Private Equity Buyouts and Employee Health

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We are grateful to Jean-Noel Barrot, Vahid Moghani, Sara Rellstab, Joacim Tåg, Felix Zwart, and seminar participants at Bayes Business School (City University, London), Erasmus University Rotterdam, Tilburg University, the Technische Universität München, the 7th HEC Paris Workshop on "Banking, Finance, Macroeconomics and the Real Economy," the 1st European meeting of the Labor and Finance Group, the 35th congress of the European Economic Association, and the Frankfurt School of Finance and Management for helpful comments and discussions.

Abstract

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

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

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Abstract

We relate employees' health to their productivity to analyze the restructuring of the labor force during Private Equity buyouts using employee-level data of 56,000 Dutch buyout employees. Employees with a worse health status before the buyout face the most substantial losses of income and employment. Health characteristics associated with lower wages in the general population are strongly predictive of job loss after buyouts. Consistent with the notion that state-level insurance substitutes for firm-level insurance, more than half of the negative effect of buyouts on employees' incomes is buffered by social transfers, and this buffer is larger for employees in poor health. We find no evidence that buyouts worsen employees' health.

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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. Our most important finding is that employees in poor health are more likely to leave target firms. We test two mutually non-exclusive hypotheses to analyze the two-way relationship between buyouts and employees' health. First, we build on the literature that links PE buyouts to operational improvements and increased firm productivity (see Kaplan and Stromberg, 2009 for a survey). We argue that buyout firms may remove employees with low productivity. Since health has been shown to be highly correlated with employees' productivity, our data can reveal such a selection of employees and allow us to study this channel. Prior literature has established a direct link between buyouts and firm productivity (e.g., Davis et al., 2014) but does not have access to data on employee characteristics that permit analyzing the channel through which firm productivity is improved. By contrast, the literature on post-buyout restructuring with access to individual-level data does not address employees' productivity (e.g., Agrawal and Tambe, 2016; Olsson and Tag, 2017; Antoni, Maug, and Obernberger, 2019). Second, when buyout firms streamline operations and strengthen incentives, they may 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. We use novel data to disentangle these selection and treatment effects of private equity buyouts, and to better understand the channels through which PE-led restructuring affects the target's workforce.

To investigate these questions, we match buyout transactions to an integrated employer-

[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.

employee data set, and to indicators for the health outcomes of individual employees. First, we document a significant loss of employment and income for all buyout employees, suggesting that PE-led restructuring of the labor force in the Netherlands is similar to what has been documented for other countries. Next, we show that a disproportionately larger part of the losses falls on buyout employees in poor health. Then 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. We use these estimates to construct a measure of the excess wages buyout employees would earn if their wages did not properly reflect this equilibrium benchmark, following a logic similar to related arguments in the literature on executive compensation (Core, Holthausen, and Larcker, 1999). We then show that the risk of job losses after buyouts increases with this measure. In addition, we show that employees in poor health have a higher likelihood to exit the labor market after buyouts, and that state-level social transfers substitute for firm-level employment insurance for this group (Ellul, Pagano, and Schivardi, 2018). Finally, we show that buyouts have no measurable impact on employees' health.

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 wages, and find that the impact is significant: Buyout employees lose about €1,300 of earnings per year by the fourth calendar year after the buyout, almost half of which is replaced through transfers of the social security system. In the next step, we investigate whether the losses of jobs and income affect employees in poor health more. We find this to be the case, and the effect is large: In addition to the baseline loss experienced by all buyout employees, employees on cardiovascular medication lose another €2,500 per year, and those on antidepressants another €2,000 per year in the fourth year after the buyout relative to matched control employees. Many of the affected employees exit the labor market and receive larger social security transfers compared to healthy employees.

Prior literature shows that employees in poor health are less productive (e.g., Currie and Madrian, 1999; Contoyannis and Rice, 2001). Similarly, the literature on private equity buyouts shows that buyout firms implement superior management practices (Bloom, Sadun, and Van Reenen, 2015) and raise productivity, among other things, through operational improvements (e.g., Kaplan and Stromberg, 2009). We hypothesize that one channel through which buyout firms effect operational improvements is 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.)

Next, we show that health influences post-buyout employment outcomes because health affects individual-level productivity by applying a two-step procedure In the first step, 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 wage 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. The estimate of this excess wage is €3.17 per day on average, with a highly skewed distribution, which indicates that some employees of buyout firms earn significantly more than what would be in line with their productivity. 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 extend this analysis to 25 groups of medications for which we have data and find a remarkably strong correlation: If buyout employees are prescribed medications that are associated with a negative impact on the wages of the entire Dutch workforce, then these employees 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.

Next, we ask to what extent earnings losses of buyout employees are mitigated by the state's social security system. In particular, we expect that employees in poor health have more comprehensive access to insurance by the state and that insurance through social transfers may substitute for insurance through implicit contracts at the firm level (Ellul, Pagano, and Schivardi, 2018). 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. Consistent with this perspective, 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 ask whether buyouts have a negative impact on employees' health. Based on prior literature, we hypothesize that buyout-related restructuring has a negative impact on employees' health by creating a more 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 negative 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., 2021) leads to more turnover of the labor force than PE-induced restructuring (Antoni, Maug, and Obernberger, 2019).

It is also well established in the health economics literature that job loss is associated with negative health outcomes (see 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. Career paths after the buyout are endogenous and we are careful not too draw any causal conclusions from the effects of career path changes on health. However, the results suggest that the consequences of PE buyouts on health hinge on the ratio of employees who switch jobs to employees who become unemployed.

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 contribute to the literature on buyouts by analyzing which employees leave the target firm, and by showing how the selection of employees who leave buyout targets can be related to measures of labor productivity. To the best of our knowledge, ours is the first study to establish this link between health and productivity, and to show how measures of productivity predict the selection of employees and their post-buyout career paths. Furthermore, we add to the literature on risk-sharing within firms and implicit contracts by documenting to what extent insurance provided through the social security system substitutes for firm-level insurance and whether it mitigates buyout-related losses in human capital. This finding may explain why employees in poor health are more likely to leave. Finally, 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.

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.

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.[2] 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 that address the heterogeneity of effects across groups of employees only recently, and only two studies use administrative 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 are confined to data on employee characteristics such as education, qualification, and occupational codes. To the best of our knowledge, our study is the first to provide evidence on how buyout firms' affect employee departures from their targets, and how these departures are related to a measure of employees' productivity. In addition, we contribute to this literature by adding health as an additional outcome that measures how buyouts are associated with employees' well-being. 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. (2020) 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

[2]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 (2007b), 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.

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.

Insurance of employment risk. The literature on employment insurance through implicit contracts builds on the theories of Baily (1974) and Azariadis (1975) (see also Rudanko, 2011; Berk and Walden, 2013). They argue that optimal risk-sharing in the firm entails that firms protect workers against shocks to their productivity, and that workers accept lower wages in return for employment protection. As a result, dynamic wage contracts do not adjust wages to adverse shocks to workers' productivity, and workers tend to earn wages in excess of their productivity over the life-cycle of long-term dynamic wage contracts (Harris and Holmstrom, 1982; Thomas and Worrall, 1988). The empirical literature supports these predictions in many contexts. Sraer and Thesmar (2007) provide evidence for wage insurance in family firms, and Kim, Maug, and Schneider (2018) show that parity-codetermined firms provide employees with insurance against industry-wide shocks. Several papers suggest that conglomerates and business groups operate internal labor markets, which help implementing employment insurance (Cestone et al., 2017; Faccio and O'Brien, 2020). The work most closely related to ours is Ellul, Pagano, and Schivardi (2018), who 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. We contribute to their findings by showing how the reduction in firm-level insurance after buyouts is substituted for 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 permanent losses to their productivity.

Restructuring and health. The literature on the health consequences of restructuring events is 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 (2011) 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.[3] The standard way to address the endogeneity between job loss and health is to

[3]There is also a small literature on how adverse effects spill over to the dependents of employees. Lindo

use exogenous variation from plant closures. Using this identification strategy, the findings are inconsistent across studies. Some studies find that job loss due to plant closure worsens employees' health, while others find no health effects.[4] A related strand of literature focuses on how job insecurity and work stress affect employees' health.[5] 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, Kárpáti and Renneboog (2021) 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.

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).

(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.

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

[5]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).

3.1 Sample construction

We assemble a list of 366 Private Equity Buyouts in the Netherlands for the period 2007-2013 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.

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

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

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.[7]
- **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,

[7] 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.

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 to measure the health status of the employees before and after a buyout. 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 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.[8] 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 expen-*

[8] 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.

ditures). Unfortunately, we only observe information on total health expenditures from 2009 onward. We first focus on *Antidepressant*, *Cardiovascular* and *Total medication*, and later extend the analysis to a broader set of health measures by grouping the detailed information on drug prescriptions into 25 categories. We use other variables (*Digestive, High medication,* and *Health expenditures*) for robustness. The first two columns of Table 6 define these additional health variables and A.2 provides additional information on the exact definition of the different variables.

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.

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). Unsurprisingly, our sample is healthier than the average Dutch population. For example, 4.3% of our sample are prescribed antidepressants compared to around 5% of Dutch men and 9% of women aged

35 to 44 in 2016 (Statistics Netherlands, 2016).[9] In the Online Appendix, we furthermore provide histograms of our main dependent variables (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:

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

In (1), Y_{ik} denotes the outcome variable in levels (labor market outcomes, social insurance, or health outcomes), α_i and γ_t are, respectively, individual and calendar-year fixed effects, i indexes individuals, t indexes calendar time, and k indexes event time, where $k = 0$ is the year in which the buyout takes place. The event-time dummy variables D_{ik} begin two years before the buyout ($k = -2$) and end four years after the buyout ($k = +4$). Our data cover all individuals from two years before to four years after the event and the dummies for the year before the event ($k = -1$) are omitted; hence, all event-time effects are measured relative to the year before the buyout.[10] The dummy variable $Target_i$ distinguishes employees of PE buyout targets from employees in the matched sample ("controls") and equals one for target employees in all sample years. We cluster standard errors at the firm level.

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

[10] 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)

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 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.

Figures 1 to 3 plot the outcome variables *Earnings*, *Daily wage* and *Days employed* from $k = -5$ to $k = +4$. They show parallel trends from *k=-5* to *k=-1* for all three outcomes.[11] This evidence also suggests there is no anticipation of the buyout, as none of the outcome variables seem to deviate from initial 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.[12]

[11] Despite parallel trends before the buyout, trends could 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.

[12] See Figure 3A in Davis et al., 2014 or Figure 3 in Antoni, Maug, and Obernberger (2019) for similar effects. We also implement the estimator proposed by Sun and Abraham (2020) that 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.

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^f, the risk factor, which measure by how much the outcome of interest differs between a target employee characterized by risk factor RF^f from control employees with the same risk factor, compared to the difference between other target and control employees who are not characterized by this risk factor.

4 Baseline analysis: labor-market effects of buyouts

This section presents the analysis on the relationship between buyouts and employment outcomes. Section 4.1 presents the baseline analysis, and Section 4.2 shows how this relationship depends on employees' pre-buyout medication status.

4.1 Baseline analysis: Income and employment

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

(3), respectively.[13]

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 declines in income after buyouts in their US sample, whereas the results from earlier studies are more mixed (Lichtenberg and Siegel, 1990; Amess and Wright, 2007a); however, none of these studies use individual-level data.

Employment and wages. Columns (2) and (3) of Table 4 decompose the results for *Earnings* into a wage component (*Daily wage*, column (2)) and an employment component (*Days employed*, column (3)). There is a statistically significant decline in employment of about two to four days in years $k = 1$ and $k = 2$, and about five days, or 1.9% of annual pre-treatment employment, in $k = 3$ and $k = 4$. By contrast, there is no significant decline in *Daily wage*, hence the decline in income should be attributed to reductions in employment, but not to reductions in wages. The findings for wages parallel those of Antoni, 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 $k = 4$ and slightly higher in the year before, much higher than

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

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,* a slight and barely significant increase in *Daily wage*, and an insignificant decline in *Earnings.* High-wage employees experience almost exactly the same decline in *Days employed* as low-wage employees, but a significant reduction in *Daily wage* of about €6.28 to €7.71 in the second to fourth year after the buyout, which 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.*[14]

4.2 Prior medication and income

In the next step, we test the hypothesis that employees in poorer health experience larger reductions in income and employment than healthy employees. There is significant evidence that adverse health events, like severe illnesses, strokes, or accidents, have a negative effect on employees' incomes.[15] While buyout firms cannot observe medical health records, which are confidential, they can observe employees' productivity and sick leave.[16] Hence, we hypothesize that productivity and health are negatively correlated. If the target firms' new

[14]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.

[15]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.

[16]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.

owners restructure the workforce in an effort to raise productivity, they will likely lay off the less productive workers, and we should therefore observe higher losses of income and employment for workers in poorer health. In this section, we provide a direct test of the relationship between health and labor-market outcomes, which takes the connection between productivity and health for granted. In the next section, we offer a more complete analysis of both parts of our hypothesis.

To test how employees' health status influences the 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 medications related to health conditions that have been found previously to be related to a loss in productivity (see Section 3.3 and Appendix 3.3): *Antidepressant* and *Cardiovascular*. In addition, we use *Total medication*, which is equal to the total number of distinct prescriptions recorded for an individual in a given year. 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 (see Table OA2 in the Online Appendix).[17]

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

[17] 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.

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

5 Productivity, health, 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 without showing the channel for this link. In this section, we hypothesize that health influences employees' productivity, and that less productive employees experience worse labor-market outcomes.

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' productivity in the later stages of their employment relationship (Harris and Holmstrom, 1982; Thomas and Worrall, 1988).[18] 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.

To investigate this hypothesis, we employ hedonic wage regressions, which have been widely used in the literature to establish the relationship between wages, job characteristics, and employee characteristics; we add medication intake as a time-varying employee characteristic to the standard regression setup.[19] 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, which is observable to the employee and to the researcher through the data we analyze here, but not to employers, is an important component of productivity. Hence, our hypothesis predicts a systematic relationship between

[18] 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).

[19] 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.

the impact of medication intake on equilibrium wages in the general workforce.

We follow Kniesner and Leeth (2010) and the literature they cite and perform the following regression of wages on employee and firm characteristics on the entire population of Dutch employees on which we have administrative data for our sample period:

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

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.[20] 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. For example, characteristic h may refer to cardiovascular medication; if employee i takes such medication in year t, then $H_{i,h,t} = 1$. We group all medications we have data on into 25 different groups. Table 6 shows how we map the different drugs into 25 groups of medications (columns (1) and (2)), the prevalence of consumption for each type of medication in the pre-buyout year in, respectively, the Dutch workforce (column (3)), among buyout employees (column (4)), and among control employees (column (5)), weighted by the number of buyout employees in a given year. Comparison of the prevalence of medication intake across groups shows that buyout employees are slightly healthier than the Dutch population across all categories of medications (e.g., see "muscoloskeletal sytem" or "other nervous system"). However, the distributions of medication intake are identical for buyout employees and for control employees.

The vector $X_{i,t}$ in equation (3) describes the job of employee i in year t with a range of occupational and employee characteristics. Specifically, 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, and indicators for ethnicity.[21] Our annual regressions include approximately ten million employ-

[20] The large sample size and the computational power provided by CBS prevent us from estimating panel regression models.

[21] The first three digits of the postal code identify small cities and districts of large cities.

ees on average and explain about 60% of the variation in our dependent variable, *Daily wage*. We depart from the literature on hedonic wage regressions cited above (footnote 19) 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.[22]

Column (6) of Table 6 shows the time-series averages $\frac{1}{T}\sum_t \widehat{\gamma}_{h,t}$ of the estimates for the medication coefficients γ_h on $H_{i,h}$ in (3), which are run separately for each calendar year from 2006 to 2012; 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 15 types of medication and significantly positive coefficients for eight types; for two types of medications, coefficients are insignificant, and both have a prevalence of only 1% in all groups (columns (2), (3), and (4)). Hence, not all types of medications indicate a negative impact on productivity. We observe the strongest (and negative) impact of medication intake on the daily wages of the Dutch workforce for antidepressants (-€8.86), drugs to treat bone diseases (-€5.59), and diabetes (-€4.40).

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 calculated as follows. First, we predict the wage $\widehat{w}_{j,t}$ of each target and control employee j based on the coefficient estimates obtained from running regression

[22]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 OA4 in the Online Appendix).

(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:

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

Accordingly, *Excess wage* estimates the predicted wage loss associated with medication consumption, based on estimates for the entire Dutch population. 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).

Next, we perform triple-difference analyses based on equation (2) with *Earnings*, *Daily wage*, and *Days employed* as dependent variables, and use *Excess wage* as a risk factor in regression (2), i.e. we interact the event-time dummies with the target indicator and *Excess wage*. The null hypothesis is that wages in buyout targets are already at their equilibrium level, and that *Excess wage* measures only random deviations of actual wages from predicted wages, e.g., because of unobservable factors not included in regression (4). 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. Table 7 reports the results for the triple interactions for the whole sample in columns (1) to (3). To interpret the results, note that the standard deviation of *Excess wage* is 4.90. It predicts declines in *Earnings* and in *Days employed*, and they are statistically highly significant. The $k = 4$ coefficient for the impact of *Excess wage* on *Earnings* is -€129.1 (column (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.*

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). We hypothesize that the influence of medication on productivity increases with age. In columns (4) to (9) of Table 7, 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, younger employees always find new employment, but sometimes at a lower wage, whereas the opposite holds for older employees.[23]

The observation that the effect of *Excess wage* on our outcome variables is dependent on age is not an issue for our analysis. Moreover, controlling for the effect of age in Table 7 by adding triple interactions with age-related variables may constitute a "bad control" in the sense of Angrist and Pischke (2009). The reason is that age picks up health-related factors that are not covered by our variables. (For example, we do not include hospitalizations,

[23]In Table OA5 in the Online Appendix, we also report results for employees who are 55 years and older and show that the interactive effects of *Excess wage* on employment and earnings are much larger for this subgroup of employees.

or medications that are given only in hospitals, such as drugs to treat cancer.) However, a problem for our analysis would arise if *Excess wage* and our medication-related variables would pick up components of employees' age that are not related to their productivity.[24] We partially address this potential issue already by including *Age* and *Tenure*, as well as their squares in the hedonic wage regression (3). To understand the extent to which such a problem may still affect our results, we perform a number of robustness checks.

First, we perform a triple-difference analysis in which we include both, *Excess wage* and a dummy variable, which equals one for employees who are above the median sample age in the pre-buyout year. We expect that age has a negative influence on labor-market outcomes, since age is related to a range of factors that may reflect employees' productivity, e.g., changes in the ability to adapt to new technological and organizational environments. Our findings in Table 8 confirm this hypothesis: The earnings of older employees decline more compared to younger employees after the buyout, by €2,342 in $k = 4$ (6.6% of median pre-buyout earnings). In Table OA7, we provide the same analysis as in Table 8, but there we add a more granular representation of age by including a dummy variable for each age quartile.[25] Finally, in Table OA6 in the Online Appendix, we report the same results with a cut-off of 55 years to identify employees who become eligible for early retirement. Figures x to x compare the confidence intervals of these three models and graphically demonstrate that there are hardly any differences between them. Note that in these analyses, the estimated effects on the health variables should be regarded as lower bounds on the true effects, because the age variables may not just pick up aspects of productivity that are independent of health, but they may also proxy for health-related factors that are not covered by our health variables. (To the extent that they do, age becomes a "bad control.") Quantitatively, these lower bounds of the true effects are approximately 40% lower than the estimates reported in Table 7, but still statistically significant.

[24] It is difficult to come up with concrete examples here, since such a factor would have to be related to age, not related to productivity, but still influence buyout firms' decision to cut either wages or employment.

[25] In Table OA6, we do not report the results for *Daily wage*, which are always insignificant.

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 7 and in column (6) of Table 6, 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 and the coefficients η_{h4}, which measure long-term employment effects. The correlation is visualized in Figure 7, 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. Note that some medications are associated with positive effects whereas others are associated with negative effects, but the average of the effects across all medications is negative (see bottom of Table 6: mean of η_{h4} is -1.30; mean of γ_h is -1.51).

Overall, these analyses support 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.

6 Labor market exit and employment insurance

A large literature in economics argues that firms and employees optimally enter implicit employment contracts, in which workers receive employment insurance against shocks to their productivity. Since these contracts are implicit, transfers of control in takeovers or buyouts may lead to a breach of such implicit contracts if the new owners do not feel bound by them (Shleifer and Summers, 1988). However, the importance of implicit employment protection for employees depends on the extent to which they may be able to rely on other insurance mechanisms, in particular, on the state social security system (Ellul, Pagano, and Schivardi, 2018). Buyout firms may take the insurance through the social security system into account when they restructure target firms.[26] In particular, they may perceive the political and reputational costs of laying off employees to be smaller to the extent that the employees are covered by the state. Thus, their decision which employees to retain may be affected by the degree to which state-level insurance is available as a substitute for firm-level insurance. Accordingly, we test two connected but separate hypotheses. First, we expect that the same health conditions that have been shown to increase the likelihood of layoffs in the previous section also increase the likelihood of exiting the labor market entirely, because employees may be laid off for two reasons: either they are a poor match for their current employers, which would not prevent them from finding new employment; or they may have suffered negative shocks to their productivity, in which case their opportunities in the labor market are reduced. Health-related conditions arguably fall into the second category, which make a permanent exit from the labor market more likely. Second, we also expect that individuals will accept a permanent exit from the labor market only if it is sufficiently attractive, i.e., if their non-labor income from disability benefits, early retirement benefits, or unemployment benefits are sufficiently high. Hence, our second hypothesis is that employees who exit from the labor market experience high benefits relative to their previous labor-market income.

6.1 Labor market exit and pre-buyout medication status

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

[26] Appendix B provides additional information about the Dutch social security system.

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 that obtain a value of one if an employee's main income comes from employment or self-employment (*Work*), unemployment insurance benefits (*Unemployment*), disability insurance benefits (*Disability*), or (early-)retirement benefits (*Retirement*). Furthermore, we group all the other sources of income, e.g., social assistance, into one category (*Other*).

We begin the analysis of employees' labor market paths 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 9 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). Employees experience a statistically highly significant likelihood of being unemployed, which is around one percentage point in all post-buyout years (column (4)). There is a weakly significant decline by 1.3 percentage points in the probability of working (column (1)). There is no general movement of buyout employees into retirement or disability insurance (columns (2) and (3)).

Next, we test the first hypothesis formulated at the beginning of this section and 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 four variables that measure health as risk factors: the three health outcome variables used in Table 5, and *Excess wage*, as defined in the previous section. We report the results in Table 10, which has four 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 four 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 Table 9, and only report the coefficients for $k = 2$ and $k = 4$ to conserve space.

Overall, we find significant results for all four health measures. The impact of health on how buyouts affect labor market paths is economically large relative to the baseline effect documented in Table 9. A one-standard deviation (= 4.9) increase in *Excess wage* (see Panel A) increases the probability of not working (i.e., not being either employed or self-employed) by 1.13 percentage points in $k = 4$ (column (1), $= 4.9 \times 0.0023$). The impact of taking either antidepressants (Panel B) or cardiovascular medication (Panel C) is much larger. Buyout employees on these two types of medications are also more likely to receive disability benefits (antidepressants: +2.78, cardiovascular: +1.33 percentage points), unemployment benefits (cardiovascular: +1.25 percentage points), or retire (antidepressants: +1.72 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 significant support for the hypothesis that employees in poor health are more likely to exit the labor market.

6.2 Social transfers versus firm-level insurance

To test the second hypothesis formulated at the beginning of this section, we analyze to what extent the state-run social security system buffers the loss of income for those buyout employees who exit the labor market. We first 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 11 performs difference-in-differences analyses similar to 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 €630 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 (+€313), and this increase is statistically also highly significant, whereas the increases in disability benefits (+€76) and retirement benefits (+190 Euros) are both insignificant. Note that there is a small gap of about €51 (=630-(313+76+190) = 51) 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 12 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 69% (=€2,231/€3,235) for employees on antidepressants and 55% (=€1,937/€3,507) for employees on cardiovascular medication.[27]

Overall, we find support for both hypotheses tested in this section. First, the health indicators that lead to large losses in earnings and employment also increase the likelihood of exiting from the labor market. Moreover, this likelihood does not increase for healthy employees, most likely because their layoffs do not result from a permanent loss of productivity but from being poorly matched to the target firm. Second, the increase in social-security income is much larger for employees in poor health and covers between 55% and 69% of the losses of labor-market income documented in the previous section.

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

7 Restructuring and health outcomes

While the discussion so far has looked at the influence of employees' health on their earnings, employment, and wages, this section investigates the reverse causality from buyouts to employees' health. The literature documents this link and shows how job loss, job insecurity, job-related stress, and other work-related factors impact employees' health (see Section 2). We hypothesize that buyouts create a more demanding and stressful work environment, as a side effect from streamlining operations and strengthening incentives, with the associated negative influence on employees' health. In addition, those employees who exit the labor market may experience a particularly negative impact on their health.

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 4, 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 Panel A and Panel B of Figure 4, 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.[28] Hence, buyouts have no measurable impact on health outcomes. We also test if these effects are zero for the group of unhealthy employees. We investigate this group in Table OA8 in the Online Appendix and confirm that the health status of this group 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 (Kárpáti and Renneboog, 2021).

The analysis in Figure 4 pools all buyout employees, including those who leave the firm after the buyout. However, the hypothesis that buyouts have stress-related negative effects on employees relates only to employees who stay with the buyout firm. Moreover, employees

[28]Table OA9 shows the estimates for the health measures presented in Figure 4 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.

who become unemployed and those who move to other firms may be affected differently. Therefore, we provide a more descriptive exercise and perform 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 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, 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. However, the descriptive analysis in Table 13 is still instructive.

In Table 13, we report the results for the coefficients for $k = 0$, $k = 2$, and $k = 4$, as before. 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 health expenditures, where they are significant and negative. These findings indicate that 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. Finally, the triple interactions reported in Table 13 are all insignificant, which suggests that buyouts are not associated with employees' health apart from influencing their career paths. After controlling for career paths, the health of target employees and control employees is similar, supporting the conclusions from the difference-in-differences analysis in Figure 4.

One of the notable findings from Table 13 is that job changes are associated with *better* employee health. The coefficients on the double interactions of event-time dummies with *Job change* are positive and highly significant for all health outcomes except for antidepressants (column (1)), for which they are economically and statistically insignificant. Hence, job changes are associated with a reduced intake of cardiovascular medication (column (2)), digestive medication (column (4)), overall medication intake (columns (3) and (5)), and health expenditures (column (6)). Similarly, the double interactions of the event-time dummies with *Not employed* are mostly highly significant and positive: *Unemployment* is associated with a deterioration of employees' health, and the estimates have about the same magnitude as the ones for *Job change*, but with the opposite sign.

Hence, Table 13 suggests that employees' health and their medication intake after a buyout is different across career paths. Employees who become unemployed are in worse health, whereas those who change jobs are in better health. We conclude that the zero effects shown in Figure 4 may be driven by potential effects on different groups of employees that cancel each other, and that there is no evidence for effects of buyouts on employees' health beyond the influence buyouts have on their careers.

8 Discussion and conclusion

We study the relationship between private equity buyouts and employees' health and earnings in the Netherlands and document three findings. First, private equity buyouts have a negative impact on the income and earnings of target employees, and these adverse effects are several times larger for employees who are in poor health prior to the buyout, measured by medication intake and other variables, compared to those for healthy employees. We predict 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 identify less productive employees and lay them off with much higher frequency than more productive and healthier employees.

Second, we find that about half of the impact of buyouts is buffered by social transfers through retirement benefits, disability benefits, and unemployment benefits, and this protection is larger for previously unhealthy workers. Therefore, total income losses are cushioned by the Dutch social transfer system. Hence, buyouts induce a partial substitution of firm-level insurance of employment risk by state-level insurance.

Third, and finally, we show that employees' health does not deteriorate after buyouts; the post-buyout development of target employees and control employees is identical. As such, we find no support for the hypothesis that buyouts create a more stressful work environment. This sets buyouts apart from other forms of corporate restructuring, for which adverse effects have been documented.

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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 6 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%.[29] 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

[29]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 Figures

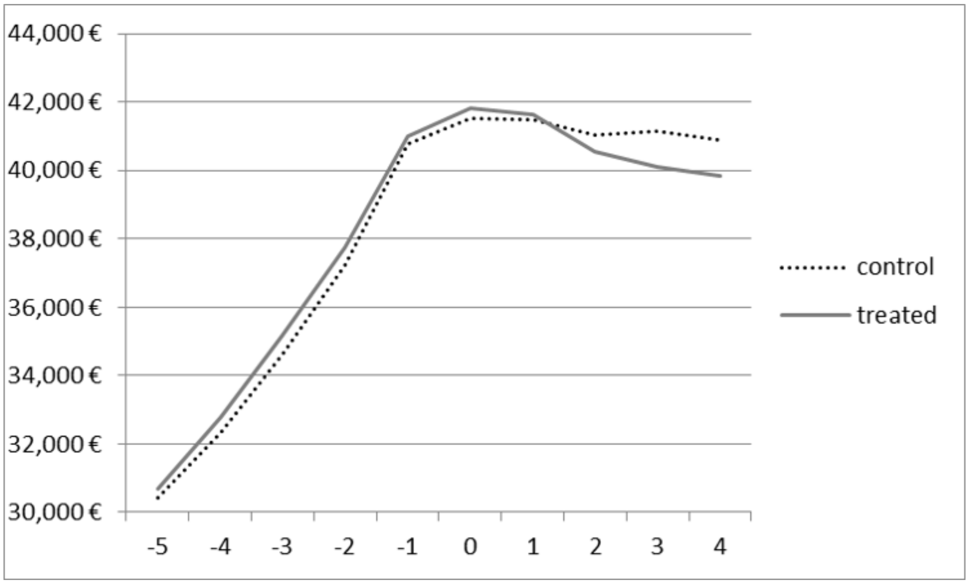

Figure 1: Parallel trends analysis: *Earnings.* This figure presents the development of *Earnings* in event time. For every event year, we compute the mean of *Earnings* for target employees and control employees separately. *Earnings* is defined in Table 1.

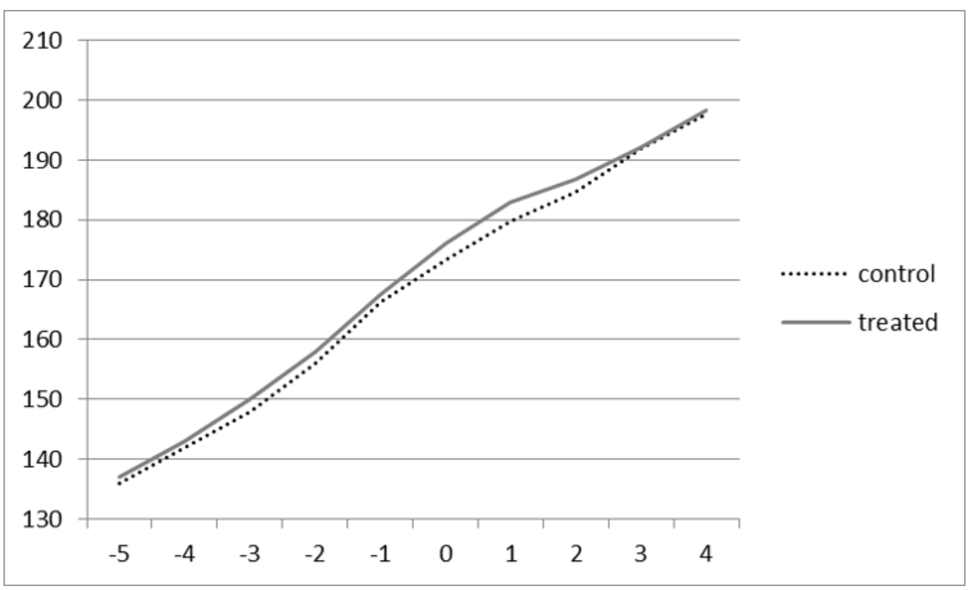

Figure 2: Parallel trends analysis: *Daily wage.* This figure presents the mean of *Daily wage* for target employees and control employees separately. *Daily wage* is defined in Table 1. *Daily wage* is set to missing if *Daily wage* of one firm in a matched pair is missing in a given year.

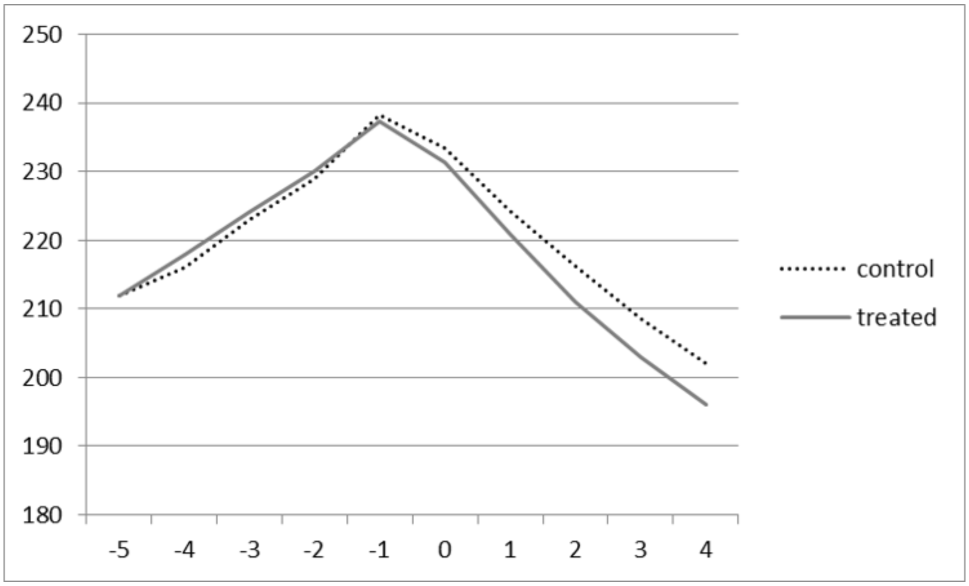

Figure 3: Parallel trends analysis: *Days employed*. This figure presents the mean of *Days employed* for target employees and control employees separately. *Days employed* is defined in Table 1. The inverted-V pattern is a mechanical consequence of the requirement that employees in both groups have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

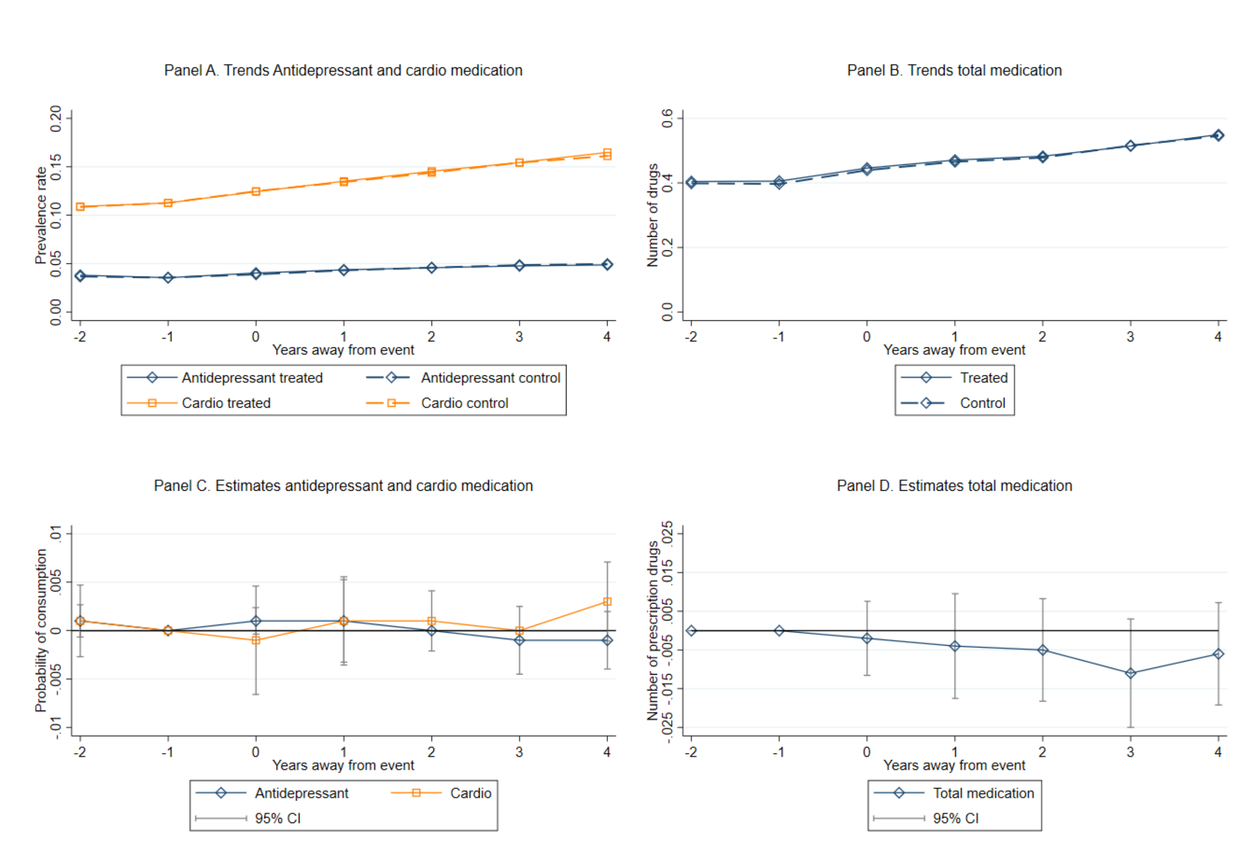

Figure 4: Buyouts and health outcomes. Panel A and Panel B plot the medication intake of *Antidepressant*, *Cardiovascular* (Panel A), and *Total medication* (Panel B), separately for buyout employees and control employees. Panel C and Panel D plot the coefficient estimates of θ_k on the interactions of event-time dummies and the target indicator from the difference-in-difference regressions (equation (2)) *for Antidepressant*, *Cardiovascular* (both Panel C), and *Total medication* (Panel D). The numerical variables are defined in Table 1.

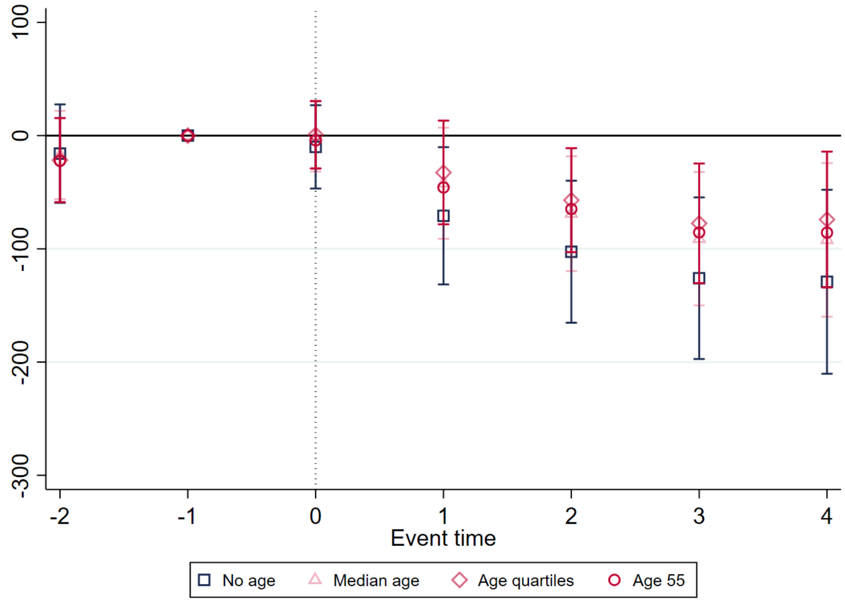

Figure 5: Earnings, excess wage, and age. This figure plots estimates from OLS-regressions on *Earnings* in a triple-difference setup from equation (2). We plot the coefficient estimates of η_k for Excess Wage as well as the 10% confidence intervals for the models presented in Table 8 (no age controls), Table 8 (dummy variable indicating above median employee age) Table OA6 (dummy variable indicating employee age of above 55, and Table OA7 (dummy variable indicating the employee's age quartile). Excess wage and age are determined in $t = -1$. Each specification contains individual and year fixed effects. The numerical variables are defined in Table 1.

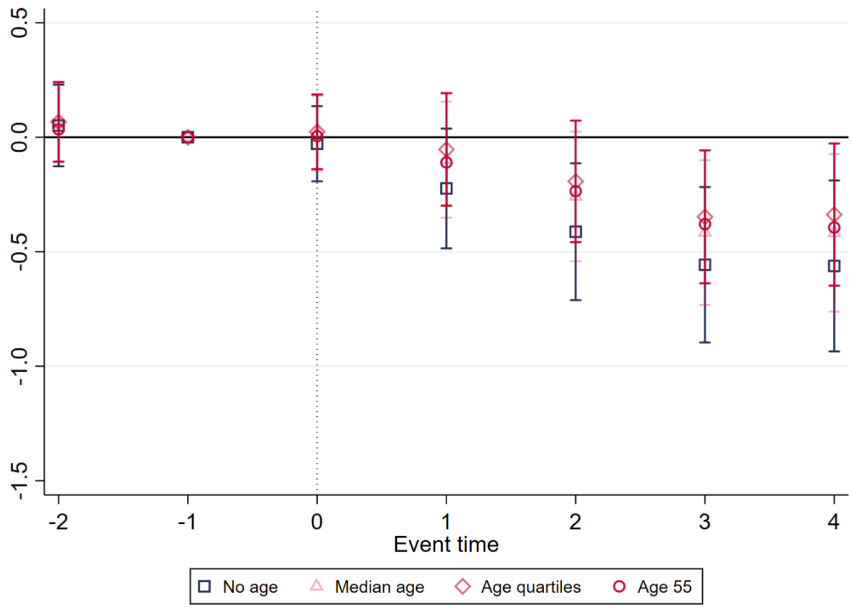

Figure 6: Days Employed, excess wage, and age. This figure plots estimates from OLS-regressions on *Days Employed* in a triple-difference setup from equation (2). We plot the coefficient estimates of η_k for Excess Wage as well as the 10% confidence intervals for the models presented in Table 8 (no age controls), Table 8 (dummy variable indicating above median employee age) Table OA6 (dummy variable indicating employee age of above 55, and Table OA7 (dummy variable indicating the employee's age quartile). Excess wage and age are determined in $t = -1$. Each specification contains individual and year fixed effects. The numerical variables are defined in Table 1.

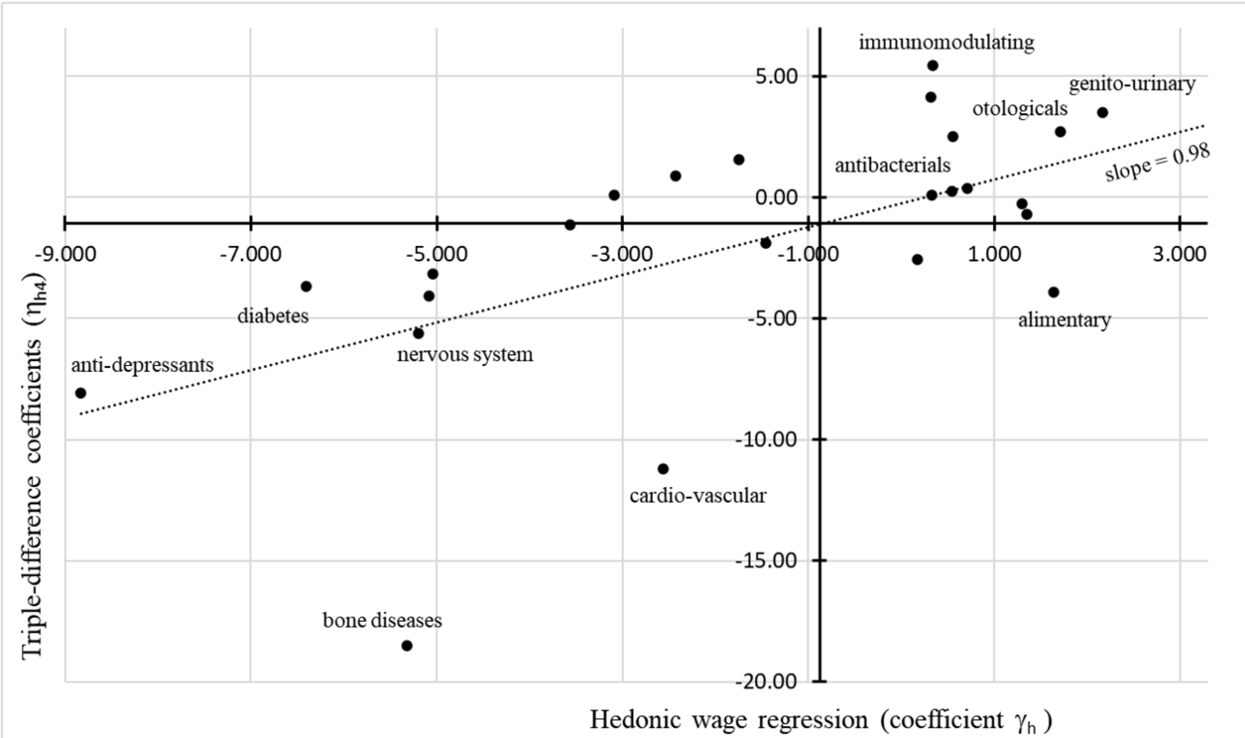

Figure 7: Hedonic wage regressions and employment effects. This figure plots the the average coefficients γ_h from the hedonic wage regressions (3) against the estimates of the coefficient η_{h4} on the interaction $Target_j \times H_{j,h} \times D_{j4}$ from the triple-difference regression (2). Medication types h refer to the 25 medication types listed in Table 6. Table 6 also provides the coefficient estimates from the hedonic wage regression in column 5 and and those from the triple-difference regressions in column 6.

D 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 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(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: Individual matching success. This table presents descriptive statistics on buyout employees and matched control employees. All variables are measured in the year prior to the private equity buyout announcement. The Imbens-Wooldridge statistic (cf. Imbens and 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.

	Earnings	Daily wage	Days employed	Tenure	Age	Firm size	Total medication	Digestive	Anti-depressant	Cardio-vascular	Residual Wage
Panel A. Matched target employees, N = 55,752											
Mean	40,776	166.20	240.95	2,940	40.10	2,999	39.68%	9.01%	3.55%	11.27%	2.88
Median	35,225	142.07	260.00	1,794	39.92	1,006	0.00%	0.00%	0.00%	0.00%	0.00
Variance	7.E+08	9710.00	1670.00	1.E+07	108.00	1.E+07	86.92%	8.20%	3.42%	10.00%	20.73
Panel B. Matched control employees, N = 55,752											
Mean	41,010	167.57	239.51	2,967	40.15	3,635	40.55%	9.01%	3.55%	11.27%	2.90
Median	35,377	143.21	260.00	1,806	39.92	1,371	0.00%	0.00%	0.00%	0.00%	0.00
Variance	7.E+08	9680.00	1683.00	1.E+07	110.00	1.E+07	91.86%	8.20%	3.42%	10.00%	21.01
Comparison to Matched target employees:											
Relative difference	-0.57%	-0.82%	0.60%	-0.94%	-0.12%	-19.15%	-2.17%	0.00%	0.00%	0.00%	-0.69%
Imbens-Wooldridge	0.01	0.01	0.02	0.01	0.00	0.14	0.01	0.00	0.00	0.00	0.00

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

	N	Mean	Median	p1	p99	Standard Deviation
Age	730,754	41.67	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 care	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%
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
Total medication	730,754	0.47	0.00	0.00	5.00	1.06
Total transfers	717,480	2,172.17	0.00	0.00	40,237.00	8,087.33
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: 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) 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) Workforce (%)	(3) Treated (%)	(4) Controls (%)	(5) Hedonic wage Regression	(6) Triple-difference regression
Alimentary tract and metabolism	A					
diabetes	A10	2%	2%	2%	-6.409***	-3.714
					-71.29	-0.70
digestive and obstipation	A02, A03, A06	12%	11%	11%	-3.091***	0.088
					-24.95	0.04
other alimentary tract and metabolism	A01, A04, A05, A07, A09, A14, A16	2%	2%	2%	1.645***	-3.940
					21.35	-1.01
vitamins and antianemic preparations	A11, A12B, B03	2%	2%	2%	-1.463***	-1.929
					-3.44	-0.43
Blood and blood forming organs	B					
blood and blood forming organs	B02, B05, B06	2%	2%	2%	1.353***	-0.698
					5.02	-0.16
Cardiovascular system	C					
cardiovascular	B01, C01-C03, C07-C10	11%	11%	11%	-2.568***	-11.230***
					-18.90	-4.00
other cardiovascular system	C04, C05	1%	1%	1%	0.331	0.087
					1.24	0.02
Dermatologicals	D					
emolients, protectives, wounds and ulcers	D02, D03	3%	3%	3%	-1.752***	1.619
					-20.84	0.48
other dermatologicals	D01, D04-D11	18%	16%	16%	0.545***	0.259
					5.18	0.19
Genito urinary system and sex hormones	G					
genito urinary system and sex hormones	G01-G04	20%	14%	15%	7.223***	5.183
					26.56	1.55
Systemic hormonal preparations	H					
systemic hormonal preparations	H01-H05	4%	4%	4%	-3.568***	-1.187
					-48.28	-0.38

Table 6: Hedonic wage regressions and labor-outcome effects for all medications (continued).

Name	(1) ATC classification	(2) % Workforce	(3) % Treated	(4) % Controls	(5) Hedonic wage Regression	(6) Triple-difference regression
Antiinfectives for systemic use	J					
antibacterials for systemic use	J01	22%	19%	19%	0.706***	0.350
					21.34	0.22
other antiinfectives for systemic use	J02, J04-J07	3%	3%	3%	0.174	-2.521
					1.01	-0.80
Immune system	L					
antineoplastics and immunomodulating agents	L01-L04	1%	1%	1%	0.342***	5.618
					4.53	1.01
Musculo-skeletal system	M					
musculo-skeletal system	M01-M04, M09	20%	18%	18%	-5.084***	-4.031**
					-25.47	-2.32
bone diseases	A12A, M05	1%	1%	1%	-5.318***	-18.653**
					-33.64	-2.41
Nervous system	N					
antidepressant	N06A	4%	4%	4%	-8.831***	-8.059**
					-58.37	-2.18
opioids	N02A	3%	3%	3%	-5.042***	-3.172
					-44.81	-0.72
other nervous system	N01-N07, ex N02A, N06A	10%	8%	8%	-5.199***	-5.609**
					-20.34	2.48
Antiparasitic products and insecticides	P					
antiparasitic, insecticides and repellents	P01-P03	1%	1%	1%	1.716***	2.660
					8.16	0.60
Respiratory system	R					
obstructive airway diseases	R03	7%	6%	6%	-2.427***	0.848
					-43.65	0.41
other respiratory system	R01, R02, R05-R07	16%	16%	16%	1.299***	-0.271
					11.85	-0.19
Sensory organs	S					
ophthalmologicals	S01	8%	7%	7%	0.558***	2.541
					13.25	1.28
otologicals	S02	3%	2%	2%	0.319***	4.194
					6.42	1.23
Various	V					
various	V01, V03, V04, V06-V08	<1%	<1%	<1%	2.164***	3.118
					6.55	0.34
Average					-1.30	-1.51

Table 7: 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 8: Excess wage and age. 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 two risk factors: *Excess wage* and a dummy variable indicating whether the employee's age is above the median age in the sample. Both risk factors are measured in the year prior to the buyout announcement. We only report the coefficient estimates of η_k. Each specification contains individual and year fixed effects. The numerical variables are defined in Table 1. The number of observations is 727,724 (*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)
Dependent variable:	Earnings	Daily wage	Days employed
D_{i-2} x Target x Excess wage	-17.2	0.069	0.063
	-0.72	0.76	0.60
D_{i0} x Target x Excess wage	-0.5	-0.005	0.018
	-0.03	-0.05	0.18
D_{i1} x Target x Excess wage	-42.0	-0.120	-0.098
	-1.41	-0.94	-0.64
D_{i2} x Target x Excess wage	-68.9**	-0.056	-0.260
	-2.24	-0.57	-1.50
D_{i3} x Target x Excess wage	-91.0**	-0.032	-0.416**
	-2.54	-0.33	-2.17
D_{i4} x Target x Excess wage	-92.1**	-0.082	-0.417**
	-2.23	-0.68	-2.00
D_{i-2} x Target x Age > median	-34.2	0.677	-2.578*
	-0.08	0.54	-1.90
D_{i0} x Target x Age > median	-552.8	0.209	-7.016***
	-1.54	0.18	-4.72
D_{i1} x Target x Age > median	-1656.6***	-0.716	-8.745***
	-3.74	-0.39	-5.02
D_{i2} x Target x Age > median	-1972.1***	-0.118	-8.125***
	-4.12	-0.70	-5.03
D_{i3} x Target x Age > median	-2093.8***	-1.312	-8.125***
	-3.63	-0.74	-4.08
D_{i4} x Target x Age > median	-2342.08***	-1.814	-8.668***
	-3.77	-0.89	-4.10

Table 9: Career paths. The table presents estimates from linear probability models of career path indicators in a difference-in-differences setup from equation (1). The binary dependent variables denote the main source of income. We only report the coefficient estimates of θ_k . The numerical variables are defined in Table 1. Each specification contains individual and year fixed effects. The number of observations is 756,658. Standard errors are clustered at the firm level. t-statistics are provided below the coefficient estimates. *, **, *** indicate significance at the 10%, 5% and 1% level, respectively.

	(1)	(2)	(3)	(4)	(5)
	Work	Retirement	Disability	Unemployment	Other
D_{i-2} x Target	0.002	0.001	0.000	-0.001	-0.002
	0.31	0.35	0.15	-0.35	-0.63
D_{i0} x Target	0.0	0.000	0.000	0.002	-0.001
	-0.10	-0.22	-0.54	1.63	-0.48
D_{i1} x Target	-0.009*	0.002	0.001	0.007***	-0.001
	-1.93	1.31	0.98	2.91	-0.44
D_{i2} x Target	-0.011*	0.002	0.001	0.010***	-0.001
	-1.90	0.85	1.07	2.76	-0.62
D_{i3} x Target	-0.013*	0.001	0.002	0.010***	-0.001
	-1.89	0.57	1.43	2.94	-0.26
D_{i4} x Target	-0.013*	0.002	0.002	0.009***	0.000
	-1.78	0.62	1.32	3.10	-0.10

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

	(1) Work	(2) Retirement	(3) Disability	(4) Unemployment	(5) Other
Panel A. Risk Factor = Excess wage					
D_{i2} x Target x Residual	-0.0016*	0.0004**	0.0005	0.0009***	-0.0003
wage	-2.10	2.01	1.23	2.54	-1.27
D_{i4} x Target x Residual	-0.0023***	0.0004	0.011**	0.0010***	-0.0030
wage	-2.65	1.27	2.04	2.83	-0.87
Panel B. Risk Factor = Antidepressant medication					
D_{i2} x Target x Antidepressant	-0.0332**	0.0152***	0.0091	0.0089	0.0000
medication	-2.40	3.74	1.16	1.16	0.01
D_{i4} x Target x Antidepressant	-0.0437***	0.0172***	0.0278***	0.0043	-0.0056
medication	-2.78	2.93	2.62	0.57	-0.96
Panel C. Risk Factor = Cardio medication					
D_{i2} x Target x Cardio	-0.0289***	0.0092*	0.0082**	0.0155***	-0.0040
medication	-3.46	1.90	2.17	3.01	-1.40
D_{i4} x Target x Cardio	-0.0357***	0.0101	0.0133***	0.0125***	-0.0002
medication	-3.02	1.26	2.87	2.63	-0.06
Panel D. Risk Factor = Total medication					
D_{i2} x Target x Total	-0.0092***	0.0022	0.0037*	0.0044***	-0.0011
medication	-2.93	1.31	1.91	2.61	-1.06
D_{i4} x Target x Total	-0.0121***	0.0026	0.0055**	0.0042**	-0.0002
medication	-2.93	1.02	2.40	2.42	-0.19

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

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

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

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

Table 13: Job changes, unemployment, and health. The table presents estimates from OLS-regressions of health variables in a triple-difference setup from equation (2). We run the regression with all observations from $k = -2$ to $k = +4$, but only report the coefficient estimates for event periods $k = 0$, $k = +2$, $k = +4$. Each specification contains individual and year fixed effects. The numerical variables are defined in Table 1. The number of observations is 727,724. Standard errors are clustered at the firm level. t-statistics are provided below the coefficient estimates. *, **, *** indicate significance at the 10%, 5% and 1% level, respectively.

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

E Online Appendix

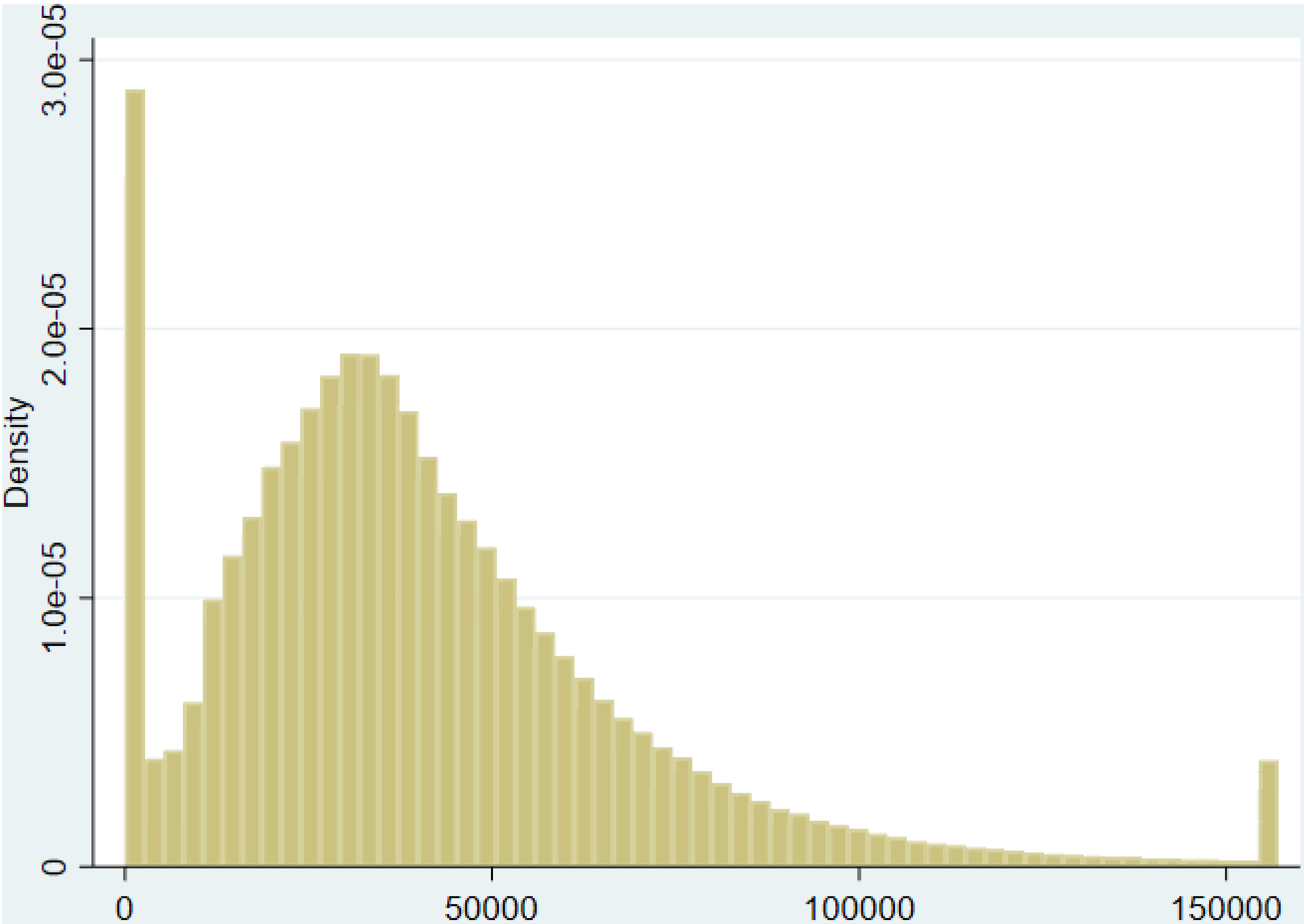

Figure OA1: Histogram of *Earnings*. This figure presents the distribution of *Earnings* for the whole sample. *Earnings* is defined in Table 1.

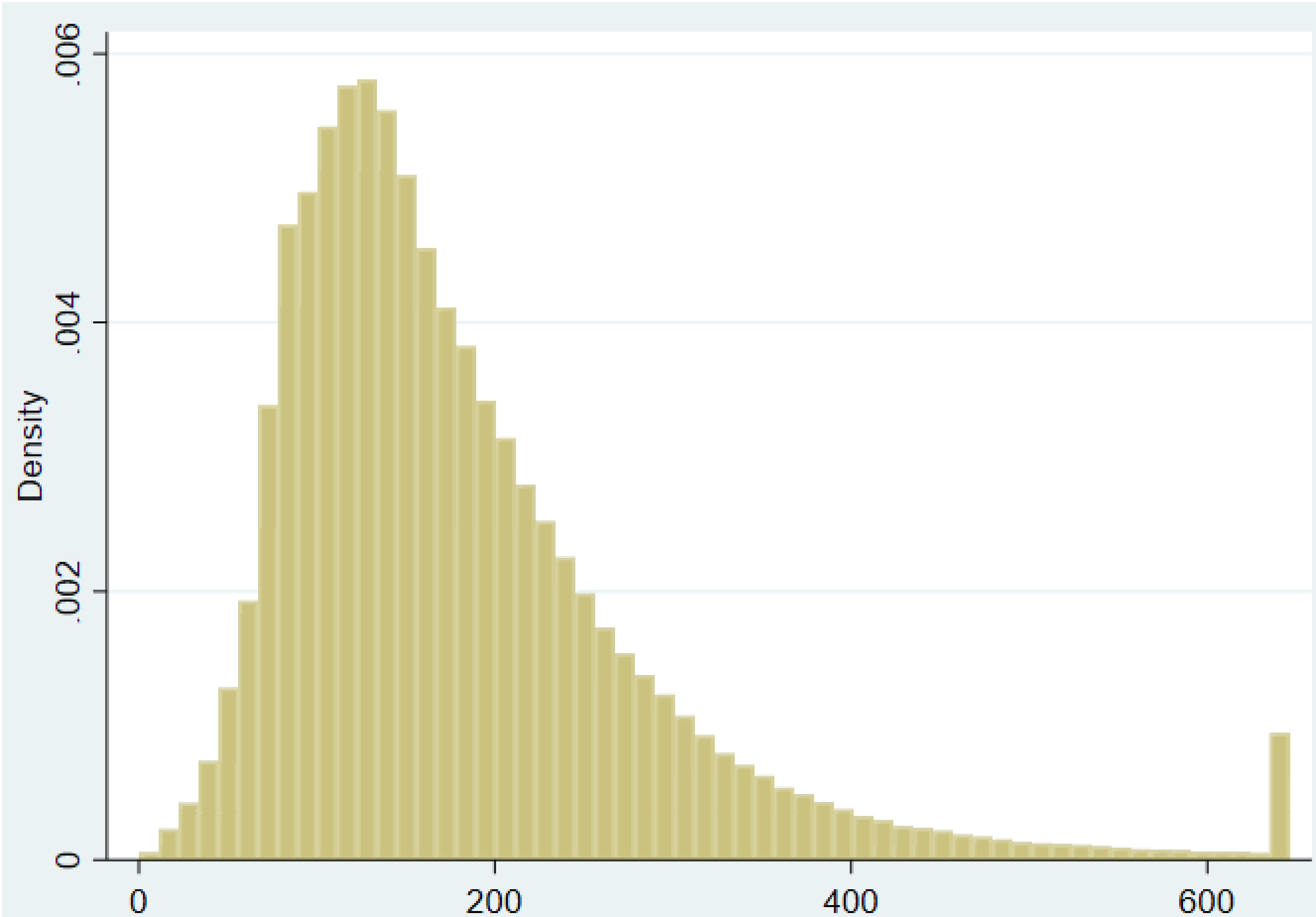

Figure OA2: Histogram of *Daily wage*. This figure presents the distribution of *Daily wage* for the whole sample. *Daily wage* is defined in Table 1. *Daily wage* is set to missing if *Daily wage* of one firm in a matched pair is missing in a given year.

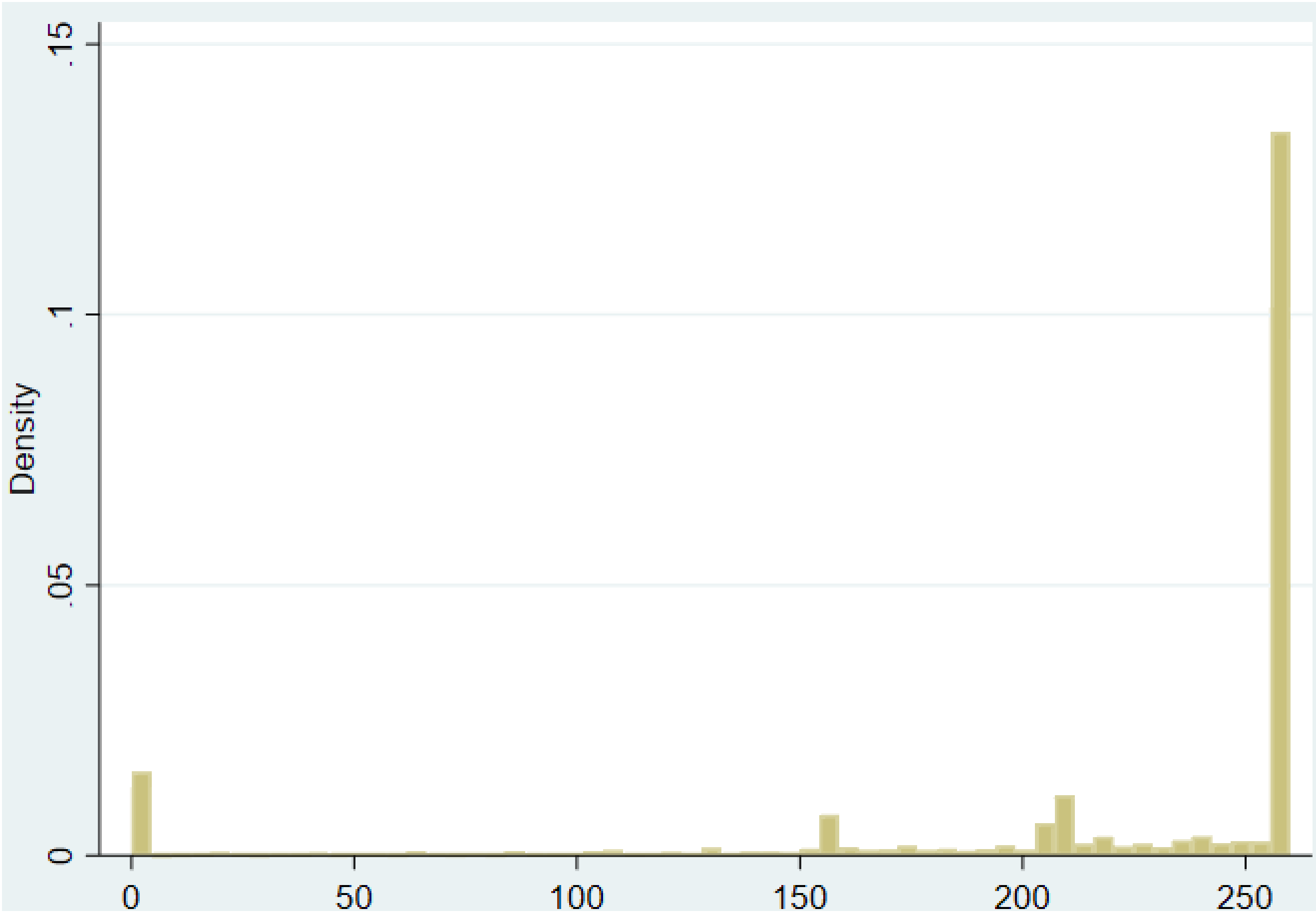

Figure OA3: Histogram pf *Days employed*. This figure presents the distribution of *Days employed* for the whole sample. *Days employed* is defined in Table 1.

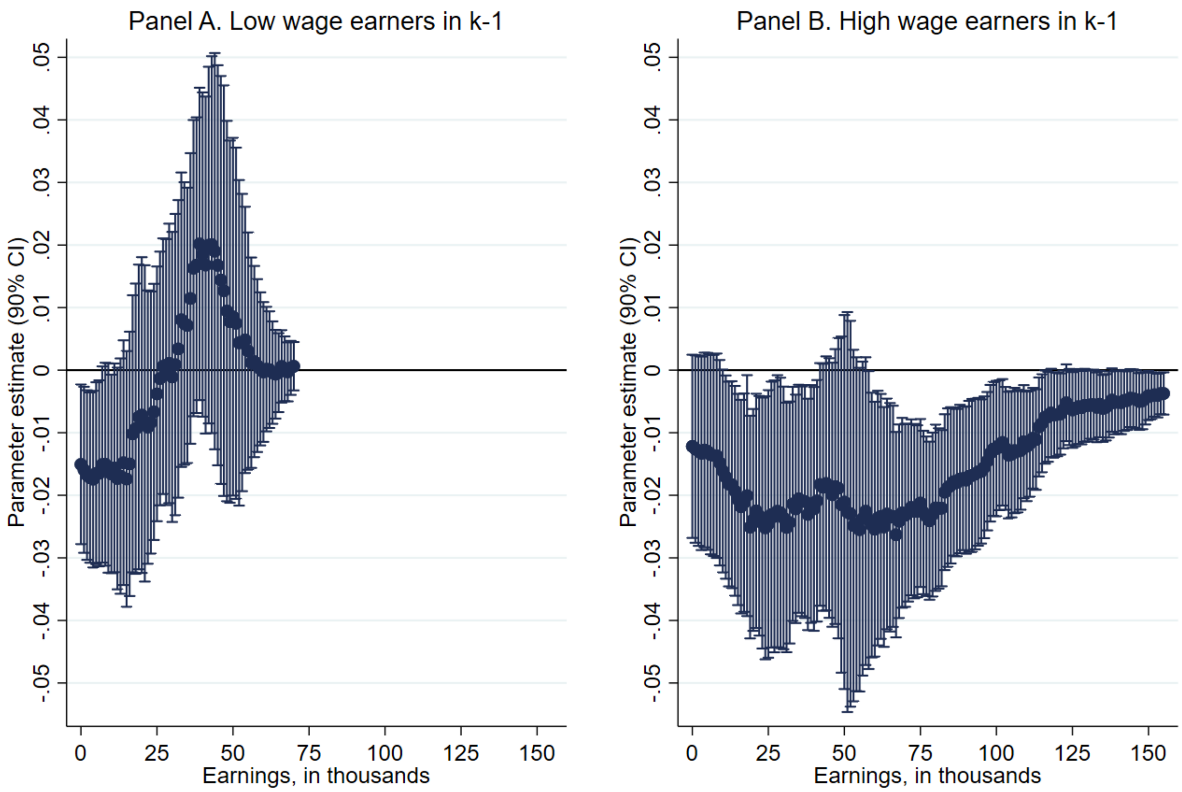

Figure OA4: Distributional regressions of *Earnings* for employees with below-median wages (Panel A) and above-median wages (Panel B). This figure plots the estimated coefficients θ_3^x (see 1) and 90% confidence intervals from a series of linear probability models, in which the dependent variable is 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)

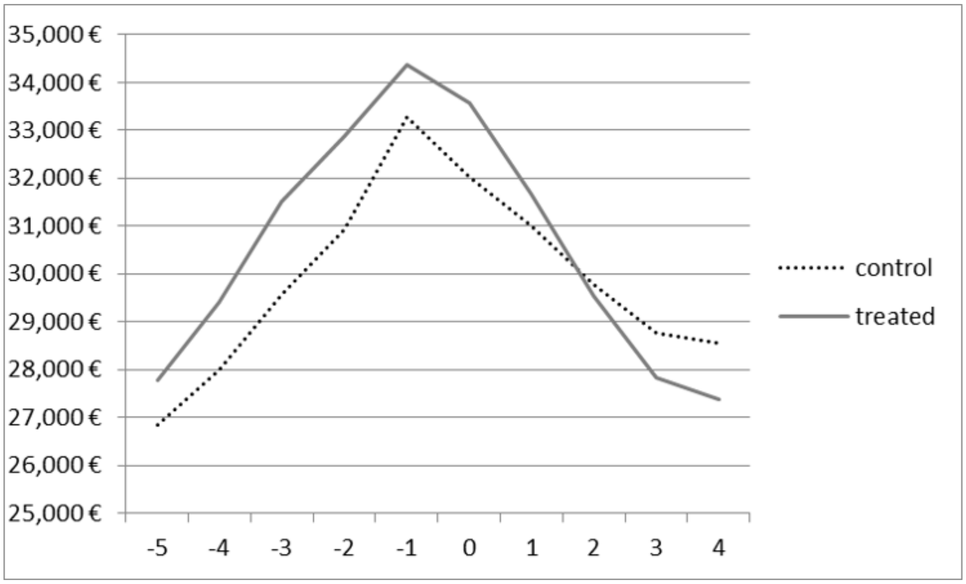

Figure OA5: Parallel trends analysis: *Earnings for antidepressant users.* This figure presents the development of *Earnings* in event time for the subset of employees who were prescribed antidepressant medication in the year before the buyout. For every event year, we compute the mean of *Earnings*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

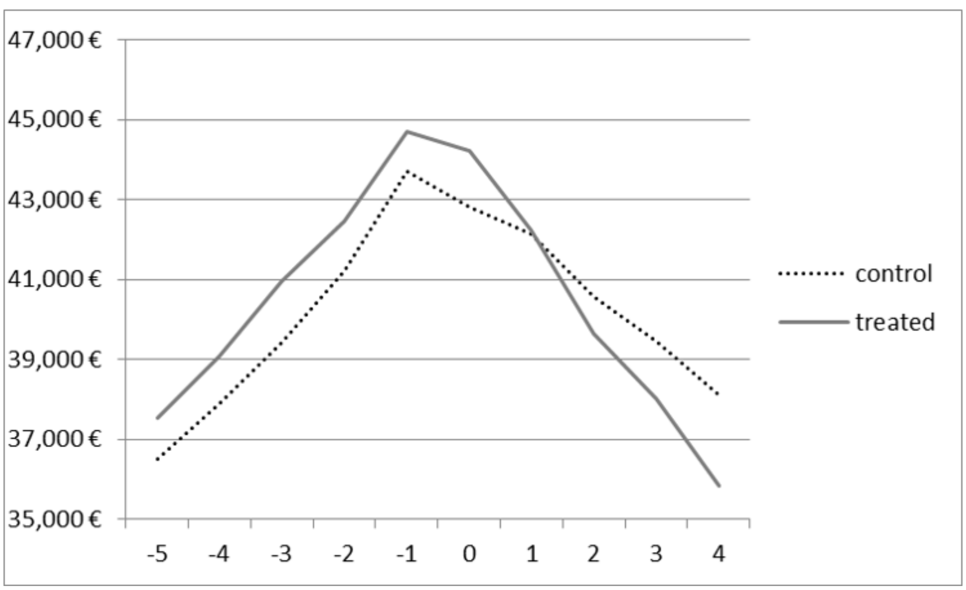

Figure OA6: Parallel trends analysis: *Earnings for cardiovascular medication users.* This figure presents the development of *Earnings* in event time for the subset of employees who were prescribed cardiovascular medication in the year before the buyout. For every event year, we compute the mean of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

Table OA1: Income and employment, accounting for heterogenous 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: 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

Table OA4: 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 OA5: 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 6, we restrict the sample to employees who were at least 55 years old in the year before the buyout. Each specification contains individual and year fixed effects. The numerical variables are defined in Table 1. The number of observations is 80,751 (*Daily wage*: 64,231 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.

Medication used: Antidep, cardio, digestive, total number

	(1) Earnings	(2) Daily wage	(3) Days employed
Sample	Age >= 55	Age >= 55	Age >= 55
D_{i-2} x Target	-10.0	0.079	0.189
x Excess wage	-0.21	0.60	0.97
D_{i0} x Target	-44.0	-0.022	-0.140
x Excess wage	-1.11	-0.14	-0.78
D_{i1} x Target	-79.6	-0.173	-0.426*
x Excess wage	-1.47	-0.99	-1.69
D_{i2} x Target	-147.5**	0.040	-0.686**
x Excess wage	-2.09	0.15	-2.29
D_{i3} x Target	-224.76***	0.085	-1.07***
x Excess wage	-2.85	0.32	-3.31
D_{i4} x Target	-120.1	0.204	-0.806**
x Excess wage	-1.33	0.56	-2.20

Table OA6: Excess wage and age. 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 two risk factors: *Excess wage* and a dummy variable indicating whether the employee's age is above or equal to 55 years one year before the buyout. Both risk factors are measured in the year prior to the buyout announcement. We only report the coefficient estimates of η_k. Each specification contains individual and year fixed effects. The numerical variables are defined in Table 1. The number of observations is 727,724 (*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.

Dependent variable:	(1) Earnings	(2) Daily wage	(3) Days employed
D_{i-2} x Target x Excess wage	-20.308	0.058	0.045
	-0.89	0.67	0.40
D_{i0} x Target x Excess wage	-4.585	-0.007	0.006
	-0.23	-0.07	0.06
D_{i1} x Target x Excess wage	-45.062	-0.131	-0.107
	-1.44	-1.04	-0.73
D_{i2} x Target x Excess wage	-65.536**	-0.073	-0.235
	-2.20	-0.76	-1.53
D_{i3} x Target x Excess wage	-87.436***	-0.060	-0.378**
	-2.71	-0.64	-2.25
D_{i4} x Target x Excess wage	-85.265**	-0.096	0.388**
	-2.41	-0.80	-2.17
D_{i-2} x Target x age >= 55	76.149	2.620	-1.502
	0.08	1.45	-0.55
D_{i0} x Target x age >= 55	-344.910	1.014	-2.95
	-0.61	0.66	-1.38
D_{i1} x Target x age >= 55	-2381.27***	0.757	-11.403***
	-3.52	0.31	-4.15
D_{i2} x Target x age >= 55	-3272.720***	3.117	-16.641***
	-3.73	0.868	-4.85
D_{i3} x Target x age >= 55	-2998.655***	2.005	-15.145***
	-2.44	0.59	-3.29
D_{i4} x Target x age >= 55	-3150.37**	-1.653	-12.684***
	-2.22	-0.47	-2.85

Table OA7: Excess wage and age quartiles. The table presents estimates from OLS-regressions on *Earnings* and *Days employed* in a triple-difference setup from equation (2). Each specification includes two risk factors: *Excess wage* and dummy variable indicating the age quartile. We only report the coefficient estimates of η_k for $k = -2, 0, 2, 4$. 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)	(2)
Dependent variable:	Earnings	Days employed
D_{i-2} x Target x Excess wage	-21.7	0.067
	-0.96	0.64
D_{i0} x Target x Excess wage	0.7	0.024
	0.04	0.24
D_{i2} x Target x Excess wage	-57.0**	-0.356
	-2.04	-0.19
D_{i4} x Target x Excess wage	-74.1**	-0.338*
	-2.03	-1.79
D_{i-2} x Target x Age Q2	441.5	2.061
	0.94	0.81
D_{i0} x Target x Age Q2	-249.7	0.127
	-0.6	0.1
D_{i2} x Target x Age Q2	-693.8	-2.584
	-1.23	-1.42
D_{i4} x Target x Age Q2	-988.7	-3.403*
	-1.43	-1.95
D_{i-2} x Target x Age Q3	200.34	-1.345
	-0.98	-0.98
D_{i0} x Target x Age Q3	-644	-2.019
	-1.23	-1.29
D_{i2} x Target x Age Q3	-1588.2**	-5.738***
	-2.15	-2.83
D_{i4} x Target x Age Q3	-1883.0**	-5.791***
	-2.24	-3.01
D_{i-2} x Target x Age Q4	383.1	-0.421
	0.54	-0.14
D_{i0} x Target x Age Q4	-699.1	-2.964
	-1.21	-1.56
D_{i2} x Target x Age Q4	-2998.4***	-14.113***
	-3.66	-5.12
D_{i4} x Target x Age Q4	-3694.5***	-14.550***
	-3.3	-4.28

Table OA8: 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 OA9: 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

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