in poor health shown in Table 9 contrast with comparatively small baseline effects for the same outcomes. This finding is consistent with the observation above that the losses of income and employment of employees in poor health (see Section 4.2) are much larger than the losses for healthy buyout employees.

5.3 Career paths and health outcomes

While the discussion so far has looked at the influence of employees' health on their careers, employment, and wages, this section investigates the reverse causality from buyouts to employees' health. Again, a large literature, partially reviewed in section 2, investigates this link and shows how job loss, job insecurity, and job-related stress as well as other characteristics of the work environment impact employees' health. The ultimate goal of buyout firms is to make target firms more valuable, and among the main strategies for achieving this objective are the streamlining of operations and the strengthening of incentives for management (see Kaplan and Stromberg (2009) for a survey). Therefore, we hypothesize that buyouts create a more demanding and stressful work environment, with the associated influence on employees' health. In addition, the career path changes imposed on employees through buyouts, which we document in the previous section, may have an impact on employees' health, in particular on those employees who lose their jobs.

First, we perform a difference-in-differences analysis based on equation (1) with the health status measures as dependent variables Figure 4 reports the results on the θ_k-coefficients on the interactions of event-time dummies and the target indicator in columns (4) to (6). The coefficients are all very close to zero with changing signs; no coefficient is statistically significant. Hence, buyouts appear to have no measurable impact on health outcomes.[22] Table OA3 shows the estimates for the other health outcome measures (*Digestive*, *High medication* and *Health Expenditures*). The results confirm that buyouts do not worsen employee's health outcomes.

Second, we study the same subsamples of employees as before and perform a triple-difference analysis by regressing the extended set of health outcomes on interactions of the event dummies, the target indicator, and career path effects. For this purpose, we use the consolidated set of career path events with two risk factors: *Job change,* which indicates whether an employee has changed jobs compared to $k = -1$; and *Not employed,* which indicates whether the employee is not working in period k (see Section 3.3 and Table 1 for more details about these variables). In Table 10, we report the coefficients on the double interactions of event-time dummies and career path events, on the interactions of event-time dummies and the target indicator, and the triple interactions, which interact event-time dummies, career path events, and the target indicator. In this case, the double interactions are of independent interest because they show how career path events are associated with employees' health generally, independently of whether they are associated with buyouts or not. We only report the coefficients for $k = 0$, $k = 2$, and $k = 4$ to conserve space.[23]

One of the remarkable findings from Table 10 is that job changes are associated with better employee health. The coefficients on the double interactions of event-time dummies with *Job change* are positive and highly significant for all health outcomes except for antidepressants (column (1)), for which they are economically and statistically insignificant. Hence, job

[22]We also test if those effects are equal to zero for the group of unhealthy employees. Our results confirm that the health status of this group also remains unaffected. These results are available upon request.

[23]We are aware that health is an important determinant of the different career paths. Therefore, we restrain from making any causality claims about the effects of career paths on health here, and just refer to associations. In order to infer causality, one would also need exogenous variation on the career path followed by each employee after the buyout.

changes are associated with a reduced intake of cardiovascular medication (column (2)), digestive medication (column (4)), overall medication intake (columns (3) and (5)), and health expenditures (column (6)). Similarly, the double interactions of the event-time dummies with *Not employed* are mostly highly significant and positive: *Unemployment* is associated with a deterioration of employees' health, and the effects have about the same magnitude as the ones for *Job change*, but with the opposite sign.

We can infer the health status of those employees who stay with the target firm from the interactions of the event-time dummies with the target indicator. These coefficients are close to zero and insignificant for all health outcomes except for health expenditure, where they are significant and negative, indicating that health expenditures are lower for those target employees who stay with their firms after the buyout. Finally, the triple interactions reported in Table 10 are all insignificant, which suggests that buyouts have no impact on employees' health apart from influencing their career paths. After controlling for career paths, the health of target employees and control employees is similar.

Hence, Table 10 suggests that employees' health and their medication intake after a buyout is different across career paths. Employees who become unemployed are in worse health, whereas those who change jobs are in better health. We conclude that the zero effects shown in Figure 4 may be driven by potential effects on different groups of employees that cancel each other out. Unfortunately, we do not have exogenous variation in career paths, so this hypothesis calls for further research.

6 Social transfers

Finally, we analyze to what extend the state-run social security system[24] buffers the loss of income for buyout employees, especially for those in poor health who experience the most adverse labor-market consequences. We compute the total income from social transfers by adding the income from disability insurance, unemployment insurance, and from retirement

[24] Appendix A provides additional information about the Dutch social security system.

benefits from CBS. First, we estimate the income from insurance (*Total* transfers) by subtracting the income from employment and self-employment from total personal income. Then we combine our estimate of personal income from transfers with the information on the main source of income, and attribute all the income from transfers to the most important source of income for a given individual in a given year. We follow this procedure to compute the following three variables: i) *Disability insurance* equals *Total transfers* if the main source of income is disability insurance benefits, and zero otherwise; ii) *Retirement income* equals *Total transfers* if the main source of income is retirement benefits, and zero otherwise; iii) *Unemployment insurance* equals *Total transfers* if the main source of income is unemployment benefits, and zero otherwise.

Then, we use these as dependent variables in difference-in-differences analyses and triple-differences analyses as described in Sections 3.6.1 and 3.6.2. Table 11 performs difference-in-differences analyses similar to Table 4, but with the different forms of transfers as dependent variables. Table 12 performs triple-difference analyses similar to those in Table 5 with the three main health variables as risk factors.

Table 11 shows that the loss in *Earnings* documented above is mitigated by higher transfers from the state. In event year $k = +4$, buyout employees receive 630 Euros of transfers more than control employees, which corresponds to about half of the loss of 1,300 Euros suffered by buyout employees overall, as reported in Table 4 (see Section 4.1). Most of the transfer income comes from unemployment insurance (+313 Euros), and this increase is statistically also highly significant, whereas the increases in disability insurance (+76 Euros) and retirement income (+190 Euros) are both insignificant. Note that there is a small gap of about 51 Euros (=630-(313+76+190) = 51) that can be attributed to other social transfers, like social assistance.

Table 12 shows that the effects of buyouts on social transfers are about four times larger for those who had been on antidepressants or cardiovascular medication prior to the buyout. We can compare the insurance effects of social transfers by relating the coefficients in columns

(1), (5), and (9) for $k = 4$ to the losses of *Earnings* for the same groups of employees and the same period reported in Table 5 to obtain estimates of the replacement ratio, which is 69% (=2,231 Euros/3,235 Euros) for employees on antidepressants and 55% (=1,937 Euros/3,507 Euros) for employees on cardiovascular medication.[25]

7 Discussion and conclusion

Private equity buyouts have a negative impact on the income and earnings of employees. The adverse effects on those with poor health prior to the buyout, measured by medication intake and other variables, are several times larger than those for healthy employees. Employees with lower predicted productivity based on hedonic wage regressions suffer higher risks of becoming unemployed. This is in line with the operational-improvement hypothesis and suggests that buyout firms identify less productive employees and lay them off with much higher frequency than healthy employees. Since medication intake increases with age, these adverse labor market outcomes of buyouts are concentrated in older employees.

We find that, on average, buyouts do not impact employees' health. We conjecture that this finding may be the result of two distinct mechanisms. First, target employees who find new jobs enjoy better health, whereas those who become unemployed are in worse health.These opposing effects cancel each other out because the size of these two groups is similar. As a result, the effect for the entire population of buyout employees may not be measurably different from zero. The association between buyouts and employees' health might, therefore, not be zero but depend on how many buyout employees become unemployed. Second, we find that about half of the impact of buyouts is buffered by social transfers through retirement benefits, disability insurance, and unemployment insurance. Therefore, total income losses are cushioned by the Dutch social transfer system, and this protection is larger for previously unhealthy workers. The cushioned income effect may also alleviate

[25] We add up θ_4 and η_4 in Table 12 to compute the total effect on social transfers, and in Table 5 to compute the total effect on earnings. The replacement ratio is the ratio of these two measures.

some of the adverse health consequences of a buyout on buyout employees. Unfortunately, we cannot further test these hypotheses with our data and within our institutional setting, so they remain for further research. In addition, to the extent that social transfers decrease the firing costs of PE investors, our finding raises the question whether the social security system lowers the restructuring costs of buyout firms.

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A Appendix: Institutional background

This appendix provides more detailed information about the main characteristics of the institutions relevant for our study applicable during the period 2007-2013.

A.1 Employment protection legislation

The Dutch employment legislation is unique in the world as it is preventive, i.e., employers need approval to dismiss an employee. An employer can request approval to dismiss an employee to either the public employment service or the civil court. These two institutions check in advance if the dismissal is fair. The public employment service considers it is a fair dismissal for economic reasons (e.g. redundancy), dysfunctional behavior, a disturbed employment relationship, illness and misconduct. A severance pay is not required if the dismissal is considered fair by the public employment service, although the employee can apply to the civil court to ask for compensation. The civil court will rescind the employment contract if a substantial reason exists. Substantial reasons are, for example, fraud, incompetence, a circumstantial change like economic conditions or long-term illness that justify dismissal. A severance payment is usually required by the civil court, and the amount depends on the salary, the age and tenure of the employee. On average, the civil court is quicker but more expensive given the severance payment, while the public employment service is slower and cheaper. Employers can choose the institution for individual dismissals, but collective dismissals (at least 20 workers) are submitted to the public employment service. Individuals cannot be dismissed during the two years on sickness leave unless they do not cooperate with the reintegration plans or after payment of a high severance payment. After the two years period, sick employees can be dismissed independently of the outcome from the disability insurance application. The Netherlands is one of the OECD countries with higher protection for individual dismissals, but not for collective ones.

A.2 Unemployment insurance

Unemployment insurance benefits are contributory in the Netherlands. An employee is entitled to unemployment insurance if s/he has worked at least 26 weeks in the last 36 weeks immediately preceding unemployment, s/he is not responsible for terminating the job contract, and s/he actively looks for a new job. The strictness of the search requirement is among the highest across OECD countries (Venn (2012)).. The amount of benefits depends on previous earnings. Unemployed are entitled to 75% of the last daily wage during the first two months, and the amount decreases to 70% thereafter, with a cap in both cases (For example, the maximum was 209.26 Euros in 2018). The duration of benefits depends on previous employment history up to a maximum of thirty-age months (The maximum period was reduced to twenty-four months in April 2019).

A.3 Disability insurance

Individuals are entitled to (partial) disability insurance (DI) if their earnings capacity is reduced by at least 35%.[26] The entitlement does not depend on previous earnings history, or whether it is a work-related illness or injury. There is a waiting period of two-years of sickness benefits before individuals can apply for DI. The employer is responsible for financing sick-pay and make efforts to reintegrate the employee during these two years if the individual had a permanent contract, and until the end of the contract if temporary. In the later case, the public employment service is responsible thereafter. The sick employee receives 70 percent of the gross wage during the period of sickness benefit, although it can go up to 100 percent of the net salary in many collective bargaining agreements (Burkhauser et al. (2008)).

The public employment service assesses DI applications with the help of a medical assessor and a vocational expert. Applicants to DI benefits assessed with a degree lower than 35% are not entitled to any benefits, and their employer can lawfully suspend their contract. The amount of benefits of those assessed with a higher degree depends discontinuously on the degree of disability: 35-55, 55-65, 65-80 and more than 80 or fully disabled. Fully disabled individuals receive 70% of their pre-sickness leave earnings, while the others receive (70*mid-point DI interval)%. In contrast to other countries, like US, DI recipients can combine DI benefits with earnings to a maximum. Similarly, they can also combine unemployment insurance and DI benefits.

A.4 Health insurance

The Netherlands has a universal social health insurance with comprehensive coverage. Compulsory health insurance contributions are the main source of funding (72%), followed by general taxation (13%) (Kroneman et al. (2016)). Individuals older than 18 pay a community-rate premium to their health insurer and an income-dependent premium to a central fund that redistributes the resources across the insurers to compensate for differences in risks. The premium for children is paid to the insurers by the government, and low-income individuals are entitled to receive a health subsidy to cover the health premium. The basic benefit package includes general practice care, hospital care, pharmaceuticals, mental health care,

[26] Earnings capacity depends on the salary of existing occupations that the applicant could potentially still perform.

maternity care and home nursing care. Except for general practice care, home nursing care, integrated care and maternity care, individuals face a deductible since 2008. The amount of the deductible was initially set at around 100 Euros, but this amount has been rising over time. The deductible includes expenditures on outpatient pharmaceuticals, but excludes the co-payments. In addition, insurers may decide not to charge the deductible to provide incentives for the insured to use the proper care. Last, the insured may decide on year basis to pay a higher deductible up to a legal maximum to decrease the premium. Overall, it is one of the most expensive systems in Europe and its quality is highly valued by the users (Kroneman et al. (2016)).

B Figures

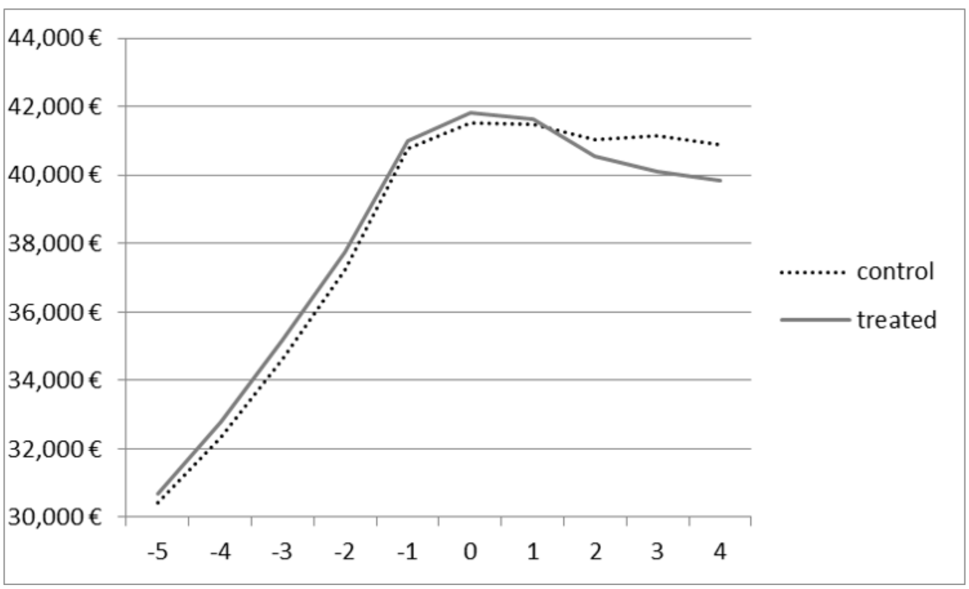

Figure 1: Parallel trends analysis: *Earnings*. This figure presents the development of *Earnings* in event time. For every event year, we compute the mean of *Earnings* for target employees and control employees separately. *Earnings* is defined in Table 1.

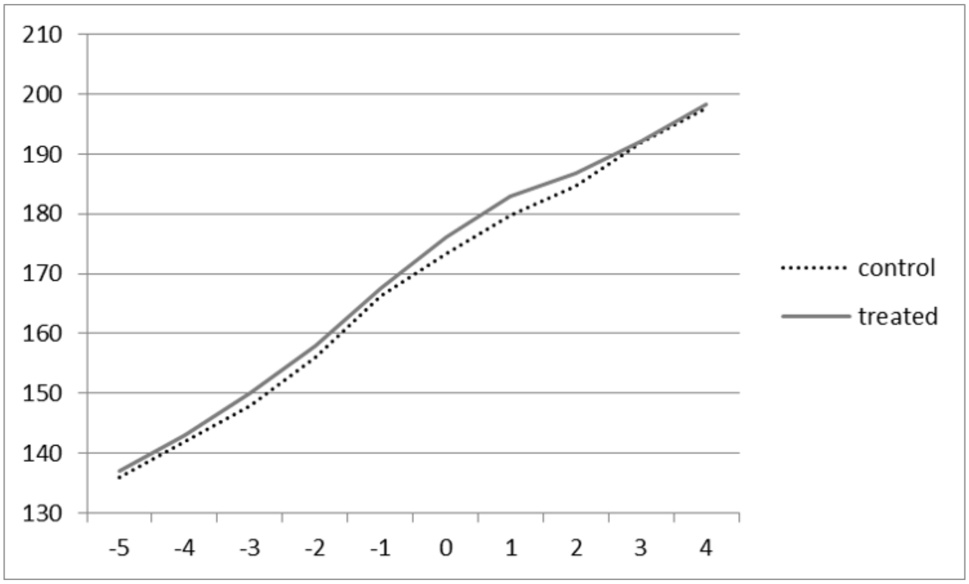

Figure 2: Parallel trends analysis: *Daily wage.* This figure presents the mean of *Daily wage* for target employees and control employees separately. *Daily wage* is defined in Table 1. *Daily wage* is set to missing if *Daily wage* of matched pair is missing in a given year.

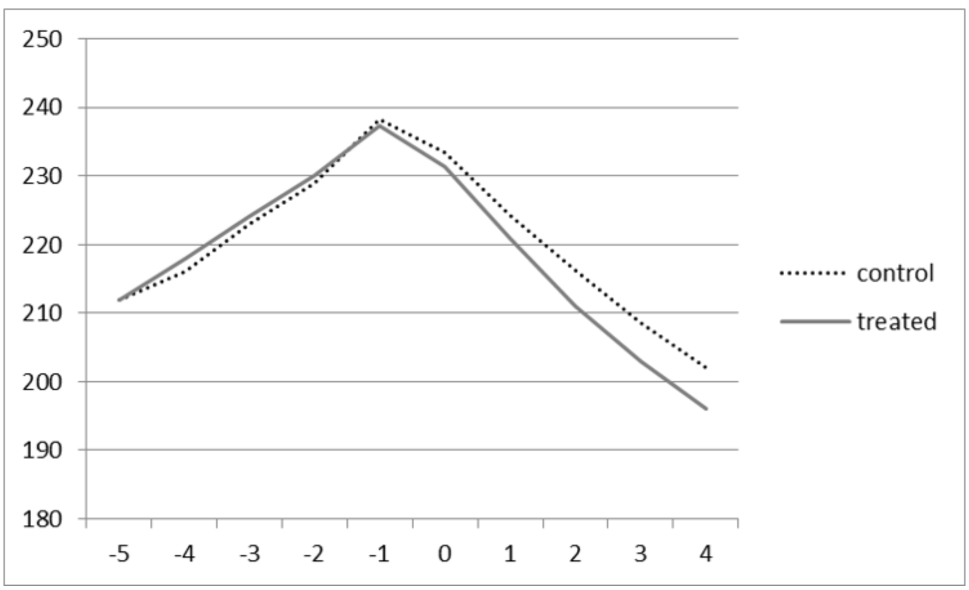

Figure 3: Parallel trends analysis: *Days employed.* This figure presents the mean of *Days employed* for target employees and control employees separately. *Days employed* is defined in Table 1. The inverted-V pattern is a mechanical consequence of the requirement that employees in both groups have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

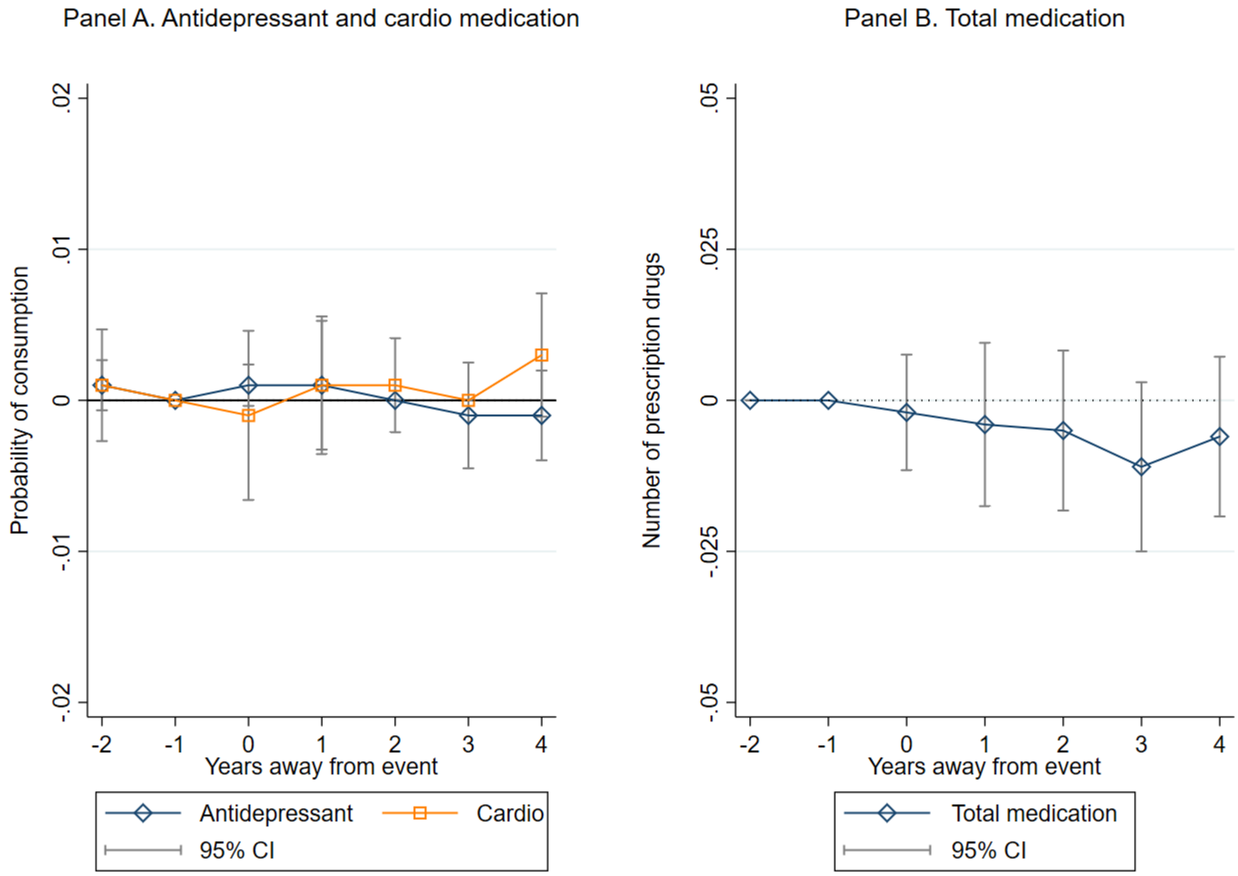

Figure 4: Buyouts and health outcomes. The figure is based on OLS-regressions of health outcome variables in a triple-difference setup from equation (2). We plot the coefficient estimates of θ_k. The numerical variables are defined in Table 1.

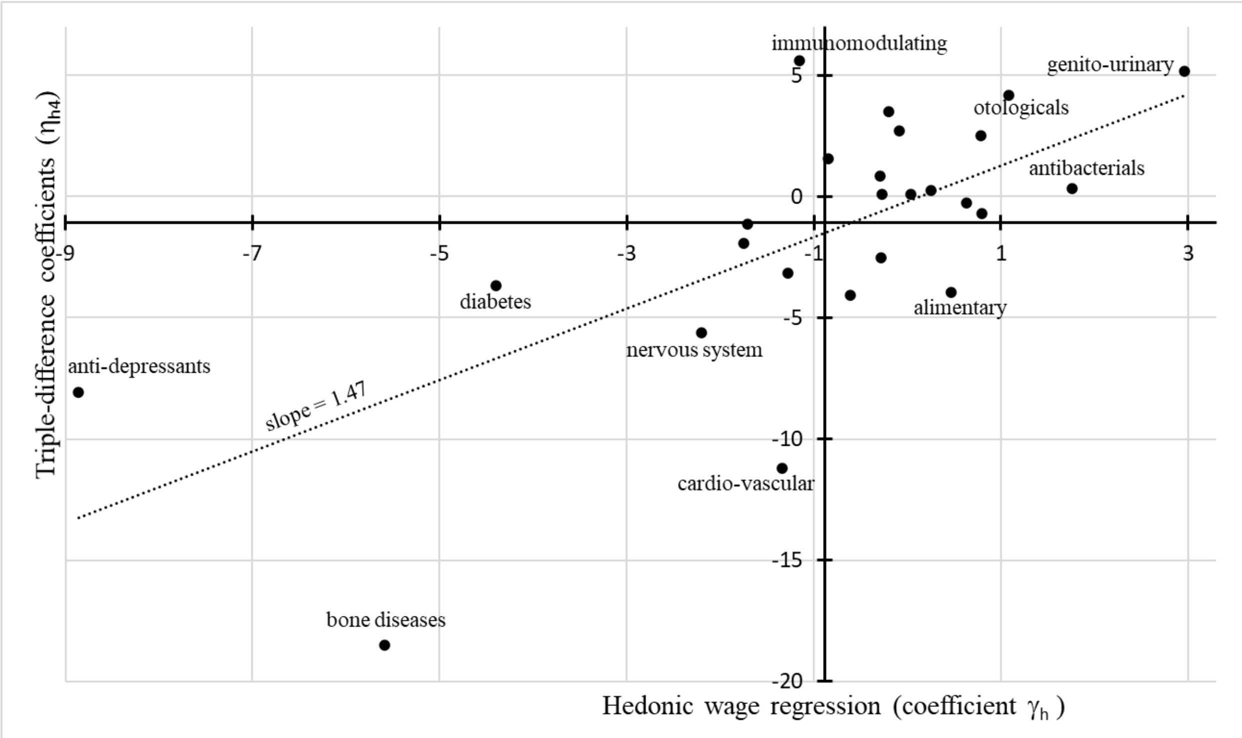

Figure 5: Hedonic wage regressions and employment effects. This figure plots the coefficient estimates for 25 medication types presented in Column (6) and Column (7) in Table 7.

C 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
Antidepressant	1 if individual is prescribed antidepressant medication (for details, see Section 3.4)	0 or 1
Age	Age of the individual in years	[0;∞]
Cardiovascular	1 if individual is prescribed cardiovascular medication (for details, see Section 3.4)	0 or 1
Digestive	1 if individual is prescribed digestive medication (for details, see Section 3.4)	0 or 1
Daily wage	*Earnings* divided by *Days employed*	]0;∞]
Days employed	Sum of days in employment over all spells in one calender year	[0;260]
Disability insurance	1 if main source of income is from disability insurance	0 or 1
Disability insurance (Euro)	*Total insurance* x *Disability insurance*	[0;∞]
Earnings	Sum of income across all spells in one calendar year	[0;∞]
Firm size	Number of employees in firm at the end of calendar year	[0;∞]
Health expenditures	Health costs covered by basic insurance package over calender year	[0;∞]
High medication	1 if Total medication is larger than 3.	0 or 1
Job change	1 as soon as buyout employee has changed jobs after the buyout	
Medication index	Medication based loss in Daily wage, indexed between 0 and 1 (details, see Section 4.3)	[0;1]
Not employed	1 if employee does not have an active employment spell at the end of the calendar year	
Other income	1 - *(Self-)employment* - *Retirement* - *Disability insurance* - *Unemploment insurance*	0 or 1
Pensions	*Total insurance* x *Retirement*	[0;∞]
Retirement	1 if main source of income is from pension	0 or 1
Residual Wage	Predicted loss in *Daily wage* because of medication intake(details, see Section 3.3)	
Stayer	1 if buyout employee is still with the buyout firm in respective calendar year	0 or 1
Switcher down	1 as soon as buyout employee has changed jobs after the buyout and the first wage in the new job is lower than the last wage in the old job	0 or 1
Switcher up	1 as soon as buyout employee has changed jobs after the buyout and the first wage in the new job is higher than the last wage in the old job	0 or 1
Tenure	Number of days in employment in current job	[0;∞]
Total insurance	Difference between income from (self-)employment and income from other sources (e.g. disability insurance, unemployment insurance, retirement)	[0;∞]
Total medication	Number of different prescriptions according to the WHO's ATC-classification	[0;∞]
Unemployment insurance	1 if main source of income is from unemployment insurance	
Unemployment insurance (Euro)	*Total insurance* x *Unemployment insurance*	[0;∞]
Work	1 if main source of income is from employment or self-employment	0 or 1

Table 2: Individual matching success. This table presents descriptive statistics on buyout employees and matched control employees. All variables are measured in the year prior to the private equity buyout announcement. The Imbens-Wooldridge statistic (cf. Imbens and Rubin (2015)) measures the normalized difference between two variables. The test divides the difference between two variables by the square root of the sum of their variances.

	Earnings	Daily wage	Days employed	Tenure	Age	Firm size	Total medication	Digestive	Anti-depressant	Cardio-vascular
Panel A. Matched target employees, N = 55,752										
Mean	40,776	166.20	240.95	2,940	40.10	2,999	39.68%	9.01%	3.55%	11.27%
Median	35,225	142.07	260.00	1,794	39.92	1,006	0.00%	0.00%	0.00%	0.00%
Variance	7.E+08	9710.00	1670.00	1.E+07	108.00	1.E+07	86.92%	8.20%	3.42%	10.00%
Panel B. Matched control employees, N = 55,752										
Mean	41,010	167.57	239.51	2,967	40.15	3,635	40.55%	9.01%	3.55%	11.27%
Median	35,377	143.21	260.00	1,806	39.92	1,371	0.00%	0.00%	0.00%	0.00%
Variance	7.E+08	9680.00	1683.00	1.E+07	110.00	1.E+07	91.86%	8.20%	3.42%	10.00%
Comparison to Matched target employees:										
Relative difference	-0.57%	-0.82%	0.60%	-0.94%	-0.12%	-19.15%	-2.17%	0.00%	0.00%	0.00%
Imbens-Wooldridge	0.01	0.01	0.02	0.01	0.00	0.14	0.01	0.00	0.00	0.00

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

	N	Mean	Median	p1	p99	Standard Deviation
Age	730,754	41.7	41.5	20.4	63.3	113.9
Antidepressant	730,754	4.3%	0.0%	0.0%	100.0%	20.3%
Cardiovascular	730,754	13.6%	0.0%	0.0%	100.0%	34.3%
Daily wage	659,416	178.5	152.6	36.7	633.9	104.8
Days employed	730,754	219.1	260.0	0.0	260.0	72.1
Digestive	730,754	9.6%	0.0%	0.0%	100.0%	29.4%
Disability insurance	717,480	0.9%	0.0%	0.0%	0.0%	9.6%
Disability insurance (Euro)	717,480	238.0	0.0	0.0	0.0	2,789.4
Earnings	730,754	40,320.1	35,388.0	0.0	154,929.0	28,346.5
Health expenditures	560,178	1,338.3	257.6	0.0	15,646.9	4,681.9
High medication	730,754	3.0%	0.0%	0.0%	100.0%	17.1%
Medication index	730,754	2.3%	0.0%	0.0%	28.8%	6.3%
Other income	717,480	2.3%	0.0%	0.0%	100.0%	14.9%
Pensions	717,480	553.9	0.0	0.0	25,541.0	5,679.2
Residual wage	730,754	-0.1	-1.0	-4.0	14.7	1.9
Retirement	717,480	1.3%	0.0%	0.0%	100.0%	11.5%
Stayer	730,754	75.3%	100.0%	0.0%	100.0%	43.1%
Switcher down	730,754	6.5%	0.0%	0.0%	100.0%	24.6%
Switcher up	730,754	10.2%	0.0%	0.0%	100.0%	30.2%
Total insurance	717,480	2,172.2	0.0	0.0	40,237.0	8,087.3
Total medication	730,754	0.5	0.0	0.0	5.0	1.1
Unemployment insurance	717,480	2.0%	0.0%	0.0%	100.0%	13.9%
Unemployment insurance (Euro)	717,480	496.3	0.0	0.0	23,106.0	3,909.4
Work	717,480	93.5%	100.0%	0.0%	100.0%	24.7%

Table 4: Income and employment. The table presents estimates from OLS-regressions on measures of human capital in a difference-in-differences setup from equation (1). We only report the coefficient estimates of θ_k . The numerical variables are defined in Table 1. Each specification contains individual and year fixed effects. The number of observations is 756,658. Standard errors are clustered at the firm level. t-statistics are provided below the coefficient estimates. *, **, *** indicate significance at the 10%, 5% and 1% level, respectively.

	(1) Earnings	(2) Daily wage	(3) Days employed
D_{i-2} x Target	540.3	0.931	2.452
	1.18	0.60	1.58
D_{i0} x Target	51.2	1.216	-1.197
	0.14	1.09	-1.10
D_{i1} x Target	-107.7	1.902	-2.451*
	-0.17	0.86	-1.67
D_{i2} x Target	-734.2	0.886	-4.201**
	-1.64	0.52	-2.12
D_{i3} x Target	-1288.8**	-1.011	-4.667**
	-2.38	-0.62	-2.04
D_{i4} x Target	-1292.3**	-0.558	-5.219**
	-2.08	-0.21	-2.30

Table 5: Medication and income. The table presents estimates from OLS-regressions on *Earnings* and *Days employed* in a triple-difference setup from equation (2). Each specification includes a risk factor (RF), which is measured in the year prior to the buyout announcement. We only report the coefficient estimates of θ_k and η_k. Each specification contains individual and year fixed effects. The numerical variables are defined in Table 1. The number of observations is 727,724. Standard errors are clustered at the firm level. t-statistics are provided below the coefficient estimates. *, **, *** indicate significance at the 10%, 5% and 1% level, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable:	Earnings	Days employed	Earnings	Days employed	Earnings	Days employed
Risk Factor (RF):	Antidepressant		Cardiovascular		Total medication	
D_{i-2} x Target	522.0	2.309	594.7	2.429	579.5	2.396
	1.15	1.51	1.34	1.64	1.27	1.57
D_{i0} x Target	52.2	-1.180	44.7	-1.077	53.2	-1.046
	0.14	-1.11	0.12	-1.04	0.14	-1.03
D_{i1} x Target	-72.6	-2.317	34.4	-1.846	47.1	-1.793
	-0.11	-1.64	0.05	-1.36	0.07	-1.37
D_{i2} x Target	-676.4	-3.919**	-484.7	-3.209*	-490.0	-3.115*
	-1.52	-2.03	-1.14	-1.75	-1.13	-1.75
D_{i3} x Target	-1211.2**	-4.331*	-1004.9**	-3.439*	-989.4*	-3.267
	-2.24	-1.94	-1.97	-1.65	-1.89	-1.61
D_{i4} x Target	-1222.7**	-4.939**	-1018.1*	-3.983*	-990.9*	-3.849*
	-1.97	-2.24	-1.72	-1.94	-1.65	-1.93
D_{i-2} x Target x RF	411.3	3.452	-520.5	-0.064	-115.5	0.016
	0.84	1.46	-1.11	-0.04	-0.79	0.03
D_{i0} x Target x RF	-27.9	-0.475	59.0	-1.070	10.6	-0.315
	-0.08	-0.24	0.18	-0.97	0.10	-0.86
D_{i1} x Target x RF	-996.9**	-3.786	-1268.2**	-5.401***	-358.9**	-1.543**
	-2.15	-1.31	-2.52	-3.25	-2.13	-2.54
D_{i2} x Target x RF	-1640.9***	-8.014**	-2235.8***	-8.892***	-572.7***	-2.563***
	-2.82	-2.44	-3.83	-4.33	-2.82	-3.39
D_{i3} x Target x RF	-2209.7***	-9.545***	-2552.1***	-11.042***	-700.6***	-3.314***
	-3.36	-2.75	-3.63	-4.32	-3.08	-3.59
D_{i4} x Target x RF	-2011.5***	-8.059**	-2489.2***	-11.230***	-691.0***	-3.185***
	-2.86	-2.18	-3.32	-4.00	-2.64	-3.16

Table 6: Productivity and employment outcome. The table presents estimates from OLS-regressions on *Earnings, Daily wage,* and *Days employed* in a triple-difference setup from equation (2). Each specification includes a risk factor (RF), which is measured in the year prior to the buyout announcement. We only report the coefficient estimates of η_k. Each specification contains individual and year fixed effects. The numerical variables are defined in Table 1. The number of observations is 727,724. Standard errors are clustered at the firm level. t-statistics are provided below the coefficient estimates. *, **, *** indicate significance at the 10%, 5% and 1% level, respectively.

	(1) Earnings	(2) Daily wage	(3) Days Employed	(4) Earnings	(5) Daily wage	(6) Days Employed
Risk Factor (RF):	Residual wage			Medication index		
D_{i-2} x Target x RF	-14.9	0.033	0.134	-152.3	5.826	6.161
	-0.58	0.40	1.08	-0.07	0.93	0.81
D_{i0} x Target x RF	18.5	0.109	0.033	-400.0	5.227	-7.092
	0.97	1.03	0.33	-0.26	0.76	-1.04
D_{i1} x Target x RF	-13.3	-0.060	-0.061	-5962.3**	-3.923	-28.1**
	-0.54	-0.62	-0.41	-2.19	-0.43	-2.37
D_{i2} x Target x RF	-57.5*	0.047	-0.249	-9333.4***	0.343	-47.1***
	-1.72	0.47	-1.57	-2.96	0.04	-3.28
D_{i3} x Target x RF	-101.9***	0.097	-0.447**	-11028.7***	2.041	-55.8***
	-2.92	0.98	-2.51	-3.08	0.29	-3.42
D_{i4} x Target x RF	-94.5**	-0.029	-0.421**	-11145.9***	-10.375	-49.6***
	-2.33	-0.23	-2.16	-2.71	-1.05	-2.69

Table 7: Hedonic wage regressions and labor-outcome effects for all medications. Column (2) presents the ATC classification codes (see Norwegian Institute of Public Health (2017)) associated with the medication type presented in Column (1). Columns (3), (4), (5) report the prevalence of a medication type in the workforce, the treated group, and the control group, respectively. Column (6) presents the average over the yearly estimates of γ_4 as presented in equation (4). The t-statistics are computed as the average coefficient divided by the standard error of the annual coefficient estimates between 2006 and 2012. The number of observations varies annually between 2.9 and 3.1 million observations. Column (7) presents OLS-regressions of *Days employed* in a triple-difference setup from equation (2). Each specification includes a risk factor (RF), which is measured in the year prior to the buyout announcement. The risk factor is the medication indicated in column (1). We only report the coefficient estimates of η_4. The specification contains individual and year fixed effects. The number of observations is 727,724. Standard errors are clustered at the firm level. t-statistics are provided below the coefficient estimates. *, **, *** indicate significance at the 10%, 5% and 1% level, respectively.

Name	(1) ATC classification	(2) Workforce (%)	(3) Treated (%)	(4) Controls (%)	(5) Hedonic wage Regression	(6) Triple-difference regression
Alimentary tract and metabolism	A					
diabetes	A10	2%	2%	2%	-4.404***	-3.714
					-37.68	-0.70
digestive and constipation	A02, A03, A06	12%	11%	11%	-0.27***	0.088
					-5.49	0.04
other alimentary tract and metabolism	A01, A04, A05, A07, A09, A14, A16	2%	2%	2%	0.471***	-3.940
					8.19	-1.01
vitamins and antianemic preparations	A11, A12B, B03	2%	2%	2%	-1.748***	-1.929
					-10.16	-0.43
Blood and blood forming organs	B					
blood and blood forming organs	B02, B05, B06	2%	2%	2%	0.8***	-0.698
					5.10	-0.16
Cardiovascular system	C					
cardiovascular	B01, C01-C03, C07-C10	11%	11%	11%	-1.34***	-11.230***
					5.19	-4.00
other cardiovascular system	C04, C05	1%	1%	1%	0.042***	0.087
					0.28	0.02
Dermatologicals	D					
emolients, protectives, wounds and ulcers	D02, D03	3%	3%	3%	-0.851***	1.619
					-13.53	0.48
other dermatologicals	D01, D04-D11	18%	16%	16%	0.254***	0.259
					4.50	0.19
Genito urinary system and sex hormones	G					
genito urinary system and sex hormones	G01-G04	20%	14%	15%	2.96***	5.183
					5.72	1.55
Systemic hormonal preparations	H					
systemic hormonal preparations	H01-H05	4%	4%	4%	-1.705***	-1.187
					-17.09	-0.38

Table 7: Hedonic wage regressions and labor-outcome effects for all medications (continued).

Name	(1) ATC classification	(2) % Workforce	(3) % Treated	(4) % Controls	(5) Hedonic wage Regression	(6) Triple-difference regression
Antiinfectives for systemic use	J					
antibacterials for systemic use	J01	22%	19%	19%	1.755***	0.350
					36.92	0.22
other antiinfectives for systemic use	J02, J04-J07	3%	3%	3%	-0.28***	-2.521
					-3.65	-0.80
Immune system	L					
antineoplastics and immunomodulating agents	L01-L04	1%	1%	1%	-1.152***	5.618
					-4.71	1.01
Musculo-skeletal system	M					
musculo-skeletal system	M01-M04, M09	20%	18%	18%	-0.609***	-4.031**
					-15.79	-2.32
bone diseases	A12A, M05	1%	1%	1%	-5.591***	-18.653**
					-15.06	-2.41
Nervous system	N					
antidepressant	N06A	4%	4%	4%	-8.861***	-8.059**
					-53.82	-2.18
opioids	N02A	3%	3%	3%	-1.283***	-3.172
					-15.29	-0.72
other nervous system	N01-N07, ex N02A, N06A	10%	8%	8%	-2.205***	-5.609**
					-10.41	2.48
Antiparasitic products and insecticides	P					
antiparasitic, insecticides and repellents	P01-P03	1%	1%	1%	-0.086***	2.660
					-1.02	0.60
Respiratory system	R					
obstructive airway diseases	R03	7%	6%	6%	-0.286***	0.848
					-4.30	0.41
other respiratory system	R01, R02, R05-R07	16%	16%	16%	0.635***	-0.271
					7.35	-0.19
Sensory organs	S					
ophthalmologicals	S01	8%	7%	7%	0.79***	2.541
					13.55	1.28
otologicals	S02	3%	2%	2%	1.084***	4.194
					12.47	1.23
Various	V					
various	V01, V03, V04, V06-V08	<1%	<1%	<1%	-0.201***	3.118
					-1.00	0.34
Average					-0.884	-1.51

Table 8: Career paths. The table presents estimates from OLS-regressions of career path indicators in a difference-in-differences setup from equation (1). We only report the coefficient estimates of θ_k . The numerical variables are defined in Table 1. Each specification contains individual and year fixed effects. The number of observations is 756,658. Standard errors are clustered at the firm level. t-statistics are provided below the coefficient estimates. *, **, *** indicate significance at the 10%, 5% and 1% level, respectively.

	Indicators based on main source of income					Indicators based on employment spell		
	(1) Work	(2) Retirement	(3) Disability insurance	(4) Unemployment insurance	(5) Other income	(6) Stayer	(7) Switcher down	(8) Switcher up
D_{i-2} x Target	0.0	0.001	0.000	-0.001	-0.002	0.013	-0.002	-0.003
	0.31	0.35	0.15	-0.35	-0.63	0.72	-0.26	-0.24
D_{i0} x Target	0.0	0.000	0.000	0.002	-0.001	-0.025	0.004	0.010
	-0.10	-0.22	-0.54	1.63	-0.48	-1.54	1.29	1.33
D_{i1} x Target	-0.009*	0.002	0.001	0.007***	-0.001	-0.031	0.007	0.017
	-1.93	1.31	0.98	2.91	-0.44	-1.24	1.04	1.23
D_{i2} x Target	-0.011*	0.002	0.001	0.010***	-0.001	-0.067**	0.016*	0.021
	-1.90	0.85	1.07	2.76	-0.62	-2.25	1.78	1.33
D_{i3} x Target	-0.013*	0.001	0.002	0.010***	-0.001	-0.047*	0.019*	0.020
	-1.89	0.57	1.43	2.94	-0.26	-1.76	1.90	1.20
D_{i4} x Target	-0.013*	0.002	0.002	0.009***	0.000	-0.045	0.020*	0.022
	-1.78	0.62	1.32	3.10	-0.10	-1.59	1.72	1.23

Table 9: Health and career paths. The table presents estimates from OLS-regressions of career path indicators in a triple-difference setup from equation (2). Each specification includes a risk factor (RF), which is measured in the year prior to the buyout announcement. We run the regression with all observations from $k = -2$ to $k = +4$, but only report the coefficient estimates of η_2, and η_4. Each specification contains individual and year fixed effects. The numerical variables are defined in Table 1. The number of observations is 727,724. Standard errors are clustered at the firm level. t-statistics are provided below the coefficient estimates. *, **, *** indicate significance at the 10%, 5% and 1% level, respectively.

	(1) Work	(2) Retirement	(3) Disability insurance	(4) Unemployment insurance	(5) Other income	(6) Stayer	(7) Switcher down	(8) Switcher up
Panel A. Risk Factor = Medication index								
D_{i2} x Target x Medication	-0.0870***	0.0299***	0.0235*	0.0396***	-0.0060	-0.1257***	0.0230*	-0.0085
index	-3.09	3.52	1.71	2.95	-0.54	-3.19	1.80	-0.31
D_{i4} x Target x Medication	-0.1015***	0.0377***	0.0425**	0.0317***	-0.0104	-0.1062**	0.0145	-0.0134
index	-3.24	2.81	2.30	2.60	-0.81	-2.47	0.84	-0.41
Panel B. Risk Factor = Residual wage								
D_{i2} x Target x Residual	-0.0010*	0.0004**	0.0003	0.0005***	-0.0002	-0.0018**	0.0000	-0.0004
wage	-1.94	2.45	0.90	2.06	-0.97	-2.44	-0.08	-0.83
D_{i4} x Target x Residual	-0.0015***	0.0005**	0.0006	0.0008***	-0.0004	-0.0016**	-0.0003	-0.0036
wage	-2.63	2.00	1.43	2.65	-1.52	-2.01	-0.65	-0.87
Panel C. Risk Factor = Antidepressant medication								
D_{i2} x Target x Antidepressant	-0.0332**	0.0152***	0.0091	0.0089	0.0000	-0.0446**	0.0016	-0.0010
medication	-2.40	3.74	1.16	1.16	0.01	-2.22	0.20	-0.10
D_{i4} x Target x Antidepressant	-0.0437***	0.0172***	0.0278***	0.0043	-0.0056	-0.0330	-0.0139	-0.0062
medication	-2.78	2.93	2.62	0.57	-0.96	-1.52	-1.41	-0.53
Panel D. Risk Factor = Cardio medication								
D_{i2} x Target x Cardio	-0.0289***	0.0092*	0.0082**	0.0155***	-0.0040	-0.0411***	0.0018	0.0030
medication	-3.46	1.90	2.17	3.01	-1.40	-2.99	0.41	0.33
D_{i4} x Target x Cardio	-0.0357***	0.0101	0.0133***	0.0125***	-0.0002	-0.0313**	0.0015	-0.0026
medication	-3.02	1.26	2.87	2.63	-0.06	-2.19	0.26	-0.26
Panel E. Risk Factor = Total medication								
D_{i2} x Target x Total	-0.0092***	0.0022	0.0037*	0.0044***	-0.0011	-0.0165***	-0.0005	0.0013
medication	-2.93	1.31	1.91	2.61	-1.06	-2.93	-0.31	0.39
D_{i4} x Target x Total	-0.0121***	0.0026	0.0055**	0.0042**	-0.0002	-0.0111*	-0.0014	0.0008
medication	-2.93	1.02	2.40	2.42	-0.19	-1.86	-0.65	0.21

Table 10: Job changes, unemployment, and health. The table presents estimates from OLS-regressions of health variables in a triple-difference setup from equation (2). We run the regression with all observations from $k = -2$ to $k = +4$, but only report the coefficient estimates for event periods $k = 0$, $k = +2$, $k = +4$. Each specification contains individual and year fixed effects. The numerical variables are defined in Table 1. The number of observations is 727,724. Standard errors are clustered at the firm level. t-statistics are provided below the coefficient estimates. *, **, *** indicate significance at the 10%, 5% and 1% level, respectively.

	(1) Antidepressant	(2) Cardio-vascular	(3) Total medication	(4) Digestive	(5) High medication	(6) Health expenditures
D_{i0} x Job change	0.003	-0.0062**	-0.0207**	-0.0069*	-0.0033***	-263.5613***
	1.03	-1.97	-2.47	-1.80	-2.58	-4.37
D_{i2} x Job change	0.001	-0.0159***	-0.0547***	-0.0103***	-0.0062***	-147.0931***
	0.37	-7.83	-9.12	-3.82	-5.95	-3.45
D_{i4} x Job change	0.000	-0.0272***	-0.0870***	-0.0108***	-0.0095***	-95.6786**
	-0.18	-13.09	-13.32	-4.24	-7.83	-2.15
D_{i0} x Not employed	0.0120***	0.0103**	0.0560***	0.0095**	0.003	355.3111***
	3.54	2.46	4.01	2.03	0.97	3.31
D_{i2} x Not employed	0.0178***	0.002	0.0357***	-0.0084***	0.0053***	515.9629***
	6.37	0.87	4.02	-2.64	3.02	5.86
D_{i4} x Not employed	0.0146***	0.0069***	0.0637***	0.000	0.0103***	162.6246**
	5.77	2.58	6.45	0.14	5.29	2.18
D_{i0} x Target	0.001	0.000	0.000	0.001	-0.001	-75.4752*
	1.25	-0.14	-0.03	0.52	-0.74	-1.90
D_{i2} x Target	0.001	0.001	-0.006	-0.004	-0.001	-39.137
	0.70	0.48	-0.84	-1.55	-0.48	-0.93
D_{i4} x Target	0.002	0.003	-0.011	-0.0051*	-0.002	-110.3313**
	0.91	1.08	-1.29	-1.77	-1.21	-2.37
D_{i0} x Target x Job change	-0.003	-0.003	-0.019	0.002	0.000	9.778
	-0.82	-0.70	-1.61	0.37	0.15	0.12
D_{i2} x Target x Job change	-0.0039*	0.001	0.001	0.0075**	0.000	-66.894
	-1.79	0.24	0.11	2.12	0.09	-1.15
D_{i4} x Target x Job change	-0.0045*	0.0051*	0.0181*	0.006	0.000	9.227
	-1.87	1.74	1.90	1.52	0.18	0.15
D_{i0} x Target x Not employed	0.003	-0.001	-0.004	-0.007	0.000	194.850
	0.60	-0.13	-0.21	-1.02	0.01	1.15
D_{i2} x Target x Not employed	-0.006	0.004	0.011	0.0119**	0.0043*	-150.009
	-1.48	1.10	0.83	2.29	1.66	-1.23
D_{i4} x Target x Not employed	-0.0077**	-0.001	0.007	0.0157***	0.003	223.2024**
	-2.21	-0.36	0.48	3.68	0.95	2.02

Table 11: Insurance and transfers. The table presents estimates from OLS-regressions of social insurance variables in a difference-in-differences setup from equation (1). We only report the coefficient estimates of θ_k. The numerical variables are defined in Table 1. Each specification contains individual and year fixed effects. The number of observations is 756,658. Standard errors are clustered at the firm level. t-statistics are provided below the coefficient estimates. *, **, *** indicate significance at the 10%, 5% and 1% level, respectively.

	(1) Total insurance	(2) Disability insurance	(3) Retirement income	(4) Unemployment insurance
D_{i-2} x Target	22.3	-5.1	51.8	2.0
	0.09	-0.22	0.35	0.03
D_{i0} x Target	315.7**	13.1	-14.6	34.7
	2.46	1.15	-0.42	1.32
D_{i1} x Target	477.2***	30.0	94.4	200.5***
	2.98	1.29	1.18	3.31
D_{i2} x Target	621.2***	48.0*	92.2	279.4***
	3.06	1.65	0.96	3.09
D_{i3} x Target	592.1**	62.6	55.6	313.5***
	2.27	1.60	0.46	3.44
D_{i4} x Target	630.4**	76.4	190.4	313.1***
	2.30	1.50	1.23	3.60

Table 12: Insurance, transfers, and health. The table presents estimates from OLS-regressions of social insurance variables in a triple-difference setup from equation (2). Each specification includes a risk factor (RF), which is measured in the year prior to the buyout announcement. We only report the coefficient estimates of θ_k and η_k for event periods $k = -2$, $k = 0$, $k = +2$, $k = +4$. The numerical variables are defined in Table 1. Each specification contains individual and year fixed effects. The number of observations is 715,369. Standard errors are clustered at the firm level. t-statistics are provided below the coefficient estimates. *, **, *** indicate significance at the 10%, 5% and 1% level, respectively.

	(1) Total insurance	(2) Disability insurance	(3) Retire-ment income	(4) Unemploy-ment insurance	(5) Total insurance	(6) Disability insurance	(7) Retire-ment income	(8) Unemploy-ment insurance	(9) Total insurance	(10) Disability insurance	(11) Retire-ment income	(12) Unemploy-ment insurance
Risk Factor (RF):	(1) - (4): Antidepressant				(5) - (8): Cardiovascular				(9) - (12): Total medication			
D_{i-2} x Target	31.4	-2.9	56.4	2.7	-9.4	-1.3	26.2	-7.4	-8.7	5.4	18.9	-7.5
	0.13	-0.14	0.39	0.04	-0.05	-0.06	0.25	-0.12	-0.04	0.33	0.18	-0.12
D_{i0} x Target	304.9**	10.0	-20.2	36.1	281.4**	1.1	-9.7	36.1	263.6**	-5.8	-19.5	38.4
	2.40	0.99	-0.58	1.39	2.48	0.11	-0.40	1.44	2.39	-0.53	-0.79	1.51
D_{i2} x Target	578.5***	43.2*	72.9	269.0***	467.1***	21.6	29.6	223.8***	428.2**	-0.1	34.3	214.0***
	2.96	1.65	0.77	3.04	2.75	0.91	0.43	2.82	2.57	0.00	0.50	2.73
D_{i4} x Target	573.2**	53.1	155.7	311.2***	470.2**	39.2	111.8	269.5***	430.4*	22.1	91.2	251.2***
	2.15	1.16	1.02	3.62	2.07	0.93	0.93	3.43	1.91	0.64	0.74	3.19
D_{i-2} x Target x RF	-175.7	-42.5	-88.0	-3.1	311.7	-22.7	232.2	92.5	87.7	-19.7	81.7	27.0
	-0.62	-0.26	-0.57	-0.03	0.77	-0.35	0.64	1.17	0.80	-0.63	0.75	1.12
D_{i0} x Target x RF	304.7	89.0	156.5	-41.0	303.9	106.2**	-42.9	-12.8	122.7*	45.6*	10.3	-9.8
	1.35	0.63	1.62	-0.49	1.55	2.04	-0.28	-0.23	1.83	1.78	0.21	-0.49
D_{i2} x Target x RF	1213.6***	143.7	543.9***	293.8	1380.1***	237.3**	561.8*	495.5***	460.0***	113.5**	135.7	159.5***
	3.08	0.72	3.05	1.61	3.73	2.33	1.85	3.21	3.52	2.26	1.33	3.32
D_{i4} x Target x RF	1657.9***	692.4**	994.6***	52.6	1467.2***	340.7**	725.2*	395.8**	468.4***	126.9*	232.6*	150.2**
	3.54	2.40	3.59	0.26	2.82	2.37	1.65	2.44	2.64	1.95	1.66	2.46

D Online Appendix

Table OA1: Medication and income. The table presents estimates from OLS-regressions on *Earnings* and *Days employed* in a triple-difference setup from equation (2). Each specification includes a risk factor (RF), which is measured in the year prior to the buyout announcement. We only report the coefficient estimates of θ_k and η_k. Each specification contains individual and year fixed effects. The numerical variables are defined in Table 1. The number of observations is 727,724. Standard errors are clustered at the firm level. t-statistics are provided below the coefficient estimates. *, **, *** indicate significance at the 10%, 5% and 1% level, respectively.

	(1)	(2)	(3)
Dependent variable:	Daily Wage	Daily Wage	Daily Wage
Risk Factor (RF):	Antidepressant	Cardio-vascular	Total medication
D_{i-2} x Target	0.873	0.879	0.789
	0.55	0.55	0.48
D_{i0} x Target	1.221	1.069	0.996
	1.08	0.90	0.79
D_{i1} x Target	1.919	1.948	2.042
	0.86	0.85	0.87
D_{i2} x Target	0.898	0.868	0.802
	0.53	0.51	0.46
D_{i3} x Target	-0.969	-1.052	-1.153
	-0.59	-0.64	-0.70
D_{i4} x Target	-0.458	-0.582	-0.548
	-0.17	-0.22	-0.20
D_{i-2} x Target x RF	1.473	0.448	0.332
	1.13	0.45	0.80
D_{i0} x Target x RF	-0.135	1.315	0.572
	-0.11	1.03	1.07
D_{i1} x Target x RF	-0.548	-0.464	-0.341
	-0.34	-0.33	-0.61
D_{i2} x Target x RF	-0.397	0.133	0.270
	-0.23	0.10	0.50
D_{i3} x Target x RF	-1.448	0.361	0.470
	-0.86	0.25	0.94
D_{i4} x Target x RF	-3.606	0.168	0.023
	-1.48	0.09	0.03

Table OA2: Other health factors and income. The table presents estimates from OLS-regressions of *Earnings* and *Days employed* in a triple-difference setup from equation (2). Each specification includes a risk factor (RF), which is measured in the year prior to the buyout announcement. We only report the coefficient estimates of θ_k and η_k. Each specification contains individual and year fixed effects. The numerical variables are defined in Table 1. The number of observations is 727,724. Standard errors are clustered at the firm level. t-statistics are provided below the coefficient estimates. *, **, *** indicate significance at the 10%, 5% and 1% level, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable:	Earnings	Days employed	Earnings	Days employed	Earnings	Days employed
Risk Factor (RF):	Digestive		High medication		High health expenditures	
D_{i-2} x Target x RF	-102.3	0.107	-258.2	1.544	259.8	0.422
	-0.29	0.06	-0.35	0.60	0.71	0.20
D_{i0} x Target x RF	-42.9	0.687	26.4	-0.624	238.2	2.8842*
	-0.18	0.63	0.04	-0.26	0.74	1.69
D_{i1} x Target x RF	-188.6	1.034	-939.9	-3.306	91.7	1.956
	-0.46	0.64	-1.17	-1.03	0.23	0.84
D_{i2} x Target x RF	-78.3	1.095	-1720.1049*	-8.4331**	-794.9	-1.472
	-0.18	0.55	-1.67	-2.10	-1.58	-0.50
D_{i3} x Target x RF	-38.7	-0.219	-2832.8003**	-13.2017***	-1022.0410*	-2.433
	-0.08	-0.10	-2.45	-2.84	-1.71	-0.74
D_{i4} x Target x RF	-145.4	-0.918	-2489.0338*	-9.7391*	-1651.7269**	-7.5412*
	-0.26	-0.37	-1.87	-1.82	-2.38	-1.79

Table OA3: Health outcomes. The table presents estimates from OLS-regressions of measures of employee health status in a difference-in-differences setup from equation (1). We only report the coefficient estimates of θ_k . The numerical variables are defined in Table 1. Each specification contains individual and year fixed effects. The number of observations is 756,658. Standard errors are clustered at the firm level. t-statistics are provided below the coefficient estimates. *, **, *** indicate significance at the 10%, 5% and 1% level, respectively.

	(1) Antidepressant	(2) Cardio-vascular	(3) Total medication	(4) Digestive	(5) High medication	(6) Health expenditures
D_{i-2} x Target	0.001	0.001	0.000	0.000	0.000	-38.033
	1.18	0.53	0.30	0.14	0.04	-0.93
D_{i0} x Target	0.001	-0.001	-0.001	0.001	-0.002	-58.141
	1.43	-0.35	-0.77	0.41	-0.41	-1.43
D_{i1} x Target	0.001	0.001	0.000	-0.001	-0.004	-70.017*
	0.43	0.46	0.44	-0.55	-0.58	-1.71
D_{i2} x Target	0.000	0.001	0.000	-0.001	-0.005	-66.637*
	-0.10	0.63	0.09	-0.45	-0.74	-1.75
D_{i3} x Target	-0.001	0.000	-0.002*	0.000	-0.011	-102.171***
	-0.56	0.01	-1.77	-0.15	-1.54	-2.65
D_{i4} x Target	-0.001	0.003	-0.001	-0.001	-0.006	-66.249
	-0.66	1.44	-1.17	-0.36	-0.89	-1.63

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