Private Equity Buyouts and Employee Health

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Abstract

We examine the role of employee health for the restructuring of the labor force during private equity buyouts using employee-level data of 56,000 Dutch buyout employees. We find no evidence that buyouts have a negative impact on the health of buyout employees. Employees lose income and employment after buyouts, and these losses are far larger for employees in poorer health. The negative effect of buyouts on employees' incomes is buffered by social transfers, and this buffer is larger for employees in poorer health, who exit the labor market at a much higher rate. Health characteristics associated with lower wages in the general population are strongly predictive of job loss after buyouts.

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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Private Equity Buyouts and Employee Health*

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Abstract

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1 Introduction

Restructuring of Private Equity (PE) targets is associated with a significant turnover and reduction of the labor force. The literature has linked restructuring after buyouts to changes in employment, wages, and financial outcomes of the firm. However, the wider implications of buyouts for employees' well-being have received little attention.[1]

In this paper, we exploit the health records of employees in Private Equity buyouts. We match buyout transactions to an integrated employer-employee data set and to indicators for the health outcomes of individual employees, and report two important findings. First, since buyout firms streamline operations and strengthen incentives (see Kaplan and Stromberg, 2009), we expect that they increase work pressure on those employees who remain with the firm and create a more demanding and stressful work environment, with the associated adverse influence on employees' health. However, we show that there is no such effect based on a large data set. Second, we observe a significant loss of employment and income for all buyout employees and show that a disproportionately larger part of these losses falls on buyout employees in poor health.

We examine two channels through which health conditions may affect post-buyout career paths. The first channel works through the state-level social security system. We show that employees are more likely to leave the target firm if they have health conditions associated with larger social security transfers. Our interpretation is that employees who are covered by social security transfers are more likely to separate from the buyout firm because social security transfers reduce the costs associated with terminating employment contracts for firms as well as for employees. For the second channel, we examine whether buyout firms remove employees with low productivity. We run population-wide regressions to estimate the precise link between health characteristics and equilibrium wages, which we take as a proxy for individual-level productivity, and show that employees are more likely to leave target

[1] For a detailed discussion of the related literature on the consequences of PE-led restructuring for employees see Section 2. Lambert et al. (2021) use survey data to study how buyouts affect employee satisfaction.

firms if their health characteristics are associated with lower individual productivity.

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 expenditures. Furthermore, we collect data on employees' employment history after buyouts and record their main source of income (employment, self-employment, disability benefits, retirement benefits, unemployment benefits), job changes, and the wages of those who are employed.

We begin by asking whether PE buyouts affect target employees' employment, income, and health. We find that the impact on income and employment is significant: Compared to the control group and by the fourth calendar year after the buyout, target employees lose on average about two percent of pre-buyout employment and €1,300 or 3.7% of pre-treatment annual earnings (median of target employees in the pre-buyout year: €35,225). We investigate how the negative effects of buyouts on earnings and employment depend on employees' health status and find that the influence of health is significant: In addition to the baseline loss experienced by all buyout employees, employees on cardiovascular medication lose an additional €2,500 per year, and those on antidepressants €2,000 per year in the fourth year after the buyout relative to matched control employees; these estimates are averages across all employees and need to be interpreted relative to the same median pre-buyout earnings as the baseline loss of income. Many of the affected employees exit the labor market and receive larger social security transfers compared to healthy employees.

Based on prior literature, we hypothesize that buyout-related restructuring has a negative impact on employees' health by creating a more stressful work environment and a higher level of job insecurity, which should lead to anxiety-related stress for those who remain employed with the target. We do not find empirical support for these predictions: The average buyout employee does not fare worse than the average control employee. Buyout employees display health levels that perfectly match those of the control group from two years before the buyout to four years after the buyout. These zero results cannot be attributed to a lack of statistical power, since the economic differences between treated and matched control employees are negligible. They cannot be attributed to changes in health insurance either, as health insurance in the Netherlands, unlike in the US, is not tied to the employer. Finally, the lack of an effect can also not be ascribed to the treatment variable itself, since Bach et al. (2021) have used similar variables to show that restructuring in M&As has a negative influence on employees' mental health. We infer that M&As create a more stressful work environment than buyouts, which is consistent with the observation that M&A-induced restructuring (Gehrke et al., 2023) leads to more turnover of the labor force than PE-induced restructuring (Antoni, Maug, and Obernberger, 2019).

In addition, job loss may result in worse health outcomes (see the discussion of this literature in Section 2). Thus, if PE buyouts increase the probability of job loss, we should observe higher levels of stress-related medication for those employees who lose their job. We find that the health of employees who become unemployed indeed deteriorates, whereas the health of those who find new jobs tends to improve. These results are not specific to PE buyouts because they also hold for the control sample. We analyze career paths after the buyouts and predict them based on the careers of control employees. We also stratify the sample based on a range of employee characteristics. We find that the null results are robust and that heterogeneous outcomes across buyout employees depend on whether buyout employees switch jobs or become unemployed.

In the next step, we analyze the impact of the social security system on how employees fare after buyouts. We expect that employees in poor health have more comprehensive access to social security payments by the state and receive larger payments compared to healthy employees. PE firms may then perceive the political and reputational costs of laying off employees to be smaller to the extent that these employees are covered by the state, which may affect their decision which employees to retain. Similarly, higher entitlements to social security payments may create incentives for employees to leave voluntarily. Consistent with our hypothesis, we find that buyout employees are more likely to receive transfers through the social security system, and that this effect is much stronger for employees in poorer health. In particular, employees in poor health are more likely to exit the labor market and receive disability benefits or retirement benefits, which cover about 60% to 80% of the income shortfall after buyouts. Hence, social insurance appears to substitute for firm-level insurance, and more so for employees in poor health.

Finally, we investigate the relationship between employees' pre-buyout health status and their labor-market outcomes further and relate them to employees' productivity. The literature on private equity buyouts shows that buyouts raise productivity, among other things, through operational improvements (e.g., Kaplan and Stromberg, 2009). In addition, prior literature also shows that employees in poor health are less productive (e.g., Currie and Madrian, 1999; Contoyannis and Rice, 2001). Hence, we hypothesize that buyout firms effect operational improvements through identifying, and then laying off less productive workers. (Note that this argument does not assume that buyout firms have access to employees' health records, which are confidential.)

Since productivity and how it is related to health is unobservable, we establish the link between health and productivity by running hedonic wage regressions (Kniesner and Leeth, 2010) on the entire Dutch population, in which we regress daily wages on firm and individual characteristics, including a range of medications. These estimates provide us with measures of how sensitive equilibrium wages are to employees' characteristics, in particular certain types of medications; we interpret the predictions from these regressions as measures of individual productivity. In the second step, these regressions are used to estimate the health-related excess wages of employees in our sample. This measure of excess wages estimates how much medicated employees would earn less compared to healthy employees if their wage would fully reflect their health status, hypothesizing that buyout employees may earn above their equilibrium wage. We then interact the estimated excess wage with the treatment indicator in triple-difference regressions and show that it predicts a significant reduction in employment and earnings. We show that those buyout employees who are prescribed medications that are associated with a negative impact on the wages of the entire Dutch workforce are also significantly more likely to lose employment at the buyout firm. Hence, we conclude that receiving wages above what is indicated by productivity predicts a higher likelihood of job loss in private equity buyouts.

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 contributes to three strands of the literature, which we review in detail in the next section. First, we complement the scarce literature on the consequences of restructuring on employee health by showing that there are no observable differences between the health outcomes of buyout employees and control employees. Second, we add to the literature on risk-sharing within firms by documenting to what extent insurance provided through the social security system affects the labor market outcomes of buyout employees. Finally, we contribute to the literature on buyouts by showing how the selection of employees who leave buyout targets can be related to measures of labor productivity constructed from health indicators.

2 Survey of related literature

Our study is broadly related to three strands of the literature, which we discuss in more detail in this section.

Restructuring and health. There is no prior paper on the health issues associated with private equity buyouts, and the broader literature on the health consequences of restructuring events is generally scarce. The only large-scale study is Bach et al. (2021). They show that M&As have a significant negative impact on employees' mental health, which contrasts with our findings and suggests that buyout-led restructuring is less stressful than M&A-induced restructuring. However, both Gornall et al. (2022) and Lambert et al. (2021) analyze job satisfaction after buyouts and show that employees are generally more dissatisfied after buyouts than after other ownership changes, such as M&As, which suggests that extreme outcomes that lead to inferior health do not provide the same indication as employee satisfaction, as measured by the Glassdoor reviews.

We are aware of only four studies on the health and psychological consequences of mergers and acquisitions, all of which study samples with a small number of firms. The earliest study is Cartwright and Cooper (1993) on a merger of two building-society 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 (N=2,225) of a merger of one Finnish company and find negative effects on subjective health. Relatedly, Currie and Tekin (2015) report similar results for mortgage foreclosures. Three of these studies document a significant negative influence on health outcomes, but none of them analyzes buyouts, all focus only on the impact of organizational change on health, and none of them incorporates the selection effect from 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.[2] The standard way to address the endogeneity between job loss and health is to

[2]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.

use exogenous variation from plant closures. Using this identification strategy, the findings are inconsistent across studies. While the majority of studies find that job loss due to plant closure worsens employees' health, some find no health effects.[3] A related strand of literature focuses on how job insecurity and work stress affect employees' health.[4] 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. Similarly, Karpati and Renneboog (2023) analyze the mental health of the employees of firms that encounter financial frictions in the wake of the great financial crisis. Their study finds an increase of antidepressant consumption for employees of these firms. We contribute 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. In addition, we show that the baseline effects of restructuring after buyouts has no impact on employees' health, which differs from other shocks such as M&As and insolvencies.

Insurance of employment risk. There is a large literature on how firms provide employment insurance to employees and thereby improve risk-sharing (see Pagano, 2020 for a survey). By contrast, the literature on how the social security system affects risk-sharing within firms and financial contracting is more limited. Ellul, Pagano, and Schivardi (2018) provide evidence that insurance through the firm and insurance through the state-level social-transfer system are substitutes in a cross-country setting. They show that wage discounts and employment stability are larger in countries with less generous employment insurance.

[3]Negative health effects: Eliason and Storrie (2009); Kuhn, Lalive, and Zweimüller (2009); Sullivan and von Wachter (2009); Schröder (2013); Bloemen, Hochguertel, and Zweerink (2018); Michaud, Crimmins, and Hurd (2016); Gathmann et al. (2021). No health effects: Browning, Moller Dano, and Heinesen (2006); Salm (2009); Böckerman and Ilmakunnas (2009); Schmitz (2011).

[4]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, Hellgren, and Näswall (2002).

Relatedly, Yehuda et al. (2023) show how the level of unemployment insurance affects the design of debt contracts. We contribute to these findings by showing how the likelihood of job separations after buyouts is affected by state-level insurance, especially for those who suffer from health issues.

The literature on firm-level wage insurance also suggests that employment insurance through firms is limited, not only because of moral hazard (Shleifer and Summers, 1988; Kim, Maug, and Schneider, 2018), but also because firms have only limited resources to provide insurance, so they insure temporary shocks, but not permanent shocks (Guiso, Pistaferri, and Schivardi, 2005). Our analysis adds to this discussion by showing how PE buyouts facilitate a shift of insurance from firms to the social security system for those employees who have suffered losses to their productivity.

Buyouts and employment. Caggese, Cuñat, and Metzger (2019) is the only study that analyzes how employee layoffs during restructuring affect the productivity of the workforce. They show that financially constrained firms tend to lay off workers who require less severance pay, even if this means laying off more productive workers. We complement their analysis by showing how health indicators that affect productivity increase employees' likelihood to being laid off after buyouts. The broader literature on the labor-market consequences of buyouts mostly confines itself to changes in employment without considering wages or earnings.[5] The earlier literature is based on either firm-level data or plant-level data and cannot distinguish the effects on individual workers from effects on the composition of the firm's (or the plant's) labor force. Davis et al. (2014) study aggregate employee flows on plant-level data and establish how these are related to plant-level total factor productivity, but without analyzing the composition of labor flows or individual-level measures of productivity. Researchers have started to analyze individual-level data sets only recently, and only two studies use admin-

[5] A non-exhaustive list of papers on the employment consequences of buyouts is: Kaplan (1989), Lichtenberg and Siegel (1990), Wright, Thompson, and Robbie (1992), Amess and Wright (2007), Boucly, Sraer, and Thesmar (2011), and Davis et al. (2014). The surveys by Kaplan and Stromberg (2009), Wright, Bacon, and Amess (2009), and Eckbo and Thorburn (2013) list additional contributions.

istrative data: Olsson and Tag (2017) from Sweden and Antoni, Maug, and Obernberger (2019) from Germany; in addition, Agrawal and Tambe (2016) analyze individual-level data for the U.S. from a job-search platform. These studies show that employees with skills that are substitutes for investments in information technology (e.g., routine tasks or offshorable tasks) suffer impairments to their human capital after private equity buyouts, whereas those whose skills complement investments in information technology experience increases in wages and employability. Hence, these studies document how employees' human capital is affected by technological change and the obsolescence of skills. The current paper complements these studies by relating the productivity of employees to their own characteristics (here: health) rather than to those of the firm and the technology it employs. Note that even perfectly healthy workers can become less productive if their skills are rendered redundant through technological obsolescence, whereas deteriorating health will have an impact even if technology remains constant

Three recent contributions explore dimensions other than income and employment. Bloom, Sadun, and Van Reenen (2015) show how PE buyouts affect management practices, Cohn, Nestoriak, and Wardlaw (2021) show that they improve workplace safety, and Gupta et al. (2023) show that PE buyouts of nursing homes reduces the quality of patient care. However, none of these studies analyzes how health affects the relationship between buyouts and labor-market outcomes.

There is a related literature on the labor-market consequences of health events, which shows that negative health events (accidents, severe illnesses) are negatively associated with *all* labor market outcomes, from hourly wages and earnings to labor force participation; we do not discuss this literature in detail here (see Currie and Madrian, 1999; Barnay, 2016; and Prinz et al., 2018 for surveys). While our paper contributes to this literature, we ask a somewhat different question by analyzing how a given health condition predisposes labor market outcomes in a restructuring event.

3 Data and methodology

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

3.1 Sample construction

We assemble a list of 366 Private Equity Buyouts in the Netherlands for the period 2007 to 2013 by combining information from Zephyr (156 transactions) and the Dutch Private Equity Association (210 transactions), which contains all transactions of the members of the European Private Equity Association in the Netherlands.[6] For 277 out of 366 PE buyouts, we can find at least one employee in the databases of the Dutch Central Bureau for Statistics (CBS). In order to enter the sample of buyout employees, we require that the individual is between 18 and 62 years old to ensure that the employee is available to the labor force throughout our observation period, and works at least 50% of full-time in the year before the buyout. These requirements leave us with 56,188 employees and 275 buyout firms. Most of these employees (N=50,590) are involved in private-to-private transactions (N=266). Hence, public-to-private transactions form only a small part of our sample.

We match buyout employees to non-treated control employees at the individual level as follows.[7] 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

[6]See Appendix A.1 for details on the selection criteria used to assemble the PE Buyout data set.

[7]Employees from the same buyout firm may be matched to control employees working in different firms. This allows us to construct the best counterfactual at the individual level.

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 for whom the firm size deviates by more than 66% from that 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 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.

3.2 Employment and social security

The CBS provides us with access to 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.[8]

[8]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.

- **Days employed:** The number of days in year t during which employee i was employed.

In addition, we use information on sources of income to identify the main source of income, and estimate the income from different social security programs. First, 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*), disability insurance benefits (*Disability*), 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. Second, we compute the total income from social transfers (*Total transfers*) by subtracting the income from employment and self-employment from total personal income. Next, we attribute all income from transfers to most important source of income, and define the following three variables (i) *Disability benefits* equals *Total transfers* if the main source of income is disability benefits, and zero otherwise; (ii) *Retirement benefits* equals *Total transfers* if the main source of income is retirement benefits, and zero otherwise; (iii) *Unemployment benefits* equals *Total transfers* if the main source of income is unemployment benefits, and zero otherwise.

3.3 Health status

We use registry data from CBS on the consumption of prescribed medication for the entire population for the period from 2006 to 2016 to measure the health status of the employees before and after a buyout.[9] In particular, we observe whether a prescribed drug covered by the Dutch basic health insurance has been dispensed to an individual by a pharmacy at least

[9]Consumption of prescribed drugs is commonly used in epidemiology as a measure of health status (e.g., Issa et al. (2023), who also use Dutch administrative data).

once in a given year. The Dutch basic health insurance provides a comprehensive coverage of drugs. We do not observe drugs provided by hospitals, e.g., drugs used in oncological treatments.[10] We use this information to compute a broad health indicator defined as 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 (see Appendix A.2). These are: (i) *Antidepressant*; (ii) *Cardiovascular*; and (iii) *Digestive*. 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, within one year, and information on total health expenditures (*Health expenditures*). Unfortunately, we only observe information on total health expenditures from 2009 onward. We first focus on *Antidepressant*, *Cardiovascular* and *Total medication*. We use other variables (*Digestive, High medication,* and *Health expenditures*) for robustness, and later extend the analysis to a broader set of health measures by grouping the detailed information on drug prescriptions into 25 categories (see Section 6).

3.4 Matching success and descriptive statistics

Table 2 compares differences between buyout employees and control employees for our measures of human capital and health, and all matching variables. 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. The Imbens-Wooldridge statistic is below the threshold of 0.25 recommended by Imbens and Wooldridge (2009) for all the variables. We conclude that our control groups match buyout employees very closely on all relevant criteria. To test the robustness of our results, we employ two alternative matching approaches. First, we use a one-to-five nearest-neighbor matching instead of the baseline one-to-one matching approach. Second, we use the Mahalanobis distance metric

[10] 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.

instead of the Euclidean distance to identify the best matched control employee. Panels C and D of Table 2 show the matching statistics for these approaches. The matching results for the one-to-five matching are slightly worse, which is unsurprising because now more control employees need to be found. The matching results for the Mahalanobis distance are virtually identical to those for the Euclidean distance, with Firm size being the only exception. Apparently, Mahalanobis distance tends to select employees from slightly smaller firms than the Euclidean distance, leading to a higher, but still acceptable Imbens-Wooldridge statistic of 0.15. We report robustness checks for our main results based on these two alternative matching approaches in the Online Appendix and discuss them briefly when we discuss these results themselves.

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 from employment and €2,172 from other sources (unemployment, disability, or retirement benefits). Hence, the average age in the sample is similar to that of comparable studies, which report 42 years for Germany (Antoni, Maug, and Obernberger, 2019) and 41.1 years for Sweden (Olsson and Tag, 2017), whereas earnings are higher in the Netherlands compared to Germany (€34,251) and Sweden (275,430 SEK, or €29,292). 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). In the Online Appendix, we provide histograms of *Earnings*, *Daily wage*, and *Days employed* (see Figures OA1, OA2, and OA3).

3.5 Methodology

This section describes the baseline difference-in-differences methodology, which we use for our main results (Section 3.5.1) and the triple-differences methodology, which we apply to estimate heterogeneous effects across subgroups based on pre-treatment characteristics (Section 3.5.2).

3.5.1 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 ($k = +4$).[11] 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 the year before the buyout.[12] 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 do not include control variables to characterize employees in our regressions, since these variables would be either time-invariant (e.g., gender, ethnicity) and therefore collinear with the individual fixed effect; or collinear with the event and calendar time dummies (e.g., age); or endogenous, as they can be affected by treatment (e.g., occupation, marital status). 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. The parameters θ_k estimate the causal effects of buyouts under three assumptions (see Sun and Abraham (2020) for

[11]Dutch medication data is only available from 2006 onward. Our sample covers all buyouts between 2007 and 2013. Hence, we do not have sufficient medication data to include a large number of pre-buyout years. To stay consistent throughout all analyses, we therefore limit all regression analyses to the period from $t-2$ to $t+4$. Note that we show parallel trends for five pre-buyout years for our main employment variables in Figure 1.

[12]PE buyouts happen at different dates in calendar time, so the event-year dummies are not collinear with calendar-year effects (see Boucly, Sraer, and Thesmar, 2011)

formal derivations): i) parallel trends in the absence of treatment; ii) no anticipation of the treatment; iii) homogeneous treatment effects. Note that, while buyouts happen to different target firms at different points in time, the setting does not constitute a case of staggered difference-in-differences. In particular, firms are either target firms or control firms, but never switch between these two groups.[13]

Figure 1 plots the outcome variables *Earnings*, *Daily wage* and *Days employed* from $k = -5$ to $k = +4$.[14] They show parallel trends from $k = -5$ to $k = -1$ for all three outcomes.[15] This evidence also suggests there is no anticipation of the buyout, as none of the outcome variables seem to deviate from the trend before the buyout. In addition, the estimates of the coefficient θ_{-2} on $Target_i \times D_{ik}$ provide 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 lower at the end of the period for the treated compared to the control employees. Similarly, treated employees work on average five days less, whereas 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.[16]

[13] We also implement the estimator proposed by Sun and Abraham (2020), which allows for heterogeneous treatment effects across cohorts. The results are shown in Table OA1 and provide suggestive evidence that our estimates are not biased due to heterogeneous treatment effects.

[14] See Antoni, Maug, and Obernberger (2019), Figures 2 - 4 for similar graphs and in a related context. In their case and ours, *Days employed* mechanically peaks that $t = 1$ because matching requires that all employees be employed at that point, but at no other point. See Davis et al. (2014), Figure 3A for a similar employment trajectory. The trends for *Earnings* arise from the combined trends for *Days employed* and for *Daily wage*.

[15] Despite parallel trends before the buyout, trends may not be parallel in the absence of the treatment if treatment was endogenous. However, it is unlikely that buyouts are endogenous to the characteristics of individual employees, except for very small firms. Employees of companies with less than 20 employees in the year before the buyout constitute less than one percent of the buyout employees in our sample.

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

3.5.2 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 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_i^f, the risk factor; note that the risk factor applies to the individual employee and can differ between the target employee and the matching control employee. The coefficients η_k measure by how much the outcome of interest differs between a target employee characterized by risk factor RF_i^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 The effect of buyouts and income and health

4.1 The effect of buyouts on earnings

This section presents the analysis on the relationship between buyouts and employment outcomes. 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 except for person and calendar-year fixed effects in columns 1, 2, and 3, respectively.

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 per year. The median income of target employees in the year before the buyout is €35,225 (see Table 2). Hence, the loss of income equals 3.7% of the median pre-treatment wage, which is slightly higher in our sample than in the sample of German workers studied by Antoni, Maug, and Obernberger (2019), who find a decline of just under €1,000 or 2.8% of the median wage in their sample. Davis et al. (2014) also find moderate declines in income and employment after buyouts in their US sample, whereas the results from earlier studies are more mixed (Lichtenberg and Siegel, 1990; Amess and Wright, 2012); 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, conditional on employment. The findings for wages parallel those of Antoni, Maug, and Obernberger (2019), whereas the decline in employment is significantly higher in their German sample at almost nine days per year. Earlier studies on buyouts either do not analyze wages, or look only at annual earnings per worker, which corresponds to our definition of *Earnings* (see Wright, Bacon, and Amess, 2009, for a survey).

We observe that the ratio of the reduction in *Earnings* divided by the reduction in *Days employed* is about €248 in $k = 4$(€1292/5.22days) and slightly higher in the year before. This ratio is much higher than the mean of €179 of *Daily wage* for the whole sample (see Table 3), which suggests that the decline in *Earnings* falls disproportionately on the higher-paid workers. To investigate this hypothesis, we perform a triple-difference analysis for all three dependent variables in columns 4 to 6 of Table 4, where we interact all effects with the dummy variable *High wage*, which equals one for an employee whose wage in $k = -1$ was above the median of his or her firm, and zero otherwise. The coefficients θ_k on the double interactions $Target_i \times D_{ik}$ reflect the changes for the low-wage employees, who experience a decline of about four to five days of *Days employed* and a slight and barely significant increase in *Daily wage*, which results in an insignificant decline in *Earnings*. While high-wage employees experience almost exactly the same decline in *Days employed* as low-wage employees, they also suffer a significant reduction in *Daily wage* of about €6.28 to €7.71 in the second to fourth year after the buyout, which compounds the decline in *Days employed* and results in much larger losses of *Earnings* of about €1,992 to €2,395 for the same period. Hence, high-wage employees experience larger percentage declines in *Earnings*.[17]

4.2 The effect of buyouts and prior medication

This section shows how the losses of employment and income shown in the previous section depend on employees' pre-buyout medication status. We hypothesize that employees in poorer health experience larger reductions in income and employment than healthy employees. There is significant evidence that adverse health events, like severe illnesses, strokes, or accidents, have a negative effect on employees' incomes.[18] While buyout firms cannot observe medical health records, which are confidential, they can observe employees' sick leave and productivity, and we hypothesize that productivity and health are negatively correlated.[19] Similarly, if the new owners of the target firm restructure the workforce, they will likely lay off the those workers whose losses of income and employment are better protected by the

[17] We investigate this aspect further in Figure OA4 in the Online Appendix, where we perform distributional regression analyses (Foresi and Peracchi, 1995; Jones, Lomas, and Rice, 2015; Chernozhukov, Fernández-Val, and Melly, 2013) by estimating linear probability models in which the dependent variable is an indicator variable $I_{ik}\,(Earnings_{ik} > e)$, which equals one if the earnings of employee i in event year k are higher than the threshold value e, and zero otherwise. We run regression (1) with this dependent variable and Figure OA4 plots the estimates of the coefficient θ_3^e against the threshold e, separately for high-wage employees and low-wage employees.

[18] See Section 2 and Currie and Madrian (1999), Contoyannis and Rice (2001), Flores, Fernández, and Pena-Boquete (2019), and Jäckle and Himmler (2010) on the impact of health on wages.

[19] Sick 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 sick leave.

social security system, which is likely correlated with their health. Hence, we expect higher losses of income and employment for workers in poorer health and provide a direct test of the relationship between health and labor-market outcomes in this section.

To test how employees' health status influences their post-buyout labor market outcomes, Table 5 performs a triple-differences analysis according to equation (2). To define risk factors RF_i^f in this equation, we use dummy variables for our three main health indicators, *Antidepressant*, *Cardiovascular*, and *Total medication*. Each health risk factor is measured in the year before the buyout $k = -1$. Hence, we ask whether the health status of employees 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 antidepressants 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 and reported in Table OA2 in the Online Appendix.[20]

We find strong support for the hypothesis that employees who were in poor health before 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 ($= \theta_4 + \eta_4 = -1,018 - 2,489$), or 10.0% of median pre-buyout earnings of €35,225. The corresponding effect for antidepressants is slightly smaller, with a combined impact of €3,235 (=1,223+2,012), or 9.2% of median pre-buyout earnings. For

[20]Before interpreting the results, we inspect whether the pre-trends are also parallel for the subgroups of employees who take antidepressants in Figure OA5, and for those who take cardiovascular medication in Figure OA6. As above, the estimate of the coefficient θ_{-2} provides a formal test of differences before the PE buyout. From both figures and the tests we can conclude that the pre-trends are remarkably parallel.

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 in addition to the €991 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 OA3 in the Online Appendix repeats the analysis of Table 5 for three additional health measures (digestive medication, high medication intake, health expenditures). The results are similar to those for our three main measures of employee health if we use health expenditures as a broader measure of employees' health or a dummy variable for high medication intake: The additional loss of earnings amounts to €2,489 for those with high medication intake and €1,652 for those with health expenditures above the median. However, 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. Hence, the results for five out of six health indicators we use here support the hypothesis that buyout employees in poorer health fare significantly worse in terms of earnings and employment after buyouts.[21]

4.3 The effect of buyouts on health

In this section we ask how buyouts affect employees' health. Section 4.1 shows that buyouts lead to reduced employment and income for the employees of buyout firms, and the literature shows that job loss and job insecurity negatively impact employees' health (see Section 2). Hence, we expect such a negative impact on employees' health after buyouts, particularly for those employees who become unemployed or exit the labor market. Moreover, we hypothesize

[21] Table OA4 repeats the analyses of Table 5 with a one-to-five nearest neighbor matching in columns 1 to 6; Table OA5 repeats the same analyses for a Mahalanobis matching in columns 1 to 6. The results are robust and estimates are in most cases marginally larger with the alternative matching approaches.

that buyouts create a more demanding and stressful work environment for those employees who continue to work for the buyout firm, as a side effect from streamlining operations and strengthening incentives. Since job-related stress has been associated with adverse health consequences, we expect that buyouts have a negative influence on the health of those employees who stay with the buyout firm. In addition, prior literature suggests that employees who become unemployed experience negative health consequences. Hence, we also expect a deterioration of health consequences for this group (See Section 2).

We begin by performing difference-in-differences analyses based on equation (1) with the health status measures *Antidepressants*, *Cardiovascular* and *Total medication* as dependent variables. In Panel C and Panel D of Figure 2, we plot the results for the θ_k-coefficients on the interactions of event-time dummies and the target indicator. The coefficients are all very close to zero with changing signs; no coefficient is statistically significant. Furthermore, in Panels A and B of Figure 2, we plot the average medication intake of buyout employees and control employees separately. The plots for both groups are indistinguishable before and after the buyout year. Hence, buyouts have no measurable impact on health outcomes.[22] We also test if these effects are zero for the group of employees in poorer health. We investigate this group in Table OA6 in the Online Appendix and confirm that the health status of of employees in poorer health remains unaffected as well. Hence, post-buyout restructuring seems to have a more benign impact on employees compared to restructuring after M&As (Bach et al., 2021) or organizational changes associated with financial frictions (Karpati and Renneboog, 2023), which is consistent with earlier observations that buyouts create new employment opportunities (e.g., Boucly, Sraer, and Thesmar, 2011) and a safer work environment (Cohn, Nestoriak, and Wardlaw, 2021).

The analysis in Figure 2 pools all buyout employees, including those who leave the firm

[22] Table OA7 in the Online Appendix shows the estimates for the health measures presented in Figure 2 and other health outcome measures (*Digestive*, *High medication* and *Health expenditures,* defined in Appendix A). The results confirm that buyouts do not worsen employee's health outcomes. Table OA8 repeats the analyses of Table OA7 and Figure 2 with a one-to-five nearest neighbor matching; Table OA9 repeats the same analyses for a Mahalanobis matching. The results are robust.

after the buyout. However, the hypothesis that buyouts have negative health consequences for buyout employees depends on two channels: the effect of a loss of employment and income applies only to employees who leave the firm, whereas the stress-related negative effects of a more demanding work environment relate only to those employees who stay with the buyout firm. Therefore, we distinguish between employees who leave the buyout firm for other firms from those who become unemployed and from those who continue to be employed by the buyout firm. Such an analysis is also useful because the null results in Figure 2 may result if there are heterogeneous treatment effects and positive health effects in one group cancel the negative effects on another group.

We note, however, that employees' career paths after buyouts depend on choices, of employees as well as firms, and are therefore endogenous. Moreover, there is no instrument that predicts these career choices and satisfies the exclusion restriction, which requires that it is uncorrelated with health outcomes. Hence, we follow three strategies to buttress our result. To begin, we interact event dummies, the target indicator, and dummies for career outcomes. Search frictions in the labor market likely prevent some employees from changing to other jobs, for example, to avoid anticipated stress, and such an analysis should be informative and also capture some exogenous variation (Section 4.3.1). Second, we repeat the same analysis, but predicting the career outcomes of target employees by using their matching controls (Section 4.3.2). Third, and finally, we use a range of variables that are arguably related to the likelihood of staying with the buyout target and interact indicators for these characteristics with the event dummies and the treatment dummy. To the extent that these characteristics predict future career paths, we should see that these indicators pick up the heterogeneity of treatment effects (Section 4.3.3).

4.3.1 Analysis of ex-post career paths

We begin by performing a triple-difference analysis by regressing six health outcomes (*Antidepressant*, *Cardiovascular, Total medication, Digestive, High medication, Health expenditures*) on interactions of the event dummies, the target indicator, and indicators of employees' post-buyout career paths. For this purpose, we define two career outcomes, and use them as risk factors in equation (2): *Job change* indicates whether an employee has changed jobs at k compared to $k = -1$; and *Not employed* indicates whether the employee is not working in period k (see Table 1 for more details about these variables). We can infer the results for those employees who stay with the target firm from the double interactions of the event-time dummies with the target indicator. Moreover, the double interactions of event-time dummies with the career path indicators 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 refrain from making any causal claims about the effects of career paths on health from this analysis, because employees and employers make choices that affect career outcomes and we do not have exogenous variation in the career paths followed by employees after the buyout.

In Panel A of Table 6, we report the long-term effects of career path changes between $k = -1$ and $k = +4$. (For the complete table, see Table OA10.) The coefficients on the double interactions of the event-time dummies with the target indicator are close to zero and insignificant for all health outcomes except for *Digestive* and *Health expenditures*, for which they are significant and negative. These findings indicate that digestive medication and health expenditures are lower for those target employees who stay with their firms after the buyout. Hence, while the analysis does not support causal claims, it provides no evidence to support the notion that buyouts create a more adverse work environment, which would imply increased medication intake and health expenditures for those who stay with the firm after the buyout.

One of the notable findings from Table 6A is that job changes are generally associated with *better* employee health employees: The coefficients on the double interactions of event-time dummies with *Job change* are negative and highly significant for all health outcomes except for antidepressants (column 1), for which they are small and insignificant. Hence, job 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) for control employees; the triple interactions show that signs of the differences between buyout employees and control employees are ambiguous and generally small (less than one percentage point).[23] Similarly, the double interactions of the event-time dummies with *Not employed* are mostly highly significant and positive: *Not employed* is associated with a deterioration of health for control employees, and the estimates have about the same magnitude as the ones for *Job change*, but with the opposite sign. Again, the differences between buyout employees and control employees are ambiguous and no clear pattern emerges.

Hence, Table 6A suggests that employees' health and their medication intake differs across career paths: Those employees who change to new jobs see their health improve, whereas those who become unemployed see their health deteriorate. Buyout employees do not differ systematically from control employees in this regard, with the exception of the noted effect of losing employment on digestive medication.

4.3.2 Predicting career outcomes using control employees

The analysis in Panel A of Table 6 is more descriptive in nature, because it combines selection effects (firms and employees choose whether to stay or not with the buyout firm) with treatment effects. In this section we partially address this issue by using the control employees to predict the career outcomes of buyout employees. In particular, if a control employee changes to another job, is not employed, or continues to work for the control firm at a certain point in time, we simply assign the same status to the buyout employee to whom the control employee was matched. Hence, this analysis substitutes the endogenous *actual* decisions employees and firms take with decisions *predicted* on the basis of those observable

[23]The only exception is the triple interaction $D_{i4} \times Target \times Job\,change$, which is 1.81pp. Hence the generally strong reduction of total medication for job changers in the control group (-8.70pp from the double interaction) is reduced by about one-fifth for job changers in buyout firms.

and unobservable employee characteristics that are controlled for through the matching process. We report the results in Panel B of Table 6, which has the exact same structure as Panel A. However, now *Job change* and *Not employed* do not reflect the actual but predicted career outcomes.

The results in Panel B of Table 6 are by and large similar to those in Panel A and the point estimates and significance levels paint a broadly similar picture. Hence, endogeneity concerns do not seem to confound the results in Panel A of Table 6. Two deviations stand out though. The predicted stayers have significantly lower *Total medication* (coefficient of -1.64 pp in column 3), whereas the actual stayers do not. Moreover, the predicted job changers (stayers) have significantly higher (lower) *Health expenditures* compared to their actual counterparts, which suggests that some predicted stayers became actual job changers, as the matching variables do not predict their departures from the buyout firm correctly. Overall, this analysis corroborates the earlier finding that those employees who stay, and are expected to stay, with the buyout firm show no signs of increased medication intake. If anything, buyouts appear to improve the health of those who stay with the firm, maybe because they improve operations in a way that improves not only workplace safety but also reduces other stress factors (see Cohn, Nestoriak, and Wardlaw, 2021).

4.3.3 Ex-ante heterogeneity of employees

Finally, we ask whether variables that are arguably correlated with the decision to retain employees predict post-buyout health outcomes. Moreover, we want to investigate whether the null result documented above obtains because the losses to some groups of employees are canceled by the gains to other groups, where groups are defined based on *ex-ante* known employee characteristics. Hence, we identify six variables that may be linked to career outcomes: (1) *Age* of employees, since we expect firms to retain younger employees more than older employees; (2) *Tenure*, which is often associated with employees' firm-specific human capital; (3) employees' pre-buyout *Daily wage*, since we observe higher losses for high-earning employees in Table 4; (4) pre-buyout wage growth; (5) firm size; and (6) gender (dummy *Female*). For each continuous variable, we construct an indicator variable, which equals one if the underlying continuous variable is above the respective sample median in the pre-buyout year.

We perform triple-difference regressions analyses based on equation (2) for the same outcome variables as in Figure 2 and the η_k-coefficients on the triple interactions of event-time dummies, the target indicator, and each of the six employee characteristics from two years before to four years after the event year in Figure 3. We plot the results for the estimates for *Antidepressants* in Panel A, those for *Cardiovascular* in Panel B, and those for *Total medication* in Panel C; the numerical estimates are shown in Table OA11 in the Online Appendix. The results are similar to those in Figure 2. There is no systematic variation of health outcomes across employees along any of the dimensions identified by the six employee characteristics. For no dimension do we observe a canceling of positive results for one group with negative results for the complement group. Note that we have seven estimates that are significant at the 5% level and another three that are significant at the 10% level, but they do not show any clear pattern. Given that we run 18 regressions and estimate six coefficient for each (108 coefficient estimates), this number of significant estimates we find is what we would expect from random variation alone. As a robustness check, we perform a similar analysis to that in Figure 3 by using interactions with indicators for pre-buyout wage quartiles and report the results in Table OA12 in the Online Appendix. The results are similar and omitted here for brevity.

Summary. We conclude that the zero effects shown in Figure 2 are robust in that they cannot be related to *ex-ante* known employee characteristics. However, they are related to ex-post career outcomes. We conclude that there is no evidence for effects of buyouts on employees' health beyond the influence buyouts have on their careers.

5 The effect of social transfers

In this section we investigate the relationship between health, career outcomes, and social transfers. We argue that buyout firms as well as employees consider social transfers in their decisions. From the point of view of buyout firms, laying off and replacing employees involves a trade-off: Buyout firms benefit from improving the matching of employees to tasks, which may deteriorate as buyout firms change their organization and technology; and from replacing existing employees with more productive new hires. At the same time, laying off employees involves financial costs (e.g., severance pay) as well as political costs that involve public resistance as well as internal frictions. We hypothesize that laying off those employees who have better access to social security income involves comparatively lower costs for buyout firms, since such layoffs are less costly for the employees and thus associated with less internal and external resistance. A similar argument holds for the buyout employees themselves, who are arguably more likely to accept departures from the firm if this is less costly for them, for example because of more generous social transfers or to avoid being tainted as having been "fired." We cannot observe whether separations from buyout firms are voluntary resignations or forced layoffs, and also believe that such distinction would be somewhat blurred by the fact that separations may be the result of bargaining between firms and employees.

The social security system is designed to (partially) insure employees against impairments of their human capital. Accordingly, we hypothesize that employees in poorer health find it easier to replace larger parts of their income through social security transfers, and that firms should find it easier to come to agreements with exactly these employees to leave the firm. Moreover, we also hypothesize that workers in poorer health are less productive, which creates additional incentives for firms to replace these employees, a topic we take up again below. Hence, in this section, we test two linked hypotheses: (1) buyout employees in poorer health are more likely to leave the firm and exit the labor market by relying on social security transfers as their main source of income (Section 5.1); and (2) buyout employees in poorer health receive more social security income, and thus can replace a larger fraction of their labor income, compared to healthier employees (5.2).

5.1 Labor market exit and pre-buyout medication status

In this section, we analyze the first hypothesis formulated above and analyze how buyouts and health status affect employees' career paths. We begin by classifying individuals' pathways after the buyout depending on their main source of income. This classification 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. In particular, we define indicator variables with a value of one if an employee's main income comes from employment with the buyout firm (*Stayer*), from employment with another firm or self-employment (*Job changer*), or from sources not related to employment. With regards to the latter, our data allow us to distinguish unemployment insurance benefits (*Unemployment*), disability insurance benefits (*Disability*), or (early-)retirement benefits (*Retirement*). Note that this classification refines the one we used in Section 4.3.1 by disaggregating *Not employed* into subcategories based on the specific reason for which an employee receives social security transfers. All these variables take a value of zero when the individual is not employed and without income or when the individual's main source of income is social assistance benefits.

We begin by estimating linear probability models with the indicators just described as dependent variables, using the same matched-sample difference-in-difference identification strategy as in equation (1). Table 7, Panel A, presents the results, such that each column corresponds to a different labor market outcome. We report only the coefficients θ_k for the double interactions $Target_i \times D_{ik}$ from equation (1), which denote the differences-in-differences between buyout employees and their matched control employees relative to $t = -1$. There is a statistically significant decline in the probability of staying with the buyout firm (column 1), which peaks two years after the buyout. The decline is less pronounced four years after the buyout (7.1 percentage points after two years vs. 4.7 percentage points after four years), suggesting that around half of the short-run effect is driven by buyout employees that would have left the firm within three and four years anyway. Most of those buyout employees who leave move to other jobs (column 2). However, employees are also more likely to have left employment by around 1.2 percentage points by the end of the second year after the buyout (column 3). The majority of these employees receives unemployment insurance (column 6).

Next, we investigate to what extent employees' labor market paths depend on their pre-buyout health status. Hence, we perform a triple-difference analysis of labor market paths by adding interactions with the three health outcome variables used in Table 5. We report the results in Table 7, which has three 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 equation (2)). We do not report the double interactions, which are similar to the corresponding interactions in Panel A.

Overall, we find significant results for all three health measures. The impact of health on how buyouts affect labor market paths is economically large relative to the baseline effect documented in Panel A. Panel B shows that a one-standard deviation increase in *Antidepressant* ($= 0.203$; see Table 3) increases the probability of not working (i.e., not being either employed or self-employed) by 0.9 percentage points (pp) in $k = 4$ (column 1, $= 0.203 \times -0.00443$). The impact of a one-standard deviation increase in *Cardiovascular* (Panel B) is 1.2 pp and that for *Total medication* (Panel C) is 1.3 pp. These effects are large relative to the average incidence of unemployment (1.99%), disability care (0.93%) and retirement (1.33%; see Table 3). Table 7 also shows that buyout employees on anti-depressants (Panel B) and cardiovascular medication (Panel C) are more likely to receive disability benefits (*Antidepressant*: +2.74, *Cardiovascular*: +1.28 percentage points), unemployment benefits (*Cardiovascular*: +1.35 percentage points), or retire (*Antidepressant*: +1.71 percentage points). These findings are 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.

Overall, we find support for the hypothesis that the health indicators that were shown to lead to large losses in earnings and employment in the previous section also increase the likelihood of exiting the labor market.

5.2 Replacement through social security transfers

In this section, we test the second hypothesis formulated above that buyout employees in poorer health receive more social security income, and thus can replace a larger fraction of their labor income, compared to healthier employees. Hence, we estimate the extent to which losses of earnings after buyouts are replaced through social security transfers. To begin, we estimate the variable *Total transfers*, which comprises all income from social security and state-level insurance and then define three variables by identifying *Total transfers* with the main source of social security: *Disability benefits* equals *Total transfers* if the main source of income is disability insurance benefits, and zero otherwise. Likewise, *Retirement benefits* (*Unemployment benefits*) equals *Total transfers* if the main source of income is retirement (unemployment) benefits. (See Section 3.2 for details.)

Table 8 performs difference-in-differences analyses similar to those in Table 4, but with these social security transfers as dependent variables. It shows that the loss in *Earnings* documented above is mitigated by higher transfers from the state. In event year $k = +4$, buyout employees receive €654 of transfers more than control employees, which corresponds to about half of the loss of €1,300 suffered by buyout employees overall, as reported in Table 4 (see Section 4.1). Most of the transfer income comes from unemployment benefits (+€312), and this increase is statistically also highly significant, whereas the increases in disability benefits (+€78) and retirement benefits (+215 Euros) are both insignificant. Note that there is a small gap of about €49 (654-(312+78+215) = 49) that can be attributed to other social transfers, like social assistance.

Based on our hypothesis, we expect a high replacement rate of social security transfers relative to pre-buyout earnings for those employees who exit the labor market. Table 9 performs triple-difference analyses similar to those in Table 5 with the three main health variables as risk factors. The table 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, compared to healthy employees. 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 71% (=€2,285/€3,235) for employees on antidepressants and 57% (=€1,987/€3,507) for employees on cardiovascular medication.[24] Overall, we find support for the hypothesis that the increase in social-security income is much larger for employees in poorer health and covers between 57% and 71% of the losses of labor-market income documented in Section 4.1.[25]

6 Productivity and labor-market outcomes

In this section, we further investigate the link between pre-buyout health and labor market outcomes. The analysis in Section 4.2 provides evidence for a link between health and labor-market outcomes. We investigate two channels for this connection, one through the social security system, which we analyze in Section 5, and one through the impact of health on employees' productivity, which we explore in this section.

We hypothesize that at least some target employees are paid above their equilibrium wage, such that their wages do not fully capture the reduced productivity from their medication intake. This may happen for a number of reasons. In particular, private equity firms may address agency issues that lead managers to overpay workers (Cronqvist et al., 2009), buyouts may improve human resource management (Bloom, Sadun, and Van Reenen, 2015), or they may break implicit dynamic wage contracts, which tend to pay wages above employees' pro-

[24]We add up θ_4 and η_4 in Tables 9 and 5 to compute the total effect earnings, respectively, on social transfers. The replacement ratio is the ratio of these two measures.

[25]Table OA13 repeats the analyses of Table 9 with a one-to-five nearest neighbor matching; Table OA14 repeats the same analyses for a Mahalanobis matching. The results are robust.

ductivity in the later stages of their employment relationship (Harris and Holmstrom, 1982; Thomas and Worrall, 1988).[26] Either of these mechanisms would lead to differences between employees' wages and their productivity. Buyout firms may improve firm-level productivity either by moving employees' wages closer to their productivity, or by laying off employees who are paid more than their productivity.

Measuring productivity based on health measures. To investigate this hypothesis, we need to establish how the health indicators we have access to are related to employees' productivity. It is *ex-ante* unclear whether the intake of certain types of medication increases, reduces, or does not affect productivity. To resolve this empirical question, we employ hedonic wage regressions, which have been widely used in the literature to establish the relationship between wages, job characteristics, and employee characteristics (see Kniesner and Leeth (2010) and the literature they cite); we add medication intake as a time-varying employee characteristic to the standard regression setup.[27] In line with the hedonic 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 or employees. We assume that firms can observe their employees' productivity, and that health is an important component of productivity. Health is not observable to employers, but to employees and to researchers through the data we analyze here. Hence, our hypothesis predicts a systematic relationship between the impact of medication intake on equilibrium wages in the general workforce.

We summarize our approach here and refer the interested reader to Appendix C for further details. To begin, we perform the following regression of wages on employee and firm characteristics on the entire population of Dutch employees on which we have administrative

[26]Note that the agency issues identified by Cronqvist et al. (2009) may be the reason why buyout firms can improve human resource management and strengthen "people management practices" in the way described by Bloom, Sadun, and Van Reenen (2015).

[27]We are grateful to Jean-Noel Barrot for suggesting this methodology. 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.

data for our sample period:

$$w_{i,t} = \alpha_t + \sum_h H_{i,h,t}\, \gamma_{h,t} + X_{i,t}\zeta_t + u_{i,t}, \tag{3}$$

where $w_{i,t}$ denotes the wages of employee i in year t and the index h refers to medications; we run these cross-sectional regressions separately for each calendar year from 2006 to 2012, since the large sample size and the computational power of our data provider prevent us from estimating panel regression models. To run regression (3), we create a broader set of health indicators and group all medications we have data on into 25 different groups. Table 10 shows how we map the different drugs into 25 groups of medications (column 1), the prevalence of consumption for each type of medication in the pre-buyout year in, respectively, the Dutch workforce (column 2), among buyout employees (column 3), and among control employees (column 4), weighted by the number of buyout employees in a given year. The distributions of medication intake are identical for buyout employees and for control employees. The medication variables $H_{i,h,t}$ are dummy variables, which equal 1 if the employee was prescribed medication of type h in year t, and zero otherwise, and the type h indexes the 25 groups defined in able 10. For example, characteristic h may refer to cardiovascular medication; if employee i takes such medication in year t, then $H_{i,h,t} = 1$.

The vector $X_{i,t}$ in equation (3) describes employee i in year t with a range of occupational and employee characteristics, including non-linear controls for employees' age and tenure. Our annual regressions include approximately ten million employees on average and explain about 60% of the variation in our dependent variable, *Daily wage*. Column 5 of Table 10 shows the time-series averages $\frac{1}{T}\sum_t \widehat{\gamma}_{h,t}$ of the estimates for the medication coefficients γ_h on $H_{i,h}$ from the cross-sectional regressions (3); the t-statistics reported below the average coefficient estimates are based on standard errors computed as the standard deviation of the seven coefficient estimates. We obtain significantly negative coefficients for 12 types of medication, with the strongest impact for antidepressants (-€8.83), drugs to treat bone diseases (-€5.32), and diabetes (-€6.41). We also find significantly positive coefficients for 11 types of medication, which is potentially puzzling. We suspect that some medications reveal behavioral patterns of employees that may be positively related to productivity, e.g., the decision to take antibiotics (included in “Antibacterials for systemic use”, ATC J01 in Table 10) to reduce the number of days lost to sick leave. To investigate this further, we ask how the 25 different classes of medications predict two health-related outcomes: mortality and the likelihood of receiving disability insurance. For both of these health comes, we run cross-sectional regressions for the Dutch population against medication intake. These analyses are based on the idea that drugs that are related to behaviors that increase productivity should also predict lower mortality and a lower incidence of disability. We generally find this to be the case and the results are shown in columns 6 and 7 of Table 10. While the correlations are not perfect, there is a clear pattern: Those drugs that predict higher productivity in column 5 are mostly those that also predict lower mortality and incidence of disability in columns 6 and 7.

Estimating Excess wage. To measure the predicted productivity impact of medication intake, we construct a new variable *Excess wage*, which is defined as the negative impact of medication intake on employees' equilibrium wage, and is calculated as follows (see Appendix C for details):

1. Use the estimates from regression (3) on the entire Dutch workforce in year t to predict the equilibrium wage $\widehat{w}_{j,t}$ that buyout employee j would have earned in year t. This step assumes that equation (3) reflects the economy-wide equilibrium wage.

2. Calculate the counterfactual wage $\widehat{w}_{j,t}^{\text{counterfactual}}$ that the same employee j would have earned in year t if s/he had been in perfect health. This estimate is obtained by using the same calculations as in the previous step, but setting the $\gamma_{h,t}$-coefficients to zero for all the 15 medications that have a negative impact on health.

3. Define *Excess wage*$_{j,t}$=$\widehat{w}_{j,t}^{\text{counterfactual}}-\widehat{w}_{j,t}$. Hence, *Excess wage* estimates the predicted wage loss associated with medication consumption, based on estimates for the entire

Dutch population.

Analysis. Next, we use *Excess wage* as a risk factor in regression (2) and perform triple-difference analyses based on equation (2) with *Earnings*, *Daily wage*, and *Days employed* as dependent variables. The null hypothesis is that wages in buyout targets are at their equilibrium level. Then the coefficients on the triple interactions in regression (2) should be zero. Alternatively, there may be systematic effects if buyout firms address agency issues, improve human resource management, or violate implicit contracts (see the discussion at the beginning of this section for details). In these cases, *Excess wage* should have a negative impact on employment or wages. Figure 4 shows the coefficient estimates for the triple interactions for $k = 4$ for the whole sample (Panels A to C) and broken down by age (Panels D to F; above median age: squares; below median age: rhombuses). Table OA15 reports the numerical results for all event dates from $k = -2$ to $k = +4$. For the whole sample, we observe declines in *Earnings* and in *Days employed*, and they are statistically highly significant. To interpret magnitude of the results, note that the standard deviation of *Excess wage* is 4.90 and the $k = 4$ coefficient for the impact of *Excess wage* on *Earnings* is −€129.1.Hence, a one-standard deviation increase in *Excess wage* results in an additional income loss of €632 (4.90 × €129) for the average buyout employee. While there is a measurable impact on employment and income, there is no impact on *Daily wage*.[28]

To further investigate the validity of our conclusions, we repeat the triple-difference analysis from Table 5 separately for each medication. We provide a summary of the results in Figure 5 and in column 7 of Table 10, which reports the coefficient η_{h4} on the interaction $Target_j \times H_{j,h} \times D_{j4}$ in regression (2), where $H_{j,h}$ equals one if employee j 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 on the Dutch population, and the coefficients η_{h4}, which measure long-term em-

[28]Table OA4 repeats the analyses of columns 1 and 3 of Table OA15 with a one-to-five nearest neighbor matching in columns 7 and 8; Table OA5 repeats the same analyses for a Mahalanobis matching in columns 7 and 8. The results are robust.

ployment effects for buyout employees. The correlation is visualized in Figure 5, which plots the estimates of 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 0.98 and a coefficient of correlation of 0.65. Hence, on average, across all medications, a one-euro medication-related reduction (increase) in *Daily wage* across the Dutch workforce translates into an 0.98-day reduction (increase) in the long-term *Days employed* of buyout employees.

Age as a confounding factor and moderator. It is well-known that medication consumption increases with age, as age is correlated with comorbidities and mortality (see Werblow, Felder, and Zweifel, 2007; Wong et al., 2012; Costa-Font and Vilaplana-Prieto, 2020). This creates a potential concern for our analysis if employees' age is correlated with unobserved characteristics of employees that are not related to their productivity or their health, but are still related to the factors that influence their separation from the firm. We address this issue by including linear and quadratic terms of *Age* and *Tenure* in equation (3). When we calculate the counterfactual wage, $\widehat{w}_{j,t}^{\text{counterfactual}}$ in step 2 above, we set only medication-related variables to zero, which should remove any direct influence of age-related factors from our estimate of *Excess wage*. The only remaining concern is that age plays a different role in wage setting and employment decisions for buyout employees compared to other employees in the Dutch labor force. Our methodology does not address this issue and we have to assume that such buyout-specific factors are not of first-order importance.[29]

Age may also be a moderating variable, since the influence of *Excess wage* on employment and income may depend on employees' age. Specifically, we hypothesize that the influence of medication on productivity increases with age, for example, because older employees generally find it harder to find new employment (see also Antoni, Maug, and Obernberger, 2019). In

[29] Note that controlling for age in Table OA15 by adding triple interactions with age-related variables is not a solution here. The correlation between medication intake and health implies also that controls for age may pick up health-related factors that are not covered by our variables, such as hospitalizations, or medications given only in hospitals, such as drugs to treat cancer. Then age would become a "bad control" in the sense of Angrist and Pischke (2009).

columns 4 to 9 of Table OA15, we report the results for the same triple interactions as in columns 1 to 3 for a sample split at the median employee age. The effects on employment tend to be concentrated in older employees (compare columns 6 and 9), whereas younger employees experience a small reduction in *Daily wage*, but not in *Days employed*. Hence, while younger employees always find new employment, but sometimes at a lower wage, the opposite holds for older employees. However, both groups suffer impairments to their human capital.

Overall, these analyses are consistent with our hypothesis 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 result 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.

7 Discussion and conclusion

We study the relationship between private equity buyouts and employees' health and earnings in the Netherlands along three dimensions. First, we examine whether buyouts create a more stressful work environment for employees. Surprisingly, employees' health does not deteriorate after buyouts even though private equity buyouts have a negative impact on the earnings of employees leaving the target. This finding sets buyouts apart from other forms of corporate restructuring, for which adverse health effects have been documented. However, the adverse effects of buyouts on earnings are several times larger for employees who are in poor health prior to the buyout, compared to the effects on healthy employees.

Second, we explore how employees' career paths after buyouts is affected by the social security system and report two findings: Buyout employees in poorer health are much more likely to exit the labor market and receive their main source of income from either retirement benefits, disability insurance, or unemployment benefits. Effectively, about half of the impact of buyouts on employees' earnings is buffered by social transfers. This protection is larger for previously unhealthy workers - up to 70% of pre-buyout income, which supports our hypothesis that the social security system affects employees' restructuring outcomes in PE buyouts.

Third, we investigate how health is related to employees' productivity. We measure the relationship between productivity and health by predicting the equilibrium wages of buyout employees conditional on their health status using hedonic wage regressions, which we estimate based on the entire Dutch workforce. We find that target employees who earn higher wages in excess of the equilibrium benchmark have higher risks of becoming unemployed. This is in line with the operational-improvement hypothesis and suggests that buyout firms are more likely to lay off less productive employees.

The findings of our paper pose interesting questions to future research. One avenue would be to investigate the external validity of our findings, in particular in contexts with different social security arrangements than those in the Netherlands. A second and more complex question is about the welfare implications of our analysis: Is it beneficial if the state instead of the firm provides insurance? Or would this lead to excess layoffs, since firms can externalize some of the costs? We leave these and other questions for future research.

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A Appendix: Data description

A.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 for which the 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 at the end of the year of the buyout. For 277 PE buyouts, we can find at least one employee in the CBS database and identify 73,323 employees in total. In order to enter the sample of buyout employees, we require that the individual is between 18 and 62 years old to ensure that the employee is available to the labor force throughout our observation period, 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.

A.2 Definition health variables

We measure the health status of our population using registry data from CBS on the consumption of prescribed medication. 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 in a given year (see Norwegian Institute of Public Health, 2017, for an explanation of the ATC4 classification system). We define medications at the ATC4 classification level and use this information to compute a broad health indicator defined as the number of different types of medications consumed in a given year (*Total medication*).

We classify different types of medications and relate them to groups of health conditions. We do this in two steps. First, 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, with their respective ATC4 classifications in parentheses: (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). Second, we extend the analysis to a larger set of health measures, by grouping the detailed information on drug prescriptions at the ATC4 level into 25 categories. The first two columns of Table 10 define these additional health variables and provide information on the corresponding ATC4 codes. Last, we also observe annual total public health expenditures at the individual level from 2009 onward. This includes information on all types of health care financed by the compulsory health insurance. As discussed in section B.4, health insurance is comprehensive in the Netherlands, and the share of private expenditures is low. For example, government and social insurance spending as a share of total spending was 88% of total health expenditures in 2013 (OECD, 2015). We only use this information for robustness analysis given the limited time span.

B 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.

B.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. The Netherlands is one of the OECD countries with higher protection for individual dismissals, but not for collective ones.

B.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 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).

B.3 Disability insurance

Individuals are entitled to (partial) disability insurance (DI) if their earnings capacity is reduced by at least 35%.[30] 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

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

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, Daly, and De Jong (2008)). 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 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.

B.4 Health insurance

The Netherlands has a universal social health insurance with comprehensive coverage. A comprehensive basic insurance package at a maximum regulated price, obligatory to all citizens, is offered by all health insurers. Individuals are then free to choose their preferred health insurer, and the government provides subsidies to low incomes to cover the health insurance premium. Therefore, unlike the US, health insurance is not tied to the employer. 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, 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, 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)).

C Appendix: Measuring productivity

This appendix explains further details on measuring productivity in Section 6 and on constructing *Excess wage* that were omitted in the main text. We run regression (3) on the Dutch workforce, where the variables in $X_{i,t}$ include *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, which identify small cities and districts of large cities; and indicators for ethnicity.

We depart from the literature on hedonic wage regressions cited above (footnote 27) in some aspects. 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. Moreover, we do not control for job-related hazards and education. We do not have access to measures of job-related hazards, and they are relevant in the literature that estimates the value of a statistical life, but are not relevant for our purpose.

We do not include controls for education, since data are available only for a non-random subsample of the Dutch workforce. We have about three million observations if we include controls for education and ten million observations if we do not include these controls. Data on education are available for younger employees and those who were asked to provide data on their education, e.g., as part of a claim for unemployment insurance. However, repeating the analysis with educational controls does not affect our conclusions (cf. Table OA16 in the Online Appendix).

Construction of *Excess wage*. To construct Excess wage, we proceed as follows. We predict the wage $\widehat{w}_{j,t}$ of each target and control employee j based on the coefficient estimates obtained from running regression (3) on the Dutch workforce, i.e.

$$\widehat{w}_{j,t} = \widehat{\alpha}_t + \sum_h H_{j,h,t} \widehat{\gamma}_{h,t} + X_{j,t} \widehat{\zeta}_t. \tag{4}$$

Next, we calculate the counterfactual wage the same employee j would earn with perfect health, obtained by setting the medication consumption of all medications that have a negative impact in the Dutch workforce in year t (i.e., $\widehat{\gamma}_{h,t} < 0$) to zero:

$$\widehat{w}_{j,t}^{\text{counterfactual}} = \widehat{\alpha}_t + \sum_h H_{j,h,t} Max \left\{0, \widehat{\gamma}_{h,t}\right\} + X_{j,t}\widehat{\zeta}_t. \tag{5}$$

Excess 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:

$$Excess\ wage_{j,t} = \widehat{w}_{j,t}^{\text{counterfactual}} - \widehat{w}_{j,t} \equiv -\sum_h H_{j,h,t} Min \left\{0, \widehat{\gamma}_{h,t}\right\}. \tag{6}$$

Note that our definition of *Excess wage* is similar to the notion of excess compensation used in the literature on CEO compensation (e.g., Core, Holthausen, and Larcker, 1999). However, differently from this literature, we estimate the counterfactual benchmark wage in equation (5) out of sample, based on the entire Dutch workforce. The distribution of *Excess wage* is skewed with a mean of €3.17, a median of zero and a value of €21.23 for the 99th percentile (Table 3).

D Figures

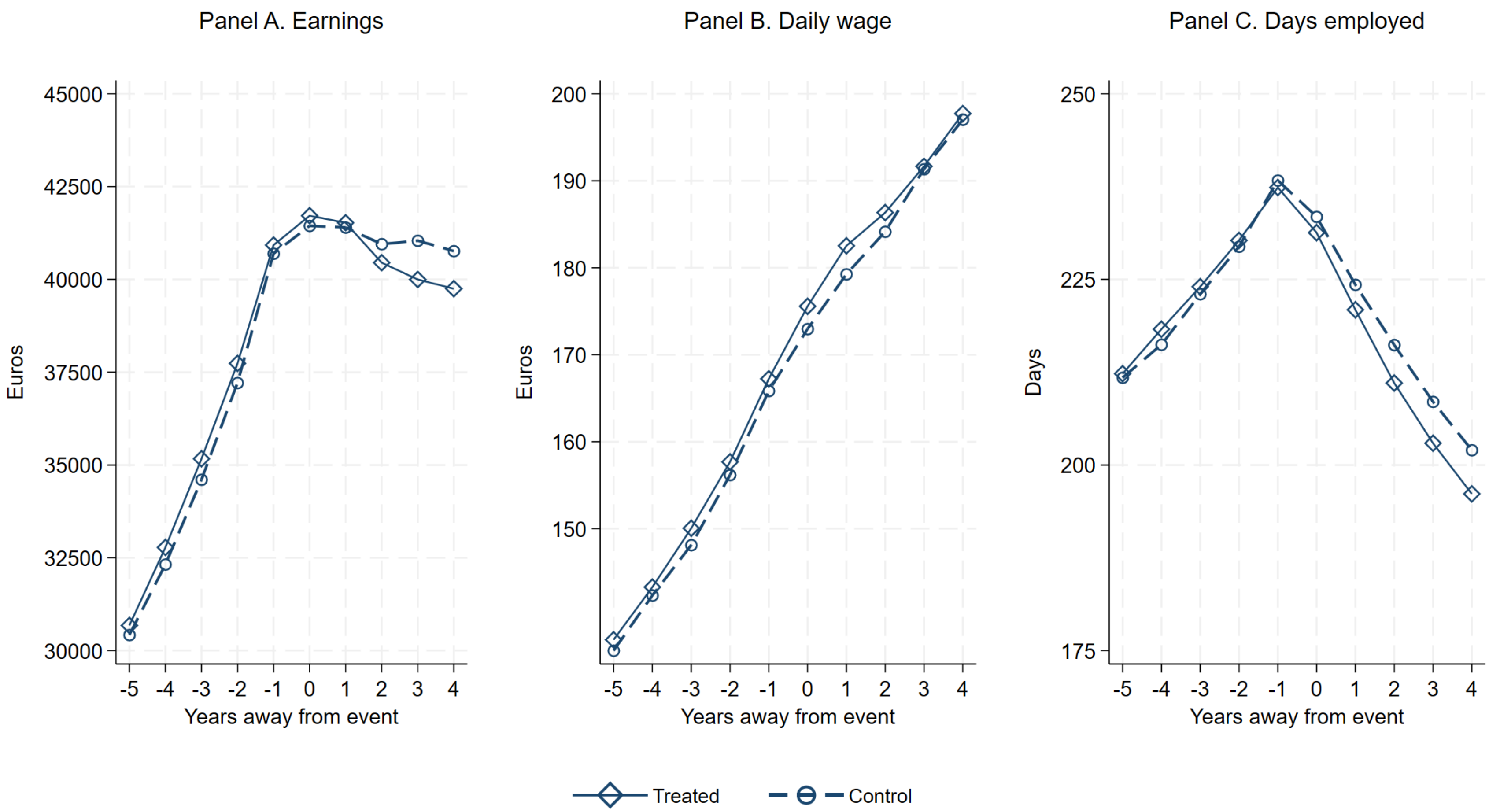

Figure 1: Employment outcomes. This figure presents the development of *Earnings (Panel A), Daily wage (Panel B), and Days employed (Panel C)* in event time. For every event year, we compute the mean of *these variables* for target employees and control employees separately. *All variables* are defined in Table 1.

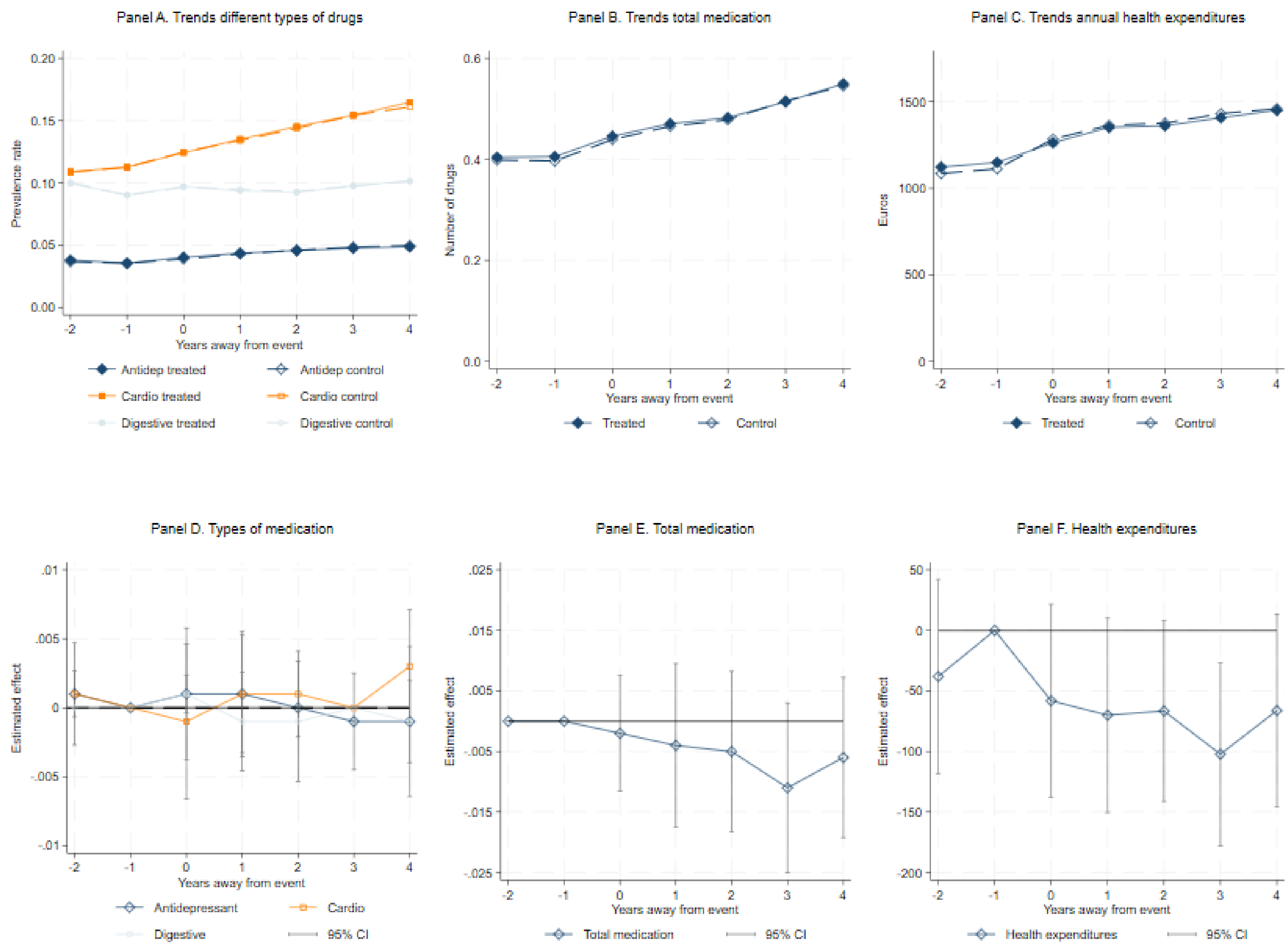

Figure 2: Buyouts and health outcomes. Panel A, Panel B, and Panel C plot the medication intake of *Antidepressant*, *Cardiovascular and Digestive* (Panel A), *Total medication* (Panel B), and *Health expenditures* (Panel C), separately for buyout employees and control employees. Panel D, Panel D and Panel E plot the coefficient estimates of θ_k on the interactions of event-time dummies and the target indicator from the difference-in-difference regressions (equation (2)) *for Antidepressant*, *Cardiovascular* and *Digestive* (Panel D), *Total medication* (Panel E) and *Health expenditures* (Panel F). The numerical variables are defined in Table 1.

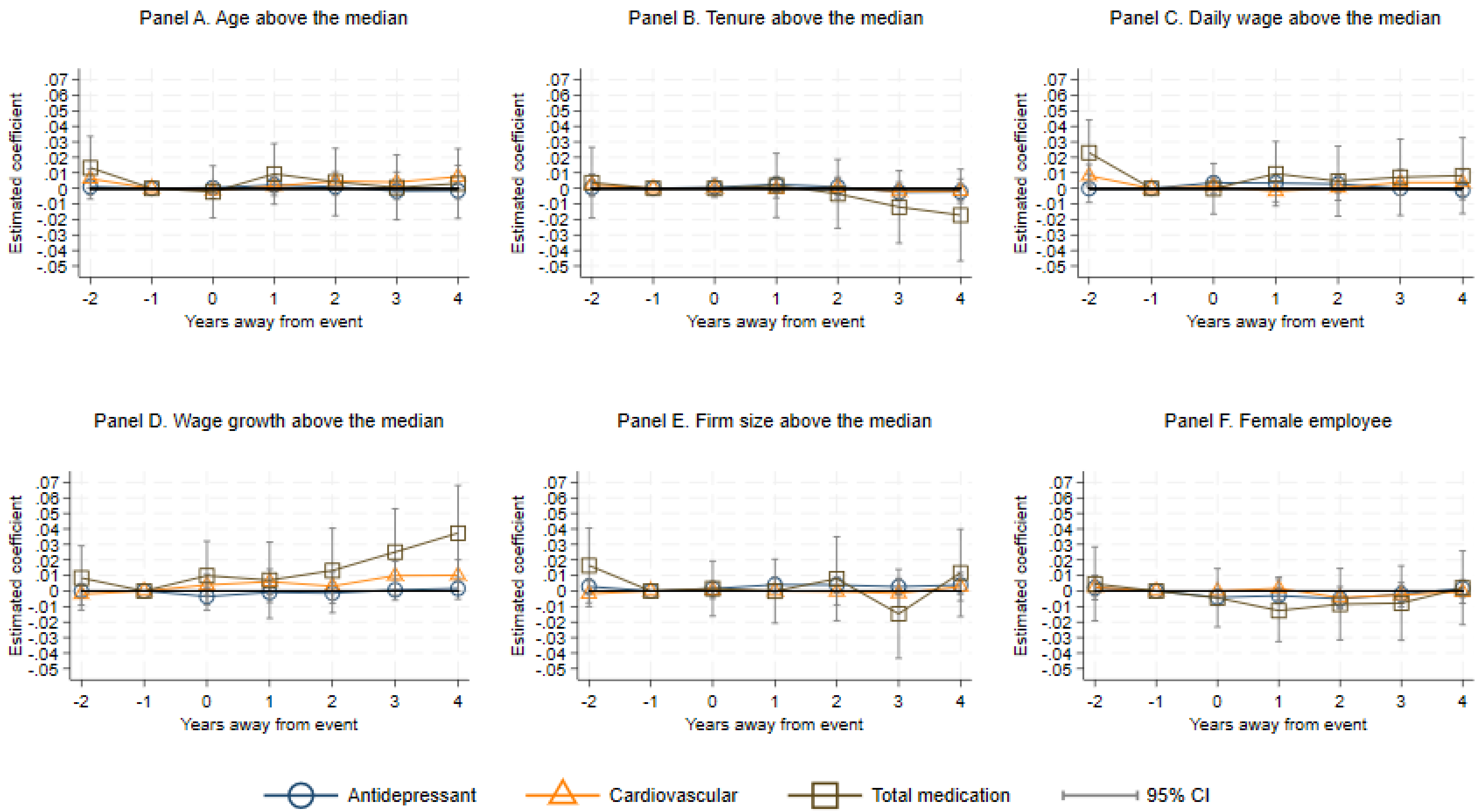

Figure 3: Employee heterogeneity. The figures present estimates from OLS-regressions on *Antidepressant, Cardiovascular*, and *Total medication* in a triple-difference setup from equation (2). Each specification includes a risk factor (RF), measured in the year prior to the buyout announcement. The risk factor is a binary variable, which is equal to one if, respectively, *Age* (Panel A), *Tenure* (Panel B), *Daily Wage* (Panel C), pre-buyout wage growth (Panel D), or *Firm size* (Panel E) are above the median. The risk factor in Panel E is equal to one if the employee is female. We report the coefficient estimates of η_k. Each specification contains individual and year fixed effects.

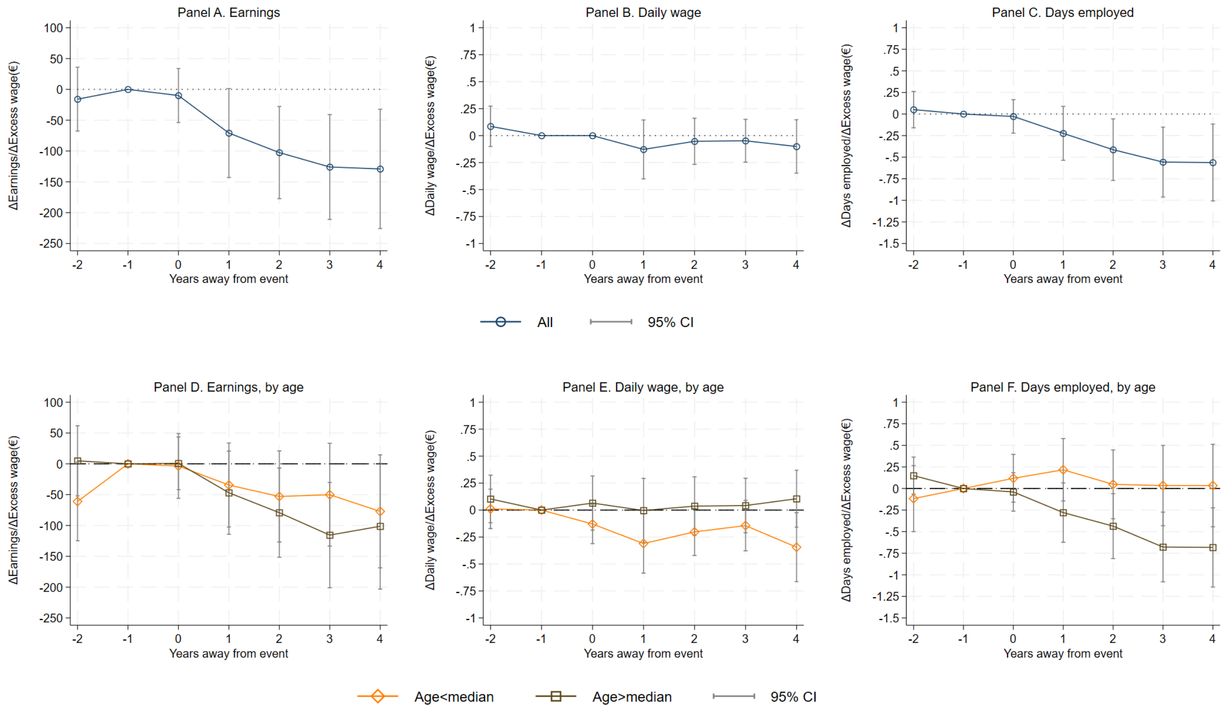

Figure 4: Productivity and employment outcomes. The figures present estimates from OLS-regressions of *Earnings* (Panels A and D), *Daily wage* (Panels B and E), and *Days employed* (Panels C and F) in a triple-difference setup from equation (2). We report the coefficient estimates of η_k, which capture the triple interactions of event time dummy, buyout indicator and a risk factor The risk factor (RF) in this figure is *Excess wage*, measured in the year prior to the buyout announcement. We show the results for the full sample (Panels A-C), and for two subsamples according to whether employees were above or below the median age in the sample (42 years) in the year before the buyout (Panels D-F). Each specification contains individual and year fixed effects. The numerical variables are defined in Table 1. Table OA15 reports the numerical results for all event dates from $k = -2$ to $k = +4$.

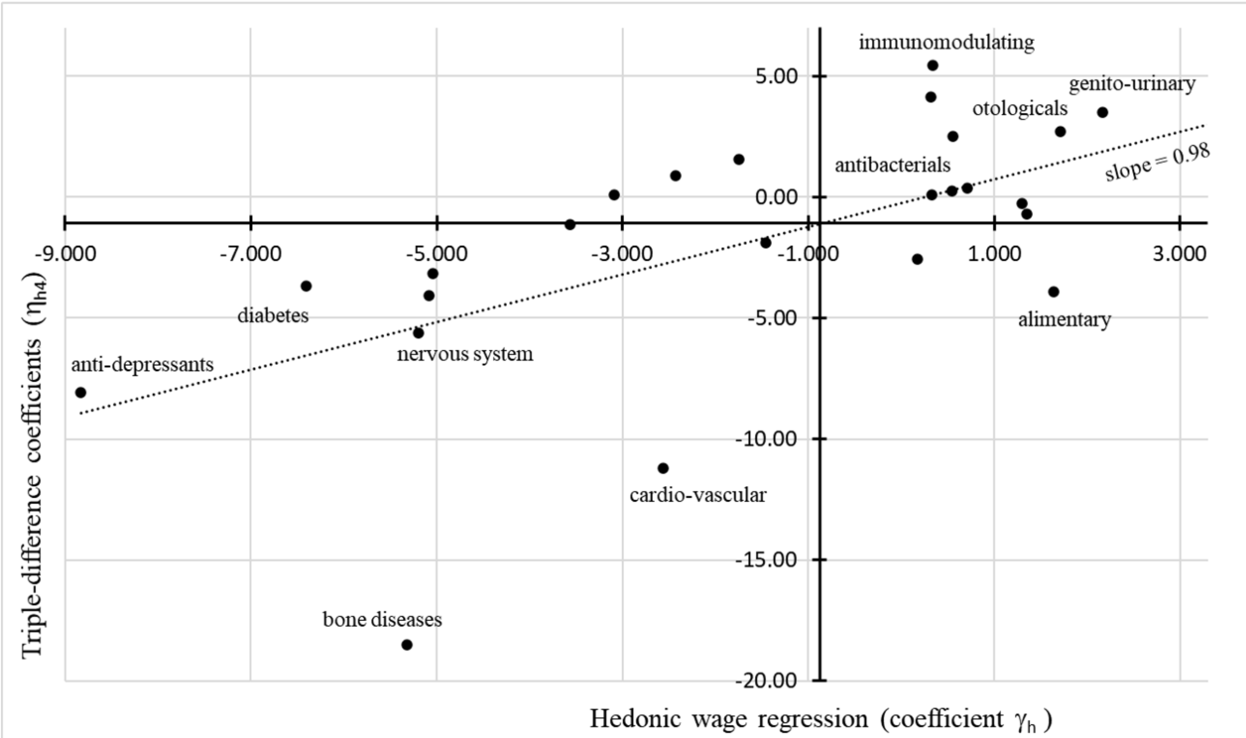

Figure 5: Hedonic wage regressions and employment effects. This figure plots the the average coefficients γ_h from the hedonic wage regressions (3) against the estimates of the coefficient η_{h4} on the interaction $Target_j \times H_{j,h} \times D_{j4}$ from the triple-difference regression (2). Medication types h refer to the 25 medication types listed in Table 10. Table 10 also provides the coefficient estimates from the hedonic wage regression in column 5 and and those from the triple-difference regressions in column 6.

E 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*, winsorized at the 99th percentile	]0;∞]
Days employed	Sum of days in employment over all spells in one calender year	[0;260]
Disability	1 if main source of income is from disability insurance	0 or 1
Disability benefits	*Total transfers* x *Disability*	[0;∞]
Earnings	Sum of income across all spells in one calendar year, winsorized at 99th percentile	[0;∞]
Excess wage	Predicted loss in *Daily wage* because of medication intake (for details see Section 4.3)	
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	
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	*Transfer income* x *Retirement*	[0;∞]
Retirement	1 if main source of income is from pension	0 or 1
Retirement benefits	*Total transfers* x *Retirement*	[0;∞]
Tenure	Number of days in employment in current job	[0;∞]
Total medication	Number of different prescriptions according to the WHO's ATC-classification	[0;∞]
Total transfers	Difference between total income and income from (self-)employment	[0;∞]
Unemployment	1 if main source of income is from unemployment insurance	
Unemployment benefits	*Total transfers* x *Unemployment*	[0;∞]
Work	1 if main source of income is from employment or self-employment	0 or 1

Table 2: Matching statistics. 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 Wooldridge (2009)) 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.

	Age	Anti-depressant	Cardio-vascular	Daily wage	Days employed	Digestive	Earnings	Excess wage	Firm size	Tenure	Total med.
Panel A. Matched target employees, N = 55,752											
Mean	40.15	3.55%	11.27%	167.57	239.51	9.01%	41,010	2.90	3,635	2,967	40.55%
Median	39.92	0.00%	0.00%	143.21	260.00	0.00%	35,377	0.00	1,371	1,806	0.00%
Variance	109.54	3.42%	10.00%	9680.00	1683.00	8.20%	7.E+08	21.01	3.E+07	1.E+07	91.86%
Panel B. Matched control employees according to Euclidean distance, one-to-one, N = 55,752											
Mean	40.10	3.55%	11.27%	166.20	240.95	9.01%	40,776	2.88	2,999	2,940	39.68%
Median	39.92	0.00%	0.00%	142.07	260.00	0.00%	35,225	0.00	1,006	1,794	0.00%
Variance	108.12	3.42%	10.00%	9710.00	1670.00	8.20%	7.E+08	20.73	3.E+07	1.E+07	86.92%
Relative difference	0.12%	0.00%	0.00%	0.82%	-0.60%	0.00%	0.57%	0.69%	19.15%	0.94%	2.17%
Imbens-Wooldridge	0.00	0.00	0.00	0.01	0.02	0.00	0.01	0.00	0.09	0.01	0.01
Panel C. Matched control employees, according to Euclidean distance, one-to-five, N = 275,944											
Mean	40.07	3.55%	11.27%	165.11	238.74	9.01%	40,537	2.88	2,985	2,947	38.03%
Variance	107.00	3.42%	10.00%	9257.00	1617.30	8.20%	6.E+08	20.65	3.E+07	1.E+07	82.28%
Relative difference	0.20%	0.00%	0.00%	1.48%	0.32%	0.00%	1.16%	0.69%	19.63%	0.68%	6.44%
Imbens-Wooldridge	0.01	0.00	0.00	0.02	0.01	0.00	0.01	0.00	0.08	0.00	0.02
Panel D. Matched control employees according to Mahalanobis distance, one-to-one, N = 55,752											
Mean	40.11	3.55%	11.27%	165.86	238.84	9.01%	40,656	2.82	2,849	2,969	39.91%
Variance	107.72	3.42%	10.00%	9533.00	1618.35	8.20%	6.E+08	20.02	3.E+00	1.E+07	88.69%
Relative difference	0.10%	0.00%	0.00%	1.03%	0.28%	0.00%	0.87%	2.80%	24.23%	-0.07%	1.62%
Imbens-Wooldridge	0.00	0.00	0.00	0.01	0.01	0.00	0.01	0.01	0.15	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	41.67	41.50	20.42	63.25	113.93
Antidepressant	730,754	4.30%	0.00%	0.00%	100.00%	20.28%
Cardiovascular	730,754	13.63%	0.00%	0.00%	100.00%	34.31%
Daily wage	659,416	178.54	152.63	36.66	633.93	104.77
Days employed	730,754	219.06	260.00	0.00	260.00	72.14
Digestive	730,754	9.59%	0.00%	0.00%	100.00%	29.44%
Disability	717,480	0.93%	0.00%	0.00%	0.00%	9.58%
Disability benefits	717,480	238.01	0.00	0.00	0.00	2,789.38
Earnings	730,754	40,320.13	35,388.00	0.00	154,929.00	28,346.47
Excess wage	730,754	3.17	0.00	0.00	21.23	4.90
Health expenditures	560,178	1,338.27	257.57	0.00	15,646.85	4,681.89
High medication	730,754	3.00%	0.00%	0.00%	100.00%	17.07%
Job changer	730,754	18.74%	0.00%	0.00%	100.00%	39.02%
Not employed	730,754	6.49%	0.00%	0.00%	100.00%	24.63%
Other income	717,480	2.28%	0.00%	0.00%	100.00%	14.93%
Pensions	717,480	553.89	0.00	0.00	25,541.00	5,679.23
Retirement	717,480	1.33%	0.00%	0.00%	100.00%	11.45%
Retirement benefits	717,480	553.89	0.00	0.00	25,541.00	4,289.39
Stayer	730,754	75.32%	100.00%	0.00%	100.00%	43.12%
Switcher down	730,754	6.46%	0.00%	0.00%	100.00%	24.57%
Switcher up	730,754	10.17%	0.00%	0.00%	100.00%	30.21%
Total insurance	717,480	2,172.17	0.00	0.00	40,237.00	8,087.33
Total medication	730,754	0.47	0.00	0.00	5.00	1.06
Unemployment	717,480	1.99%	0.00%	0.00%	100.00%	13.95%
Unemployment benefits	717,480	496.34	0.00	0.00	23,106.00	3,909.38
Work	717,480	93.48%	100.00%	0.00%	100.00%	24.69%

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). Columns 4 to 6 include a risk factor (RF), which is measured in the year prior to the buyout announcement and denotes all employees whose wage is above the median wage at the firm at which they are employed. We only report the coefficient estimates of θ_k and η_k. The numerical variables are defined in Table 1. Each specification contains individual and year fixed effects. The number of observations is 727,724 for *Earnings* and *Days employed* and 658,584 for *Daily wage*, respectively . The number of observations is lower for *Daily wage* because we require that the variable is available for both the buyout employee and the control employee in a given year. If that requirement is not met, we exclude the observation. 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):				High wage	High wage	High wage
D_{i-2} x Target	540.3	0.931	2.452	699.9	2.022	2.274
	1.18	0.60	1.58	1.52	1.48	1.18
D_{i0} x Target	51.2	1.216	-1.197	500.5	2.700	-1.132
	0.14	1.09	-1.10	1.48	2.91	-0.81
D_{i1} x Target	-107.7	1.902	-2.451*	557.8	3.691*	-2.393
	-0.17	0.86	-1.67	0.88	1.70	-1.40
D_{i2} x Target	-734.2	0.886	-4.201**	282.7	4.065**	-4.14**
	-1.64	0.52	-2.12	0.56	2.19	-2.00
D_{i3} x Target	-1288.8**	-1.011	-4.667**	-50.2	2.942*	-4.42*
	-2.38	-0.62	-2.04	-0.09	1.70	-1.90
D_{i4} x Target	-1292.3**	-0.558	-5.219**	-57.1	3.340	-4.677**
	-2.08	-0.21	-2.30	-0.10	1.53	-2.06
D_{i-2} x Target x RF				-307.5	-2.147	0.192
				-0.56	-1.00	0.14
D_{i0} x Target x RF				-883.2*	-2.958	-0.110
				-1.71	-1.58	-0.11
D_{i1} x Target x RF				-1305.3***	-3.55**	-0.126
				-3.01	-2.11	-0.11
D_{i2} x Target x RF				-1991.6***	-6.281***	-0.134
				-3.37	-2.87	-0.11
D_{i3} x Target x RF				-2425.8***	-7.841***	-0.502
				-3.28	-3.21	-0.34
D_{i4} x Target x RF				-2394.7***	-7.714**	-1.068
				-2.68	-2.47	-0.71

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 (*Daily wage*: 658,584 observations). 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: 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 estimate for $k = +4$, which captures the long-term change from $k = -1$ to $k = +4$.. The intermediate treatment effects are included in Figure 6. For all other interaction effects, we refer the reader to Table OA10. In Panel A, we define our career path variables on the basis of the actual changes in the career paths of buyout employees. In Panel B, we predict the counterfactual career path of a buyout employee in the absence of the buyout by assuming that the buyout employee would have had the same career path as the control employee. Each specification contains individual and year fixed effects. The numerical variables are defined in Table 1. The number of observations is 727,724 (for *Health expenditures*: 556,830). 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
Panel A. Actual career paths						
D_{i4} x Job change	-0.0003	-0.0272***	-0.0870***	-0.0108***	-0.0095***	-95.6786**
	-0.18	-13.09	-13.32	-4.24	-7.83	-2.15
D_{i4} x Not employed	0.0146***	0.0069***	0.0637***	0.0004	0.0103***	162.6246**
	5.77	2.58	6.45	0.14	5.29	2.18
D_{i4} x Target (Stayer)	0.0017	0.0028	-0.0110	-0.0051*	-0.0017	-110.3313**
	0.91	1.08	-1.29	-1.77	-1.21	-2.37
D_{i4} x Job change x Target	-0.0045*	0.0051*	0.0181*	0.0055	0.0003	9.2268
	-1.87	1.74	1.90	1.52	0.18	0.15
D_{i4} x Not employed x Target	-0.0077**	-0.0013	0.0065	0.0157***	0.0026	223.2024**
	-2.21	-0.36	0.48	3.68	0.95	2.02
Panel B.Predicting career outcomes using control employees						
D_{i4} x Job change	-0.0003	-0.0272***	-0.0870***	-0.0108***	-0.0095***	-95.6705**
	-0.18	-13.09	-13.32	-4.24	-7.83	-2.15
D_{i4} x Not employed	0.0146***	0.0069***	0.0637***	0.0004	0.0103***	162.6454**
	5.77	2.58	6.45	0.14	5.29	2.18
D_{i4} x Target (Stayer)	-0.0006	0.0000	-0.0164**	-0.0038	-0.0025*	-146.3433***
	-0.34	0.00	-2.04	-1.36	-1.84	-3.28
D_{i4} x Job change x Target	0.0025	0.0107***	0.0241***	-0.0001	0.0013	119.6108*
	0.98	3.41	2.76	-0.02	0.77	1.89
D_{i4} x Not employed x Target	-0.0071**	0.0006	0.0122	0.0159***	0.0034	235.5395**
	-2.03	0.17	0.89	3.75	1.25	2.14

Table 7: Health and career paths. Panel A presents estimates from linear probability models of career path indicators in a difference-in-differences setup from equation (1) and reports the coefficient estimates of θ_k. Panels B, C, and D present estimates from a triple-difference setup from equation (2). These panels report only the coefficients η_k on the triple interactions with a risk factor (RF), which is measured in the year prior to the buyout announcement. The dependent variables are binary variables that indicate the main source of income. All regressions contain individual and year fixed effects. The numerical variables are defined in Table 1. The number of observations is 703,756. 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) Stayer	(2) Job changer	(3) Not employed	(4) Retirement	(5) Disability	(6) Unemployment
Panel A. Career Paths						
D_{i-2} x Target	0.0140	-0.0120	-0.0010	0.0010	0.0000	0.0000
	0.79	-0.72	-0.27	0.38	0.09	-0.25
D_{i0} x Target	-0.027*	0.0260	0.0010	0.0000	0.0000	0.002*
	-1.67	1.64	0.17	-0.18	-0.60	1.74
D_{i1} x Target	-0.0350	0.0250	0.009**	0.0020	0.0010	0.007***
	-1.45	1.10	2.02	1.34	0.98	2.98
D_{i2} x Target	-0.071**	0.059**	0.012**	0.0020	0.0010	0.010***
	-2.46	2.26	2.00	0.97	1.10	2.79
D_{i3} x Target	-0.050*	0.0370	0.013*	0.0020	0.0020	0.010***
	-1.89	1.61	1.95	0.68	1.42	2.93
D_{i4} x Target	-0.047*	0.0330	0.014*	0.0020	0.0030	0.009***
	-1.70	1.37	1.85	0.75	1.36	3.08
Panel B. Risk Factor = Antidepressant medication						
D_{i0} x Target x Antidepressant	-0.0096	0.0008	0.0088	0.0049**	0.0025	0.0008
medication	-0.69	0.06	0.96	2.02	0.46	0.19
D_{i1} x Target x Antidepressant	-0.0229	0.0066	0.0163	0.0089***	0.0031	0.0049
medication	-1.42	0.42	1.24	2.84	0.39	0.73
D_{i2} x Target x Antidepressant	-0.0429**	0.0102	0.0327**	0.0155***	0.0092	0.0083
medication	-2.29	0.49	2.33	3.75	1.17	1.08
D_{i3} x Target x Antidepressant	-0.0311	-0.0172	0.0483***	0.0177***	0.0210**	0.0198**
medication	-1.57	-0.88	3.19	3.64	2.25	2.51
D_{i4} x Target x Antidepressant	-0.0282	-0.0161	0.0443***	0.0171***	0.0274**	0.0061
medication	-1.37	-0.74	2.83	2.90	2.57	0.81
Panel C. Risk Factor = Cardiovascular						
D_{i0} x Target x Cardiovascular	-0.0132	0.0102	0.0029	-0.0010	0.0039**	-0.0005
	-1.48	1.19	0.74	-0.35	1.99	-0.22
D_{i1} x Target x Cardiovascular	-0.0189*	0.0012	0.0177***	0.0067	0.0044	0.0085**
	-1.69	0.11	2.81	1.60	1.32	2.02
D_{i2} x Target x Cardiovascular	-0.0364***	0.0081	0.0283***	0.0088*	0.0084**	0.0151***
	-2.94	0.58	3.37	1.81	2.21	2.92
D_{i3} x Target x Cardiovascular	-0.0310**	0.0033	0.0278***	0.0067	0.0091**	0.0136***
	-2.46	0.24	2.77	1.09	2.11	2.96
D_{i4} x Target x Cardiovascular	-0.0281**	-0.0071	0.0351***	0.0097	0.0128***	0.0135***
	-2.16	-0.45	2.96	1.21	2.75	2.81
Panel D. Risk Factor = Total medication						
D_{i0} x Target X Total	-0.0049	0.0036	0.0013	0.0001	0.0013	-0.0005
medication	-1.57	1.12	0.82	0.13	1.33	-0.70
D_{i1} x Target X Total	-0.0059	-0.0005	0.0063***	0.0027*	0.0024	0.0022
medication	-1.49	-0.11	2.59	1.87	1.29	1.62
D_{i2} x Target x Total	-0.0141***	0.0051	0.0090***	0.0022	0.0038*	0.0041**
medication	-2.89	0.89	2.86	1.30	1.93	2.46
D_{i3} x Target X Total	-0.0104**	-0.0010	0.0114***	0.0017	0.0057***	0.0048***
medication	-2.12	-0.18	3.05	0.82	2.59	2.85
D_{i4} x Target x Total	-0.0091*	-0.0031	0.0122***	0.0025	0.0054**	0.0047***
medication	-1.71	-0.49	2.93	0.98	2.33	2.63

Table 8: 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 703,756. 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 transfers	(2) Disability benefits	(3) Retirement benefits	(4) Unemployment benefits
D_{i-2} x Target	21.979	-6.502	53.338	4.757
	0.09	-0.28	0.35	0.07
D_{i0} x Target	318.105**	13.06	-14.127	37.207
	2.47	1.14	-0.4	1.41
D_{i1} x Target	487.648***	31.15	95.961	205.586***
	3.03	1.31	1.18	3.37
D_{i2} x Target	634.987***	49.114*	98.169	287.298***
	3.1	1.65	1.01	3.15
D_{i3} x Target	609.705**	62.332	68.005	318.038***
	2.31	1.56	0.55	3.46
D_{i4} x Target	654.440**	77.97	214.806	312.266***
	2.37	1.51	1.37	3.57

Table 9: 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 703,756. 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 transfers	(2) Disability benefits	(3) Retirement benefits	(4) Unemployment benefits	(5) Total transfers	(6) Disability benefits	(7) Retirement benefits	(8) Unemployment benefits	(9) Total transfers	(10) Disability benefits	(11) Retirement benefits	(12) Unemployment benefits
Risk Factor (RF):	(1) - (4): Antidepressant				(5) - (8): Cardiovascular				(9) - (12): Total medication			
D_{i-2} x Target	29.6	-4.7	57.6	5.5	-10.5	-2.7	28.3	-5.7	-8.6	3.5	21.4	-4.4
	0.12	-0.22	0.39	0.09	-0.05	-0.13	0.26	-0.09	-0.04	0.21	0.20	-0.07
D_{i0} x Target	305.3**	9.9	-20.6	38.1	281.1**	0.4	-10.1	38.0	261.9**	-6.4	-20.7	39.9
	2.39	0.97	-0.58	1.46	2.47	0.04	-0.42	1.50	2.36	-0.58	-0.84	1.57
D_{i1} x Target	459.3***	31.0	84.8	196.4***	397.7***	21.2	56.8	176.4***	376.2***	9.5	44.2	172.6***
	2.95	1.48	1.06	3.30	2.94	1.07	1.01	3.31	2.85	0.52	0.79	3.29
D_{i2} x Target	590.2***	43.2	78.0	277.0***	479.4***	20.6	35.8	232.1***	439.1***	-2.5	40.3	222.5***
	2.99	1.62	0.82	3.10	2.80	0.85	0.52	2.89	2.62	-0.12	0.59	2.81
D_{i3} x Target	553.9**	44.1	41.9	295.7***	438.5**	24.2	14.3	259.5***	388.0*	-11.6	-2.2	240.9***
	2.16	1.24	0.35	3.28	2.03	0.75	0.16	3.12	1.83	-0.46	-0.02	2.92
D_{i4} x Target	594.3**	53.7	179.7	308.7***	484.4**	40.1	132.2	264.3***	445.5*	21.7	113.9	245.0***
	2.21	1.15	1.16	3.57	2.11	0.94	1.09	3.34	1.96	0.62	0.91	3.10
D_{i-2} x Target x RF	-127.2	-29.4	-76.0	-4.3	316.6	-21.2	223.9	100.9	86.1	-17.9	78.3	26.0
	-0.44	-0.18	-0.49	-0.04	0.79	-0.32	0.61	1.28	0.79	-0.57	0.72	1.10
D_{i0} x Target x RF	350.4	85.9	176.5*	-26.1	321.7	110.1**	-36.3	-8.1	130.1*	46.2*	13.8	-7.6
	1.51	0.61	1.82	-0.32	1.64	2.12	-0.23	-0.15	1.92	1.83	0.28	-0.39
D_{i1} x Target x RF	783.2**	3.6	306.7**	255.0*	784.7***	86.2	341.7	254.5**	257.4***	48.5	120.8	78.2**
	2.41	0.02	2.22	1.73	2.93	1.03	1.40	2.13	2.77	1.08	1.54	2.02
D_{i2} x Target x RF	1240.6***	163.8	556.7***	285.2	1360.7***	249.6**	545.610*	483.1***	456.8***	119.1**	131.9	155.4***
	3.14	0.82	3.09	1.56	3.66	2.41	1.79	3.13	3.47	2.34	1.29	3.22
D_{i3} x Target x RF	1544.5***	505.5**	722.8***	620.5***	1499.8***	334.0***	469.8	512.8***	512.1***	172.7***	156.6	184.4***
	3.34	2.06	2.78	2.97	2.93	2.75	1.19	3.40	2.94	2.92	1.22	3.35
D_{i4} x Target x RF	1690.2***	688.4**	989.1***	96.0	1502.6***	335.7**	732.034*	423.4***	473.4***	126.8*	228.1	159.8***
	3.61	2.41	3.56	0.47	2.88	2.33	1.66	2.58	2.66	1.95	1.63	2.58

Table 10: Hedonic wage regressions and labor-outcome effects for all medications. For each medication type presented in column 1, the subsequent columns contain the following information: column 1 shows the ATC classification codes (see Norwegian Institute of Public Health (2017)); columns 2, 3, and 4 report the prevalence of each medication type in the workforce, the treated group, and the control group, respectively; column 5 shows the average of the yearly estimates of $\gamma_{h,t}$ in equation (3), estimated for the entire Dutch workforce for each year from 2006 to 2012, where the t-statistics are computed as the average coefficient divided by the standard error of the annual coefficient estimates; column 6 (column 7) shows estimated coefficients from logit models that regress an indicator for cumulative mortality (disability insurance) in 2012 on each medication type in 2009, using the entire Dutch workforce aged 16 to 64 alive at the end of 2009 and not receiving DI benefits for disability insurance in 2009; column 8 shows estimates of the triple-difference coefficients η_{4h} for $k = 4$ in regressions as in equation (2), with *Daily wage* as the dependent variable and the medication group h shown in Column 1 as a risk factor; the risk factor RF^f is measured in the year prior to the buyout announcement. The regression in column 6 column (7) uses 14,420,039 (13,422,416) observations and includes age and gender controls. The regression in column 8 uses 727,724 observations and contain individual and year fixed effects. Standard errors are clustered at the firm level. t-statistics are provided below the coefficient estimates. *, **, *** indicate significance at the 10%, 5% and 1% level, respectively.

Name	(1) ATC classification	(2) Workforce (%)	(3) Treated (%)	(4) Controls (%)	(5) Hedonic wage Regression	(6) Mortality estimates	(7) Disability estimates	(8) Triple-difference regression
Alimentary tract and metabolism	A							
diabetes	A10	2%	2%	2%	-6.409***	0.317***	0.206***	-3.714
					-71.29	24.60	13.62	-0.70
digestive and obstipation	A02, A03, A06	12%	11%	11%	-3.091***	0.484***	0.572***	0.088
					-24.95	51.51	67.78	0.04
other alimentary tract and metabolism	A01, A04, A05, A07,	2%	2%	2%	1.645***	1.062***	0.336***	-3.940
	A09, A14, A16				21.35	82.68	21.30	-1.01
vitamins and antianemic preparations	A11, A12B, B03	2%	2%	2%	-1.463***	0.189***	0.268***	-1.929
					-3.44	5.45	7.00	-0.43
Blood and blood forming organs	B							
blood and blood forming organs	B02, B05, B06	2%	2%	2%	1.353***	0.122***	0.047	-0.698
					5.02	3.49	1.21	-0.16
Cardiovascular system	C							
cardiovascular	B01, C01-C03,	11%	11%	11%	-2.568***	0.340***	0.458***	-11.230***
	C07-C10				-18.90	37.82	53.86	-4.00
other cardiovascular system	C04, C05	1%	1%	1%	0.331	-0.051*	0.023	0.087
					1.24	-1.91	1.03	0.02
Dermatologicals	D							
emolients, protectives, wounds and ulcers	D02, D03	3%	3%	3%	-1.752***	0.302***	0.077***	1.619
					-20.84	0.016	4.99	0.48
other dermatologicals	D01, D04-D11	18%	16%	16%	0.545***	-0.111***	0.104***	0.259
					5.18	-11.30	13.26	0.19
Genito urinary system and sex hormones	G							
genito urinary system and sex hormones	G01-G04	20%	14%	15%	7.223***	-0.073***	0.189***	5.183
					26.56	-5.95	22.52	1.55

Table 10: 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) Mortality estimates	(7) Disability estimates	(8) Triple-difference regression
Systemic hormonal preparations	H							
systemic hormonal preparations	H01-H05	4%	4%	4%	-3.568***	0.296***	0.176***	-1.187
					-48.28	26.59	16.48	-0.38
Antiinfectives for systemic use	J							
antibacterials for systemic use	J01	22%	19%	19%	0.706***	0.306***	0.249***	0.350
					21.34	34.86	33.30	0.22
other antiinfectives for systemic use	J02, J04-J07	3%	3%	3%	0.174	0.157***	0.100***	-2.521
					1.01	8.73	6.89	-0.80
Immune system	L							
antineoplastics and immunomodulating agents	L01-L04	1%	1%	1%	0.342***	1.216***	0.818***	5.618
					4.53	76.13	47.65	1.01
Musculo-skeletal system	M							
musculo-skeletal system	M01-M04, M09	20%	18%	18%	-5.084***	-0.162***	0.475***	-4.031**
					-25.47	-17.61	63.73	-2.32
bone diseases	A12A, M05	1%	1%	1%	-5.318***	0.120***	0.033*	-18.653**
					-33.64	7.08	1.81	-2.41
Nervous system	N							
antidepressant	N06A	4%	4%	4%	-8.831***	0.237***	1.162***	-8.059**
					-58.37	20.00	132.64	-2.18
opioids	N02A	3%	3%	3%	-5.042***	0.479***	0.502***	-3.172
					-44.81	41.09	49.06	-0.72
other nervous system	N01-N07, ex N02A, N06A	10%	8%	8%	-5.199***	0.598***	0.755***	-5.609**
					-20.34	59.23	86.62	2.48
Antiparasitic products and insecticides	P							
antiparasitic, insecticides and repellents	P01-P03	1%	1%	1%	1.716***	-0.280***	0.238***	2.660
					8.16	-8.97	12.18	0.60
Respiratory system	R							
obstructive airway diseases	R03	7%	6%	6%	-2.427***	0.362***	0.188***	0.848
					-43.65	32.71	18.77	0.41
other respiratory system	R01, R02, R05-R07	16%	16%	16%	1.299***	-0.205***	0.022***	-0.271
					11.85	-19.97	2.74	-0.19
Sensory organs	S							
ophthalmologicals	S01	8%	7%	7%	0.558***	-0.107***	0.022**	2.541
					13.25	-8.66	2.11	1.28
otologicals	S02	3%	2%	2%	0.319***	-0.146***	0.024	4.194
					6.42	-7.10	1.52	1.23
Various	V							
various	V01, V03, V04, V06-V08	<1%	<1%	<1%	2.164***	0.717***	0.211***	3.118
					6.55	18.61	5.17	0.34
Average					-1.30	0.247	0.2902	-1.51

F Online Appendix

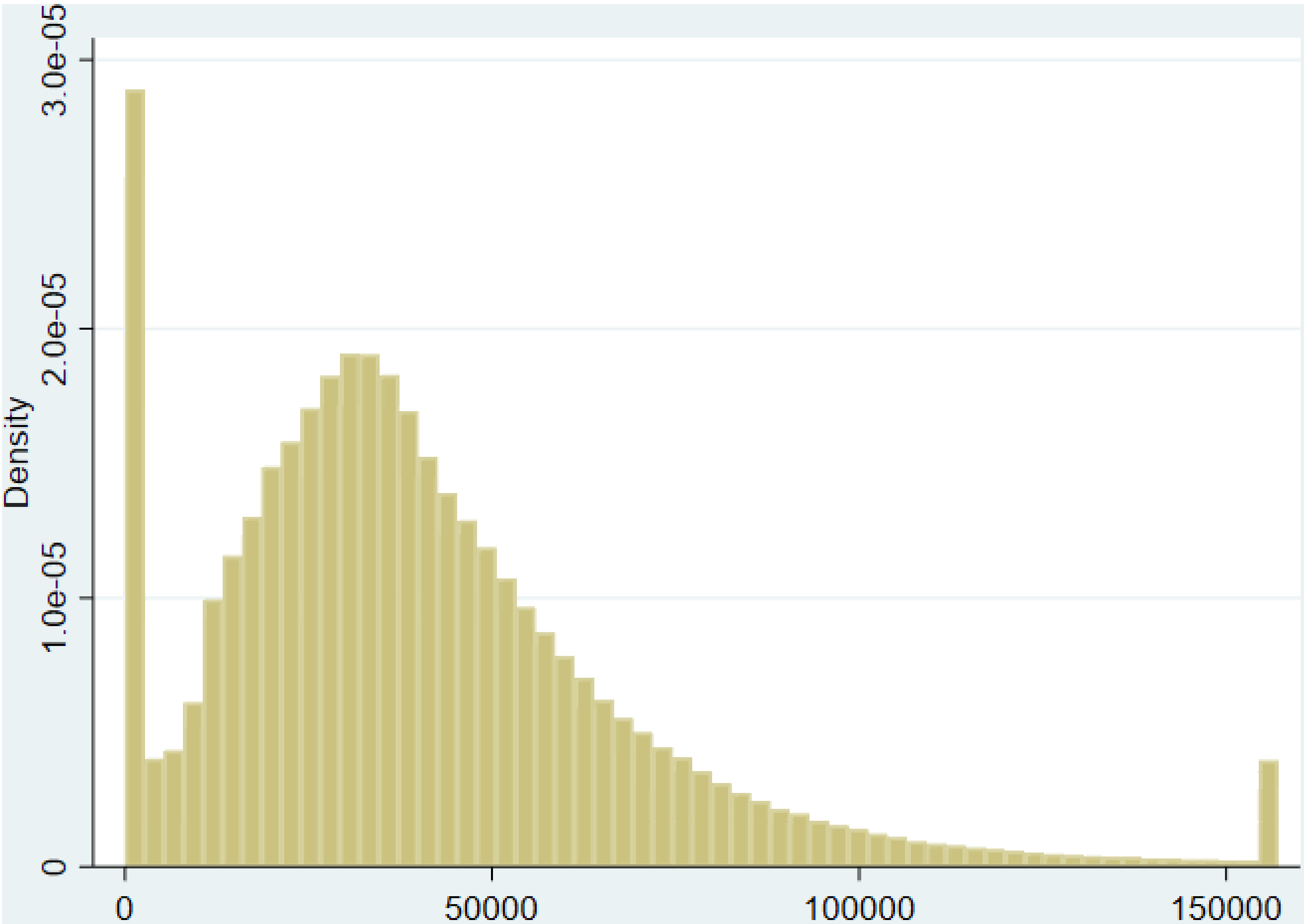

Figure OA1: Histogram of *Earnings*. This figure presents the distribution of *Earnings* for the whole sample. *Earnings* is defined in Table 1.

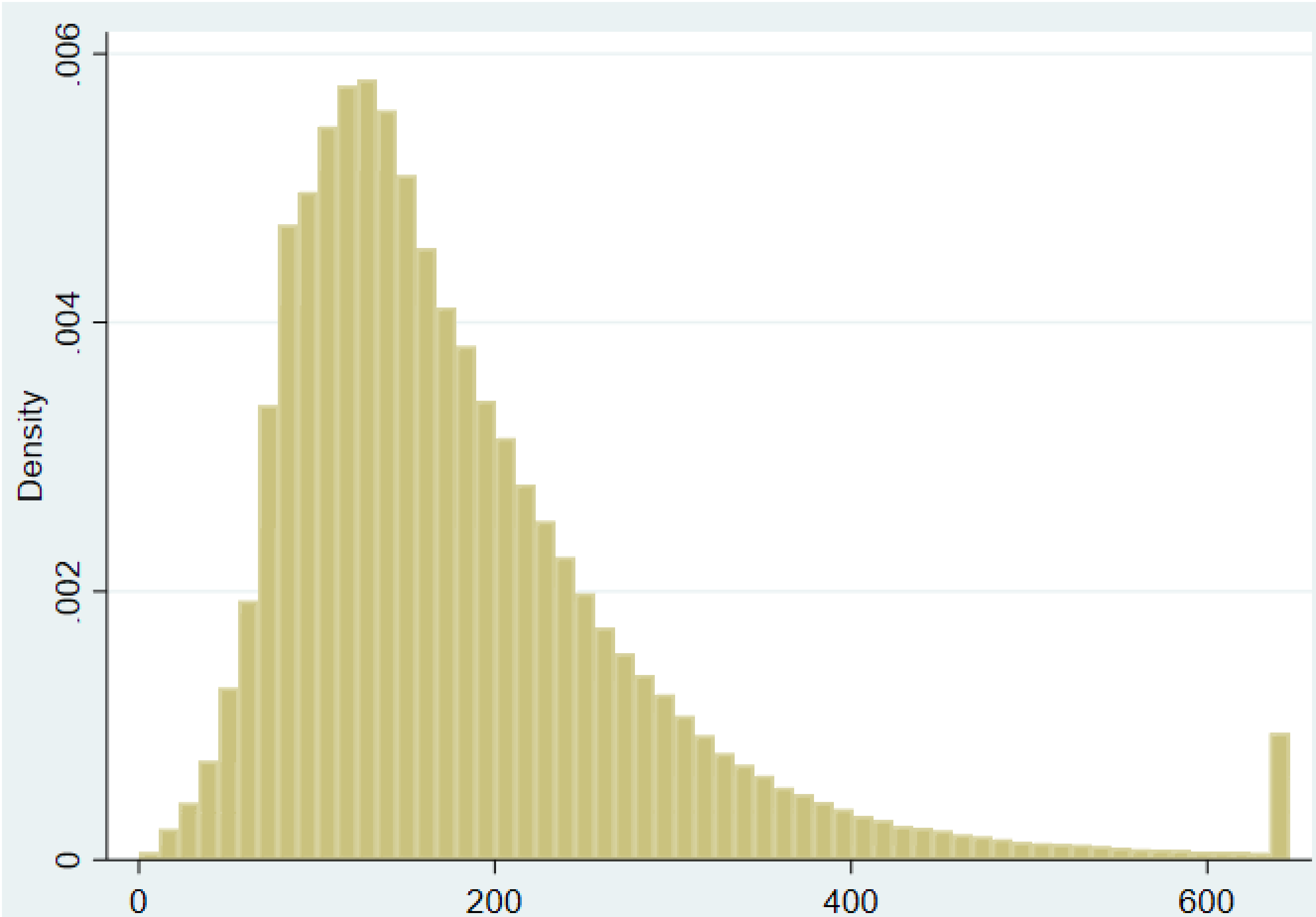

Figure OA2: Histogram of *Daily wage*. This figure presents the distribution of *Daily wage* for the whole sample. *Daily wage* is defined in Table 1. *Daily wage* is set to missing if *Daily wage* of one firm in a matched pair is missing in a given year.

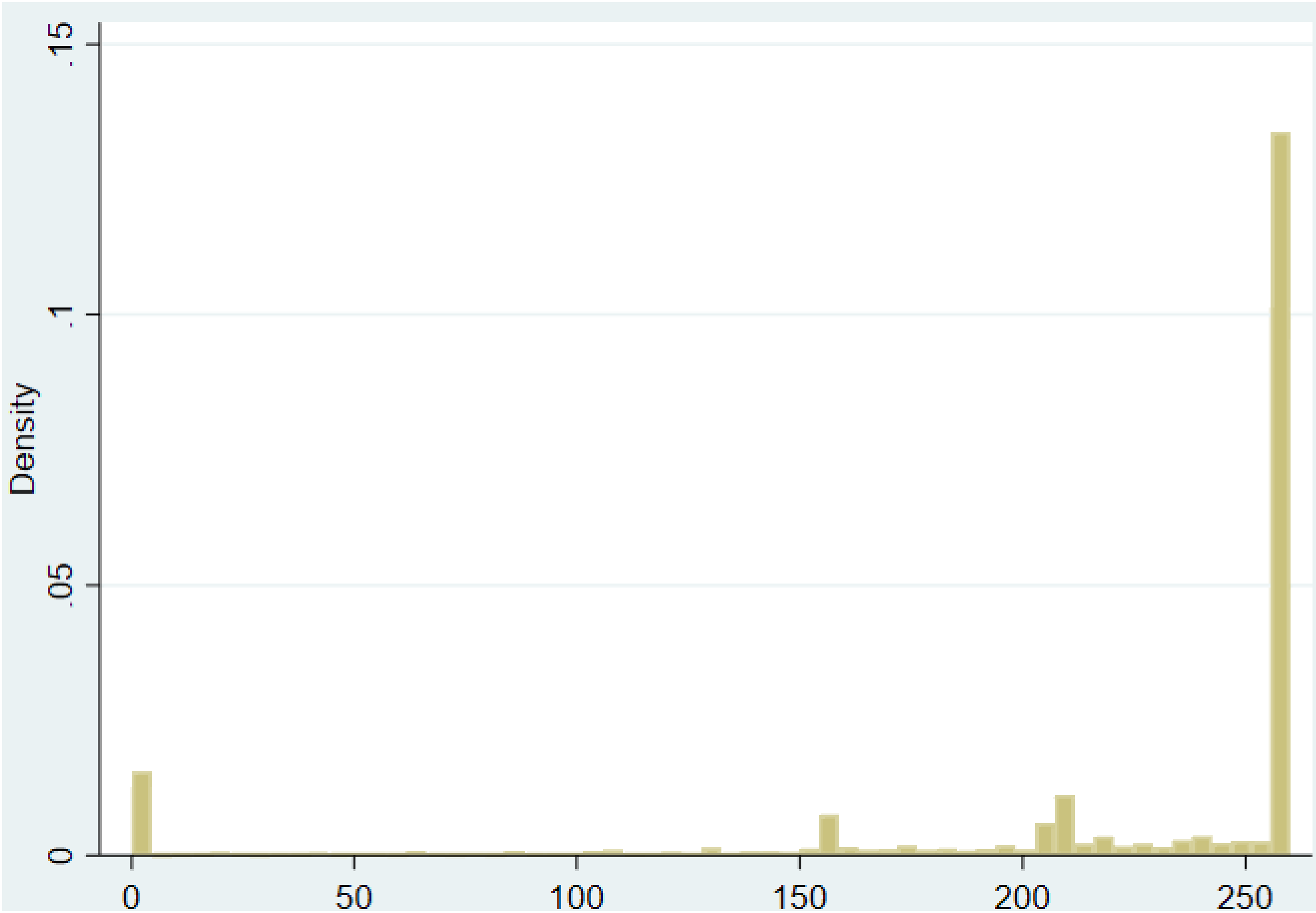

Figure OA3: Histogram of *Days employed*. This figure presents the distribution of *Days employed* for the whole sample. *Days employed* is defined in Table 1.

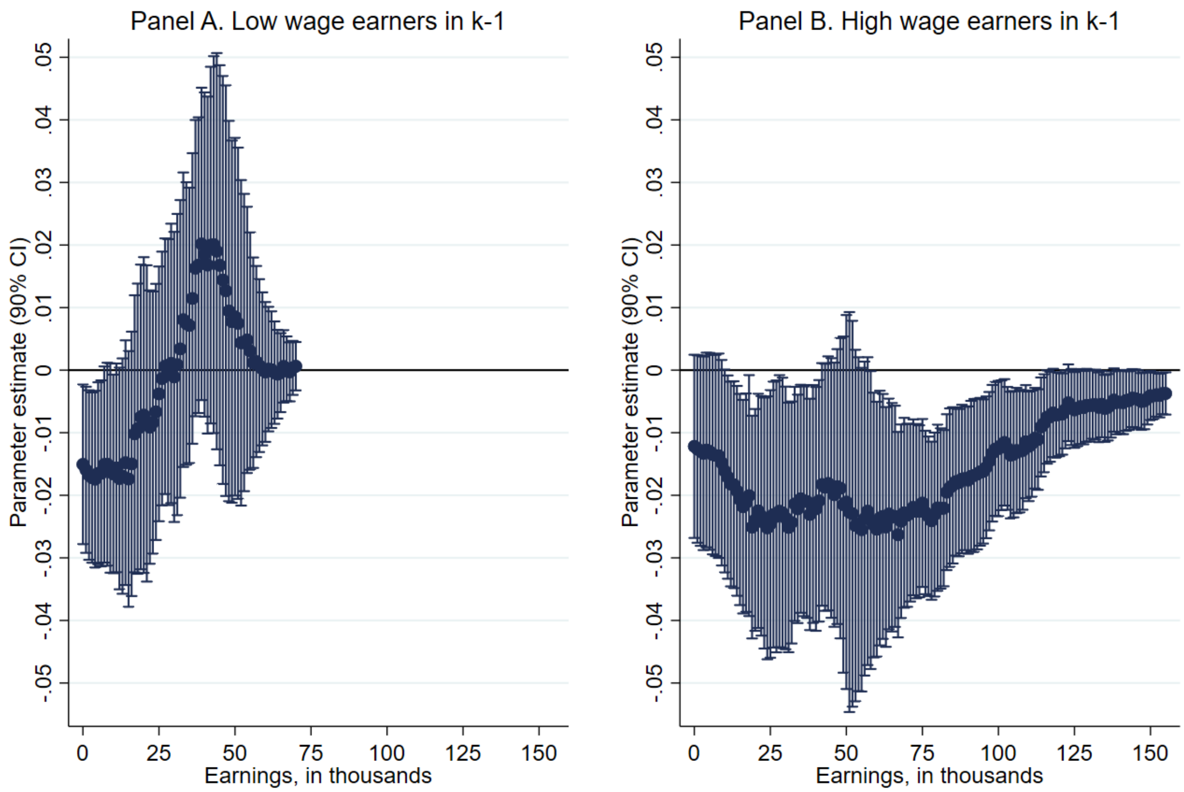

Figure OA4: Distributional regressions of *Earnings* for employees with below-median wages (Panel A) and above-median wages (Panel B). This figure plots the estimated coefficients θ_3^x (see 1) and 90% confidence intervals from a series of linear probability models, in which the dependent variable is an indicator variable $I_{i3}\left(Earnings_{i3} > e\right)$, which equals one if the earnings of employee i in event year k are higher than the threshold value e, and zero otherwise. We select rounded values from zero to the top of the earnings distribution in intervals of €1,000. We estimate these regressions using the subsample of employees with below-median wages in $k = -1$ (Panel A) or above-median wages in $k = -1$ (Panel B)

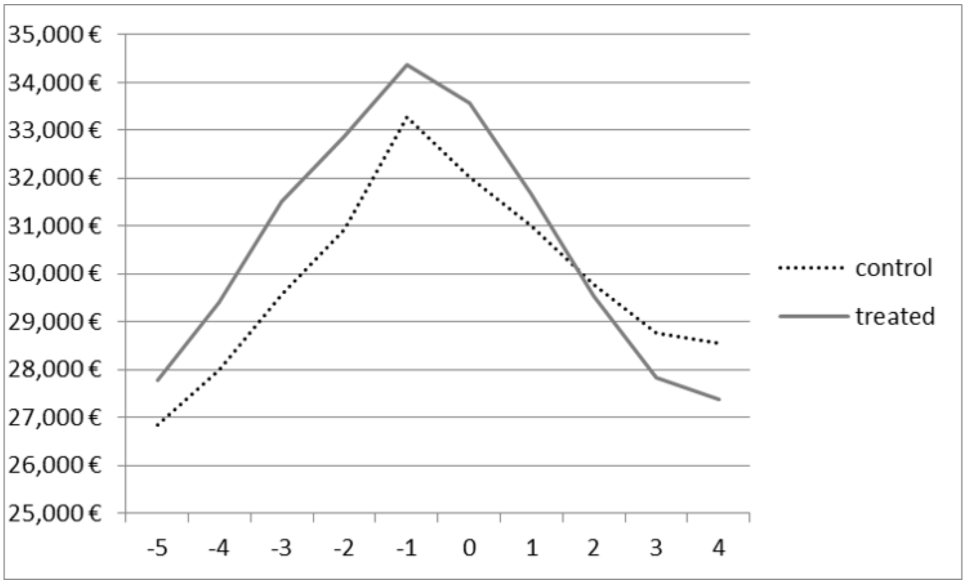

Figure OA5: Parallel trends analysis: *Earnings for antidepressant users.* This figure presents the development of *Earnings* in event time for the subset of employees who were prescribed antidepressant medication in the year before the buyout. For every event year, we compute the mean of *Earnings* for target employees and control employees separately. *Earnings* is defined in Table 1.

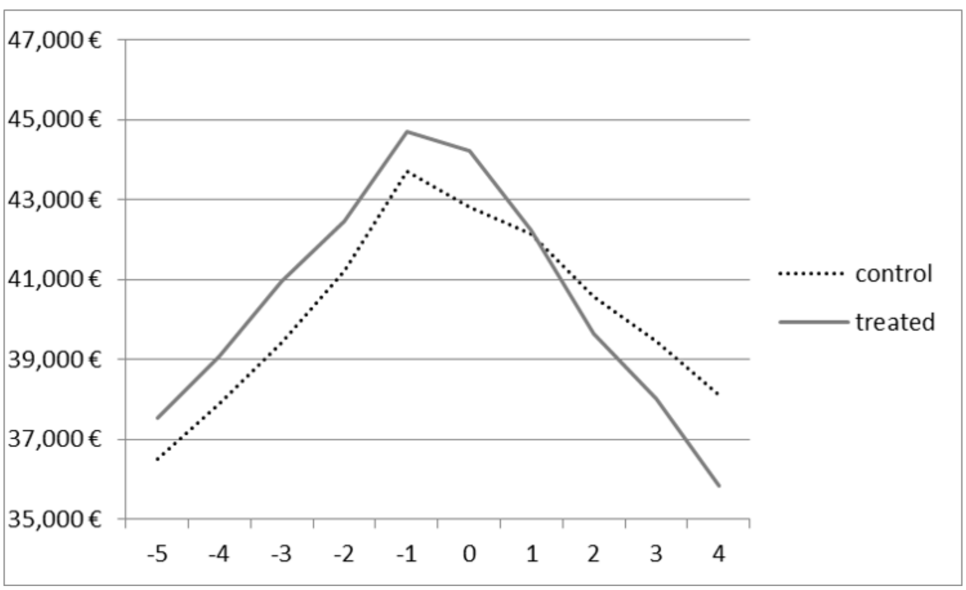

Figure OA6: Parallel trends analysis: *Earnings for cardiovascular medication users.* This figure presents the development of *Earnings* in event time for the subset of employees who were prescribed cardiovascular medication in the year before the buyout. For every event year, we compute the mean of *Earnings* for target employees and control employees separately. *Earnings* is defined in Table 1.

Table OA1: Income and employment, accounting for heterogeneous treatment effects. The table presents estimates from regressions on measures of human capital in a difference-in-differences setup from equation (1). We use the estimator proposed by Sun and Abraham (2020) that allows for heterogeneous treatment effects across cohorts. 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 727,724 for *Earnings* and *Days employed* and 658,584 for *Daily wage*, respectively. The number of observations is lower for *Daily wage* because we require that the variable is available for both the buyout employee and the control employee in a given year. If that requirement is not met, we exclude the observation. 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
Risk Factor (RF):			
D_{i-2} x Target	490.8	0.576	2.294
	1.38	0.49	1.52
D_{i0} x Target	51.3	1.215	-1.197
	0.13	1.10	-1.14
D_{i1} x Target	-107.6	1.908	-2.45*
	-0.16	0.81	-1.78
D_{i2} x Target	-734.1	0.898	-4.201**
	1.53	0.54	-2.32
D_{i3} x Target	-1288.5**	-1.008	-4.667**
	-2.18	-0.62	-2.09
D_{i4} x Target	-1292.0*	-0.754	-4.866**
	-1.85	0.28	-2.10

Table OA2: Medication and income. The table presents estimates from OLS-regressions on *Daily wage* 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 658,584. 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 OA3: 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 OA4: Robustness test of Figure 4 and Tables 5 and OA15 based on a one-to-five nearest-neighbor matching. This table repeats the analysis presented in Table **5** in columns 1 to 6, and the analysis in Table **OA15** column 1 in column 7, and the analysis of Table **OA15** column 3 in column 8, allowing for up to five control employees for each buyout employee. The number of observations in all regressions is 2,167,374. t-statistics are provided below the coefficient estimates. *, **, *** indicate significance at the 10%, 5% and 1% level, respectively.

	(1) Earnings	(2) Days employed	(3) Earnings	(4) Days employed	(5) Earnings	(6) Days employed	(7) Earnings	(8) Days employed
Risk Factor (RF):	(1) - (2): Antidepressant		(3) - (4): Cardiovascular		(5) - (6): Total medication		(7) - (8): Excess wage	
D_{i-2} x Target x RF	426.3	3.3084*	-466.5	0.072	-139.0	-0.136	-18.1	0.025
	1.02	1.67	-1.08	0.05	-1.03	-0.30	-0.74	0.27
D_{i0} x Target x RF	-248.4	-0.784	-93.7	-1.8657**	-26.5	-0.5191*	-14.6	-0.100
	-0.86	-0.47	-0.33	-2.05	-0.27	-1.71	-0.71	-1.12
D_{i1} x Target x RF	-1118.6***	-3.973	-1350.8***	-5.6482***	-379.6**	-1.5210***	-78.0**	-0.2764*
	-2.75	-1.54	-2.93	-3.94	-2.39	-2.76	-2.20	-1.86
D_{i2} x Target x RF	-1895.9***	-9.6356***	-2171.9***	-9.4966***	-598.8***	-2.7489***	-107.7***	-0.4555***
	-3.57	-3.27	-3.89	-4.97	-3.07	-3.91	-2.94	-2.68
D_{i3} x Target x RF	-2185.7***	-9.8952***	-2463.3***	-10.8667***	-663.1***	-3.1029***	-122.9***	-0.5256***
	-3.71	-3.22	-3.88	-4.62	-3.07	-3.67	-2.93	-2.72
D_{i4} x Target x RF	-2334.5***	-10.3011***	-2621.1***	-12.0190***	-646.9***	-2.9629***	-118.9**	-0.5103**
	-3.91	-3.18	-3.89	-4.80	-2.65	-3.24	-2.52	-2.41

Table OA5: Robustness test of Figure 4 and Tables 5 and OA15 using Mahalanobis matching. This table repeats the analysis presented in Table 5 in columns 1 to 6, in Table 11 (1) in column 7, and Table 11 (3) in column 8, using the Mahalanobis distance instead of the euclidean distance to match each buyout employee to one control employee. The number of observations in all regressions is 728,699. t-statistics are provided below the coefficient estimates. *, **, *** indicate significance at the 10%, 5% and 1% level, respectively.

	(1) Earnings	(2) Days employed	(3) Earnings	(4) Days employed	(5) Earnings	(6) Days employed	(7) Earnings	(8) Days employed
Risk Factor (RF):	(1) - (2): Antidepressant		(3) - (4): Cardiovascular		(5) - (6): Total medication		(7) - (8): Excess wage	
D_{i-2} x Target x RF	135.9	1.289	-754.8	-0.573	-144.9	-0.160	-31.6	-0.027
	0.28	0.55	-1.59	-0.33	-0.92	-0.31	-1.14	-0.26
D_{i0} x Target x RF	-15.1	0.171	-143.6	-1.906	-29.7	-0.564	-15.9	-0.044
	-0.04	0.09	-0.42	-1.64	-0.24	-1.43	-0.69	-0.45
D_{i1} x Target x RF	-1046.3**	-3.780	-1699.1***	-7.3027***	-476.0***	-2.0783***	-91.6**	-0.3139**
	-2.23	-1.30	-3.34	-4.62	-2.61	-3.45	-2.42	-2.04
D_{i2} x Target x RF	-2030.5***	-9.3136***	-2477.6***	-10.5514***	-753.9***	-3.4391***	-122.8***	-0.5221***
	-3.49	-2.81	-4.27	-5.01	-3.63	-4.58	-3.13	-2.95
D_{i3} x Target x RF	-2336.9***	-9.4232***	-2782.9***	-11.9742***	-888.5***	-4.0230***	-142.7***	-0.5929***
	-3.59	-2.70	-4.00	-4.64	-3.74	-4.37	-3.19	-2.92
D_{i4} x Target x RF	-2202.1***	-7.8421**	-2739.2***	-12.8874***	-875.6***	-3.9647***	-152.6***	-0.6426***
	-3.12	-2.11	-3.65	-4.50	-3.31	-3.86	-3.07	-2.85

Table OA6: Health outcomes for medicated employees. The table presents estimates from OLS-regressions of measures of employee health status in a difference-in-differences setup from equation (1). In column 1, we analyze the subset of employees who were prescribed antidepressant medication in t-1 and in column 2, we analyze the subset of employees who were prescribed cardiovascular medication in t-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. 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
D_{i-2} x Target	0.029	0.002
	1.52	0.21
D_{i0} x Target	0.007	0.002
	0.48	0.19
D_{i1} x Target	0.021	-0.006
	1.34	-0.62
D_{i2} x Target	0.014	-0.001
	0.91	-0.08
D_{i3} x Target	0.004	-0.800
	0.30	-0.80
D_{i4} x Target	0.007	0.00
	0.46	0.220
Observations	25,748	81,508

Table OA7: 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 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_{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

Table OA8: Robustness test of Table OA7 and Figure 2 based on a one-to-five nearest-neighbor matching. This table repeats the analysis presented in Figure 2 and Table OA7, but uses a one-to-five nearest neighbor matching instead of the one-to-one matching in the baseline analysis. t-statistics are provided below the coefficient estimates. *, **, *** indicate significance at the 10%, 5% and 1% level, respectively.

	(1) Antidepressant	(2) Cardiovascular	(3) Total medication	(4) Digestive	(5) High medication	(6) Health expenditures
D_{i-2} x Target	0.001	0.002	0.003	0.000	-0.001	24.680
	0.66	1.50	1.30	-0.08	-0.17	0.72
D_{i0} x Target	0.001	0.000	0.002	0.000	-0.003	-23.537
	1.06	0.41	1.30	-0.56	-0.70	-0.69
D_{i1} x Target	0.001	0.002	-0.001	0.000	-0.005	-31.579
	1.15	0.93	-0.45	0.29	-0.89	-0.97
D_{i2} x Target	0.001	0.002	0.001	0.000	-0.005	-10.729
	0.56	1.05	0.40	-0.15	-0.83	-0.34
D_{i3} x Target	0.000	0.001	0.001	-0.002**	-0.009	-28.652
	0.16	0.68	0.78	-2.06	-1.44	-0.93
D_{i4} x Target	0.000	0.002	-0.001	-0.002	-0.010	-24.959
	-0.34	1.18	-0.38	-1.62	-1.51	-0.73
Observations	2,167,374	2,167,374	2,167,374	2,167,374	2,167,374	1,660,273

Table OA9: Robustness test of Table OA7 and Figure 2 using Mahalanobis distance to match control employees. This table repeats the analysis presented in Figure 2 and Table OA7, using the Mahalanobis distance instead of the Euclidean distance to match each buyout employee to one control employee. t-statistics are provided below the coefficient estimates. *, **, *** indicate significance at the 10%, 5% and 1% level, respectively.

	(1) Antidepressant	(2) Cardiovascular	(3) Total medication	(4) Digestive	(5) High medication	(6) Health expenditures
D_{i-2} x Target	0.001	0.001	0.001	0.000	0.003	-13.432
	0.79	0.70	0.36	0.44	0.50	-0.33
D_{i0} x Target	0.000	-0.001	0.000	-0.001	-0.002	-26.990
	0.47	-0.39	0.16	-0.86	-0.34	-0.68
D_{i1} x Target	0.000	0.001	-0.002	0.000	-0.002	-29.416
	0.33	0.69	-0.79	0.18	-0.25	-0.75
D_{i2} x Target	-0.001	0.002	-0.001	0.000	-0.003	-22.749
	-1.00	0.98	-0.47	-0.10	-0.46	-0.60
D_{i3} x Target	-0.001	0.002	0.000	-0.002*	-0.005	-49.191
	-1.00	0.71	0.14	-1.92	-0.71	-1.27
D_{i4} x Target	-0.002	0.003	-0.001	-0.001	-0.005	-33.881
	-1.19	1.19	-0.44	-0.58	-0.64	-0.84
Observations	728,699	728,699	728,699	728,699	728,699	558,216

Table OA10: Job changes, unemployment, and health. The table presents estimates from OLS-regressions of health variables in a triple-difference setup from equation (2). *Job Change* can take the value of one only after the buyout. Therefore, we cannot estimate the effect of "job change" on the outcome variables for $k = -2$. In Panel A, we define our career path variables on the basis of the actual changes in the career paths of buyout employees. In Panel B, we predict the counterfactual career path of a buyout employee in the absence of the buyout by assuming that the buyout employee would have had the same career path as the control employee. Each specification contains individual and year fixed effects. The numerical variables are defined in Table 1. The number of observations is 727,724 (for *Health expenditures*: 556,830). Standard errors are clustered at the firm level. t-statistics are provided below the coefficient estimates. *, **, *** indicate significance at the 10%, 5% and 1% level, respectively.

Panel A. Actual career Paths	(1) Antidepressant	(2) Cardio-vascular	(3) Total medication	(4) Digestive	(5) High medication	(6) Health expenditures
D_{i0} x Job change	0.0025	-0.0062**	-0.0207**	-0.0069*	-0.0033***	-263.5613***
	1.03	-1.97	-2.47	-1.80	-2.58	-4.37
D_{i1} x Job change	0.0001	-0.0097***	-0.0377***	-0.0096***	-0.0025**	-133.7378**
	0.07	-4.31	-5.80	-2.98	-2.31	-2.21
D_{i2} x Job change	0.0006	-0.0159***	-0.0547***	-0.0103***	-0.0062***	-147.0931***
	0.37	-7.83	-9.12	-3.82	-5.95	-3.45
D_{i3} x Job change	-0.0001	-0.0207***	-0.0681***	-0.0075***	-0.0084***	-53.4137
	-0.04	-10.40	-11.35	-3.09	-7.62	-1.28
D_{i4} x Job change	-0.0003	-0.0272***	-0.0870***	-0.0108***	-0.0095***	-95.6786**
	-0.18	-13.09	-13.32	-4.24	-7.83	-2.15
D_{i-2} x Not employed	0.004	0.0176***	0.0453***	-0.0069	0.0047**	297.5833**
	1.0285	4.1081	3.4813	-1.2593	2.11	2.4421
D_{i0} x Not employed	0.0120***	0.0103**	0.0560***	0.0095**	0.0034	355.3111***
	3.54	2.46	4.01	2.03	0.97	3.31
D_{i1} x Not employed	0.0175***	0.0083***	0.0368***	0.0016	0.0035*	517.2201***
	5.69	2.75	3.40	0.40	1.86	4.54
D_{i2} x Not employed	0.0178***	0.0023	0.0357***	-0.0084***	0.0053***	515.9629***
	6.37	0.87	4.02	-2.64	3.02	5.86
D_{i3} x Not employed	0.0189***	0.0050*	0.0569***	0.0022	0.0097***	366.2575***
	6.65	1.87	5.99	0.72	5.37	4.75
D_{i4} x Not employed	0.0146***	0.0069***	0.0637***	0.0004	0.0103***	162.6246**
	5.77	2.58	6.45	0.14	5.29	2.18
D_{i-2} x Target	0.0014	0.0008	-0.0012	0.0000	0.0001	-35.7496
	1.20	0.45	-0.20	-0.01	0.06	-0.85
D_{i0} x Target	0.0013	-0.0002	-0.0001	0.0012	-0.0007	-75.4752*
	1.25	-0.14	-0.03	0.52	-0.74	-1.90
D_{i1} x Target	0.0011	0.0012	-0.0042	-0.0019	0.0003	-49.3511
	0.77	0.66	-0.63	-0.77	0.31	-1.16
D_{i2} x Target	0.0011	0.0010	-0.0060	-0.0042	-0.0006	-39.1367
	0.70	0.48	-0.84	-1.55	-0.48	-0.93
D_{i3} x Target	0.0014	0.0001	-0.0073	-0.0004	-0.0019	-50.6138
	0.84	0.05	-0.96	-0.16	-1.51	-1.15
D_{i4} x Target	0.0017	0.0028	-0.0110	-0.0051*	-0.0017	-110.3313**
	0.91	1.08	-1.29	-1.77	-1.21	-2.37
D_{i0} x Target x Job change	-0.0026	-0.0028	-0.0187	0.0019	0.0003	9.7783
	-0.82	-0.70	-1.61	0.37	0.15	0.12
D_{i1} x Target x Job change	-0.0028	0.0000	0.0010	0.0058	-0.0013	-72.2475
	-1.18	0.01	0.11	1.38	-0.85	-0.93
D_{i2} x Target x Job change	-0.0039*	0.0007	0.0009	0.0075**	0.0001	-66.8942
	-1.79	0.24	0.11	2.12	0.09	-1.15
D_{i3} x Target x Job change	-0.0037*	0.0035	0.0063	0.0004	0.0020	-96.3595*
	-1.80	1.26	0.72	0.13	1.33	-1.78
D_{i4} x Target x Job change	-0.0045*	0.0051*	0.0181*	0.0055	0.0003	9.2268
	-1.87	1.74	1.90	1.52	0.18	0.15
D_{i-2} x Target x Not employed	-0.0001	0.0047	0.0310*	0.0066	0.0058**	-87.2316
	-0.0094	0.7911	1.7471	0.8618	2.089	-0.5023
D_{i0} x Target x Not employed	0.0027	-0.0006	-0.0035	-0.0065	0.0000	194.8496
	0.60	-0.13	-0.21	-1.02	0.01	1.15
D_{i1} x Target x Not employed	-0.0018	-0.0026	0.0090	-0.0011	0.0033	-109.1876
	-0.42	-0.60	0.59	-0.21	1.25	-0.71
D_{i2} x Target x Not employed	-0.0057	0.0040	0.0106	0.0119**	0.0043*	-150.0089
	-1.48	1.10	0.83	2.29	1.66	-1.23
D_{i3} x Target x Not employed	-0.0085**	-0.0026	-0.0216*	0.0012	-0.0027	-168.6222*
	-2.22	-0.73	-1.71	0.28	-1.09	-1.69
D_{i4} x Target x Not employed	-0.0077**	-0.0013	0.0065	0.0157***	0.0026	223.2024**
	-2.21	-0.36	0.48	3.68	0.95	2.02

Panel B. Employees' career paths	(1) Antidepressant	(2) Cardio-vascular	(3) Total medication	(4) Digestive	(5) High medication	(6) Health expenditures
D_{i0} x Job change	0.0025	-0.0062**	-0.0207**	-0.0069*	-0.0033***	-263.5533***
	1.03	-1.97	-2.48	-1.80	-2.58	-4.37
D_{i1} x Job change	0.0001	-0.0097***	-0.0377***	-0.0096***	-0.0025**	-133.7460**
	0.07	-4.31	-5.79	-2.98	-2.31	-2.21
D_{i2} x Job change	0.0006	-0.0159***	-0.0547***	-0.0103***	-0.0062***	-147.0879***
	0.37	-7.83	-9.12	-3.82	-5.95	-3.45
D_{i3} x Job change	-0.0001	-0.0207***	-0.0681***	-0.0075***	-0.0084***	-53.4071
	-0.04	-10.40	-11.35	-3.09	-7.62	-1.28
D_{i4} x Job change	-0.0003	-0.0272***	-0.0870***	-0.0108***	-0.0095***	-95.6705**
	-0.18	-13.09	-13.32	-4.24	-7.83	-2.15
D_{i-2} x Not employed	0.0040	0.0176***	0.0453***	-0.0069	0.0047**	297.6562**
	1.03	4.11	3.48	-1.26	2.11	2.44
D_{i0} x Not employed	0.0120***	0.0103**	0.0560***	0.0095**	0.0034	355.2981***
	3.54	2.46	4.01	2.03	0.97	3.31
D_{i1} x Not employed	0.0175***	0.0082***	0.0368***	0.0016	0.0035*	517.1737***
	5.69	2.75	3.39	0.40	1.86	4.54
D_{i2} x Not employed	0.0178***	0.0023	0.0357***	-0.0084***	0.0053***	515.8777***
	6.37	0.86	4.02	-2.64	3.02	5.86
D_{i3} x Not employed	0.0189***	0.0050*	0.0569***	0.0022	0.0097***	366.2512***
	6.64	1.87	5.98	0.72	5.37	4.75
D_{i4} x Not employed	0.0146***	0.0069***	0.0637***	0.0004	0.0103***	162.6454**
	5.77	2.58	6.45	0.14	5.29	2.18
D_{i-2} x Target	0.0014	0.0009	-0.0010	0.0000	0.0001	-34.6852
	1.18	0.49	-0.16	0.00	0.10	-0.81
D_{i0} x Target	0.0013	-0.0008	-0.0036	0.0003	-0.0008	-96.2804**
	1.22	-0.55	-0.77	0.14	-0.86	-2.46
D_{i1} x Target	0.0006	0.0009	-0.0076	-0.0030	0.0003	-104.4950***
	0.43	0.50	-1.21	-1.21	0.30	-2.67
D_{i2} x Target	0.0001	-0.0007	-0.0155**	-0.0049*	-0.0010	-114.9356***
	0.10	-0.33	-2.28	-1.86	-0.86	-2.75
D_{i3} x Target	-0.0004	-0.0010	-0.0139*	-0.0010	-0.0018	-96.7463**
	-0.24	-0.45	-1.95	-0.38	-1.48	-2.23
D_{i4} x Target	-0.0006	0.0000	-0.0164**	-0.0038	-0.0025*	-146.3433***
	-0.34	0.00	-2.04	-1.36	-1.84	-3.28
D_{i0} x Target x Job change	-0.0024	0.0018	0.0108	0.0110**	0.0007	182.3489**
	-0.79	0.43	0.93	2.14	0.37	2.23
D_{i1} x Target x Job change	-0.0003	0.0002	0.0147*	0.0116***	-0.0020	241.1634***
	-0.12	0.05	1.79	2.85	-1.31	2.62
D_{i2} x Target x Job change	-0.0002	0.0049	0.0327***	0.0104***	0.0005	244.4381***
	-0.09	1.61	4.11	2.98	0.37	3.85
D_{i3} x Target x Job change	0.0025	0.0045	0.0200**	0.0012	0.0005	57.5681
	1.15	1.60	2.43	0.36	0.34	0.98
D_{i4} x Target x Job change	0.0025	0.0107***	0.0241***	-0.0001	0.0013	119.6108*
	0.98	3.41	2.76	-0.02	0.77	1.89
D_{i-2} x Target x Not employed	0.0004	0.0044	0.0309*	0.0066	0.0056**	-71.1894
	0.06	0.75	1.74	0.86	2.00	-0.41
D_{i0} x Target x Not employed	0.0029	0.0008	0.0024	-0.0059	0.0005	233.0883
	0.65	0.15	0.14	-0.93	0.13	1.38
D_{i1} x Target x Not employed	-0.0015	-0.0017	0.0126	-0.0008	0.0037	-86.3929
	-0.36	-0.39	0.84	-0.15	1.39	-0.56
D_{i2} x Target x Not employed	-0.0054	0.0056	0.0164	0.0122**	0.0050*	-125.1492
	-1.40	1.55	1.28	2.33	1.91	-1.03
D_{i3} x Target x Not employed	-0.0081**	-0.0011	-0.0162	0.0018	-0.0021	-151.0053
	-2.12	-0.31	-1.28	0.41	-0.87	-1.51
D_{i4} x Target x Not employed	-0.0071**	0.0006	0.0122	0.0159***	0.0034	235.5395**
	-2.03	0.17	0.89	3.75	1.25	2.14

Table OA11: Health outcomes and employee characteristics. The table presents estimates from OLS-regressions on *Antidepressant, Cardiovascular*, and *Total medication* in a triple-difference setup from equation (2). Each specification includes a risk factor (RF), measured in the year prior to the buyout announcement. The risk factors are a binary variables, which are equal to one if, respectively, *Age*, *Tenure* (both Panel A), *Daily Wage,* pre-buyout wage growth (both Panel B), or *Firm size* (Panel C) is above the median. The second risk factor in Panel C is equal to one if the employee is female. We only report the coefficient estimates of η_k on the triple interaction. Each 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.

	(1) Antidepressant	(2) Cardiovascular	(3) Total Medication	(4) Antidepressant	(5) Cardiovascular	(6) Total Medication
Panel A.						
Risk Factor (RF):	Age	Age	Age	Tenure	Tenure	Tenure
D_{i-2} x Target x RF	0.0010	0.0062**	0.0133	0.0005	0.0023	0.0037
	0.38	1.98	1.29	0.18	0.65	0.32
D_{i0} x Target x RF	0.0004	-0.0013	-0.0023	0.0007	0.0003	0.0000
	0.18	-0.47	-0.27	0.34	0.10	0.01
D_{i1} x Target x RF	0.0021	0.0014	0.0093	0.0025	-0.0003	0.0019
	0.87	0.45	0.93	0.98	-0.10	0.18
D_{i2} x Target x RF	0.0011	0.0047	0.0041	0.0010	0.0005	-0.0035
	0.38	1.54	0.37	0.38	0.14	-0.31
D_{i3} x Target x RF	-0.0017	0.0040	0.0006	-0.0025	-0.0020	-0.0120
	-0.62	1.19	0.06	-0.95	-0.65	-1.01
D_{i4} x Target x RF	-0.0015	0.0075**	0.0031	-0.0022	-0.0017	-0.0172
	-0.53	2.04	0.27	-0.80	-0.43	-1.14
Panel B.						
Risk Factor (RF):	Daily wage	Daily wage	Daily wage	Wage growth	Wage growth	Wage growth
D_{i-2} x Target x RF	-0.0001	0.0081**	0.0230**	-0.0001	-0.0021	0.0084
	-0.02	2.19	2.15	-0.05	-0.60	0.80
D_{i0} x Target x RF	0.0036*	0.0013	-0.0002	-0.0035	0.0039	0.0099
	1.86	0.53	-0.02	-1.28	1.12	0.88
D_{i1} x Target x RF	0.0034	-0.0019	0.0094	-0.0009	0.0060	0.0070
	1.41	-0.55	0.89	-0.27	1.45	0.56
D_{i2} x Target x RF	0.0028	0.0004	0.0047	-0.0014	0.0031	0.0132
	1.00	0.10	0.41	-0.41	0.68	0.95
D_{i3} x Target x RF	0.0000	0.0038	0.0072	0.0007	0.0099**	0.0251*
	0.01	0.90	0.57	0.21	2.09	1.76
D_{i4} x Target x RF	-0.0013	0.0037	0.0082	0.0016	0.0101**	0.0373**
	-0.43	0.87	0.66	0.46	1.99	2.38
Panel C.						
Risk Factor (RF):	Firm size	Firm size	Firm size	Female	Female	Female
D_{i-2} x Target x RF	0.0029	-0.0018	0.0165	0.0021	0.0023	0.0046
	1.18	-0.43	1.34	0.81	0.57	0.38
D_{i0} x Target x RF	0.0015	0.0005	0.0016	-0.0041	0.0000	-0.0044
	0.76	0.18	0.18	-1.54	0.00	-0.46
D_{i1} x Target x RF	0.0041*	0.0000	-0.0001	-0.0030	0.0017	-0.0126
	1.73	0.01	-0.01	-1.26	0.46	-1.24
D_{i2} x Target x RF	0.0039	-0.0004	0.0079	-0.0050**	-0.0043	-0.0085
	1.55	-0.09	0.57	-2.04	-1.19	-0.72
D_{i3} x Target x RF	0.0028	-0.0015	-0.0147	-0.0021	-0.0025	-0.0078
	1.03	-0.33	-1.01	-0.74	-0.62	-0.64
D_{i4} x Target x RF	0.0039	0.0028	0.0117	0.0016	-0.0006	0.0020
	1.29	0.60	0.82	0.56	-0.16	0.16

	(1) Antidepressant	(2) Cardiovascular	(3) Total Medication	(4) Antidepressant	(5) Cardiovascular	(6) Total Medication
Panel A.						
Risk Factor (RF):	Age	Age	Age	Tenure	Tenure	Tenure
D_{i-2} x Target x RF	0.0010	0.0062**	0.0133	0.0005	0.0023	0.0037
	0.38	1.98	1.29	0.18	0.65	0.32
D_{i0} x Target x RF	0.0004	-0.0013	-0.0023	0.0007	0.0003	0.0000
	0.18	-0.47	-0.27	0.34	0.10	0.01
D_{i1} x Target x RF	0.0021	0.0014	0.0093	0.0025	-0.0003	0.0019
	0.87	0.45	0.93	0.98	-0.10	0.18
D_{i2} x Target x RF	0.0011	0.0047	0.0041	0.0010	0.0005	-0.0035
	0.38	1.54	0.37	0.38	0.14	-0.31
D_{i3} x Target x RF	-0.0017	0.0040	0.0006	-0.0025	-0.0020	-0.0120
	-0.62	1.19	0.06	-0.95	-0.65	-1.01
D_{i4} x Target x RF	-0.0015	0.0075**	0.0031	-0.0022	-0.0017	-0.0172
	-0.53	2.04	0.27	-0.80	-0.43	-1.14
Panel B.						
Risk Factor (RF):	Daily wage	Daily wage	Daily wage	Wage growth	Wage growth	Wage growth
D_{i-2} x Target x RF	-0.0001	0.0081**	0.0230**	-0.0001	-0.0021	0.0084
	-0.02	2.19	2.15	-0.05	-0.60	0.80
D_{i0} x Target x RF	0.0036*	0.0013	-0.0002	-0.0035	0.0039	0.0099
	1.86	0.53	-0.02	-1.28	1.12	0.88
D_{i1} x Target x RF	0.0034	-0.0019	0.0094	-0.0009	0.0060	0.0070
	1.41	-0.55	0.89	-0.27	1.45	0.56
D_{i2} x Target x RF	0.0028	0.0004	0.0047	-0.0014	0.0031	0.0132
	1.00	0.10	0.41	-0.41	0.68	0.95
D_{i3} x Target x RF	0.0000	0.0038	0.0072	0.0007	0.0099**	0.0251*
	0.01	0.90	0.57	0.21	2.09	1.76
D_{i4} x Target x RF	-0.0013	0.0037	0.0082	0.0016	0.0101**	0.0373**
	-0.43	0.87	0.66	0.46	1.99	2.38
Panel C.						
Risk Factor (RF):	Firm size	Firm size	Firm size	Female	Female	Female
D_{i-2} x Target x RF	0.0029	-0.0018	0.0165	0.0021	0.0023	0.0046
	1.18	-0.43	1.34	0.81	0.57	0.38
D_{i0} x Target x RF	0.0015	0.0005	0.0016	-0.0041	0.0000	-0.0044
	0.76	0.18	0.18	-1.54	0.00	-0.46
D_{i1} x Target x RF	0.0041*	0.0000	-0.0001	-0.0030	0.0017	-0.0126
	1.73	0.01	-0.01	-1.26	0.46	-1.24
D_{i2} x Target x RF	0.0039	-0.0004	0.0079	-0.0050**	-0.0043	-0.0085
	1.55	-0.09	0.57	-2.04	-1.19	-0.72
D_{i3} x Target x RF	0.0028	-0.0015	-0.0147	-0.0021	-0.0025	-0.0078
	1.03	-0.33	-1.01	-0.74	-0.62	-0.64
D_{i4} x Target x RF	0.0039	0.0028	0.0117	0.0016	-0.0006	0.0020
	1.29	0.60	0.82	0.56	-0.16	0.16

Table OA12: Employee health and wage quartiles. The table presents estimates from OLS-regressions of *Antidepressant, Cardiovascular, Total medication, and Health expenditures* in a differences-in-differences setup which estimates the effects of a buyout on employee health for each wage quartile. Each specification contains individual and year fixed effects. The number of observations is 727,724 (for *Health expenditures*: 556,830). 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) Cardiovascular	(3) Total Medication	(4) Health expenditures
D_{i-2} x Target	0.0005	-0.0040	-0.0072	-114.4787
	0.17	-1.07	-0.79	-1.59
D_{i0} x Target	0.0002	-0.0045	-0.0050	-86.7592
	0.07	-1.61	-0.48	-1.03
D_{i1} x Target	-0.0036	-0.0011	-0.0105	-35.6285
	-1.32	-0.30	-0.93	-0.40
D_{i2} x Target	-0.0047	-0.0002	-0.0126	-176.1092**
	-1.57	-0.04	-1.11	-2.13
D_{i3} x Target	-0.0023	-0.0044	-0.0292***	-170.2215**
	-0.75	-1.10	-2.63	-2.22
D_{i4} x Target	-0.0022	0.0007	-0.0139	-45.3409
	-0.65	0.16	-1.05	-0.50
D_{i-2} x Target x Wage Q2	0.0016	0.0028	-0.0057	190.9328*
	0.41	0.54	-0.40	1.81
D_{i0} x Target x Wage Q2	-0.0009	0.0065*	0.0068	158.5724
	-0.30	1.73	0.49	1.38
D_{i1} x Target x Wage Q2	0.0052	0.0056	0.0044	-39.9712
	1.48	1.26	0.30	-0.35
D_{i2} x Target x Wage Q2	0.0067*	0.0026	0.0108	228.7497**
	1.80	0.53	0.72	2.02
D_{i3} x Target x Wage Q2	0.0032	0.0047	0.0292*	149.1331
	0.90	0.95	1.95	1.42
D_{i4} x Target x Wage Q2	0.0037	0.0010	0.0058	-4.3214
	0.95	0.18	0.33	-0.04
D_{i-2} x Target x Wage Q3	0.0003	0.0089*	0.0255*	89.7266
	0.08	1.65	1.66	0.87
D_{i0} x Target x Wage Q3	0.0046	0.0050	0.0187	37.4686
	1.50	1.36	1.41	0.34
D_{i1} x Target x Wage Q3	0.0065*	0.0040	0.0253	-35.8670
	1.80	0.83	1.41	-0.30
D_{i2} x Target x Wage Q3	0.0076*	0.0036	0.0240	145.8835
	1.94	0.73	1.48	1.35
D_{i3} x Target x Wage Q3	0.0016	0.0091*	0.0446**	70.1788
	0.42	1.71	2.57	0.65
D_{i4} x Target x Wage Q3	0.0012	0.0083	0.0325*	6.5600
	0.28	1.42	1.88	0.06
D_{i-2} x Target x Wage Q4	0.0016	0.0100*	0.0151	42.9766
	0.47	1.85	1.06	0.35
D_{i0} x Target x Wage Q4	0.0014	0.0041	-0.0132	-89.1287
	0.44	1.17	-1.07	-0.81
D_{i1} x Target x Wage Q4	0.0052*	-0.0024	-0.0024	-62.8478
	1.68	-0.50	-0.17	-0.51
D_{i2} x Target x Wage Q4	0.0042	-0.0006	-0.0047	72.9317
	1.12	-0.10	-0.30	0.66
D_{i3} x Target x Wage Q4	0.0013	0.0033	-0.0009	64.7432
	0.36	0.59	-0.06	0.59
D_{i4} x Target x Wage Q4	0.0000	0.0002	-0.0096	-72.2746
	-0.01	0.04	-0.54	-0.61

Table OA13: Robustness test of Table 9 based on a one-to-five nearest-neighbor matching. This table repeats the analysis presented in Table **9**, but uses a one-to-five nearest neighbor matching instead of the one-to-one matching in the baseline analysis. The number of observations in all regressions is 2,126,583. t-statistics are provided below the coefficient estimates. *, **, *** indicate significance at the 10%, 5% and 1% level, respectively.

	(1) Total transfers	(2) Disability benefits	(3) Retire-ment benefits	(4) Unemploy-ment benefits	(5) Total transfers	(6) Disability benefits	(7) Retire-ment benefits	(8) Unemploy-ment benefits	(9) Total transfers	(10) Disability benefits	(11) Retire-ment benefits	(12) Unemploy-ment benefits
Risk Factor (RF):	(1) - (4): Antidepressant				(5) - (8): Cardiovascular				(9) - (12): Total medication			
D_{i-2} x Target	-7.0	-8.4	40.6	-4.2	-39.7	-8.6	18.8	-14.1	-46.1	-0.4	14.9	-19.7
	-0.03	-0.42	0.28	-0.07	-0.20	-0.44	0.18	-0.24	-0.22	-0.03	0.14	-0.31
D_{i0} x Target	335.5***	13.6	0.2	30.6	303.1***	6.6	4.1	33.5	289.5***	-1.2	0.5	36.4
	2.66	1.57	0.01	1.22	2.69	0.75	0.18	1.38	2.63	-0.12	0.02	1.50
D_{i1} x Target	471.2***	31.3*	103.5	190.3***	406.2***	16.9	77.3	173.0***	388.1***	3.1	67.5	177.1***
	3.04	1.70	1.33	3.20	3.02	0.95	1.45	3.29	2.95	0.20	1.27	3.41
D_{i2} x Target	596.6***	48.3*	103.6	255.0***	482.2***	16.5	70.4	219.6***	436.1***	-9.9	63.9	218.4***
	3.05	1.95	1.10	2.88	2.83	0.73	1.05	2.80	2.62	-0.57	0.96	2.81
D_{i3} x Target	612.2**	60.5*	96.3	289.6***	488.8**	32.9	51.3	269.0***	439.5**	-4.5	39.4	246.5***
	2.42	1.81	0.82	3.27	2.26	1.09	0.60	3.31	2.08	-0.20	0.45	3.06
D_{i4} x Target	603.2**	63.4	174.2	307.3***	474.5**	34.0	123.0	261.5***	435.3*	15.7	96.1	255.7***
	2.26	1.45	1.16	3.69	2.10	0.86	1.06	3.42	1.93	0.50	0.81	3.36
D_{i-2} x Target x RF	-112.4	-145.2	20.8	-14.0	264.0	-38.2	196.2	84.5	89.9	-27.6	63.0	37.0*
	-0.45	-0.96	0.16	-0.16	0.66	-0.68	0.54	1.15	0.85	-0.95	0.59	1.67
D_{i0} x Target x RF	383.1**	127.0	142.0*	8.2	405.3**	100.8**	9.4	-23.7	137.7**	45.7**	9.0	-14.8
	1.99	1.03	1.75	0.12	2.43	2.24	0.07	-0.48	2.27	2.03	0.21	-0.83
D_{i1} x Target	899.9***	22.2	338.1***	298.5**	859.3***	132.6*	340.0	246.9**	262.6***	65.2*	110.4	54.7
	3.18	0.13	2.90	2.26	3.54	1.93	1.44	2.20	3.08	1.69	1.48	1.50
D_{i2} x Target x RF	1429.3***	139.4	688.6***	435.3***	1470.2***	323.9***	517.5*	451.4***	490.8***	146.0***	145.9	123.2***
	3.96	0.80	4.40	2.84	4.30	3.73	1.82	3.10	3.90	3.25	1.51	2.76
D_{i3} x Target	1789.5***	410.8*	844.9***	777.3***	1683.3***	374.4***	683.1*	432.5***	545.8***	185.6***	197.8*	169.0***
	4.50	1.91	3.89	4.31	3.71	3.58	1.87	3.18	3.42	3.49	1.65	3.40
D_{i4} x Target x RF	1740.6***	607.1**	1117.3***	96.8	1732.2***	453.2***	836.8**	449.2***	518.8***	155.9***	268.2**	129.9**
	4.18	2.32	4.60	0.54	3.63	3.57	2.08	3.20	3.19	2.68	2.12	2.36

Table OA14: Robustness test of Table 9 using Mahalanobis distance to match control employees.. This table repeats the analysis presented in Table 9, using the Mahalanobis distance instead of the Euclidean distance to match each buyout employee to one control employee. The number of observations in all regressions is 715,684. t-statistics are provided below the coefficient estimates. *, **, *** indicate significance at the 10%, 5% and 1% level, respectively.

	(1) Total transfers	(2) Disability benefits	(3) Retire-ment benefits	(4) Unemploy-ment benefits	(5) Total transfers	(6) Disability benefits	(7) Retire-ment benefits	(8) Unemploy-ment benefits	(9) Total transfers	(10) Disability benefits	(11) Retire-ment benefits	(12) Unemploy-ment benefits
Risk Factor (RF):	(1) - (4): Antidepressant				(5) - (8): Cardiovascular				(9) - (12): Total medication			
D_{i-2} x Target	10.2	-9.0	38.5	-1.3	-33.7	-7.3	2.5	-7.3	-23.7	1.5	0.9	-9.0
	0.04	-0.42	0.26	-0.02	-0.17	-0.35	0.02	-0.12	-0.11	0.09	0.01	-0.14
D_{i0} x Target	352.8***	13.5	-1.7	35.9	313.8***	3.7	-1.6	39.4	287.3***	-6.3	-12.9	40.9
	2.79	1.25	-0.05	1.38	2.77	0.35	-0.07	1.55	2.61	-0.54	-0.53	1.62
D_{i1} x Target	495.9***	33.6	109.5	194.4***	409.0***	22.3	75.3	164.9***	372.2***	2.3	50.2	161.0***
	3.17	1.61	1.40	3.23	3.01	1.13	1.38	3.06	2.80	0.13	0.92	3.04
D_{i2} x Target	601.8***	35.8	99.7	279.1***	488.4***	14.0	60.6	239.4***	418.0**	-7.6	29.3	233.1***
	3.02	1.30	1.04	3.14	2.85	0.57	0.90	3.01	2.46	-0.37	0.43	2.96
D_{i3} x Target	552.1**	40.2	39.3	291.6***	425.3*	19.4	13.2	256.0***	341.7	-12.8	-40.7	239.3***
	2.14	1.13	0.32	3.23	1.95	0.60	0.15	3.06	1.58	-0.49	-0.45	2.89
D_{i4} x Target	637.7**	60.1	181.7	324.7***	495.5**	42.1	120.2	276.5***	443.5*	18.1	79.7	267.1***
	2.38	1.30	1.17	3.79	2.17	0.98	1.00	3.52	1.95	0.50	0.65	3.40
D_{i-2} x Target x RF	-62.8	-24.1	-64.8	5.4	380.7	-17.5	291.4	62.0	89.5	-22.1	88.1	22.1
	-0.23	-0.14	-0.43	0.05	0.90	-0.27	0.77	0.75	0.80	-0.72	0.79	0.92
D_{i0} x Target x RF	291.0	166.3	104.9	-46.5	443.1**	141.3***	32.7	-43.6	186.4***	63.9**	36.2	-16.1
	1.32	1.25	1.12	-0.55	2.32	2.81	0.22	-0.77	2.85	2.35	0.80	-0.83
D_{i1} x Target x RF	832.6***	132.9	377.4***	171.4	1050.5***	152.2*	424.0*	318.4***	376.9***	90.0**	177.3**	97.4***
	2.70	0.70	2.81	1.11	3.97	1.84	1.77	2.68	4.20	2.02	2.33	2.58
D_{i2} x Target x RF	1358.1***	342.4*	565.5***	291.3	1465.6***	315.3***	535.1*	450.2***	570.2***	138.1***	221.0**	138.7***
	3.36	1.70	2.92	1.58	3.87	3.05	1.76	2.90	4.39	2.74	2.24	2.92
D_{i3} x Target x RF	1638.3***	524.6**	820.1***	520.8**	1688.4***	369.2***	509.1	488.1***	661.2***	178.9***	266.1**	174.6***
	3.51	2.07	3.08	2.44	3.32	3.02	1.27	3.23	3.89	3.03	2.10	3.19
D_{i4} x Target x RF	1618.5***	851.5***	990.3***	-127.4	1837.7***	450.3***	883.8**	402.6**	615.5***	178.9***	332.4**	131.2**
	3.32	2.81	3.39	-0.59	3.52	3.08	2.02	2.45	3.47	2.74	2.43	2.15

Table OA15: 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 *Excess wage* as a risk factor (RF), measured in the year prior to the buyout announcement. We only report the coefficient estimates of η_k. In columns 4 to 9, we split the sample according to whether employees were older than the median age in the sample (42 years) in the year before the buyout. Each specification contains individual and year fixed effects. The numerical variables are defined in Table 1. 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	(7) Earnings	(8) Daily wage	(9) Days employed
Sample	All	All	All	Age =< median	Age <= median	Age <= median	Age > median	Age > median	Age > median
D_{i-2} x Target	-15.9	0.085	0.051	-61.075*	0.011	-0.12	4.8	0.103	0.15
x Excess wage	-0.60	0.90	0.47	-1.88	0.12	-0.6	0.17	0.92	1.36
D_{i0} x Target	-10.0	0.000	-0.028	-3.5	-0.129	0.118	0.7	0.065	-0.040
x Excess wage	-0.44	0.00	-0.28	-0.13	-1.39	0.84	0.03	0.51	-0.35
D_{i1} x Target	-70.8*	-0.128	-0.223	-34.4	-0310**	0.216	-46.9	-0.004	-0.279
x Excess wage	-1.92	-0.92	-1.41	-0.99	-2.21	1.17	-1.36	-0.03	-1.59
D_{i2} x Target	-102.5***	-0.053	-0.413**	-52.9	-0.202*	0.048	-79.201**	0.037	-0.436**
x Excess wage	-2.68	-0.49	-2.27	-1.40	-1.81	0.24	-2.14	0.27	-2.27
D_{i3} x Target	-125.9***	-0.048	-0.557***	-50.1	-0.144	0.034	-115.618***	0.042	-0.678***
x Excess wage	-2.90	-0.470	-2.70	-1.17	-1.21	0.14	-2.65	0.33	-3.29
D_{i4} x Target	-129.1***	-0.101	-0.562**	-77.133*	-0.344**	0.034	-101.21*	0.105	-0.683***
x Excess wage	-2.61	-0.80	-2.47	-1.65	-2.11	0.14	-1.95	0.78	-2.92
Observations	727,724	658,584	727,724	365,595	336,725	365,595	361,679	321,859	361,679

Table OA16: Hedonic wage regressions and labor-outcome effects for all medications (continued).

Name	(1) ATC classification	(5) Hedonic wage Regression	(6) Triple-difference regression
Antiinfectives for systemic use	J		
antibacterials for systemic use	J01	1.89***	0.350
		47.88	0.22
other antiinfectives for systemic use	J02, J04-J07	-0.286***	-2.521
		-3.45	-0.80
Immune system	L		
antineoplastics and immunomodulating agents	L01-L04	-1.57***	5.618
		-5.70	1.01
Musculo-skeletal system	M		
musculo-skeletal system	M01-M04, M09	-0.619***	-4.031**
		-17.67	-2.32
bone diseases	A12A, M05	-5.675***	-18.653**
		-14.56	-2.41
Nervous system	N		
antidepressant	N06A	-9.05***	-8.059**
		-58.98	-2.18
opioids	N02A	-1.397***	-3.172
		-16.54	-0.72
other nervous system	N01-N07, ex N02A, N06A	-2.226***	-5.609**
		-11.34	2.48
Antiparasitic products and insecticides	P		
antiparasitic, insecticides and repellents	P01-P03	0.042	2.660
		0.45	0.60
Respiratory system	R		
obstructive airway diseases	R03	-0.311***	0.848
		-4.58	0.41
other respiratory system	R01, R02, R05-R07	0.669***	-0.271
		7.14	-0.19
Sensory organs	S		
ophthalmologicals	S01	0.793***	2.541
		11.83	1.28
otologicals	S02	0.998***	4.194
		11.01	1.23
Various	V		
various	V01, V03, V04, V06-V08	-0.202	3.118
		-0.96	0.34
Average		-0.85	-1.51

Table OA16: Hedonic wage regressions and labor-outcome effects for all medications, including controls for education. For each medication type presented in column 1, the subsequent columns contain the following information: column 1 shows the ATC classification codes (see Norwegian Institute of Public Health, 2017); column 2 shows the average of the yearly estimates of $\gamma_{h,t}$ in equation (3), estimated for the Dutch workforce with data on education (roughly three million observations per year) for each year from 2006 to 2012, where the t-statistics are computed as the average coefficient divided by the standard error of the annual coefficient estimates; column 3 shows estimates of *Daily wage* for the period $k = 4$ triple-difference coefficients η_{4h}, using the medication group h shown in Column 1 as a risk factor in equation (2), where the risk factor RF^f is measured in the year prior to the buyout announcement. The regressions in column 6 are estimated using 727,724 observations and contain individual and year fixed effects. Standard errors are clustered at the firm level. t-statistics are provided below the coefficient estimates. *, **, *** indicate significance at the 10%, 5% and 1% level, respectively.

Name	(1) ATC classification	(2) Hedonic wage Regression	(3) Triple-difference regression
Alimentary tract and metabolism	A		
diabetes	A10	-4.911***	-3.714
		-36.03	-0.70
digestive and obstipation	A02, A03, A06	-0.315***	0.088
		-6.15	0.04
other alimentary tract and metabolism	A01, A04, A05, A07,	0.573***	-3.940
	A09, A14, A16	10.84	-1.01
vitamins and antianemic preparations	A11, A12B, B03	-1.462***	-1.929
		-7.73	-0.43
Blood and blood forming organs	B		
blood and blood forming organs	B02, B05, B06	0.874***	-0.698
		5.05	-0.16
Cardiovascular system	C		
cardiovascular	B01, C01-C03,	-1.785***	-11.230***
	C07-C10	-5.89	-4.00
other cardiovascular system	C04, C05	-0.006	0.087
		-0.04	0.02
Dermatologicals	D		
emolients, protectives, wounds and ulcers	D02, D03	-0.719***	1.619
		-13.47	0.48
other dermatologicals	D01, D04-D11	0.335***	0.259
		6.39	0.19
Genito urinary system and sex hormones	G		
genito urinary system and sex hormones	G01-G04	4.807***	5.183
		8.89	1.55
Systemic hormonal preparations	H		
systemic hormonal preparations	H01-H05	-1.753***	-1.187
		-15.48	-0.38

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