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Job Loss, Firm-Level Heterogeneity and Mortality: Evidence from Administrative Data

Hans Bloemen[1]
Stefan Hochguertel[1]
Jochem Zweerink[2]

[1] Faculty of Economics and Business Administration, VU University Amsterdam, and Tinbergen Institute, the Netherlands;
[2] Utrecht University, the Netherlands.

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Evidence from administrative data

Hans Bloemen[a,b], Stefan Hochguertel[a] and Jochem Zweerink[c]

a VU Amsterdam, Tinbergen Institute, Netspar
b IZA Bonn
c Utrecht University

Abstract

This paper estimates the effect of job loss on mortality for older male workers with strong labor force attachment. Using Dutch administrative data, we find that job loss due to sudden firm closure increased the probability to die within five years by a sizable 0.60 percentage points. Importantly, this effect is estimated using a model that controls for firm-level worker characteristics, such as firm-level average mortality rates for mortality during the four years prior to the year of observation. On the mechanism driving the effect of job loss on mortality, we provide evidence for an effect running through stress and changes in life style.

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Keywords: job loss, mortality, treatment effect

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Correspondence should be directed to Jochem Zweerink, Utrecht School of Economics, Faculty of Law, Economics and Governance, Utrecht University, Kriekenpitplein 21-22, 3584 EC Utrecht, The Netherlands. Email: j.r.zweerink@uu.nl.

1. Introduction

Job loss is a frequent event that many individuals experience in their lives. In the United States alone, there were about 850,000 private sector job separations due to mass layoffs in 2012 (Bureau of Labor Statistics, 2013).[1] First-order effects on workers' economic circumstances may be expected due to the associated income loss that can have long-lasting, permanent effects (Jacobson, LaLonde, and Sullivan, 1993; Couch and Placzek, 2010). Job loss, and in particular, layoffs, can be associated with strong effects on people's lives in general and health in particular, through a variety of channels. Workers may get surprised by a sudden career disruption, experience subsequent job instability (Stevens, 1997), have difficulties coping with the change and experience detrimental effects on their life style (Deb et al., 2011; Falba et al., 2005), financial situation (Jacobson, LaLonde, and Sullivan, 1993; Couch and Placzek, 2010), physical (Black, Devereux, and Salvanes, 2015) and mental health (Kuhn, Lalive, and Zweimueller, 2009). In the extreme, involuntary job loss may result in death (Sullivan and Von Wachter, 2009; Eliason and Storrie, 2009; Browning and Heinesen, 2012).[2] We reinvestigate the effect of job loss on mortality, using administrative data from the Netherlands. We find evidence of a sizable positive effect.

Estimating the effect of job loss on mortality is challenging, as job loss is likely to be endogenous to mortality. Studies in the literature estimate the mortality effects of job loss due to firm closure (Eliason and Storrie, 2009; Browning and Heinesen, 2012; Michaud, Crimmins, and Hurd,2014) and job loss in firms experiencing large firm-level employment declines (Sullivan and Von Wachter, 2009). They use workers employed in firms that stay open as the control group and workers laid off in closing firms or firms experiencing large employment declines as the treatment group. Workers employed in closing firms and firms experiencing large employment declines may have poorer health than those employed in firms that do not close, however. These pre-existing differences in firm-level worker health and mortality rates may exist due to selective hiring or worker outflow prior to firm closure or large firm-level employment declines. If these pre-existing differences in firm-level worker health and mortality rates are not controlled for, the estimate of the effect of job loss on mortality may be biased upwards. We control for pre-existing differences in firm-level worker health and mortality rates and other firm-level worker characteristics. To our knowledge, this is the first paper in the literature to do so.

[1] Excluding farm job separations and separations due to seasonal work or vacation periods.
[2] The general economic circumstances that may cause job loss, such as a recession, may have an effect on health as well (Ruhm, 2000).

We use employee-employer matched Dutch administrative data for the period 2003-2010. We estimate the effect of job loss on the probability to die within five years. There are a number of reasons why mortality is an outcome variable of core interest. First, length of life is directly relevant to the experienced welfare of consumers. Second, mortality is an event which is distinctly and objectively observed in our data and does not raise issues of interpretation or subjectivity, unlike, say, self-reported health as available in surveys. We use job loss due to sudden firm closure as the treatment. The sudden nature of firm closures is imposed by the way we select observations. Job loss due to sudden firm closure is a clean treatment, not suffering from selectivity of job loss and unlikely to suffer from anticipation of job loss. The control group in our approach includes observations on workers who stayed on their jobs in the year of observation. The treatment group consists of observations on workers who were laid off in the year of observation specifically because of firm closure. For male workers with strong labor force attachment, we find that job loss due to firm closure increased the probability to die in the first five years after job loss by 0.60 percentage points or 34 percent. This result is in relative terms similar to what Eliason and Storrie (2009) and Browning and Heinesen (2012) find, who do not control for firm-level worker characteristics. We find that without controlling for firm-level worker characteristics, job loss increased the probability to die within five years by 0.83 percentage points or 46 percent. This suggests that not controlling for pre-existing differences in worker characteristics such as mortality rates between workers employed in closing firms and those employed in firms that stayed open indeed biases the estimate of the mortality effect of job loss upwards. Cause-specific mortality analysis shows that cerebrovascular diseases and smoking-related cancers are important drivers of the effect.

A limited number of earlier studies find a positive effect of job loss on mortality as well (Sullivan and Von Wachter, 2009; Eliason and Storrie, 2009; Browning and Heinesen, 2012; Michaud, Crimmins, and Hurd, 2014). Kuhn, Lalive, and Zweimueller (2009), Browning and Heinesen (2012) and Black, Devereux, and Salvanes (2015) find that job loss has a positive effect on hospitalization and a negative effect on health. Browning, Danø, and Heinesen (2006) and Salm (2009), conversely, do not find an effect of job loss on hospitalization or health, however.

What is still not clear in the literature is how job loss affects health and mortality. There is evidence that job loss increases self-harm (Keefe et al., 2002; Browning and Heinesen, 2012), mental illness (Kuhn, Lalive, and Zweimueller, 2009; Browning and Heinesen, 2012), traffic accidents (Browning and Heinesen, 2012), smoking (Black, Devereux, and Salvanes, 2015), drinking (Eliason and Storrie, 2009; Deb et al., 2011; Browning and Heinesen, 2012) and induces having an unhealthier BMI (Deb et al., 2011). There is evidence that job loss affects mortality via reduced lifetime earnings (Sullivan and Von Wachter, 2009) as well.

This paper makes several contributions to this recent literature. First, we use employer-employee matched administrative data that allow us to construct workers' characteristics at the firm-level, such as firm-level average mortality rates and firm-level average hospitalization rates. Controlling for these and other firm-level worker characteristics when estimating the effect of job loss on mortality avoids bias of our result due to workers employed in closing firms having had poorer health than those employed in firms that stayed open.

Second, there is debate about the mechanisms driving the effect of job loss on mortality. We provide new evidence on the health mechanism driving the effect of job loss on mortality as we are explicitly able to exploit information from cause of death registers. These pieces of evidence are complemented by estimates of the effect of job loss on mortality for time horizons of one until five years.

Third, as we have end dates of jobs and month and year of death available, we can measure the time period between the date of job loss and the date of mortality more precisely than studies that have only the year of job loss and the year of death available. This avoids bias of the effect of job loss on mortality due to differences in time horizons whereon mortality is considered.

The remainder of this paper is organized as follows. Section 2 briefly reviews related literature. Section 3 provides background on the data and furnishes insightful descriptives. Section 4 discusses the institutional setting. Section 5 explains the identification strategy and Section 6 presents the empirical results. Section 7 concludes.

2. Literature review

The main point of discussion in the existing literature is how job loss affects health and mortality. Several papers provide evidence for the positive sign of the effect of job loss on mortality, but especially cause-specific (mortality) analyses do not give consistent evidence on a mechanism that is at work. It is not clear why this is the case. We observe considerable differences between studies in terms of data selection, estimation methods, definitions of job loss, definitions and classifications of causes of death and time horizons on which the effects of job loss on mortality are measured. These differences between studies may arguably explain at least part of the differences in results.

Sullivan and Von Wachter (2009), Eliason and Storrie (2009), Browning and Heinesen (2012) and Michaud, Crimmins, and Hurd (2014) all study the effect of job loss on mortality. Similar to us, the first three articles study the effect for workers with strong labor force attachment. They do not focus on older workers, though. Michaud, Crimmins, and Hurd (2014) do focus on older workers, yet not selecting workers based on having strong labor force attachment.

Sullivan and Von Wachter (2009) study the effect of job loss in plants experiencing large employment declines on mortality of high-tenure male workers. They estimate a logit model on administrative quarterly data for workers in Pennsylvania born in any of the years 1920-1959 for the period 1974-2006. They find a 10-15 percent increase in the probability to die for the next 20 years. This implies that the life expectancy of a worker displaced at age forty decreased by one to one and a half years compared to a nondisplaced counterpart if the increases in mortality hazard were sustained indefinitely. The authors find that the positive effect of job loss on mortality was larger for workers with larger predicted earnings declines, suggesting that the effect of job loss on mortality runs through persistent losses in earnings. This is consistent with the authors' finding that the effect of job loss on mortality was smaller for older workers. As unemployment insurance (UI) benefits were generous in the Netherlands and most laid off workers in closing firms received severance pay, we do not expect job loss to have had a large effect on (lifetime) income and to have had a large effect on mortality through income for in particular the older workers we study.

Eliason and Storrie (2009) and Browning and Heinesen (2012) study the effect of job loss due to plant closure on overall and cause-specific mortality on time horizons that are somewhat similar to ours. They use the same definitions of cause-specific mortality categories, except those for the categories alcohol-related mortality and suicide mortality. Eliason and Storrie (2009) employ a Cox proportional hazard model to estimate the effect of job loss due to plant closure in 1987/88 on mortality. They use administrative data for male workers aged 25-64 from Sweden for the years 1983-1999. They find that job loss increased the probability to die within four years by 44 percent for men. The authors do not find an effect for women. Their analysis by cause of death shows that the increase in the mortality probability was driven by suicides and alcohol-related causes of death.

Browning and Heinesen (2012) study the effect of job loss due to plant closure on mortality and hospitalization for male workers with strong labor market attachment. They employ propensity score weighting combined with nonparametric duration analysis on Danish administrative data for male workers aged 20-60 for the years 1980-2006. They find that job loss increased overall mortality, mortality due to diseases of the circulatory system, suicide mortality and mortality and hospitalization due to traffic accidents, alcohol-related diseases and mental illness. They find that job loss increased the risk of overall mortality by 84 percent in the first year after displacement, 36 percent in the first four years after displacement, 17 percent in the first ten years after displacement and 10 percent during the first 20 years after displacement.

The study by Michaud, Crimmins, and Hurd (2014) focusses on older workers and compares the effects of layoffs and job terminations due to plant closure on self-reported health and mortality.[3] Layoffs here include mass layoffs and more individualized layoffs. The authors use biennial survey data for older workers from the Health and Retirement Study (HRS) waves 1992-2008 and HRS biomarker data from the waves 2006 and 2008 for workers who were in the age category 59-70 in 2006-2008. Their study is based on a relatively small number of observations. This does not allow them to estimate the effect of job termination on mortality directly. Instead, in a first step they estimate the effect of job termination on the count of high-risk biomarkers using Poisson models, and next they employ a Poisson model to estimate the effects of the predicted count of high-risk markers, incorporating age and socio-economic characteristics on the probability to die within two years. They find no effect of self-reported job termination due to plant closure on mortality and a positive 9.4 percent long run effect of self-reported layoff on mortality.

Browning, Danø, and Heinesen (2006), Kuhn, Lalive, and Zweimueller (2009), Salm (2009) and Black, Devereux, and Salvanes (2015) study the effect of job loss on health, using different health outcomes. Browning, Danø, and Heinesen (2006) match workers who lost their jobs due to plant closure or mass layoff to those who did not lose their jobs but had the same expected probability to lose their jobs. They use a 10 percent sample of the full-time employed Danish male population aged 20-63 for the period 1981-1999. They find no effect of job loss on hospitalization for diseases of the circulatory system and diseases of the digestive system during the four years after displacement.

[3] Schröder (2013) also distinguishes between the effects of job termination due to layoff and job termination due to plant closure on subjective health.

Kuhn, Lalive, and Zweimueller (2009) and Black, Devereux, and Salvanes (2015) estimate the effect of job loss on health for workers who were slightly younger than those we study. Kuhn, Lalive, and Zweimueller (2009) employ propensity score matching to compare the health of workers who were employed at closing plants to the health of those who were employed at continuing plants. Using Austrian health insurance data on private sector employment linked with social security data for prime aged male workers for the period 1998-2002, they find that job loss increased hospitalization for mental health reasons.

Black, Devereux, and Salvanes (2015) employ a difference-in-difference approach to study how job displacement due to plant closure or plant-level employment downsizing affects cardiovascular health. They use a Norwegian sample of full-time employed men and women predominantly aged in their early forties. The data are repeated cross-section data for the period 1986-1999, including survey data on health and health behaviors and linked administrative data on person and firm characteristics. They find that job displacement had a negative effect on health seven years ahead, for both men and women. They find evidence that the effect was driven by an increase in the probability of smoking on a daily basis.

Salm (2009) uses a difference-in-difference approach to estimate the effect of job loss due to plant closure on health. He uses HRS data for the waves 1994-2002. He does not find an effect of self-reported job loss on various self-reported measures of physical and mental health.

3. Data

We use Dutch administrative micro panel data for the period 2003-2010. The data are administered by Statistics Netherlands and cover the entire Dutch population. We have access to data on job and personal characteristics, mortality and hospital stays. These data can be linked through a personal identifier.[4] The data on job characteristics can also be linked through a job and employer identifier. The personal characteristics file contains information on demographic characteristics such as marital status, number of children, country of birth, birth year and birth month. The job characteristics file provides

[4] The original file names are *SSB Banen (1999-2006), BEONTTAB (2003-2005), SSB Personen (2002-2005), Doodsoorzaken (2003-2010)* and *Landelijke Medische Registratie* (*LMR, 2002-2004*). Statistics Netherlands only provides data that are administered by governmental institutions. The data that are administered are not always administered for the years we are interested in. Hospital stays data, for instance, are incomplete after 2005, so that it is not possible to estimate the effect of job loss on alternative health measures created from hospital data.

information on all jobs any individual had been employed in. For every job, both start and end date, the industry code and the annual wage are available. The job characteristics file also provides information on whether job terminations were due to firm bankruptcy. The mortality file contains information such as month, year and primary cause of death. The hospital stay file provides for every hospital stay information such as the start and end date of the spell, the reason for the stay and where the patient went after being released from the hospital. We study the effect of job loss on mortality for job losses due to firm closure that occurred in the period 2003-2005. We can track subsequent mortality events up to and including the year 2010.

For our analysis we make some baseline selection of observations. This is for various reasons discussed in this section including that we want to restrict ourselves to a group of workers for whom we may reasonably expect a mortality effect of job loss. In Section 6.4.2, we verify whether our result is robust to changing the various data selection criteria. In general, we find that our result is robust. Our baseline sample is restricted to observations on male workers in the age category 45-59 who had the Dutch nationality and who had stable employment relationships.[5] We define stable employment as having continuous job tenure of at least five years at the same firm on January 1st of the year of observation and having a wage income of at least 20,000 Euros in the year prior to the year of observation.[6] Following Browning and Heinesen (2012), we exclude observations on workers employed in firms that had less than five workers on January 1st of the year of observation. We do so in order to remove self-employed and their employees from the sample. Those small firms may be inherently unstable. We also exclude observations on workers employed in large firms that had 400 workers or more on January 1st of the year of observation, because large firms may be older, more stable and less likely to close, so they predominantly end up in the control group without having a counterpart in the treatment group. We exclude observations on job departures other than layoffs due to firm closure from our sample, because workers departing their jobs for other reasons than firm closure receive a treatment, but a different one than the one we are primarily interested in.

[5] We do not estimate the effect of job loss on mortality for women, because there are too few observations on women meeting our selection criteria. Partly this has to do with the high prevalence of part-time employment among women in the country.

[6] As is customary, our register data do not contain information on hours worked. The income value of 20,000 euro corresponds to about 130 percent of the annual minimum wage in 2004 at full-time employment.

We exclude observations on workers who were laid off due to firm closure if at least 40 percent of the workforce of the closing firm got employed in one particular firm within one year after firm closure.[7] We do so to rule out takeovers or restarts of firms after closure (also see Browning and Heinesen, 2012). Lastly, closing firms may have laid off significant shares of their workforces prior to closure. This is a potential threat to the validity of our approach for various reasons. First, layoffs of co-workers and the threat of layoffs of workers may impose stress on workers and may possibly affect workers' health. Second, prior firm-level layoffs may make workers expect that the firm goes bankrupt or closes in general. Especially relatively productive workers with good job prospects elsewhere may leave the firm and find a job elsewhere once they expect the firm to shut down (Henningsen and Hægeland, 2008; Schwerdt, 2011). Third, prior firm-level layoffs may be selective, as the least productive workers may be fired when the firm experiences hard times (Pfann, 2006). Ill-health workers may be among the least productive workers. Whether the aggregate outflow of workers from the firm prior to closure is positively or negatively selected on workers' health is unclear. We exclude observations on workers employed in firms that reduced their workforce by at least 20 percent during the four years prior to the year of observation.

Table 1 compares workers that stayed on the job (our control group) with workers who were laid off due to firm closure (our treatment group). It shows that workers who were laid off due to firm closure were on average similar in terms of age, country of birth, marital status, number of children, hospitalization status (at t-1) and wage income (at t-1) to those who stayed on their jobs.[8] Workers who lost their jobs

[7] On the one hand, we do not want to use too strict a definition of restarting firms or firms being taken over, because including observations on workers employed in such firms may bias our result. On the other hand, we do not want to impose too loose a definition of restarting or taken over firms either. We do not want to use a definition with a minimum level of re-employment that is much lower than 40 percent. This is because if the lower bound would be, say, 20 percent, this would imply that we consider a closing firm that counted, say, five workers in the year of closure, as having made a restart if any of the five workers got employed somewhere within one year after closure.

[8] Age is measured on December 31st of the year of observation and is measured in years. Country of birth is measured by a dummy that equals one if the worker and worker's parents were born in the Netherlands and zero otherwise. Hospitalization status (at t-1) is a dummy that is one if an individual was hospitalized during the previous year of observation and zero otherwise. Wage income is measured in thousands of euros. Job tenure is measured on January 1st of the year of observation and is measured in years. Marital status is a dummy that is one if an individual was married on January 1st of the year of observation and zero otherwise. Being married also includes having a registered partnership. A registered partnership is a partnership that has a legal status similar to marriage. Cohabiting without being married or without having a registered partnership is not included in our definition of being married. Firm size is measured in number of workers employed at the firm on January 1st of the year of observation. Year indicates the year of observation.

involuntarily in closing firms had on average lower job tenure and were employed in smaller firms than workers who stayed in their jobs. The relatively low job tenure and low firm size may indicate that closing firms were in general younger than firms that did not shut down. This would be consistent with the evidence from the US on most firms dying young, at least in manufacturing (e.g. Evans, 1987; Dunne, Roberts, and Samuelson, 1988). Job loss due to firm closure was relatively frequent in commercial services. In transportation and communication and construction, relatively few workers lost their jobs.

Firm-level averages of worker characteristics are annual averages based on observations from the whole population of workers for the period 2003-2005, so not only on the sample of male workers we study in this paper. Firm-level means of most worker characteristics were on average similar for workers employed in closing firms as for workers employed in firms that stayed open. Firm-level fractions of workers having died during the four years preceding the year of observation were on average higher for closing firms than for firms that stayed open. This suggests that workers employed in closing firms had on average poorer health than those employed in firms that did not close. Average firm-level mean job tenure was half as low for closing firms than for firms that stayed open, reflecting that closing firms were typically younger than firms that stayed open.

Figure 1 shows that the fraction of workers dying within five years was larger for workers who were laid off due to firm closure than for workers who stayed on their jobs for workers of all ages.[9] The fraction of laid off workers dying within five years was quite volatile, because the number of mortality cases involved was small.[10]

4. Institutional setting

The institutional setting that accommodates job loss due to firm closure may affect the effect of job loss on (lifetime) earnings. Sullivan and Von Wachter (2009) find that the effect of job loss on mortality runs through (lifetime) earnings. This implies that the institutional setting may affect the size and direction of the effect of job loss due to firm closure on mortality as well.

[9] The five year mortality rate was significantly larger for workers who were laid off due to firm closure than for those workers who were not laid off due to firm closure at the one percent level.

[10] The total number of (unique) fatalities underlying the graph for workers who were laid off due to firm closure is 232.

Laid-off workers with employment tenures as the ones in our dataset were eligible for UI benefits of up to five years, irrespective of whether the firm they were employed in closed down.[11] The potential UI benefit duration depended positively on the employment tenure of the worker.[12] The replacement rate was 70 percent of the final wage (Dutch government, 2015a).[13] So, even if workers were not re-employed soon after job loss, the short-run effects of job loss on income were modest.

In addition to UI benefits, laid off workers may have received severance pay. Workers who were laid off by firms that closed down but did not go bankrupt did in certain cases receive severance pay from their employer. Workers employed in this type of closing firms could generally speaking be laid off via the court (Dutch Government, 2015b) or via the Employee Insurance Agency (Dutch Government, 2015c). Layoffs via the former route typically resulted in severance pay for the laid off workers. The levels of these compensations depended strongly and positively on age, job tenure and wage income. Compensations for workers aged 45 with job tenure of five years, i.e. the youngest workers with the lowest job tenure in our dataset, were typically about half an annual wage. In case of layoffs via the Employee Insurance Agency, employers had to ask the Employee Insurance Agency for permission to lay off workers. If permission was granted, employers could typically lay off workers without giving them any compensation. Workers may in this case have gone to court to seek compensations from their (former) employers.

Workers laid off due to firm bankruptcy may have received severance pay in two cases. First, if some inventory remained after the trustee and preferred creditors were paid. Second, if a board member of the employer inappropriately subtracted financial wealth from the firm prior to bankruptcy and he or she was held liable for this (Dutch Government, 2015d). Both cases were not common, however, so workers did typically not receive any severance pay for job loss due to firm bankruptcy (Berntsen and Mulder, 2014; Van Riet, 2014).[14]

[11] Eligibility was conditional on, for instance, actively looking for a job.

[12] The data of Statistics Netherlands do not go back far enough in the past to determine the maximum duration for unemployment benefit per individual.

[13] Workers did not build up occupational pension capital during unemployment.

[14] Workers may have asked the judge to get severance pay in case of firm bankruptcy. This severance pay was paid out of what was left of the inventory after the trustee and preferred creditors had been paid. However, as there was typically nothing left of the inventory, chances to get severance pay by making a request to the judge were very low.

The oldest workers in our dataset may have used unemployment insurance as an alternative retirement path.[15] The social security eligibility age in the Netherlands was 65 during the period studied. Early retirement pensions of occupational pension funds generally offered benefits as of age 60, 61 or 62 to the oldest workers in our dataset, depending on the pension fund. They typically required workers to have been employed continuously for a certain number of years prior to early retirement. As workers in our dataset did not reach the age of 60 yet, they were not eligible to retire early after job loss without getting re-employed beforehand. However, UI benefits may have provided the opportunity to close part of the gap in earnings between the time of job loss and reaching the normal retirement age. Long-run income loss effects through career disruption may, however, have been important even if workers could have claimed UI benefits, especially for the somewhat younger workers in our sample.

5. Identification strategy

We use a standard treatment effect estimation framework to estimate the average treatment effect as specified in (1).

(1) $$\alpha = \text{ATE} = \text{E}(Y_{it}|X_{it}, D_{it} = 1) - \text{E}(Y_{it}|X_{it}, D_{it} = 0)$$
$$\text{for individual i, i} = 1, 2, \ldots, \text{N and year t, t} = 2003, 2004, 2005$$

Y is a dummy that equals one if an individual stayed employed on the job during the whole year of observation and died within the five years after December 31st of the year of observation. It equals one as well if an individual who lost his job in the year of observation died within five years after the exact date of job loss. Y equals zero otherwise. X is a vector including first, second and third order terms for age, a dummy for being born in the Netherlands, a dummy for being married, number of children, a dummy for hospitalization (at t-1), wage income (at t-1), job tenure, firm size and industry dummies.[16] X also includes firm-level fractions of workers having died during the four years preceding the year of observation, being female, being born in the Netherlands, being married and having been hospitalized (at t-1). It includes firm-level averages for worker's age, number of children, wage income (at t-1) and

[15] Chan and Stevens (2001) find that job loss among older workers has strong and lasting effects on employment rates. Tatsiramos (2010) finds that older workers who are displaced in countries with relatively more generous UI provisions have relatively lower re-employment rates.

[16] We use the public sector as the base industry. The first, second and third order age terms are defined as $(age - 45)$, $(age - 45)^2$ and $(age - 45)^3$. The industry dummies are dummies for each industry as classified by Nomenclature statistique des Activités économiques dans la Communauté Européenne (NACE) 1993 codes (Statistics Netherlands, 2004).

job tenure as well. We use annual firm-level averages for worker characteristics rather than firm-level averages over the whole period of observations, because annual firm-level averages capture developments in firm-level worker characteristics across time. These may be important, as they may be related to firm closure. D is a dummy that equals one if an individual lost his job due to firm closure in the year of observation and zero otherwise.

The validity of our approach relies on the conditional independence assumption as specified in (2).

(2) $$\mathrm{Y_{0,it}, Y_{1,it} \perp D_{it} | X_{it}}$$
for each individual i and year t

The conditional independence assumption (2) asserts that conditional on the control variables included in X, the treatment job loss due to firm closure was as good as randomly assigned. Within closing firms, all workers were fired, so whether these workers were laid off did certainly not depend on workers' health. However, it may well be that workers employed in closing firms differed from workers employed in firms that did not close. One concern is that closing firms were relatively young and that these firms may have hired a certain selection of workers that had relatively poor health and relatively high probability to die within five years. We observe that closing firms actually had on average a higher fraction of workers dying during the four years preceding the year of observation than firms that did not close. We control for pre-existing between-firm differences in health and mortality by including the fractions of workers having been hospitalized (at t-1) and having died during the four years preceding the year of observation as control variables. We control for job tenure, firm size and firm-level average job tenure as well, which we expect to be strongly related to the firms' age and to pick up most of the effects related to being employed in young firms.

If (2) holds, this implies (3).

(3) $$\mathrm{E}\left(Y_{0,it} \middle| X_{it}\right) = \mathrm{E}(Y_{it}|X_{it}, D_{it} = 0) \text{ and } \mathrm{E}\left(Y_{1,it} \middle| X_{it}\right) = \mathrm{E}(Y_{it}|X_{it}, D_{it} = 1)$$
for each individual i and year t

Or, in words, if the conditional independence assumption holds, the expected probability to die within five years, conditional on observables, is the same for those who actually received the treatment "job loss due to firm closure" as for those who received the treatment hypothetically, irrespective of whether they actually received the treatment.

If (2) holds, then (1) is equivalent to (4).

(4) $\alpha = \text{ATE} = \text{E}(Y_{1,it}|X_{it}) - \text{E}(Y_{0,it}|X_{it})$

for each individual i and year t

The average treatment effect is defined as the difference between the expected probability to die within five years for individuals who were laid off due to firm closure and those who were not laid off due to firm closure, conditional on observables.

We estimate the average treatment effect specified in (1) by estimating the linear probability model specified in (5) using the administrative data as pooled cross section data.

(5) $Y_i = \beta_0 + X_i\beta_1 + D_i\alpha + \epsilon_i$

We also include year in X here. The linear probability model compares mortality outcomes for workers in the control group with those in the treatment group, controlling for differences in observables. It is important to notice that workers in the control and treatment group are on average similar in many respects. This takes away the need to use a method such as propensity score matching, that only uses those observations in the control group with those in the treatment group that compare best in terms of predicted probability to lose jobs due to firm closure. As many workers were observed in multiple years, we cluster standard errors at the individual level to account for serial correlation in the error terms. We perform sensitivity checks on our functional form specification in Section 6.4.3. We find that our result is in particular sensitive to excluding age terms and firm-level worker characteristics from X.

6. Results

6.1 Baseline estimate

Table 2 shows that job loss due to firm closure increased the probability to die within five years by 0.60 percentage points or 34 percent. This effect is significant at the one percent level. We discuss in Section 6.3 that our results on the effects of job loss on mortality on alternative time horizons are similar to those of Eliason and Storrie (2009) and Browning and Heinesen (2012). The coefficient estimates on most control variables show signs and p-values as we expect them to be. The probability to die within five years is positively related with age and being hospitalized (at t-1) and negatively related with wage income (at t-1). Firm-level average probability to die during the four years preceding the year of observation shows a positive relation with the probability to die within five years. This captures firm-level mortality effects, including the mortality effects of firm-level stress, work conditions, health related hiring policy and other selection of workers into firms. Firm-level average age and hospitalization status (at t-1) have negative coefficients. This is the consequence of controlling for firm-level mortality (during the period t-4 until t-1).

6.2 Effects of job loss on cause-specific mortality

Specific causes of death may be related to working or being laid off. For instance, if job loss would induce high stress levels, the effect of job loss on mortality may run through, amongst others, diseases of the circulatory system. We estimate the linear probability model in (5) using a dummy variable for dying within five years due to a specific frequent cause as a dependent variable to get more insight in the mechanism through which job loss affects mortality.[17] The causes of death are grouped in so called chapters, according to the 10th Revision of the International Statistical Classification of Diseases and Related Health Problems (ICD-10). The ICD is a health status classification system by the World Health Organization (WHO, 2010). For the most frequent causes of death, cancer and diseases of the circulatory system, we also estimate the model for those subchapters, so called blocks, that frequently caused death. We estimate the model for intentional self-harm as well, because this is an interesting cause of death in the context of this paper. As job loss may induce workers to change their life style, we estimate the model for alcohol-related mortality and mortality due to smoking-related cancers as well. There are no chapters or blocks on alcohol-related diseases and smoking-related cancers included in the ICD classification. As Eliason and Storrie (2009) do for both of these groups of diseases and Browning and Heinesen (2012) do for alcohol-related diseases only, we create categories on these causes of death ourselves. The category mortality due to smoking-related cancers includes fatalities due to cancers of the respiratory system, with lung cancer being the most frequent cancer of the respiratory system. According to Albert and Samet (2003), 90 percent of the lung cancer cases can be attributed to active smoking. The category mortality due to alcohol-related diseases includes fatalities that can directly be attributed to alcohol use.[18]

Table 3 shows that the effect of job loss on cause-specific mortality is positive and significant at the five percent level for mortality due to diseases of the circulatory system and positive and significant at the ten percent level for mortality due to neoplasms (cancers). The effect on mortality due to neoplasms is driven by mortality due to smoking-related cancers and the effect on mortality due to diseases of the

[17] We consider a cause to be frequent if it causes death of at least 250 individuals in our dataset.
[18] The causes of death classification with the corresponding ICD-10 codes is included in Table A1 of the Appendix.

circulatory system is driven by mortality due to cerebrovascular diseases. Cerebrovascular diseases can cause acute mortality. Hypertension is the most important modifiable risk factor for this type of diseases.[19] Other risk factors include diabetes, obesity, alcohol use, smoking, lack of physical exercise, high cholesterol, high blood glucose and low fruit and vegetable intake (WHO, 2009).[20] Risk factors for hypertension include obesity, smoking, alcohol consumption, physical inactivity and stress (Kaplan and Nunes, 2003; Appel et al., 2006; Truelsen, Begg, and Mathers, 2006). We find that job loss decreased the probability to die within five years due to external causes of death excluding intentional self-harm. This effect is significant at the five percent level and may be explained by workers who stayed on their jobs dying in work-related accidents, such as accidents that occur when commuting between home and work.

The positive effect of job loss on mortality due to cerebrovascular diseases and mortality due to diseases of the circulatory system in general is consistent with Browning and Heinesen (2012). They suggest that the effect of job loss on mortality due to diseases of the circulatory system runs through stress. Black, Devereux, and Salvanes (2015) find that job loss affects health through smoking-related diseases. We find this as well.

6.3 Mortality effects of job loss by year since layoff

So far, we have focused on mortality within five years as the outcome variable. Estimating the model specified in (5) with mortality within fewer years as the outcome variable may provide insights on how long it takes for job loss to affect mortality. Table 4 shows that job loss has a positive 0.22 percentage point or 86 percent strong effect on mortality in the first year after job loss. The effect of job loss on mortality in the first two years after job loss is 0.34 percentage point or 61 percent and the effect of job loss on mortality in the first three years after job loss is 0.36 percentage point or 39 percent. The effect of job loss on mortality in the first four years after job loss is 0.55 percentage point or 41 percent. The relative sizes of these effects across time horizons are consistent with Browning and Heinesen (2012), who find that job loss increased mortality by 84 percent in the first year after displacement, 36 percent in the first four years after displacement and 17 percent in the first ten years after displacement. The relative size of the effect estimated by Eliason and Storrie (2009), who find that job loss increased the

[19] Aging is an important risk factor for these and most of the other diseases discussed as well, but is not modifiable.
[20] Risk factor indicates a factor that is correlated with the prevalence of a disease. There is not necessarily a causal relation between a risk factor and the prevalence of a disease.

probability to die within four years by 44 percent, is consistent with our result as well. One possible explanation for the strong effect in the first year after is that job loss induced stress, causing cerebrovascular diseases that resulted in death among some of the laid off workers. The effect of job loss on mortality in later years after job loss is consistent with our earlier evidence suggesting that the effect runs through changes in smoking behavior.

6.4 Robustness checks

6.4.1 Robustness checks on the type of job loss

We estimate the mortality effects of job loss due to firm closure. There are other types of job loss that can be applied in the estimation of the effect of job loss on mortality as well. These other types of job loss include job loss in firms experiencing large employment declines as applied in Sullivan and Von Wachter (2009), and job loss due to firm bankruptcy that has to our knowledge not been applied before. The effects of job loss on mortality may differ across treatments and the workers receiving the treatments may differ in terms of characteristics. This may make between-study comparisons of the mortality effects of job loss using the different types unsuitable. Our data allow us to distinguish the three types of job loss and to compare their effects on mortality. The three types of job loss directly relate to each other, as job losses due to bankruptcy of firms are a subset of all job losses due to firm closures and job losses due to firm closures are a subset of all departures from firms that were experiencing employment declines of at least 40 percent compared to the previous year of observation.

Table 5, variation a, shows that for the baseline dataset excluding observations on cases of job loss due to firm closure that did not involve firm bankruptcy, there is no effect of job loss on the probability to die within five years. For the baseline selection of observations including observations on workers employed in firms that experienced employment declines of at least 40 percent in the year of observation, we find that the effect of job loss on mortality is about one-third smaller than the baseline effect (variation b).[21]

[21] The same criteria for selection of these observations apply as for the observations in the baseline dataset, except for the criterion on the treatment the worker received.

6.4.2 Robustness checks on data selection criteria

We find an effect of job loss on mortality for a particular selection of observations that we believe represents a clean sample. We investigate whether our baseline result is sensitive to changing data selection criteria.

Our baseline selection only includes observations on workers in the age category 45-59. Table 6, variation a, shows that the mortality effect of job loss estimated on the sample excluding observations on workers aged 52 (median age) or older is almost identical to the effect estimated on the baseline sample. If observations on workers younger than age 52 are excluded from the sample, the effect is similar to the baseline effect as well (variation b).[22]

We exclude observations for workers employed in firms with fewer than five or more than 400 workers on January 1st of the year of observation. We do so to keep our control group and treatment group comparable. Very large firms may rarely close, making workers employed at these firms end up in the control group only. Firms with less than five workers include self-employed and may so be considered as unstable firms. Workers employed at these firms may end up disproportionally often in the treatment group. The effect of job loss on mortality for the extended sample including observations on workers employed in firms with less than five or more than 400 workers is larger than but similar to the baseline estimate (variation c).

Similar to Sullivan and Von Wachter (2009), we exclude observations on workers with job tenures of less than five years to ensure that workers had stable employment relationships. There is no effect if we include observations on workers with job tenures of at least one year in our dataset as well (variation d). Excluding observations on workers with job tenures shorter than ten years results in an effect that is larger than the baseline effect (variation e). Workers with shorter job tenures may be more flexible and better able to deal with job loss than workers with longer job tenures. This may have resulted in a stronger effect of job loss on mortality for workers with longer job tenures and the absence of an effect of job loss on mortality for workers with shorter job tenures.

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[22] Sullivan and Von Wachter (2009) find that the effect of job loss on mortality is smaller for older workers. We have verified whether this is also true in our case, estimating (5) with the dummy for job loss and the dummy for job loss interacted with age included in D. We find that the coefficient on job loss interacted with age is not significant at the ten percent level, however.

We also only select observations with wage incomes (at t-1) of at least 20,000 euros to ensure that workers in our dataset had strong labor force attachment. Lowering this threshold by 10,000 euros hardly affects the number of observations in our dataset and has a negligible impact on the size of the effect (variation f). The small effect of lowering the income threshold on the number of observations in our dataset reflects that older male workers in their career jobs typically did not work only few hours. Conversely, excluding observations on workers who earned less than 30,000 euros (at t-1) strongly reduces the number of observations. The effect of job loss on mortality based on observations that had wage income levels above this increased threshold is smaller than the baseline estimate (variation g). One potential explanation is that workers who earn relatively little are more likely to earn just enough to break even. Job loss (slightly) decreased income for these workers that brought some of them in trouble paying their daily expenses. This resulted in stress that negatively affected their health and increased their probability to die within five years.

We exclude observations on workers employed at firms experiencing large employment declines or growth prior to closure to minimize the risk that job loss is endogenous to mortality. As a robustness check, we estimate the effect of job loss on mortality for a sample that includes observations on workers employed in firms that experienced any employment reductions or employment growth in the years preceding closure. We find that this effect is smaller than the baseline effect (variation h). Workers employed in firms that experienced prior mass layoffs may have become better able to handle layoff related stress by their prior experiences. This may have made the effect of job loss on mortality smaller for these workers.

We exclude observations on workers who were laid off due to firm closure if at least 40 percent of the workforce got employed in one particular firm within one year after closure. We do so to rule out that closing firms may have restarted or may be taken over by another firm, effectively keeping workers in employment. Estimating the effect of job loss on mortality for the dataset including these observations gives an effect that is almost identical to the baseline estimate (variation i).

6.4.3 Robustness checks on functional form specification

We verify whether our baseline result is sensitive to functional form specification changes. We estimate the treatment effect using a specification that is identical to the baseline specification, except that one of the independent variables or one group of independent variables is left out. Table 7, variation a, shows that the effect of job loss on mortality estimated using the functional form excluding firm-level worker characteristics is 0.83 percentage points or 46 percent. This is much larger than the baseline estimate. Excluding individual firm-level worker characteristics from our functional form generally does not affect our result much (variations b-j), except for excluding firm-level mortality (during the period t-4 until t-1).The effect of job loss on mortality estimated using the functional form excluding firm-level mortality is much larger than the baseline estimate as well (variation b). These results suggest that our result is biased upwards if we do not control for pre-existing differences in worker characteristics in general and pre-existing differences in worker mortality rates in particular.

Our baseline result is in general robust to leaving an independent variable or group of independent variables out of the functional form (variations k-t). The only exceptions are leaving out the nonlinear age terms and job tenure, giving estimates that are larger than the baseline estimate (variations k and q). If we include age dummies instead of nonlinear age terms as independent variables in our functional form, our result is similar to the baseline estimate (variation u).

7. Conclusions

We study the effect of job loss on mortality. As ill-health workers may be more likely to lose their jobs than healthier workers, simply regressing job loss on mortality will result in a coefficient estimate on job loss that is biased upwards. Studies in the literature attempt to avoid such endogeneity bias by using job loss due to firm closure (Eliason and Storrie, 2009; Browning and Heinesen, 2012; Michaud, Crimmins, and Hurd, 2014) and job loss in firms experiencing large firm-level employment declines (Sullivan and Von Wachter, 2009) as treatments to estimate mortality effects of job loss. These treatments may be endogenous to mortality as well, however. We actually find that firm-level mortality rates during the four years preceding the year of observation were higher in closing firms than in firms that did not close. These pre-existing differences in firm-level worker mortality rates may exist due to selective hiring or selective worker outflow prior to firm closure or large firm-level employment declines. We control for pre-existing differences in firm-level worker health and mortality rates and other firm-level worker characteristics. To our knowledge, we are the first paper in the literature to do so.

We study the effect of job loss due to sudden firm closure on mortality for older male workers with strong labor force attachment, using employee-employer matched administrative data from the Netherlands. We find that job loss due to firm closure increased the probability to die within five years by 0.60 percentage point or 34 percent. This is similar to the findings of Eliason and Storrie (2009) and

Browning and Heinesen (2012). We find that job loss increased the probability to die within five years by 0.83 percentage point or 46 percent if we do not control for firm-level worker characteristics. This effect being larger than the effect we find after controlling for firm-level worker characteristics suggests that the coefficient estimate of job loss on mortality is biased if we do not control for firm-level worker characteristics.

Taking a closer look at the mechanism driving the effect of job loss on mortality, we find that job loss due to firm closure may run through stress, as cause-specific mortality analysis shows that (acute) diseases of the circulatory system are important drivers of the effect. The strong effect of job loss on mortality in the first year after job loss is consistent with an effect running through stress and (acute) diseases of the circulatory system. Eliason and Storrie (2009) and Browning and Heinesen (2012) find that diseases of the circulatory system are important drivers of the effect of job loss on mortality as well. Changes in lifestyle seem to be relevant too, because smoking-related cancers account for part of the effect of job loss on mortality. This is consistent with Black, Devereux, and Salvanes (2015), who find that job loss negatively affects health through smoking-related diseases.

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Tables and Figure

Table 1: Descriptive statistics

Workers who stayed in their jobs in the year of observation (control group)			Workers who lost their jobs due to closure of firms in the year of observation (treatment group)		
Variable	**Mean**	**Std. Dev.**	**Variable**	**Mean**	**Std. Dev.**
Age	51.74	4.27	Age	52.33	4.39
Born in the Netherlands	0.90	0.30	Born in the Netherlands	0.89	0.32
Married	0.82	0.39	Married	0.81	0.39
Number of children	1.92	1.21	Number of children	1.89	1.20
Hospitalized [t-1]	0.050	0.218	Hospitalized [t-1]	0.050	0.217
Wage income [t-1]	44.65	23.60	Wage income [t-1]	45.57	24.80
Job tenure	16.18	8.02	Job tenure	14.12	7.99
Firm size	126.68	109.97	Firm size	121.71	103.47
Year	2004.00	0.81	Year	2004.04	0.84
Industry			Industry		
Agriculture	0.018	0.131	Agriculture	0.009	0.093
Asset Management	0.028	0.164	Asset Management	0.023	0.149
Banking/Insurance	0.013	0.115	Banking/Insurance	0.026	0.160
Catering	0.006	0.079	Catering	0.006	0.076
Commercial Services	0.073	0.261	Commercial Services	0.164	0.370
Construction	0.159	0.366	Construction	0.113	0.316
Education	0.074	0.261	Education	0.081	0.273
Health Care	0.015	0.121	Health Care	0.022	0.147
Manufacturing	0.283	0.450	Manufacturing	0.268	0.443
Other Care	0.056	0.231	Other Care	0.056	0.229
Public Sector	0.062	0.240	Public Sector	0.063	0.243
Retail	0.126	0.332	Retail	0.101	0.301
Transportation/ Communication	0.082	0.274	Transportation/ Communication	0.049	0.217
Temporary work	0.005	0.069	Temporary work	0.019	0.136
Firm-level worker characteristics			Firm-level worker characteristics		
Died [t-4 until t-1]	0.0088	0.0141	Died [t-4 until t-1]	0.0105	0.0168
Age	40.99	3.72	Age	41.92	4.28
Female	0.25	0.22	Female	0.26	0.23

Table 1 (continued)

Workers who stayed in their jobs in the year of observation (control group)			Workers who lost their jobs due to closure of firms in the year of observation (treatment group)		
Variable	**Mean**	**Std. Dev.**	**Variable**	**Mean**	**Std. Dev.**
Born in the Netherlands	0.87	0.11	Born in the Netherlands	0.86	0.13
Married	0.59	0.13	Married	0.60	0.13
Number of children	1.21	0.43	Number of children	1.21	0.44
Hospitalized [t-1]	0.024	0.024	Hospitalized [t-1]	0.025	0.025
Wage income [t-1]	32.66	14.99	Wage income [t-1]	34.94	15.26
Job tenure	8.08	3.27	Job tenure	8.49	3.97
N	840,915		N	8,394	

Table 2: Linear Probability Model (LPM) estimates for the probability to die within five years (in percentage points) (Ordinary Least Squares, OLS) *

Variable	Coef.	Std. Err.	P-value
Job loss	0.5968	0.1763	0.001
$(Age - 45)$	0.1512	0.0308	0.000
$(Age - 45)^2$	-0.0051	0.0059	0.383
$(Age - 45)^3$	0.0007	0.0003	0.018
Born in the Netherlands	0.1623	0.0720	0.024
Married	-0.7989	0.0636	0.000
Number of children	-0.1121	0.0189	0.000
Hospitalized [t-1]	1.0749	0.0855	0.000
Wage income [t-1]	-0.0086	0.0008	0.000
Job tenure	-0.0354	0.0033	0.000
Firm size	0.0003	0.0002	0.188
Year	0.0150	0.0148	0.310
Industry			
Agriculture	0.1302	0.1778	0.464
Asset Management	-0.0131	0.1446	0.928
Banking/Insurance	0.3077	0.1978	0.120
Catering	-0.1409	0.2842	0.620
Commercial Services	0.0953	0.1143	0.404
Construction	-0.1114	0.1103	0.312
Education	-0.0985	0.1116	0.378
Health Care	-0.1862	0.1594	0.243
Manufacturing	-0.0705	0.0998	0.480
Other Care	0.0507	0.1222	0.678
Retail	-0.0349	0.1055	0.741
Transportation/ Communication	-0.0538	0.1217	0.659
Temporary work	-0.4932	0.2908	0.090
Base industry: Public Sector			
F-test on joint significance industry dummies:	F = 1.26, p=0.2314		

* Standard errors are clustered at the individual level.

Table 2 (continued)

Variable	Coef.	Std. Err.	P-value
Firm-level worker characteristics			
Died [t-4 until t-1]	0.0150	0.0003	0.000
Hospitalized [t-1]	-1.4799	0.7012	0.035
Age	-0.1437	0.0086	0.000
Female	0.3910	0.2096	0.062
Born in the Netherlands	-0.6322	0.2077	0.002
Married	0.9026	0.2574	0.000
Number of children	0.0770	0.0982	0.433
Wage income [t-1]	0.0030	0.0011	0.005
Job tenure	0.0672	0.0092	0.000
Constant	-24.120	29.709	0.417
N	849,309		

Table 3: LPM estimates for the effect of job loss on the probability to die within five years due to a specific cause of death (in percentage points) (OLS) *

Cause of death	**Coef.**	**Std. Err.**	**P-value**	**Rel. effect (%)**	**#Deaths**
Alcohol-related diseases	0.0461	0.0293	0.115	228.1	113
Neoplasms	0.2377	0.1280	0.063	24.8	4,695
Malignant neoplasms of digestive organs	0.0885	0.0762	0.246	26.8	1,608
Malignant neoplasms of intrathoracic organs	0.0243	0.0673	0.718	8.4	1,424
Smoking-related cancer	0.0520	0.0265	0.050	767.7	36
Malignant neoplasms of urinary tract	-0.0301	0.0245	0.220	-47.4	296
Malignant neoplasms, stated or presumed to be primary, of lymphoid, haematopoietic and related tissue	-0.0033	0.0297	0.912	-5.2	309
Other malignant neoplasms	0.1062	0.0650	0.102	51.7	1,022
Diseases of the circulatory system	0.2453	0.0975	0.012	52.8	2,260
Ischaemic heart diseases	0.0882	0.0650	0.175	39.4	1,075
Cerebrovascular diseases	0.0875	0.0427	0.041	152.9	286
Other forms of heart disease	0.0696	0.0597	0.243	37.9	899
External causes of morbidity and mortality except intentional self-harm	-0.0389	0.0176	0.027	-68.4	277
Intentional self-harm	0.0063	0.0291	0.830	11.4	271
Other diseases	0.1001	0.0670	0.135	44.3	1,126
Total	0.5968	0.1763	0.001	33.5	8,741

* Each estimate is a coefficient estimate for job loss ($\hat{\alpha}$) for the model as specified in (5) with a dummy for cause-specific mortality within five years as the dependent variable. The independent variables are job loss, $(age-45)$, $(age-45)^2$, $(age-45)^3$, being born in the Netherlands, marital status, number of children, hospitalization status (at t-1), wage income (at t-1), job tenure, firm size, industry dummies, year and firm-level averages on the probability to die during the four years preceding the year of observation, age, gender, being born in the Netherlands, marital status, number of children, hospitalization status (at t-1), wage income (at t-1) and job tenure. Standard errors are clustered at the individual level. The relative effect is the coefficient estimate on job loss times 100 divided by the base fractions of workers who died within five years due to the relevant cause for workers who stayed in their jobs.

Table 4: LPM estimates for the probability to die within less than five years (in percentage points) (OLS) *

The probability to die within	**1 year**	**2 years**	**3 years**	**4 years**	**5 years**
Coefficient estimate job loss ($\hat{\alpha}$)	0.2229	0.3435	0.3645	0.5471	0.5968
Standard error	0.0812	0.1116	0.1335	0.1583	0.1763
P-value	0.006	0.002	0.006	0.001	0.001
Relative effect (%)	85.8	60.9	39.4	41.2	33.5

* Each estimate is a coefficient estimate for job loss ($\hat{\alpha}$) for the model as specified in (5) with a dummy for dying within the indicated number of years as the dependent variable. The independent variables are job loss, $(age - 45), (age - 45)^2, (age - 45)^3$, being born in the Netherlands, marital status, number of children, hospitalization status (at t-1), wage income (at t-1), job tenure, firm size, industry dummies, year and firm-level averages on the probability to die during the four years preceding the year of observation, age, gender, being born in the Netherlands, marital status, number of children, hospitalization status (at t-1), wage income (at t-1) and job tenure. Standard errors are clustered at the individual level. The relative effect is the coefficient estimate on job loss times 100 divided by the base fractions of workers who died within the relevant number of years for workers who stayed in their jobs.

Table 5: LPM estimates for the probability to die within five years for alternative definitions of job loss (in percentage points) (OLS) *

Variation	Definition of job loss	Coef.	Std. Err.	P-value	N treatment group	Rel. eff. (%)
	Job loss due to firm closure (baseline)	0.5968	0.1763	0.001	8,394	33.5
a.	Job loss due to firm bankruptcy	0.1117	0.3879	0.773	1,552	6.3
b.	Job loss due to a firm-level employment declines of at least 40%	0.3889	0.1504	0.010	10,735	21.8

* Each estimate is a coefficient estimate for job loss ($\hat{\alpha}$) for the model as specified in (5) with a dummy for dying within five years as the dependent variable. The independent variables are job loss, $(age - 45)$, $(age - 45)^2$, $(age - 45)^3$, being born in the Netherlands, marital status, number of children, hospitalization status (at t-1), wage income (at t-1), job tenure, firm size, industry dummies, year and firm-level averages on the probability to die during the four years preceding the year of observation, age, gender, being born in the Netherlands, marital status, number of children, hospitalization status (at t-1), wage income (at t-1) and job tenure. The control groups are the same in variations a and b as in the baseline case. Standard errors are clustered at the individual level. The relative effect is the coefficient estimate on job loss times 100 divided by the base fractions of workers who died within five years.

Table 6: Robustness checks on data selection: LPM estimates for the probability to die within five years (in percentage points) (OLS) *

Variation	Robustness check	Coef.	Std. Err.	P-value	N
	Baseline	0.5968	0.1763	0.001	849,309
a.	Excl. workers aged 52 (median age) or older	0.5792	0.2200	0.008	414,931
b.	Excl. workers aged younger than 52 (median age)	0.5758	0.2622	0.028	434,378
c.	Incl. workers employed in firms of any size	0.9252	0.1537	0.000	2,113,677
d.	Incl. workers with job tenures of at least 1 year	0.1492	0.1319	0.258	971,028
e.	Excl. workers with job tenure less than 10 years	0.7450	0.2286	0.001	631,478
f.	Incl. workers with wage income[t-1] of at least 10,000 euros	0.6032	0.1746	0.001	872,120
g.	Excl. workers with wage income[t-1] lower than 30,000 euros	0.5032	0.1862	0.007	702,352
h.	Incl. firms with layoffs prior to the year of observation	0.3769	0.1450	0.009	971,354
i.	Incl. cases of job loss due to firm closure if at least 40% of the workers employed in a closing firm got employed in one particular firm within one year after firm closure	0.5807	0.1617	0.000	850,814

* Each estimate is a coefficient estimate for job loss ($\hat{\alpha}$) for the model as specified in (5) with a dummy for dying within five years as the dependent variable. The independent variables are job loss, $(age-45)$, $(age-45)^2$, $(age-45)^3$, being born in the Netherlands, marital status, number of children, hospitalization status (at t-1), wage income (at t-1), job tenure, firm size, industry dummies, year and firm-level averages on the probability to die during the four years preceding the year of observation, age, gender, being born in the Netherlands, marital status, number of children, hospitalization status (at t-1), wage income (at t-1) and job tenure. Standard errors are clustered at the individual level.

Our dataset (baseline) includes observations on male workers in the age category 45-59 who had the Dutch nationality in the year of observation, who had a continuous job tenure of at least five years at the same firm on January 1st of the year of observation and who had a wage income of at least 20,000 euros in the year prior to the year of observation. We exclude observations on workers employed in firms that have less than five or at least 400 workers. We exclude observations on job departures other than forced layoffs due to firm closure. We exclude observations on job loss due to firm closure as well if at least 40 percent of the workers employed in a closing firm got employed in one particular company within one year after closure. We also exclude observations on closing firms that experienced workforce declines of at least 20 percent during the four years prior to the year of observation.

Table 7: Robustness checks on functional form specification: LPM estimates for the probability to die within five years (in percentage points) (OLS) *

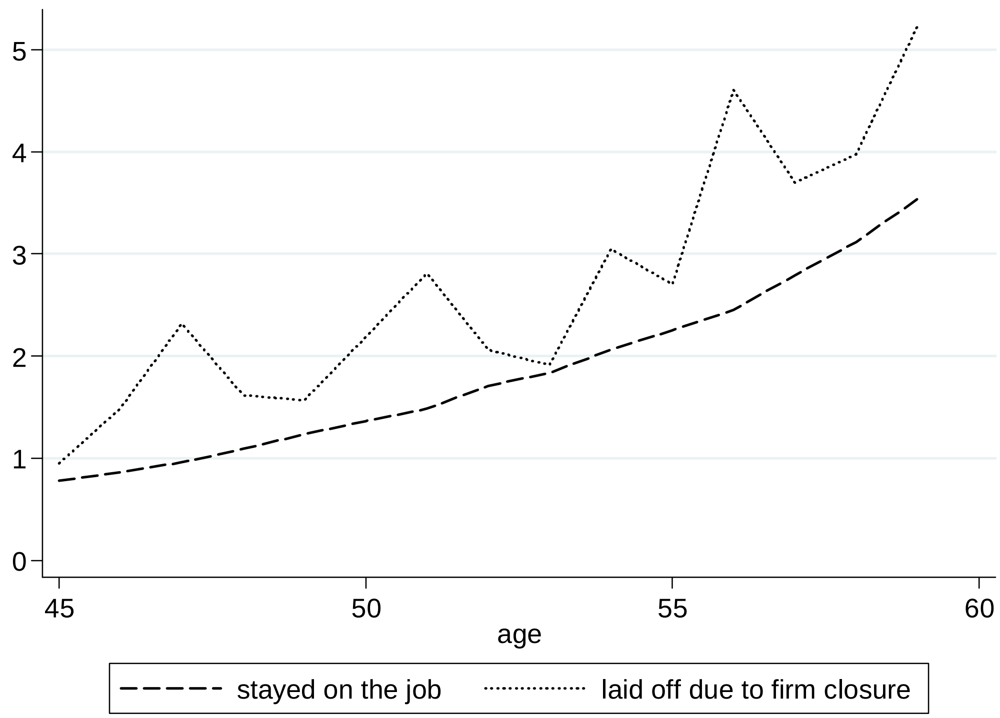
Variation	Robustness check	Coef.	Std. Err.	P-value	Rel. effect (%)
	Baseline	0.5968	0.1763	0.001	33.5
a.	Do not control for any firm-level worker characteristic	0.8257	0.1787	0.000	46.4
b.	Do not control for firm-level average mortality rates [t-4 until t-1]	0.7763	0.1788	0.000	43.6
c.	Do not control for firm-level average hospitalization rates [t-1]	0.5966	0.1763	0.001	33.5
d.	Do not control for firm-level average age	0.5649	0.1764	0.001	31.7
e.	Do not control for firm-level fraction of female workers	0.5952	0.1764	0.001	33.4
f.	Do not control for firm-level fraction of workers born in the Netherlands	0.6006	0.1763	0.001	33.7
g.	Do not control for firm-level fraction of married workers	0.5943	0.1764	0.001	33.4
h.	Do not control for firm-level average number of children	0.5967	0.1763	0.001	33.5
i.	Do not control for firm-level average wage income [t-1]	0.5990	0.1763	0.001	33.6
j.	Do not control for firm-level average job tenure	0.6358	0.1763	0.000	35.7

* Each estimate is a coefficient estimate for job loss ($\hat{\alpha}$) for the model as specified in (5) with a dummy for dying within five years as the dependent variable. The independent variables are job loss, $(age-45)$, $(age-45)^2$, $(age-45)^3$, being born in the Netherlands, marital status, number of children, hospitalization status (at t-1), wage income (at t-1), job tenure, firm size, industry dummies, year and firm-level averages on the probability to die during the four years preceding the year of observation, age, gender, being born in the Netherlands, marital status, number of children, hospitalization status (at t-1), wage income (at t-1) and job tenure. Standard errors are clustered at the individual level.

Table 7 (continued)

Variation	Robustness check	Coef.	Std. Err.	P-value	Rel. effect
k.	Do not control for age	0.7574	0.1767	0.000	42.5
l.	Do not control for being born in the Netherlands	0.5977	0.1763	0.001	33.6
m.	Do not control for marital status	0.5942	0.1764	0.001	33.4
n.	Do not control for number of children	0.5979	0.1764	0.001	33.6
o.	Do not control for hospitalization[t-1]	0.5994	0.1764	0.001	33.7
p.	Do not control for wage income[t-1]	0.6082	0.1764	0.001	34.2
q.	Do not control for job tenure	0.6816	0.1761	0.000	38.3
r.	Do not control for firm size	0.5936	0.1763	0.001	33.3
s.	Do not control for industry dummies	0.6063	0.1763	0.001	34.0
t.	Do not control for year	0.5967	0.1764	0.001	33.5
u.	Control for age fixed effects instead of nonlinear age effects	0.5967	0.1764	0.001	33.5

Figure 1: Probability to die within five years for men (%), by labor force state



Appendix

Table A1: Cause of death classification

Cause of death	ICD-10 codes*
Alcohol-related diseases	F10, G31.2, G62.1, K29.2, K70, K85.2, K86.0, T51, X45, Y15
Neoplasms	C00-C97, D00-D48
Malignant neoplasms of digestive organs	C15-C26
Malignant neoplasms of intrathoracic organs	C37-C38, C39.9
Smoking-related cancer	C30-C34, C39.0, C39.8
Malignant neoplasms of urinary tract	C64-C68
Malignant neoplasms, stated or presumed to be primary, of lymphoid, haematopoietic and related tissue	C81-C96
Other malignant neoplasms	C00-C14, C40-C63, C68-C80, C79, D00-D48
Diseases of the circulatory system	I00-I99
Ischaemic heart diseases	I20-I25
Cerebrovascular diseases	I60-I69
Other forms of heart disease	I00-I19, I26-I59, I70-I99
Symptoms, signs and abnormal clinical and laboratory findings, not elsewhere classified	R00-R99
External causes of morbidity and mortality except intentional self-harm	V00-V99, W00-W99, X00-59, X86-X98
Intentional self-harm	X60-X85
Other diseases	A00-A99, B00-B99, D50-D89, E00-E90, F00-F99, G00-G99, H00-H95, J00-J99, K00-K93, L00-L99, M00-M99, N00-N99, O00-O99, P00-P96, Q00-Q99, R00-R99, S00-S99, T00-T99, U00-U85, Z00-Z99

* More information on the ICD-10 classificiation can be found on http://apps.who.int/classifications/icd10/browse/2015/en.