# Sick Happens: The Effect of Worker Health Shocks on Coworkers' Employment and Health Behavior*

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**Abstract**

We analyze how a worker's severe health shock affects the employment and health behavior of their older coworkers. We link comprehensive administrative data on labor market histories and health records from Austria to identify coworker networks and severe health shocks in small firms, which cause substantial increases in healthcare expenditures, absenteeism, and mortality, as well as persistent reductions in the labor supply of affected workers. Combining a matching approach with a difference-in-difference framework, we find a significant impact of a health shock on the labor market outcomes and health behavior of older coworkers. Affected coworkers are about 2.3 percentage points more likely to be employed in the shock firm and tend to delay retirement. Although there is no change in daily earnings and earnings growth, coworkers are more likely to receive special bonus payments after leaving the firm. The employment effects are larger when the health shock affects a high-skilled worker and when the shocked worker leaves the firm after the health shock. Finally, we find that female coworkers in the treatment group are more likely to have a mammography, especially in response to health shocks due to cancer. We find no statistically significant effects on participation in general health check-ups and PSA tests, or on coworker absenteeism.

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*Keywords:* coworker health shock · employment · retirement · health behavior · difference-in-difference

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# 1 Introduction

Every year, millions of individuals around the world suffer a major health shock. In 2021 alone, an estimated 66.8 million cases of cardiovascular diseases and 23.6 million cases of cancer were recorded. Both diseases are among the top three causes of death.[1] While the far-reaching and detrimental consequences of such health shocks are well documented for the affected individuals (see Prinz et al. 2018 for a literature review), there is less evidence on the spillover effects of such events on those surrounding them. Existing studies mainly focus on family members (e.g., Fadlon and Nielsen, 2019; Frimmel et al., forthcoming), while networks outside the household, such as coworkers, have received little attention. However, adults spend a significant amount of their time at work. In the US, individuals aged between 20 and 64 spend an average of 2.9 hours per day with their coworkers. This is about the same amount spent with their partner or children, and considerably less than the time spent with the extended family or friends.[2,3]

A health shock of a worker can therefore also affect their coworkers in several dimensions. A priori, the effect of a severe health shock on coworkers' employment is ambiguous. Witnessing a major deterioration in a worker's health makes coworkers' own health risks more salient (Fadlon and Nielsen, 2019). Worried about their own health and that the health shock could be attributed to occupational risk and workload, coworkers may try to reduce their own risk of suffering a severe health shock by reducing their labor supply, switching to less demanding jobs, or retiring earlier. In this case, a health shock is expected to have a negative effect on coworkers' employment and earnings and to lead to a higher probability of (early) retirement. At the same time coworkers may adjust their preventive health behavior accordingly. Coworkers may be more likely to participate in health screening programs to learn about their own health risks, or they may try to reduce risky health behaviors. Alternatively, a worker's health may affect the labor supply of coworkers through firm-level effects. A deterioration in a worker's health may result in a partial loss of human capital and productivity. Firms may try to compensate for reduced productivity either by hiring new external workers or by relying more on existing workers. If these two groups are imperfect substitutes, firms' demand for the labor of incumbent workers should increase (Jäger and Heining, 2022), resulting in increased employment of coworkers (at least in the shock firm) or delayed retirement.

In this study, we analyze how a worker's severe health shock affects the labor market outcomes and health behavior of their coworkers. We link comprehensive administrative data on labor market histories and health records from Austria to identify coworker net-

[1] Source: Institute for Health Metrics and Evaluation: Cardiovascular Diseases, Cancer.

[2] The values are 2.9 hours per day spent with a partner, 2.5 hours with children, 1.2 hours with extended family, and 0.8 hours with friends. Source: Our World in Data, own calculations.

[3] Time use data from Austria suggest that the average day of a 20 to 64 year old includes 4.1 hours of work, 3 hours of care work, and 5 hours of social contacts and leisure time (Statistik Austria, 2023, own calculations).

works and severe health shocks in small firms. Health shocks are defined as a worker's first hospitalization for cancer or acute cardiovascular events, including stroke, myocardial infarction, and heart failure. We show that these health shocks are indeed severe, leading to substantial increases in healthcare expenditures, absenteeism, and mortality, as well as to significant and persistent reductions in labor supply for those who suffer them. To construct an appropriate control group, we follow the method implemented by Jäger and Heining (2022). For each worker-firm pair with an actual shock we use a combination of exact and propensity score matching to find a worker-firm pair in the pool of worker-firm pairs that do not experience a health shock and add information on coworkers. Using a difference-in-difference framework, we compare coworkers who experience a severe health shock of a colleague to coworkers without such an experience, but who have a colleague who is similar to an actual shock worker on key observable characteristics, before and after the (placebo) health shock. For our main analysis, we focus on coworkers aged 50 or older at the time of the health shock. This group may be particularly affected because the likelihood of experiencing a severe health shock increases with age, and thus learning about one's own health risks may be greater for more similar older coworkers (Fadlon and Nielsen, 2019). Moreover, firms may rely on seniority to find reliable replacements for shock workers (Bianchi et al., 2023).

We find a significant impact of a health shock on older coworkers' labor market outcomes and health behavior. Coworkers in the treatment group are 2.3 percentage points more likely to be employed in the shock firm after the health shock. This corresponds to an increase of 2.5 percent relative to the pre-shock employment probability. Consistently, employment days increase and retirement days decrease. Although there is no change in daily earnings and earnings growth, treated coworkers are more likely to receive special severance payments (i.e., golden handshakes) when they leave the firm. The effects on employment and retirement are larger when the health shock affects a high-skilled worker and when the shock worker leaves the firm after the health shock. Regarding health behavior, we find that female coworkers in the treatment group are 9.7 percent more likely to have a mammography after the health shock than women in the control group. This effect appears to be larger when coworkers and shock workers are more similar to each other in terms of age and gender. Since mammographies are used to detect breast cancer, we find that the increase is mainly driven by cancer-related health shocks. We find no statistically significant effects on participation in general health check-ups and prostate-specific antigen (PSA) tests. Coworker absenteeism is also unaffected. While these results all focus on coworkers who are over 50 at the time of the health shock, we find no or only considerably smaller effects for younger coworkers.

We contribute to several strands of the literature. First, we extend the largely grown literature on the spillover effects of health shocks. Existing studies have mainly focused on the family, in particular on the effects on household wealth (e.g., Wu, 2003), spouses' labor supply (e.g., Coile, 2004; Fadlon and Nielsen, 2021; García-Gómez et al., 2013; Jeon and Pohl, 2017; Nahum, 2007), and well-being (e.g., Angelini and Costa-Font, 2023).[4] There is considerably less evidence on spillovers to groups outside the household, such as coworkers. One exception to this is the paper by Fadlon and Nielsen (2019), which is based on administrative data from Denmark. Although the authors are primarily interested in the effect of health shocks on spouses' and children's health behavior, they find that the increase in preventive care associated with a health shock also extends to coworkers. They show that the results are driven by coworkers who are similar to the shock worker in terms of age and occupation class. Similarly, Kapeller (2021) uses data from Austria to show that a worker's cancer diagnosis leads coworkers who are similar to the shock worker to participate more frequently in health screening programs, such as general health check-ups, PSA tests, and gynecological check-ups. These effects even cascade to coworkers' spouses. Pruckner et al. (2020) find evidence that worker health behaviors (i.e., the participation in different forms of health screening) are highly correlated. This is particularly true when workers have similar characteristics.

Second, we extend the literature on the labor market response of coworkers to health shocks, where the study by Jäger and Heining (2022) is most closely related to our work. They use the death of a worker to estimate the magnitude of personnel replacement cost for firms in the German labor market.[5] Their results suggest that incumbent workers and external hires are not perfect substitutes, as the death of a worker increases the earnings of coworkers and the probability of being employed by the same firm. At the same time, the probability of employment in other firms decreases. Larger effects emerge when the deceased worker has more specific human capital and when there are only few workers in the deceased worker's occupation in the local external labor market.[6] Using administrative data from Brazil, Fietz and Schmeißer (2024) show how changes in the racial composition of a firm's workforce caused by worker deaths affect the retention of employees. They find that, relative to the death of a white worker, the loss of a non-white worker reduces the probability of remaining at the firm by 1.8 percent for non-white coworkers. Since the effect is driven by quits, this suggests that the change in the probability of staying is due to changes in labor supply rather than changes in labor demand. Other studies using deaths as an exogenous variation in worker exits focus on spillover effects in very specific settings, such as the effect of CEOs on firm performance (e.g., Becker and Hvide, 2022)

[4] Several studies also document the effects of parental health shocks on (adult) children's labor supply and/or well-being (e.g., Frimmel et al., forthcoming; Glaser and Pruckner, 2023; Maestas et al., 2024; Norén, 2020; Ramirez Lizardi et al., 2024; Rellstab et al., 2020).

[5] Bertheau et al. (2022) also use worker deaths to estimate replacement costs. They find that worker exits due to unexpected deaths reduce the profits of Danish firms by a total of 41,000 Euros. This profit loss is mainly due to a loss of value added and an increase in incumbent workers' earnings.

[6] Spillover effects on coworkers' careers have also been found for changes in labor supply that are not related to workers' deaths. For example, Bianchi et al. (2023) study an Italian pension reform to show that the delayed retirement leads to lower wage growth and promotion rates for coworkers. The effects are larger when senior workers are in a higher position and for coworkers over the age of 55.

and the effect of individual inventors and superstar academics on the success of their team members (e.g., Azoulay et al., 2010; Jaravel et al., 2018; Oettl, 2012).

Finally, we also complement the literature on peer effects on different dimensions of individual behavior. Studies in this area have estimated peer effects in welfare use and participation in social security programs (e.g., Åslund and Frederiksson, 2009; Dahl et al., 2014; Markussen and Røed, 2015), risky health behavior (e.g., Eisenberg et al., 2014; Fletcher, 2012; Gaviria and Raphael, 2001), preventive healthcare (e.g., Bouckaert et al., 2020; Francetic et al., 2022; Goldberg et al., 2023; Redler and Reichel, 2024), worker absenteeism (e.g., Bradley et al., 2007; Hesselius et al., 2009; Ichino and Maggi, 2000), productivity (e.g., Bandiera et al., 2010; Guryan et al., 2009; Mas and Moretti, 2009), and wages (e.g., Cornelissen et al., 2017). Our results on the negative impact of severe health shocks on shock workers' own employment also add to the existing evidence on the adverse employment and earnings consequences of health shocks for those experiencing them (e.g., Ahammer et al., 2024; Bíró et al., 2024; Dobkin et al., 2018; García-Gómez et al., 2013; García-Gómez, 2011; Halla and Zweimüller, 2013; Parro and Pohl, 2021; Trevisan and Zantomio, 2016).

Our study contributes to the existing literature in three ways. First, we extend the existing evidence by considering the impact of severe health shocks on coworkers' labor market outcomes and health behavior. Other studies mainly focus on worker deaths, especially those interested in the labor market responses of coworkers. We believe that health shocks are a more relevant treatment than deaths because they occur more frequently and therefore represent a more realistic scenario for a wide range of firms. Second, we consider responses to an event that mainly leads to a partial loss of productivity and human capital. In the case of worker deaths, firms face a total loss of productivity, which is not necessarily the case for severe health shocks. Firms and coworkers may react differently to a partial and/or temporary substitution of shock workers than to a full and/or permanent substitution. Finally, we synthesize the two strands of the literature that consider the spillover effects of health shocks on health behavior and labor market responses. Our detailed and comprehensive administrative data on individual-level labor market activity and healthcare utilization allow us not only to study changes in coworkers' labor market outcomes, but also to consider healthcare utilization and preventive health behavior as relevant outcomes of interest.

The remainder of the paper is organized as follows. Section 2 summarizes the institutional setting of this study and describes the data used in the empirical analysis. Section 3 provides a detailed description of the empirical strategy. Section 4 presents our main results, effect heterogeneity, and different robustness checks. Section 5 concludes the paper.

# 2 Institutional Setting and Data

## 2.1 Institutional Setting

*Austrian Labor Market:* The Austrian labor market is characterized by a combination of strong institutional regulation of wages and working conditions and relative flexibility in terms of dismissals and turnover. Wages are generally set by collective agreements negotiated between union and employer representatives (Pollan, 2009). About 98 percent of the Austrian workforce is covered by a collective bargaining agreement.[7] At the same time, turnover is relatively high and employment protection against dismissal is relatively weak (Böheim, 2017). Firms can unilaterally dismiss workers without having to give a specific reason, subject to a statutory notice period.

The Austrian economy is characterized by small firms rather than large corporations. Our analysis focuses on firms that employ fewer than 50 workers four quarters before the health shock. In 2021, firms with a firm size between 1 and 49 workers accounted for about 36 percent of all firms. Together, they employed 34 percent of all workers.[8] Our sample therefore covers a non-negligible part of the Austrian labor market.

*Social Security:* Austria has a *Bismarckian* social security system that offers universal access to healthcare, pensions, disability, and unemployment benefits. Individuals are assigned to one of the health insurance funds based on their place of residence and occupation. The *Austrian Health Insurance Fund* (*Österreichische Gesundheitskasse*) is the largest insurer, covering about three-quarters of all private-sector workers.[9] Health insurance provides access to a wide range of healthcare services, including outpatient visits, inpatient care, prescription drugs, and health and cancer screening programs.[10] Expenditures on outpatient care and medication are financed mainly by wage-based social security contributions paid by employees and employers. Hospital costs are also partly financed by tax revenues. General practitioners (GPs) have traditionally been the first point of entry into the healthcare system. They can refer patients to specialists in the outpatient sector and/or to a hospital if necessary. GPs also usually certify sick leave.

Employees are also automatically enrolled in Unemployment Insurance (UI), which is funded by a payroll tax shared equally between workers and employers. UI benefits are available regardless of the reason for separation, including dismissal for poor performance

[7] Source: OECD Employment and Labour Market Statistics.

[8] Source: Statistics Austria, own calculations.

[9] Before being merged into one entity in 2020, the *Austrian Health Insurance Fund* was divided into nine separate provincial health insurance funds (*Gebietskrankenkassen*).

[10] Insured individuals over the age of 18 can participate in a general health check-up program. This is usually performed by GPs and includes a medical history and a series of age- and gender-specific diagnostic and laboratory tests to detect health risks and diseases at an early stage. Men have the option of a regular prostate-specific antigen (PSA) blood test, while women over the age of 40 are entitled to a mammography screening every two years to detect breast cancer (Pruckner et al., 2020).

or misconduct. Laid-off workers are entitled to benefits immediately upon becoming unemployed, while workers who quit their jobs receive benefits only after a one-month waiting period. The minimum replacement rate is 55 percent of the daily net wage before unemployment. There are higher replacement rates for workers with dependents. The duration of UI benefits depends on the age and the labor market history of the worker. After UI benefits are exhausted, means-tested income support is available (Ahammer et al., 2023).

The public pension system covers all private-sector workers in Austria and provides early retirement, old-age, and disability pensions. Public pensions are the main source of income for retirees in Austria. The amount of the pension depends on the number of insurance months accumulated during the individual's working life and their income history (Frimmel, 2021). Compared to other countries, Austrian pensions are relatively generous. The replacement rate of 74.1 percent is well above the OECD average of 50.7 (OECD, 2023). The statutory retirement age is 65 for men and 60 for women. However, the actual retirement age is much lower. In 2022, the average retirement age was 61.6 for men and 59.7 for women (Dachverband der Sozialversicherungsträger, 2023). This gap is mainly explained by early retirement due to disability and schemes that were introduced to smooth the transition to retirement (e.g., part-time retirement schemes). Employers also influence their workers' decision to retire by offering special severance payments (i.e., golden handshakes), especially when wages for older workers are high (Frimmel et al., 2018).

## 2.2 Data Sources

We use two comprehensive administrative data sets to analyze the effect of severe health shocks of workers on their coworkers. Detailed information on daily labor market activity is available from the *Austrian Social Security Database* (*ASSD*). The *ASSD* is structured as a matched worker-firm data set. The data record all employment, unemployment, and retirement spells of all private sector workers in Austria between 1972 and 2018 (Zweimüller et al., 2009). Based on these spells, we identify the main employer of each worker in a given quarter, which is important for identifying the coworkers of those workers who suffer a health shock. The data set also provides information on firm characteristics, workers' socio-demographic characteristics, and earnings.

The *ASSD* data can be linked to health record data from the *Upper Austrian Health Insurance Fund* (*UAHIF*). These data provide detailed information on outpatient visits, prescribed medications, and hospital visits for more than one million private sector employees and their dependents in Upper Austria from 2005 to 2018.[11] The data on hospital visits also include the admission diagnoses according to the International Classification of

[11] With more than 1.5 million inhabitants (16.73 percent of the total Austrian population), Upper Austria is the third largest of Austria's nine federal states (see Statistics Austria).

Diseases (ICD-10) scheme. We use this information to identify the relevant health shocks (see Section 3.1).

## 2.3 Outcome Variables

*Labor Market Outcomes:* Regarding the labor market activity of coworkers, we examine employment, earnings, and retirement. For employment, we use the *ASSD* to obtain the number of days of employment in a quarter. We also consider the probability of being employed in the firm where the health shock occurred. To do this, we obtain a worker's main employer in each quarter and define a binary indicator that equals one if a worker is employed in the shock firm in a given quarter and zero otherwise. The *ASSD* also provides information on workers' earnings. These earnings are top-coded by the maximum social security contribution base.[12] Because no information on hours worked is available, we can only consider daily earnings. We also analyze wage growth by calculating the difference in log daily earnings between two consecutive quarters. Retirement is measured by the number of retirement days in a given quarter. This includes all types of retirement (i.e., regular, disability and early retirement).

It has been shown that Austrian firms offer golden handshakes to influence their workers' retirement decisions (Frimmel et al., 2018). Using data from the Austrian Ministry of Finance, we also examine whether coworkers receive a golden handshake after the health shock. The data include information on special severance payments. We classify these special severance payments as golden handshakes if the amount of the severance payment exceeds the monthly earnings by a factor of 8 or more. Since the data from the Ministry of Finance are only available until 2012, we analyze this outcome only for health shocks that occurred before 2010.

*Health Outcomes:* The link to the *UAHIF* data allows us to study the effects of health shocks on coworkers' health (behavior). We use information on sick leaves to measure coworkers' absenteeism. We consider both the number of sick leave days associated with all diagnoses in a given quarter and the number of days associated with easy-to-fake diagnoses, which are associated with symptoms that are not readily visible and therefore difficult for physicians to verify.[13] Previous research has shown that these sick leaves are related to shirking (Ahammer, 2018), so this outcome can also be interpreted as a surrogate measure of worker effort.

With respect to health behavior, we consider the use of various health screening programs as an outcome. Specifically, we use the information on outpatient expenditures pro-

[12] The maximum social security contribution base changes from year to year. In 2024, it is 6,060 Euros per month, which is equivalent to 202 Euros per day (see oesterreich.gv.at).

[13] These diagnoses include only sick leaves related to the common cold (ICD-10 J04 and J06), low back pain (ICD-10 M54.5), and headache (ICD-10 R51).

vided by the *UAHIF* data to examine the probability of having a general health check-up, which includes several tests for cardiovascular and cancer diseases, a PSA test (only for men), or a mammography (only for women) in a given quarter. For each screening program, we define separate binary indicators that are equal to one if expenditures for that screening program are greater than zero in a given quarter, and equal to zero otherwise.

# 3 Empirical Strategy

To estimate the causal effect of a worker's severe health shock on coworkers' labor market and health outcomes, we would ideally compare coworkers in a firm who experience such a shock with identical coworkers who do not. We address the key challenge of constructing a valid counterfactual for treated coworkers by combining a matching strategy with a difference-in-difference model. We first describe the identification of health shocks and the matching strategy in detail before explaining the construction of the estimation sample and the empirical model.

## 3.1 Identification of Health Shocks and Matching

To construct an appropriate control group for those coworkers who experience a severe health shock of one of their colleagues, we follow the approach implemented by Jäger and Heining (2022). First, we identify severe health shocks of workers in the administrative health data. This allows us to divide firms and their workers into those that experience a severe health shock of a worker and those that do not. Second, we implement a combination of exact and propensity score matching to find a statistical twin for each worker-firm pair with an actual health shock in the pool of those workers employed in firms without a health shock. The treatment group comprises all worker-firm pairs where a worker actually experiences a severe health shock. The control group consists of firms where no health shock occurs, but where there is a placebo shock worker who is similar to an actual shock worker. Finally, we add information on the coworkers to construct our final estimation sample. We describe each step in more detail below.

*Identification of Severe Health Shocks:* In a first step, we identify severe health shocks based on the admission diagnoses recorded in the hospital data. The health shocks we consider are cancer and circulatory diseases. Specifically, we consider all types of malignant neoplasms (ICD-10 C), myocardial infarction (ICD-10 I21 and I22), heart failure (ICD-10 I50), and cerebrovascular disease (ICD-10 I60 to I66). For each individual, we keep the first severe health shock diagnosis observed in the data. We then keep only those individuals, who were employed and aged between 15 and 65 at the time of the shock, and aggregate the individual worker-level information on the severe health shocks at the firm level. To do this, we keep the first shock diagnosis observed among all workers with health shocks in a given firm. To ensure that we can accurately identify the worker responsible for the health shock, we drop firms with multiple first shocks in the same quarter.

We then combine this sample of firms with a health shock with the sample of all other firms. In order to have a consistent sample of firms, we include only firms that (i) employ more than three workers in all quarters that a firm is observed, (ii) are observed without any gaps, (iii) are observed for at least four consecutive quarters, and (iv) where all workers are insured in Upper Austria. The last restriction is necessary to ensure that firms in the donor pool do not actually experience a health shock. Since the hospital data are only observed for workers insured in Upper Austria, we can only be sure that no relevant hospital diagnoses were recorded for a worker in a given firm only if all workers in the firm are insured in Upper Austria.

*Matching Procedure:* Based on the information on whether a firm experiences a severe health shock of a worker or not, we divide the sample into a treatment group and a donor pool. The treatment group includes all worker-firm pairs that experience a worker's first severe health shock between 2006 and 2018. The donor pool includes all worker-firm pairs of firms that do not experience a severe health shock until 2018.[14]

For each worker-firm pair with an actual severe health shock experienced in quarter $s$, we select a worker-firm pair from the donor pool with similar lagged observable characteristics. We do this using a combination of exact and propensity score matching. In this way, for each worker-firm pair treated in quarter $s$, we construct a control group consisting of similar worker-firm pairs from firms that do not experience a severe health shock. The list of variables used in the matching procedure includes firm and worker characteristics. The firm characteristics include the *number of workers*, *industry at the 2-digit level*, the median wage, the age composition of the workforce, the age of the firm, the share of women, the share of white-collar workers, the share of blue-collar workers, and the share of non-Austrian citizens in the workforce. For the worker characteristics we choose *age in five-year categories*, daily earnings, and binary indicators for workers' *collar*, *gender*, *citizenship*, and for *whether a worker is insured in Upper Austria*.[15] All variables are measured four quarters before the health shock (i.e., in $s-4$). Thus, we are matching on the levels and not on the trends of these variables, which means that the pre-trends can still be used as an appropriate validity check for the parallel trends assumption.

Throughout the matching process, which we perform separately for each shock quarter

[14] To ensure that the firms in the donor pool are truly untreated, we use information on hospital diagnoses from 1998 onward. This allows us to remove firms from the donor pool that could have experienced a health shock at any time after 1998 but before 2006.

[15] The variables printed in *italics* are used in the exact matching, while the other characteristics were used to compute the propensity score.

$s$, we ensure that each firm and worker in the donor pool is used only once as a matching pair. In cases where there are multiple closest matches in the donor pool, one of them is selected randomly. Using this procedure, we can find a match for 45 percent of all worker-firm pairs with a severe health shock. Table A.1 in the Appendix compares firm and worker characteristics of matched and unmatched worker-firm pairs. Matched firms are on average smaller than unmatched firms.[16] They also employ more women and have a smaller share of blue collar workers and foreigners in their workforce. Average daily wages are 7.5 Euros lower in matched firms than in non-matched firms. Consistent with these firm-level differences, there are more women and fewer non-Austrian citizens among the matched workers, and their wages are lower than those of non-matched workers. Matched workers are also more likely than non-matched workers to be employed in the shock firm four quarters before the health shock. They also have lower healthcare expenditures, but these differences are not statistically significant. Panel (a) of Figure 1 shows that matching success is relatively stable over time. The propensity scores in the treatment and control group are very similar, indicating that our approach leads to close matches (Panels (b) and (c)).

*Construction of the Estimation Sample:* So far, we have focused mainly on those workers who experience a severe health shock, identifying a statistical twin for each of them in the data. However, we are ultimately interested in the outcomes of the coworkers of these shock workers and their matched controls. Therefore, in a final step, we add information on all workers employed at the firm where a (placebo) health shock occurs in quarter $s$, excluding of course the observations of the workers responsible for the health shock. In the end, we compare the coworkers of workers who actually experience a severe health shock with coworkers who have a colleague without a health shock but who is similar to an actual shock worker on key observable characteristics and who is employed in a firm without a health shock that has similar characteristics to a firm where an actual health shock occurred.

For our main estimation sample, we construct a panel at the coworker-quarter level that tracks coworkers for up to 12 quarters before and 12 quarters after the (placebo) health shock. We impose several sample restrictions. First, we restrict the sample to firms that employ no more than 50 workers four quarters before the health shock. This should increase the likelihood that coworkers are actually aware of their colleague's health shock. Since we do not have information on the detailed organization of firms and workers' occupations, we cannot be sure that workers actually know each other. However, this is more likely to be the case in smaller firms than in larger ones. This restriction also allows

[16] One explanation for the difference in firm size between matched and unmatched firms is that we match on the exact firm size. Since the probability of finding an untreated firm with the exact same firm size as a treated firm is higher for smaller firms than for larger firms, the sample of matched firms tends to include more small firms.

us to clearly identify the first severe health shock in a firm, as larger firms are likely to experience multiple health shocks of workers.

Second, we restrict the sample to include only shock workers and coworker who have been employed at the shock firm for at least four quarters prior to the health shock (including the quarter of the health shock). Again, this should increase the likelihood that workers actually know each other and are aware of a colleague's severe health shock.

Third, because firms and coworkers may experience a second health shock of a worker during the sample period, we only include observations up to the quarter of the second shock at the firm and/or coworker level. In our sample, about 18 percent of treated firms experience a second health shock of a worker, with an average duration of 12 quarters between the two events. Only 0.8 percent of coworkers experience a second health shock of one of their colleagues.[17]

Lastly, our main estimation sample focuses on coworkers who are older than 50 years at the time of the (placebo) health shock. We believe that this age group is most relevant for the outcomes considered in this paper. The labor market outcomes include the number of retirement days in a given quarter. Since retirement decisions are usually made at the end of a career, this outcome is only relevant for older workers. In addition, health screening programs are tend to target older individuals. For PSA tests, men over the age of 45 are advised to undergo them regularly.[18] A mammography should be done every two years from the age of 40 onward. Women between the ages of 45 and 74 receive an invitation letter from the health insurance to remind them to undergo the procedure.[19] For completeness, we also show the results for younger coworkers in the Appendix.[20]

*Descriptive Statistics:* Table 1 provides descriptive statistics comparing the characteristics of coworkers in the treatment and control group before the (placebo) health shock. Coworkers are about 55 years old at the time of the (placebo) health shock, work on average 89 days per quarter, and earn approximately 72 Euros per day. The probability of being employed in the shock firm before the health shock is 0.92. 51.6 percent of all coworkers are women and 6.3 percent are non-Austrian citizens. Overall, the differences between coworkers in the treatment and control group are small and not statistically significant. One exception is medication expenditure. Coworkers in the control group spend about 9 Euros more per quarter than those in the treatment group. The average firm in the sample employs 16.7 workers before the health shock. Firms in the treatment group have a lower share of women and blue-collar workers than firms in the control group,

[17] Note also that a coworker may appear as a coworker for multiple (placebo) shock workers at different times. In our sample, however, 99.58 percent of coworkers appear only once.

[18] See Österreichische Krebshilfe for details.

[19] See Österreichische Krebshilfe for details.

[20] Note that we apply the age restriction only to coworkers. Shock workers can be younger than 50 at the time of the (placebo) health shock.

while average wages are slightly higher in treatment firms than in control firms. None of the differences are statistically significant.

In Table A.2 in the Appendix, we report descriptive statistics for the characteristics of the shock workers included in the main estimation sample. The average shock worker is about 50 years old at the time of the (placebo) health shock. Like their coworkers, they are highly attached to the labor market and the shock firm. Approximately half of the shock workers are women and 2.6 percent of them are non-Austrian citizens. Compared to the coworkers, the shock workers earn slightly less. Again, the differences between the treatment and control group group are small and not statistically significant. However, as would be expected, workers who actually experience a health shock have higher healthcare expenditures than workers with a placebo shock.

Figure 2 shows the distribution of the different types of severe health shocks in the main estimation sample. More than two-thirds of all health shocks are associated with malignant neoplasms. As Table A.3 in the Appendix shows, the most common types of cancer are malignant neoplasms of the breast (ICD-10 C50), skin (ICD-10 C43 and C44), male genital organs (ICD-10 C60 to C62), and digestive organs (ICD-10 C15 to C26). 17.95 percent of health shocks are related to cerebrovascular diseases, while myocardial infarction accounts for 11.45 percent. 2.78 percent suffer from heart failure.

## 3.2 Estimation Strategy

To estimate the effect of a severe health shock on the labor market and health outcomes of coworkers, we estimate the following two-way fixed effects (TWFE) model:

$$y_{iq} = \alpha_i + \beta \cdot \mathbb{1}\left(q \geq 0\right)_q + \delta \cdot \left[\mathbb{1}\left(q \geq 0\right)_q \times HS_i\right] + \boldsymbol{\gamma}\boldsymbol{X}_{iq} + \varepsilon_{iq} \tag{1}$$

where $y_{iq}$ is the outcome of interest of coworker $i$ observed in the relative quarter $q$. The relative quarter is defined as the difference between the calendar quarter $t$ and the quarter of the (placebo) health shock $s$ (i.e., $t - s$). $\alpha_i$ are coworker fixed effects, $\mathbb{1}\left(q \geq 0\right)_q$ is a binary indicator that is equal to one for the post-treatment period and zero for the pre-treatment period, and $HS_i$ is a binary indicator equal to one for those coworkers who actually experience the health shock of one of their colleagues and zero for those coworkers with a placebo health shock. $\delta$, the coefficient of the interaction term $\mathbb{1}\left(q \geq 0\right)_q \times HS_i$, is the coefficient of interest. It measures the average effect of experiencing an actual severe health shock on the outcome of interest as compared to no shock occurring. $\boldsymbol{X}_{iq}$ includes calendar year $\times$ birth year fixed effects to flexibly control for age and general trends in the outcome variable. Since all coworkers in a shock firm are either treated or not treated, standard errors are clustered at the shock firm level.

Interpreting $\delta$ as the causal effect of a severe health shock on coworkers requires that the outcomes of coworkers in the treated group would have evolved in the same way as in the control group in the absence of the health shock. While the parallel trends assumption is not directly testable, one can check whether coworkers' outcomes are already on different trends before the health shock. If this is not the case, it is likely that the outcomes would have actually evolved in the same way in the absence of the health shock. We show that coworkers' outcomes are not on diverging trends before the (placebo) health shock by estimating the following dynamic TWFE model:

$$y_{iq} = \alpha_i + \sum_{k=-12,\ k\neq-4}^{11} \beta_k \cdot \mathbb{1}\left(q=k\right)_q + \sum_{k=-12,\ k\neq-4}^{11} \delta_k \cdot \left[\mathbb{1}\left(q=k\right)_q \times HS_i\right] + \boldsymbol{\gamma} \boldsymbol{X}_{iq} + \varepsilon_{iq} \quad (2)$$

where $\mathbb{1}\left(q=k\right)_q$ is a series of binary indicators for the different relative time periods. The coefficients $\delta_k$ denote the treatment effects of interest relative to the reference period of four quarters before the (placebo) health shock. Setting the reference period to $q=-4$ allows us to estimate treatment effects for the last quarters before the health shock and to test for the existence of anticipation effects. All other variables are defined as in Equation (2).

In our empirical setting, not all coworkers experience a severe health shock of one of their colleagues at the same time. Such variation in treatment timing may be problematic if treatment effects vary over time (Goodman-Bacon, 2021). To address this potential concern, we also provide estimates for the model specified in Equation (2) based on the method developed by Sun and Abraham (2021) in the Appendix.

# 4 Results

We first focus on those workers that actually suffer a severe health shock. This initial analysis shows that the health shocks considered in this study are indeed a relevant and significant event in the life of these workers in terms of their health and labor market activity. This analysis also helps to understand the extent of the need to replace a shocked worker within a firm. Knowing about the magnitude and relevance of the treatment, we then examine the impact of the severe health shocks on coworkers' labor market outcomes and health behavior.

## 4.1 Effect of Health Shocks on Shock Workers

We consider different types of healthcare expenditures to verify that our empirical strategy indeed identifies severe and unexpected health shocks that have a long-lasting negative impact on the health of those who suffer them. The first column of Figure 3 plots average inpatient, outpatient, and medication expenditures for each relative period separately for treatment and control shock workers. The second column shows the estimates from the dynamic TWFE model specified in Equation (2). Prior to the health shock, there are no differences in expenditures between workers in the treatment and control group, suggesting that the health shocks are unexpected. However, in the quarter of the health shock, expenditures in all three categories increase substantially for treated workers. The average health shock increases inpatient expenditures by about 9,700 Euros, outpatient expenditures by about 78 Euros, and medication expenditures by about 297 Euros in the quarter of the shock. Although the estimated coefficients decrease in subsequent periods, persistent differences in health status between treated and control workers remain. Even 12 quarters after the health shock, inpatient expenditures of treated workers are about 850 Euros higher than those of control group workers compared to four quarters before the health shock. Outpatient expenditures are about 40 Euros higher and medication expenditures are about 215 Euros higher. We also find that expenditures for PSA tests, mammography, dermatology, and radiology (see Figure A.1 in the Appendix) increase significantly in the quarter before and the quarter of the health shock. This suggests that standard preventive screening tools are responsible for the detection of the health shock diagnoses.

Another indicator for the severity of the health shock is the increase in mortality. As Panel (a) of Figure A.2 in the Appendix shows, treated workers have an 11 percent chance of dying within 12 quarters after the shock, while it is virtually zero for the control group. Panel (b) shows that the type of the health shock has important implications for the increase in mortality. While the immediate increase in mortality associated with cardiovascular diseases is higher than that associated with cancer, the long-run increase is much larger for those with a cancer diagnosis as their first health shock. Also, those aged below 50 at the the time of the health shock have a lower probability of dying within 12 quarters after the shock (Panel (c)).

The severe health consequences for those who suffer the shocks considered in this paper also have a significant impact on their labor market participation. As Panels (a) and (b) of Figure 4 show, those workers who suffer a health shock considerably reduce the number of days of employment. The largest decrease of about 30 days is observed two quarters after the health shock. This corresponds to a 33 percent decrease compared to the pre-shock sample average. Although this gap narrows over time, 12 quarters after the health shock, treated workers spend 10.87 fewer days in employment than those without a health shock. Most importantly, this reduction in employment is a relevant factor for the firm in which shock workers are employed at the time of the shock. Panels (c) and (d) of Figure 4 show that workers with a health shock are 25 percentage points less likely to work at the shock firm two quarters after the health shock. Although some workers appear to return after some time, there is still a gap of 7.3 percentage points relative to the control group after 12 quarters. Thus, these health shocks do indeed represent a significant loss of human capital that must be replaced by new hires or compensated by current coworkers. There is some heterogeneity in the employment response with respect to the shock type and the age at the time of shock. While the immediate reduction in employment is larger for cancer patients, they face a smaller long-term effect than those suffering from a cardiovascular disease (Panel (e)). Also, workers under 50 tend to return to the labor market more often in the long run than older workers, although the immediate employment responses are similar for both groups (Panel (f)).

Even if workers with a health shock remain in the labor market, there is a substantial reduction in their actual labor supply. As Panel (a) of Figure A.3 in the Appendix shows, the health shock leads to a persistent increase in absenteeism. The average increase in sick leave days is about 36 days in the quarter of the health shock, which is 14 times higher than the pre-shock average. Even 12 quarters after the health shock, treated workers have 2.1 more sick leave days than workers in the control group. Importantly, Panel (b) shows that this increase in sick leave days is not driven by sick leaves associated with easy-to-fake diagnoses, such as back pain. This implies that the increase in absenteeism is not due to shirking, but is actually caused by a reduction in work capacity.

Panel (c) of Figure A.3 in the Appendix shows that the reduction in labor supply of those who suffer the severe health shocks is also reflected in an increase in retirement days. Five quarters after the health shock, workers in the treatment group spend about 4 to 6 days more per quarter in retirement than workers in the control group. The average control group worker spends 3.8 days in retirement in the fourth quarter after the health shock, implying that workers with a health shock spend more than twice as many days in retirement.[21] The increase in the total number of retirement days is driven by an increase in disability retirement (Panel (d)).

Overall, the health shocks are a significant event not only for the affected workers, but also for the firms in which these workers are employed at the time of the shock. Firms are affected both by an increased probability of workers leaving the firm and dropping out of the labor market and by workers taking more sick leaves. Thus, firms need to compensate for this reduction in their labor force. Because of the partial nature of the reduction in employment, the compensation is more likely to come from existing coworkers than from new hires.

## 4.2 Effect of Health Shocks on Coworkers

*Labor Market Outcomes:* The previous section shows that the health shocks studied in this paper have substantial and long-lasting negative effects on the labor market and health outcomes of those who suffer them. We now turn to the effects of these health

[21] To construct our estimation sample, we require that workers be employed at the shock firm in the quarter of the shock and the three quarters preceding the shock. Because of this requirement, the number of retirement days prior to the health shock is close to zero. For all outcomes related to retirement, we report the sample average of the control group in the post-period or in relative quarter 4 as the mean of the outcome in all tables and figures, as we believe this is a more appropriate value for gauging the size of the estimated coefficients.

shocks on coworkers by estimating Equation (1) for the labor market and health outcomes of coworkers. Table 2 reports the average treatment effects for coworkers' employment, earnings, and retirement. Compared to coworkers in the control group, those who actually experience a severe health shock of one of their colleagues have on average 1.07 more days of employment per quarter after the health shock. This corresponds to an increase of 1.20 percent relative to the average number of quarterly employment days before the treatment. The increase in employment days is also reflected in the probability of being employed in the firm where the health shock occurred. The health shock increases coworkers' probability of working at the shock firm by 2.32 percentage points or 2.52 percent. Both coefficients are statistically significant at the 10 percent level (p-values 0.083 and 0.095). Consistent with the finding that treated coworkers are more attached to the labor market, Column (3) shows that the coworkers with a health shock spend, on average, 1.05 fewer days per quarter in retirement than coworkers in the control group. The point estimate is statistically significant at the 5 percent level and is of a similar absolute size as the increase in employment days.

Columns (4) and (5) of Table 2 report the coefficients for coworkers' daily earnings and earnings growth. The estimated coefficients are not statistically significant for either outcome. Although this suggests that firms do not compensate coworkers for staying longer at the shock firm with higher wages, there may be other forms of remuneration that firms could choose to reward their employees. One such remuneration could be special severance payments (i.e., golden handshakes) when coworkers leave the firm. We analyze the probability of receiving a golden handshake from the shock firm after the health shock by estimating a cross-sectional regression including a binary treatment indicator, a set of coworker and shock worker characteristics, and shock year fixed effects.[22] Table A.4 in the Appendix reports the results for all coworkers employed at the shock firm in the quarter of the health shock in Columns (1) and (2), and for the subsample of coworkers whose last employment spell before retirement was at the shock firm in Columns (3) and (4). The estimated coefficients suggest that coworkers who experience a severe health shock of one of their colleagues are more likely to receive a golden handshake after the health shock occurred. This is also true for coworkers who leave the shock firm for retirement. This implies that rather than adjusting base pay to compensate coworkers for staying longer at the shock firm, firms reward treated coworkers through other forms of remuneration.

*Health Outcomes:* Coworkers' labor market outcomes may not be the only dimension that can be affected by a health shock. The experience of a health shock may induce coworkers to reconsider their own health situation and, at least temporarily, increase

[22] Information on special severance payments is only available until 2012. We therefore restrict the estimation sample for this analysis to all health shocks prior to 2010. We classify a special severance pay as a golden handshake if the amount of the special severance pay exceeds the monthly earnings by a factor of 8 or more.

their preventive health behavior. Therefore, we also analyze the effect of the health shock on coworkers' health (behavior). Table 3 reports the estimated average effects for sick leave days and participation in different forms of health screening programs. Column (1) shows that there is no statistically significant effect on the number of sick leave days. This is also true for sick leave days related to easy-to-fake diagnoses, such as the common cold, lower back pain, and headache. Since sick leaves resulting from these diagnoses have been shown to be related to shirking (Ahammer, 2018), the absences of a statistically significant effect suggests that the health shock does not lead to a change in coworkers' effort.

With respect to coworkers' health behavior, we find some evidence of an increase in the participation in health screening programs.[23] Although the estimated coefficients for the probability of participating in the general health check and undergoing a PSA test in Columns (3) and (4) of Table 3 are positive and non-negligible in magnitude, they are imprecisely estimated. For mammographies, we find that female coworkers with a health shock are 0.64 percentage points (9.73 percent) more likely to have the procedure compared to coworkers in the control group. The estimated coefficient is statistically significant at the 10 percent level (p-value 0.060).

*Discussion:* Our main results suggest that a severe health shock of a worker generally increases the employment of their coworkers. In particular, it increases the probability of working at the firm where the shock occurred. We also find a small but statistically significant delay in retirement. This implies that firms compensate for the loss of the shock worker's working capacity with with incumbents rather than with external hires. While this does not lead to an increase in wages, firms still reward coworkers with special severance payments when they leave the firm for retirement. Importantly, the health shock does not increase coworker absenteeism, and we find suggestive evidence that it does not affect effort. However, witnessing the severe health shock of a coworker does induce changes in health behavior. We find an increase in the probability of participating in health screening programs, especially for mammographies.

Compared to the estimates in Jäger and Heining (2022), our point estimates for the probability of being employed at the shock firm are considerably larger. While Jäger and Heining (2022) find that the death of a worker increases coworkers' probability of being employed at the firm where the death occurred by 0.3 percentage points, or 0.39 percent, we find an increase of 2.32 percentage points, or 2.52 percent.[24] We also find no statistically significant effect on daily wages and wage growth. However, we do find that treated workers are more likely to receive a golden handshake when they leave the

[23] Note that PSA tests are analyzed only for men and mammographies are analyzed only for women.

[24] It is important to note that our main results refer only to coworkers aged over 50 at the time of the health shock, which may limit the comparability of the estimated coefficients. Also, the partial lack of statistical precision does not allow a strong statement about the comparison of effect sizes.

shock firm for retirement. This implies that there is still an earnings gain associated with the health shock, but that this does not seem to be reflected in base pay as in Jäger and Heining (2022).

So far we have only considered coworkers who were older than 50 at the time of the health shock. In the Appendix, we also provide estimates for the same set of outcome variables for younger coworkers. We find that the health shock has, if anything, only a muted effect on young coworkers' labor market outcomes (see Table A.5 in the Appendix). While the number of employment days increases by 0.72 days (p-value 0.079), there is no statistically significant change in the probability of being employed by the shock firm. The number of retirement days increases by 0.12 days. Given that retirement is hardly relevant for this age group, we are cautious about the causal interpretation of this coefficient. As for older coworkers, there is no statistically significant effect on wages and wage growth. We also find no statistically significant effects for sick leave days and participation in health screening programs (see Table A.6 in the Appendix).

## 4.3 Effect Heterogeneity

Our main results suggest that coworkers compensate for the firm's loss of labor supply associated with the severe health shock of one of their colleagues by being more likely to be employed at the shock firm. In general, the health shock increases employment and reduces retirement. It also affects the health behavior of female coworkers by increasing the probability of having a mammography. As a next step we shed more light on potential mechanisms and treatment heterogeneity of our effects.[25] First, we consider the skill level of the shock worker. Productivity losses of higher skilled workers are expected to be more "damaging" to firms and therefore open a larger need for compensation, especially for equally experienced workers. Second, because several papers show that the similarity between coworkers is relevant for behavioral responses (Fadlon and Nielsen, 2019; Pruckner et al., 2020), we estimate the effects separately by the age and gender of the shock worker and the coworker. Finally, because more stress-related cardiovascular health shocks may trigger coworkers differently than cancer diagnoses, we also consider treatment heterogeneity by the type of the health shock.

*Shock Workers' Skill:* Compensating for the reduced labor supply with an external hire may be more difficult for firms when the shock worker is highly skilled. This implies that the increase in the probability of incumbent workers being employed by the shock firm should be larger when a more skilled worker suffers a severe health shock. Since worker skill is not directly observable in our data, we use two surrogate measures. First, since

[25] As the estimated coefficients for some outcomes are not statistically significant at any conventional level, we present the results of the heterogeneity analysis only for those labor market and health outcomes for which the coefficients are statistically significant at least at the 10 percent level in the full sample.

wages should be correlated with workers' skills, we split the sample by the shock worker's position in the shock firm's wage distribution. Table 4 reports the results separately for coworkers of shock workers who are below or above the 75$^{\text{th}}$ percentile of the within-firm wage distribution. As expected, the labor market effects are driven by high-wage (i.e., high-skilled) shock workers. For their coworkers, the health shock increases the probability of being employed at the shock firm by 5.86 percentage points (6.34 percent). This increase in labor supply to the shock firm is reflected in an overall increase in employment days and a decrease in retirement days. For the coworkers of less skilled shock workers, the point estimates have the same sign as those for the coworkers of more skilled shock workers, but they are considerably smaller in size and also not statistically significant. The effect on the probability of having a mammography is positive and non-negligible in magnitude for both groups of coworkers, but is only statistically significant at the 10 percent level for coworkers of less skilled shock workers (p-value 0.071).

As a second measure of worker skill, we use estimated Abowd et al. (1999) (AKM) worker wage fixed effects. A higher AKM worker fixed effect should reflect higher worker productivity, which should be positively correlated with the skill level. Separate results for workers with AKM worker fixed effects above and below the median are reported in Table 5. Again, the estimated coefficients on labor market outcomes are larger for coworkers of productive shock workers than those for coworkers of less productive shock workers. For the latter group, none of the estimated coefficients are statistically significant at any conventional level. The effect on mammographies is of similar magnitude for both groups, but neither coefficient is statistically significant.

*Age:* Panel (f) of Figure 4 shows that the shock workers' employment response to the severe health shock differs by the age of the shock worker at the time of the shock. We therefore estimate Equation (1) separately for coworkers whose colleague with the health shock is below or above 50 at the time of the shock. Table A.7 in the Appendix reports the results, which show that health shocks to younger workers increase their coworkers' employment by 1.91 days, increase the probability of being employed at the shock firm by 3.32 percentage points (p-value 0.083), and reduce retirement by 1.74 days. For the coworkers of older shock workers, the point estimates are smaller in size and not statistically significant at any conventional level. The effect for mammographies, on the other hand, is driven by shock workers over the age of 50. In this group, coworkers with a health shock are 0.95 percentage points (15.4 percent) more likely to have a mammography than coworkers in the control group. The point estimate for coworkers of younger shock workers is smaller and not statistically significant. Overall, these results suggest that older coworkers compensate for the reduced work capacity of younger shock workers and that the similarity between shock workers and coworkers is important for the change in health behavior.

That older coworkers play an important role in cushioning the adverse effects of the severe health shock on the available labor force is further supported by the results in Table A.8 in the Appendix. It reports separate estimates for coworkers younger than 55 (i.e., aged 50 to 54) and those older than 55 at the time of the health shock. The point estimates, although some are imprecisely estimated, suggest that the increase in labor supply is concentrated among coworkers aged 55 and older. For mammographies, the estimated coefficients are non-negligible in size, although only the estimate for coworkers younger than 55 is statistically significant at the 10 percent level (p-value 0.10).

*Gender:* Table A.9 in the Appendix reports the estimated coefficients separately for women and men.[26] We find that the increase in employment days and employment at the shock firm is concentrated among men, while the reduction in retirement days is statistically significant only for women. This implies that the increase in employment for men is not reflected in the decrease in their retirement, and the decrease in retirement for women is not reflected in their employment. Two factors could explain this result. First, the statutory retirement age for women is lower for women than for men (60 vs. 65), which means that, at any age above 50, retirement is a more relevant substitute for employment for women than for men. This is reflected in the outcome means reported in Table A.9 in the Appendix, as women in the control group spend on average 4.2 more days in retirement in the post-treatment period than men in the control group. Thus, the increase in men's employment could be reflected in a decrease in alternative labor market spells other than retirement, such as unemployment. Second, the signs of the point estimates for women's employment and men's retirement are consistent with the estimates for women's retirement and men's employment, but they are imprecisely estimated. For example, the lower bound of the 95 percent confidence interval of the point estimate for men's retirement would suggest a reduction of 1.20 days. This is similar in magnitude to the increase in employment of 1.67 days. Similarly, the upper bound for women's employment days is an increase of 2.07 days, which is similar in absolute magnitude to the reduction in retirement of 1.42 days.

To take the gender of the shock worker into account, we evaluate the effects separately according to whether the shock worker and the coworkers have the same gender or not in Table A.10 in the Appendix. While the increase in employment at the shock firm appears to be driven by cases where the coworkers are of the same gender as the shock worker (p-value 0.075), the coefficients are not estimated with sufficient precision to conclude that the labor market effects are indeed different between the two groups. Consistent with the argument that the changes in health behavior are more likely to occur when coworkers are more similar to the shock worker, we find that the probability of having a

[26] Note that mammographies are analyzed only for women. This outcome is therefore omitted from Table A.9 in the Appendix.

mammography increases by 0.91 percentage points (13.39 percent) when the shock worker is also a woman. Women do not change their participation in mammographies when the health shock affects a man.

*Shock Type:* Panel (e) of Figure 4 shows that the employment responses of shock workers differ between severe health shocks due to cancer and those due to cardiovascular disease, especially in the long run. We therefore also split the sample of coworkers along this dimension and report the results in Table A.11 in the Appendix. The estimates for the labor market outcomes do not suggest a concentration of the effects among a particular group. While the increase in employment days and the reduction in retirement days are larger for cancer-related health shocks, the increase in the probability of being employed in the shock firm is larger for cardiovascular diseases. However, most of the coefficients and the differences between them are not statistically significant. As expected from the fact that mammographies are used to detect breast cancer, we find that the increase in the probability of having one is clearly driven by health shocks due to cancer. Female coworkers who experience the cancer diagnosis of a colleague are 0.88 percentage points (13.49 percent) more likely to have a mammography than those in the control group. For health shocks due to by cardiovascular diseases, the point estimate is negative but not statistically significant.

*Summary:* The heterogeneity analysis shows that the increase in coworkers' employment is particularly strong when a severe health shock affects a highly skilled worker. We find larger point estimates in cases where the shock worker is in the upper part of the wage distribution and for shock workers with an above-median AKM worker fixed effect. This is consistent with the hypothesis that it is more difficult for firms to compensate for the loss of labor supply caused by the health shock with an external hire if the shock worker is more skilled. Instead, incumbent workers step in. In particular, we find that older coworkers fill the gap when the shock worker is relatively young. In addition, the employment effects seem to be concentrated among male coworkers and in cases where the shock worker and coworker are of the same gender. It is important to note, however, that the coefficients for each subgroup are sometimes imprecisely estimated, which is why they provide at best suggestive evidence.

For the effects on health behavior, our results suggest that the similarity between the shock worker and the coworkers is important. We find larger increases in the probability of having a mammography when both groups of workers are in the same age group and are of the same gender. As expected from the intention of the procedure, cancer-related health shocks are mainly responsible for the overall increase. Again, the lack of statistical precision prevents us from drawing stronger conclusions.

## 4.4 Robustness

*Dynamic TWFE Estimates:* So far, we have only considered the average post-treatment effect of a severe health shock on coworkers' outcomes estimated from Equation (1). This specification does not allow us to analyze the development of the outcome variables in the pre-treatment period, which is commonly used as a validity check for the parallel trends assumption. It is also not possible to determine whether the estimated treatment effects are constant or change over time. Therefore, to provide a more complete picture of the main results, we also estimate the dynamic TWFE model specified in Equation (2).

Figure A.4 in the Appendix shows the results for coworkers' labor market outcomes. Overall, the estimated coefficients in the period before the (placebo) health shock are not statistically significant. The outcomes of coworkers in the treatment and control group do not follow different trends before the treatment, suggesting that this is likely to have been the case in the absence of the treatment. Regarding the post-treatment period, we find positive point estimates for employment days (Panel (a)) and the probability of working in the shock firm (Panel (b)) in all quarters after the health shock. Although this generally confirms the effects documented in Table 2, the estimated coefficients are imprecisely estimated and not statistically significant. Consistent with the positive point estimates for employment, coworkers in the treatment group spend fewer days in retirement in all quarters after the health shock (Panel (c)). The point estimates appear to increase slightly over time and are only statistically significant at the 5 percent level in later quarters. The dynamic point estimates for log daily wages (Panel (d)) and wage growth (Panel (e)) are close to zero and statistically insignificant throughout the entire post-treatment period.

The results for coworkers' health outcomes are shown in Figure A.5 in the Appendix. Again, the assumption of parallel trends is credibly fulfilled for these outcomes. As for the average effects in Table 3, the dynamic estimates show no statistically significant effects for all sick leaves (Panel (a)) and easy-to-fake sick leaves (Panel (b)). While not reflected in the average effect for the probability of participating in the general health screening, positive and statistically significant point estimates emerge in the quarter before and the quarter of the (placebo) health shock (Panel (c)). This suggests that the increased participation of shock workers in health screening programs prior to the health shock induces their coworkers to also participate in the general health check-up. The probability of having a PSA test does not increase as a result of the health shock. If anything, some of the statistically significant point estimates go in the opposite direction (Panel (d)). The point estimates for the probability of having a mammography fluctuate around zero and are not statistically significant (Panel (e)).

Not all coworkers in our estimation sample experience the severe health shock of one of their colleagues at the same time. In case of treatment effect heterogeneity, this variation in treatment timing may lead to biased estimates (Goodman-Bacon, 2021). To address this concern, we re-estimate Equation (2) for all our main outcomes using the method developed by Sun and Abraham (2021). Figures A.6 and A.7 in the Appendix show the results. There are no quantitatively significant differences in the estimated coefficients between our main estimates and the Sun and Abraham (2021) coefficients.

*Other Robustness Checks:* The main results are robust to (i) restricting the sample to coworkers where the shock worker leaves the firm after the health shock, (ii) weighting the regressions, and (iii) using an estimation sample where the constraint on employment in the shock firm before the health shock is relaxed.

First, we show that the estimated effects are driven by those cases where the shock worker leaves the firm after the health shock. If our finding that coworkers compensate for the reduction in working capacity by being more likely to stay at the shock firm, the effects should be driven by those cases where the loss in labor supply is even greater (i.e., when the shock worker leaves the firm). We therefore estimate Equation (1) separately for coworkers where the shock worker leaves the firm within one quarter after the health shock and for coworkers where the shock worker stays at the shock firm. Table A.12 in the Appendix shows the results for the labor market outcomes. As expected, the employment responses are driven by cases where the shock worker leaves the firm. Coworkers are 4.41 percentage points (4.83 percent) more likely to be employed in the shock firm after the health shock if the shock worker leaves the firm, while we find no statistically significant effects when the shock worker remains in the firm. For both groups, we find no statistically significant effects on wages and wage growth.[27]

Second, our main results are robust to weighting the regression in Equation (1) by the inverse of the size of the shock firm in the quarter of the shock. This ensures that each shock receives the same weight.[28] Table A.14 in the Appendix shows the results of the weighted regressions. We find employment effects of a similar size and level of statistical significance to our main estimates.

Finally, we find qualitatively similar results when we relax the employment restriction imposed in the construction of the main estimation sample (see Section 3.1). Shock workers and coworkers in our main estimation sample must be employed in the firm where the health shock occurs for at least four quarters prior to the health shock (including the quarter of the health shock). In Table A.15 in the Appendix, we relax this constraint by reducing the employment requirement to two quarters (i.e., the quarter before and the quarter of the health shock) and find positive point estimates for the number of employment days and the probability of being employed in the shock firm, although

[27] The coefficients for the health outcomes are reported in Table A.13 in the Appendix. However, none of the estimated coefficients are statistically significant.

[28] As larger firms contribute more observations to our estimation sample, shocks to larger firms are implicitly given more weight than shocks to smaller firms when estimating an unweighted version of Equation (1)

the latter is somewhat smaller in size than the main estimate. In addition, coworkers in the treatment group spend fewer days in retirement and are more likely to have a mammography as a result of the health shock.

# 5 Conclusion

In this study, we provide new evidence on the spillover effects of health shocks outside family networks by extending the setting to coworkers and firms. First, we show that these health shocks are severe for those who suffer them, leading to substantial increases in healthcare expenditures, absenteeism, and mortality, as well as to significant and persistent reductions in labor supply. Combining a matching approach to construct a control group and a difference-in-difference framework, we then analyze how a worker's severe health shock affects the labor market outcomes and health behavior of their older coworkers. We find a significant impact on both dimensions. Coworkers are about 2.3 percentage points more likely to be employed in the shock firm and significantly delay retirement. Although we find no evidence for changes in daily earnings or wage profiles, firms compensate their loyal employees with special severance payments when they leave the firm. Treatment heterogeneity analysis shows that the effects on employment and retirement are larger when the health shock affects a high-skilled worker and when the shock worker leaves the firm after the health shock. In terms of health behavior, we find that female coworkers in the treatment are about 9.73 percent more likely to have a mammography after a cancer-related health shock. However, we find no statistically significant effects on participation in general health check-ups and PSA tests, or on coworker absenteeism.

Our results are consistent with the hypothesis that older coworkers step in to compensate for the (partial) loss of labor supply to the firm associated with the severe health shock. For example, we find larger employment effects when the shock worker is highly skilled. As these workers are more difficult to replace and require experienced substitutes, firms tend to rely more on incumbent workers rather than hiring external workers. As a monetary compensation, incumbent workers receive higher severance payments when they leave the firm. The findings that older coworkers expand their labor supply and adjust their preventive health behavior only marginally in the short run after experiencing a health shock in the firm speak against the alternative hypothesis that the health shocks affect coworkers through an increase in workplace stress, as this should induce coworkers to leave the shock firm.

There are important limitations to this study that somewhat limit the external validity of our findings. First, we only focus on small firms with less than 50 workers four quarters before the (placebo) health shock. These firms are often family-run and may come with better working conditions and closer relationships between coworkers. In contrast, health shocks in larger firms may elicit different reactions from coworkers, with potentially ambiguous results, as health shocks in larger firms may be less perceived or differently attributed to workplace stress. Second, we only consider deteriorations in workers' physical health, and not their mental health, as health shocks. Therefore, our findings cannot necessarily be generalized to all types of health shocks. As mental health is becoming increasingly important, especially in the workplace, more evidence is needed to provide a more complete picture of the spillover effects of health shocks in the workplace.

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# 6 Figures (to be placed in the article)

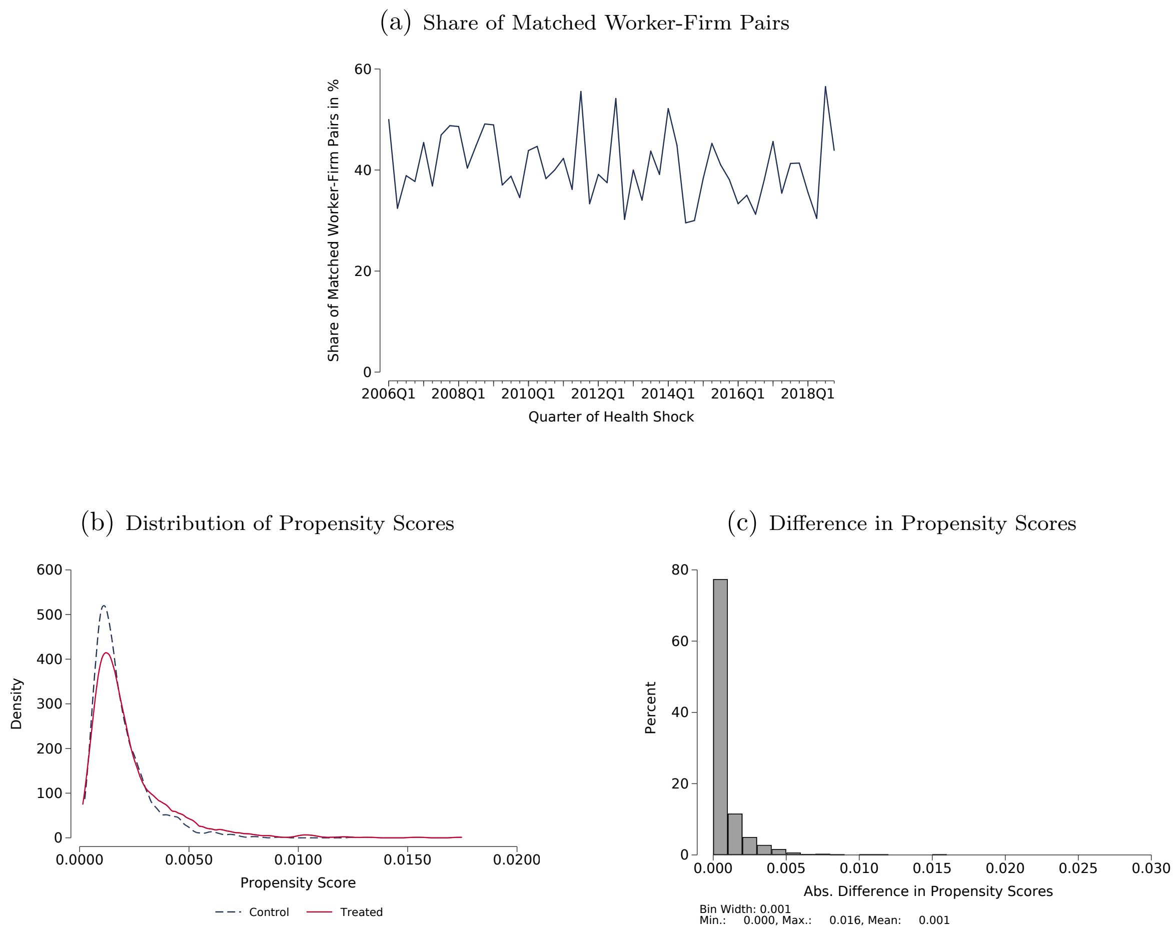



**Figure 1** Matching Diagnostics

*Note* — This figure shows several diagnostics for the propensity score matching described in Section 3.1. Panel (a) shows the share of matched worker-firm pairs over time. Panel (b) shows the distribution of propensity scores separately for treated shock workers (red solid line) and the matched control shock workers (blue dashed line). Panel (c) shows the distribution of the difference in propensity scores between shock workers in the treatment and matched control group.

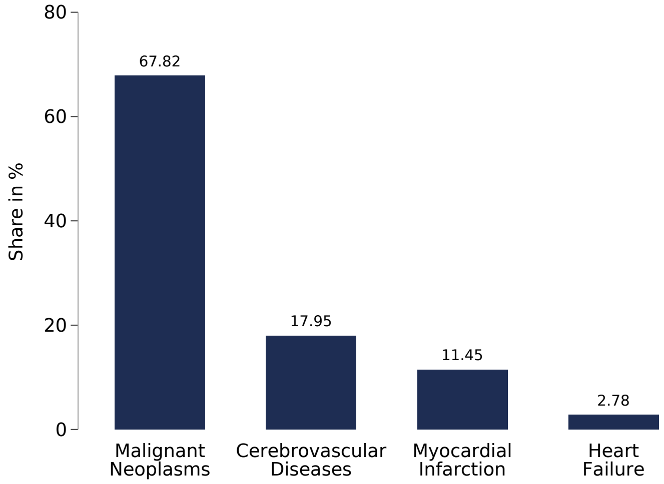
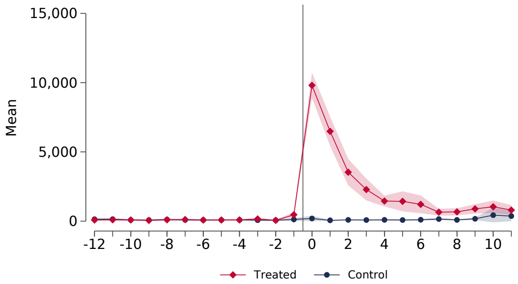


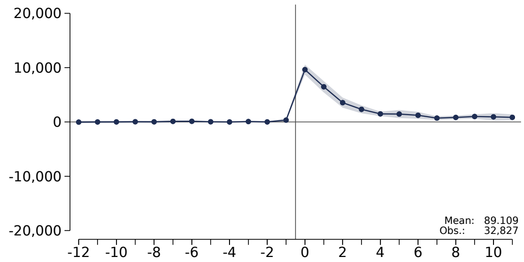

**Figure 2** Distribution of Health Shock Types

*Note* — The figure shows the distribution of the different types of severe health shocks in the main estimation sample. The health shocks are identified based on the admission diagnoses recorded in the hospital data. The first bar shows the share of health shocks associated with malignant neoplasms (ICD-10 C). The remaining bars show the share of different cardiovascular diseases, including cerebrovascular disease (ICD-10 I60 to I66), myocardial infarction (ICD-10 I21 and I22), and heart failure (ICD-10 I50).

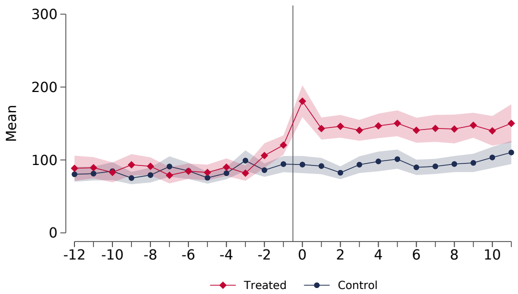
(a) Inpatient Expenditures



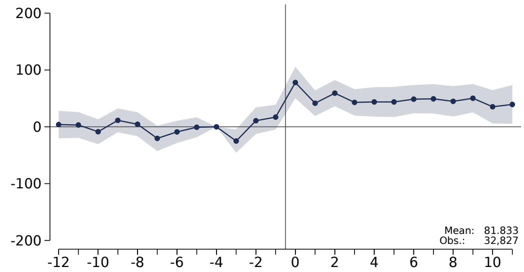

(b) Inpatient Expenditures



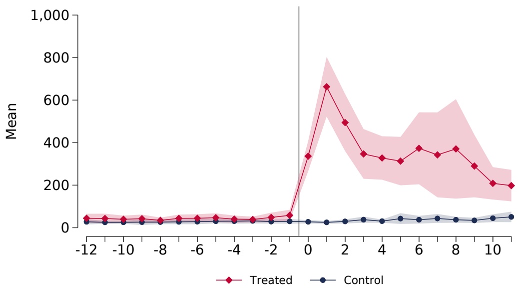

(c) Outpatient Expenditures




(d) Outpatient Expenditures




(e) Medication Expenditures




(f) Medication Expenditures

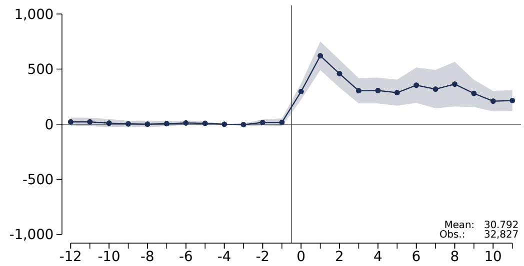
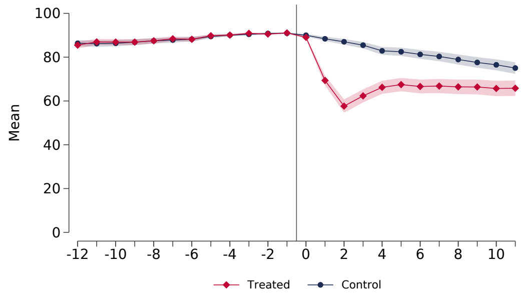


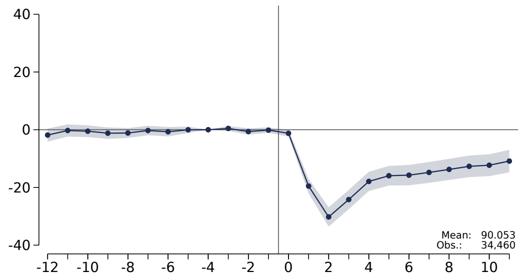

**Figure 3** Shock Workers – Healthcare Expenditures

*Note* — The first column of this figure shows sample averages with 95 percent confidence intervals for different types of shock workers' healthcare expenditures. The averages are shown separately for the treatment (red diamonds) and the control (blue dots) group. The second column shows the estimated coefficients with 95 percent confidence intervals for the effect of a severe health shock on different types of shock workers' healthcare expenditures. The estimates are based on Equation (2). Panels (a) and (b) consider the amount of inpatient expenditures in a given quarter. Panels (c) and (d) use outpatient expenditures as an outcome. Medication expenditures are considered in Panels (e) and (f). The estimates can be interpreted as the change in expenditures in a given quarter in Euros. Estimates are shown for 12 quarters before and 12 quarters (including the quarter of the health shock) after the health shock. Zero indicates the quarter of the health shock. The relative period -4 has been chosen as a reference period. In Panels (b), (d), and (f), the number of observations and the mean of the outcome variable for control shock workers in the reference period are reported in the bottom right corner. Standard errors are clustered at the shock firm level.

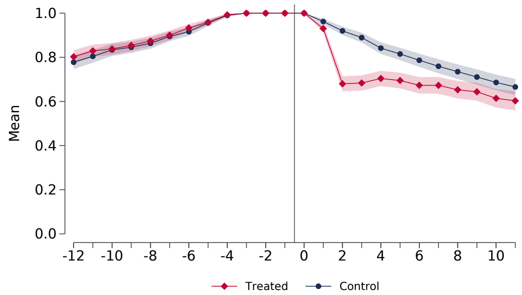
(a) Employment Days



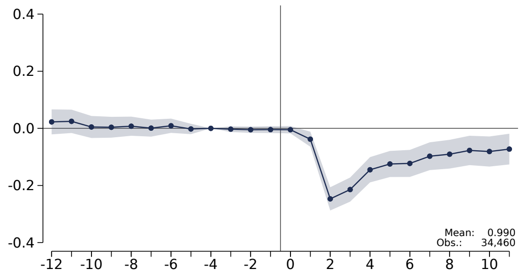

(b) Employment Days



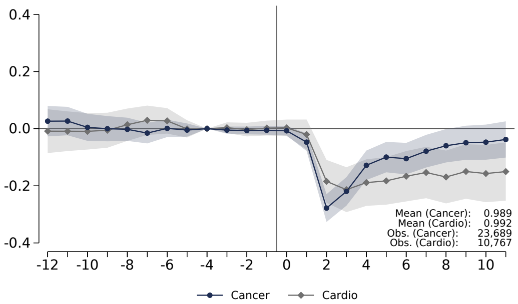

(c) **P**(Employed at Shock Firm)




(d) **P**(Employed at Shock Firm)




(e) **P**(Employed at Shock Firm) By Shock Type




(f) **P**(Employed at Shock Firm) By Age of Shock Worker

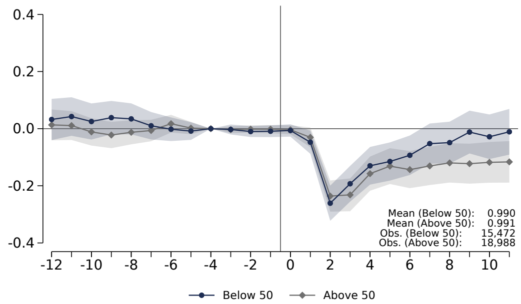



**Figure 4** Shock Workers – Employment

*Note* — Panels (a) and (c) show sample averages with 95 percent confidence intervals for different employment outcomes of shock workers separately for the treatment (red diamonds) and the control (blue dots) group. The other panels show the estimated coefficients with 95 percent confidence intervals for the effect of a severe health shock. The estimates are based on Equation (2). Panels (a) and (b) consider the number of employment days in a given quarter. In Panels (c), (d), (e), and (f) we define a binary indicator that equals one if the shock workers is employed in the shock firm in a given quarter and zero otherwise. Panel (d) shows the coefficients for all shock workers in the estimation sample. In Panel (e) we estimate the regression separately for health shocks due to cancer (blue dots) and cardiovascular disease (gray diamonds). Panel (f) shows separate estimates for shock workers below (blue dots) and above (gray diamonds) the age of 50 at the time of the health shock. Estimates are shown for 12 quarters before and 12 quarters (including the quarter of the health shock) after the health shock. Zero indicates the quarter of the health shock. The relative period -4 has been chosen as a reference period. In Panels (b), (d), (e), and (f), the number of observations and the mean of the outcome variable for control shock workers in the reference period are reported in the bottom right corner. Standard errors are clustered at the shock firm level.

# 7 Tables (to be placed in the article)

**Table 1** Comparing Treatment and Control Coworkers

| | Ø Full | Ø Treatment | Ø Control | Diff. | Stand. Diff. in % | N |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| **Shock Firm Characteristics** | | | | | | |
| Firm Size | 16.721 | 16.886 | 16.548 | 0.337 | 3.11 | 49,600 |
| % Blue Collar | 38.761 | 38.232 | 39.318 | −1.086 | −3.39 | 49,600 |
| % Women | 50.276 | 48.759 | 51.869 | −3.110 | −9.08 | 49,600 |
| Avgerage Age | 43.133 | 43.262 | 42.996 | 0.266 | 5.06 | 49,600 |
| % Non-Austrian Citizens | 7.513 | 7.110 | 7.936 | −0.826 | −6.52 | 49,600 |
| Average Daily Wage | 77.156 | 77.715 | 76.569 | 1.147 | 3.72 | 49,600 |
| **Coworker Characteristics** | | | | | | |
| Treated | 0.512 | 1.000 | 0.000 | $1.000^{***}$ | 0.00 | 49,600 |
| *Demographic Characteristics* | | | | | | |
| Female | 0.516 | 0.504 | 0.528 | −0.024 | −4.77 | 49,600 |
| Age in Years | 52.992 | 52.943 | 53.045 | −0.102 | −2.85 | 49,600 |
| Age at Shock in Years | 54.572 | 54.523 | 54.624 | −0.101 | −2.91 | 49,600 |
| Non-Austrian Citizen | 0.063 | 0.061 | 0.065 | −0.004 | −1.71 | 49,600 |
| *Labor Market Outcomes* | | | | | | |
| Employment Days | 88.736 | 88.765 | 88.705 | 0.060 | 0.44 | 49,600 |
| Employed at Shock Firm | 0.921 | 0.920 | 0.922 | −0.002 | −0.83 | 49,600 |
| Blue Collar | 0.433 | 0.424 | 0.443 | −0.019 | −3.82 | 48,807 |
| Daily Wage | 72.088 | 72.405 | 71.754 | 0.651 | 1.82 | 48,173 |
| Tenure in Quarters | 36.205 | 36.120 | 36.294 | −0.173 | −0.53 | 48,807 |
| *Health Outcomes* | | | | | | |
| Sick Leave Days (All Causes) | 2.357 | 2.277 | 2.442 | −0.164 | −2.00 | 48,950 |
| Easy-to-Fake Sick Leave Days | 0.114 | 0.109 | 0.118 | −0.009 | −0.62 | 48,934 |
| Inpatient Exp. | 95.907 | 93.676 | 98.255 | −4.579 | −0.59 | 48,321 |
| Outpatient Exp. | 94.230 | 92.933 | 95.596 | −2.663 | −1.57 | 48,321 |
| Medication Exp. | 30.099 | 25.547 | 34.893 | $-9.346^{***}$ | −6.94 | 48,321 |
| Exp. Preventative Health Check-Up | 2.910 | 2.858 | 2.965 | −0.107 | −0.73 | 48,935 |
| Exp. PSA Screening | 0.486 | 0.476 | 0.498 | −0.022 | −1.10 | 23,561 |
| Exp. Mammography | 4.079 | 4.021 | 4.136 | −0.116 | −0.72 | 25,381 |
| Dermatology Exp. | 1.668 | 1.585 | 1.756 | −0.171 | −1.57 | 48,321 |
| Radiology Exp. | 5.183 | 5.025 | 5.350 | −0.326 | −1.61 | 48,321 |

*Note* — The table reports descriptive statistics for shock firm characteristics as well as the demographic, labor market, and health characteristics of the coworkers included in our main estimation sample. The definition of the main estimation sample and the construction of the treatment and control group are described in Section 3.1. The sample period comprises the 12 quarters preceding the (placebo) health shock. Note that health characteristics can only be observed for coworkers insured in Upper Austria. Prostate-specific antigen (PSA) tests are analyzed only for men, while mammographies are analyzed only for women. Column (1) reports the mean for the full sample. The means for treated and control coworkers are reported in Columns (2) and (3). Column (4) reports the difference in means between the treatment and control group. Column (5) reports the standardized difference in means. The standardized difference is defined as the difference in means between the treated and control group for a given variable $(\mu_{treated} - \mu_{control})$ divided by the average standard deviation $\left(\sqrt{0.5 \cdot (\sigma^2_{treated} + \sigma^2_{control})}\right)$ and multiplied by 100. The number of observations is given in Column (6). Standard errors are clustered at the shock firm level. $^{*}$ $p < 0.1$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.001$.

**Table 2** Average Effects of Health Shocks on Coworkers' Labor Market Outcomes

| | Employment | | Retirement | Wages | |
|---|---|---|---|---|---|
| | Days | **P**(Employed at Shock Firm) | Days | Log Daily Wage | Wage Growth |
| | (1) | (2) | (3) | (4) | (5) |
| Health Shock × Post | 1.0676* | 0.0232* | -1.0506** | 0.0022 | -0.0002 |
| | (0.6159) | (0.0139) | (0.4612) | (0.0059) | (0.0015) |
| Individual FE | ✓ | ✓ | ✓ | ✓ | ✓ |
| Year × Birth Year FE | ✓ | ✓ | ✓ | ✓ | ✓ |
| N | 93,058 | 93,058 | 93,058 | 84,129 | 79,482 |
| # Clusters | 1,605 | 1,605 | 1,605 | 1,604 | 1,604 |
| p-Value of Pre-Coefficients | 0.218 | 0.541 | 0.246 | 0.687 | 0.579 |
| Outcome Mean | 88.705 | 0.922 | 7.829 | 4.130 | 0.006 |
| Effect in % of Mean | 1.204 | 2.520 | -13.419 | 0.053 | -3.746 |

*Note* — The table reports the estimated coefficients for the effect of a health shock on coworkers' labor market outcomes. The results are based on Equation (1). Column (1) shows the results for the number of employment days in a given quarter. Column (2) uses a binary indicator that equals one if a coworker is employed at the firm where the (placebo) health shock occurred and zero otherwise. Column (3) reports the results for the quarterly number of retirement days. Column (4) uses the log daily wage as an outcome. Column (5) considers wage growth, which is defined as the difference in log daily wages between two consecutive quarters, as an outcome. The sample period comprises the 12 quarters preceding and the 12 quarters (including the quarter of the health shock) following the (placebo) health shock. The number of observations, the number of clusters, the p-value of an F-test for the joint significance of all pre-treatment coefficients of Equation (2), the mean of the outcome variable for the control group, and the coefficient in percent of the outcome mean are reported at the bottom of the table. For all outcomes except retirement, the outcome mean is based on observations from the pre-treatment period. The outcome mean for retirement is based on the post-treatment period. Standard errors are clustered at the shock firm level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.001$.

**Table 3** Average Effects of Health Shocks on Coworkers' Health Outcomes

| | Sick Leave Days | | $\mathbf{P}$(Expenditures > 0) | | |
|---|---|---|---|---|---|
| | All Causes | Easy-to-Fake Diagnoses | General Health Check-Up | PSA Screening | Mammography |
| | (1) | (2) | (3) | (4) | (5) |
| Health Shock × Post | -0.1002 | 0.0043 | 0.0009 | 0.0044 | 0.0064* |
| | (0.1912) | (0.0283) | (0.0022) | (0.0044) | (0.0034) |
| Individual FE | ✓ | ✓ | ✓ | ✓ | ✓ |
| Year × Birth Year FE | ✓ | ✓ | ✓ | ✓ | ✓ |
| N | 91,862 | 91,830 | 91,829 | 43,788 | 48,050 |
| # Clusters | 1,605 | 1,605 | 1,605 | 958 | 1,128 |
| p-Value of Pre-Coefficients | 0.009 | 0.442 | 0.293 | 0.061 | 0.645 |
| Outcome Mean | 2.442 | 0.118 | 0.040 | 0.060 | 0.065 |
| Effect in % of Mean | -4.103 | 3.593 | 2.186 | 7.300 | 9.726 |

*Note* — The table reports the estimated coefficients for the effect of a health shock on coworkers' health outcomes. The results are based on Equation (1). Column (1) shows the results for the number of sick leave days in a given quarter. Column (2) considers only sick leaves associated with easy-to-fake diagnoses, including the common cold (ICD-10 J04 and J06), lower back pain (ICD-10 M54.5), and headache (ICD-10 R51). In Column (3), we define a binary indicator that equals one if the outpatient expenditures for the general health screening program in a given quarter are larger than zero and zero otherwise. Columns (4) and (5) use analogous binary indicators for expenditures related to PSA tests and mammography. These outcomes are analyzed only for men and women, respectively. The sample period comprises the 12 quarters preceding and the 12 quarters (including the quarter of the health shock) following the (placebo) health shock. The number of observations, the number of clusters, the p-value of an F-test for the joint significance of all pre-treatment coefficients of Equation (2), the mean of the outcome variable for the control group in the pre-treatment period, and the coefficient in percent of the outcome mean are reported at the bottom of the table. Standard errors are clustered at the shock firm level. $^{*}$ $p < 0.1$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.001$.

**Table 4** Average Effects of Health Shocks on Coworkers' Outcomes by Wage Percentile of Shock Worker

| | Employment | | | | Retirement | | **P**(Expenditures > 0) | |
|---|---|---|---|---|---|---|---|---|
| | Days | | **P**(Employed at Shock Firm) | | Days | | Mammography | |
| Wage Percentile of Shock Worker | $\leq 75^{\text{th}}$ | $> 75^{\text{th}}$ | $\leq 75^{\text{th}}$ | $> 75^{\text{th}}$ | $\leq 75^{\text{th}}$ | $> 75^{\text{th}}$ | $\leq 75^{\text{th}}$ | $> 75^{\text{th}}$ |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Health Shock × Post | 0.4024 | 2.5464** | 0.0080 | 0.0586** | -0.4244 | -2.2705*** | 0.0076* | 0.0048 |
| | (0.7581) | (1.0516) | (0.0161) | (0.0267) | (0.5487) | (0.7921) | (0.0042) | (0.0059) |
| Individual FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Year × Birth Year FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| N | 61,342 | 31,715 | 61,342 | 31,715 | 61,342 | 31,715 | 30,729 | 17,321 |
| # Clusters | 1,039 | 570 | 1,039 | 570 | 1,039 | 570 | 734 | 397 |
| p-Value of Pre-Coefficients | 0.232 | 0.471 | 0.203 | 0.498 | 0.102 | 0.488 | 0.181 | 0.945 |
| Outcome Mean | 88.419 | 89.179 | 0.921 | 0.925 | 7.310 | 8.590 | 0.066 | 0.064 |
| Effect in % of Mean | 0.455 | 2.855 | 0.869 | 6.337 | -5.805 | -26.432 | 11.515 | 7.526 |

*Note* — The table reports the estimated coefficients for the effect of a health shock on coworkers' outcomes by the within-firm wage percentile of the shock worker at the time of the health shock. We estimate Equation (1) separately for coworkers of shock workers with daily earnings below the $75^{\text{th}}$ percentile of the wage distribution of the shock firm and for coworkers of shock workers with daily earnings above the $75^{\text{th}}$ percentile. Columns (1), (3), (5), and (7) report the results for the first group, Columns (2), (4), (6), and (8) those for the second group. Columns (1) and (2) show the results for the number of employment days in a given quarter. Columns (3) and (4) use a binary indicator that equals one if a coworker is employed at the firm where the (placebo) health shock occurred and zero otherwise. Columns (5) and (6) report the results for the quarterly number of retirement days. In Columns (7) and (8), we define a binary indicator that equals one if the outpatient expenditures for mammography in a given quarter are larger than zero and zero otherwise. This outcome is only analyzed for women. The sample period comprises the 12 quarters preceding and the 12 quarters (including the quarter of the health shock) following the (placebo) health shock. The number of observations, the number of clusters, the p-value of an F-test for the joint significance of all pre-treatment coefficients of Equation (2), the mean of the outcome variable for the control group, and the coefficient in percent of the outcome mean are reported at the bottom of the table. For all outcomes except retirement, the outcome mean is based on observations from the pre-treatment period. The outcome mean for retirement is based on the post-treatment period. Standard errors are clustered at the shock firm level. $^{*}$ $p < 0.1$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.001$.

**Table 5** Average Effects of Health Shocks on Coworkers' Outcomes by Productivity of Shock Worker

| | Employment | | | | Retirement | | **P**(Expenditures > 0) | |
|---|---|---|---|---|---|---|---|---|
| | Days | | **P**(Employed at Shock Firm) | | Days | | Mammography | |
| Productivity of Shock Worker | Low | High | Low | High | Low | High | Low | High |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Health Shock × Post | 0.2493 | 2.4080*** | 0.0132 | 0.0371* | -0.3556 | -2.1753*** | 0.0062 | 0.0058 |
| | (0.8641) | (0.8401) | (0.0178) | (0.0213) | (0.6328) | (0.6143) | (0.0044) | (0.0054) |
| Individual FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Year × Birth Year FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| N | 49,322 | 43,472 | 49,322 | 43,472 | 49,322 | 43,472 | 29,195 | 18,714 |
| # Clusters | 865 | 736 | 865 | 736 | 865 | 736 | 650 | 474 |
| p-Value of Pre-Coefficients | 0.033 | 0.403 | 0.564 | 0.843 | 0.213 | 0.949 | 0.646 | 0.837 |
| Outcome Mean | 88.136 | 89.292 | 0.921 | 0.923 | 7.891 | 7.726 | 0.062 | 0.069 |
| Effect in % of Mean | 0.283 | 2.697 | 1.430 | 4.022 | -4.506 | -28.155 | 9.955 | 8.340 |

*Note* — The table reports the estimated coefficients for the effect of a health shock on coworkers' outcomes by the productivity of the shock worker. We estimate Equation (1) separately for coworkers of shock workers with a below median AKM worker fixed effect and for coworkers of shock workers with an above median AKM worker fixed effect. Columns (1), (3), (5), and (7) report the results for the first group, Columns (2), (4), (6), and (8) those for the second group. Columns (1) and (2) show the results for the number of employment days in a given quarter. Columns (3) and (4) use a binary indicator that equals one if a coworker is employed at the firm where the (placebo) health shock occurred and zero otherwise. Columns (5) and (6) report the results for the quarterly number of retirement days. In Columns (7) and (8), we define a binary indicator that equals one if the outpatient expenditures for mammography in a given quarter are larger than zero and zero otherwise. This outcome is analyzed only for women. The sample period comprises the 12 quarters preceding and the 12 quarters (including the quarter of the health shock) following the (placebo) health shock. The number of observations, the number of clusters, the p-value of an F-test for the joint significance of all pre-treatment coefficients of Equation (2), the mean of the outcome variable for the control group, and the coefficient in percent of the outcome mean are reported at the bottom of the table. For all outcomes except retirement, the outcome mean is based on observations from the pre-treatment period. The outcome mean for retirement is based on the post-treatment period. Standard errors are clustered at the shock firm level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.001$.

# Web Appendix

This Web Appendix provides additional material discussed in the unpublished manuscript “Sick Happens: The Effect of Worker Health Shocks on Coworkers’ Employment and Health Behavior” by Wolfgang Frimmel and Rene Wiesinger.

# A Additional Figures and Tables

## A.1 Additional Figures

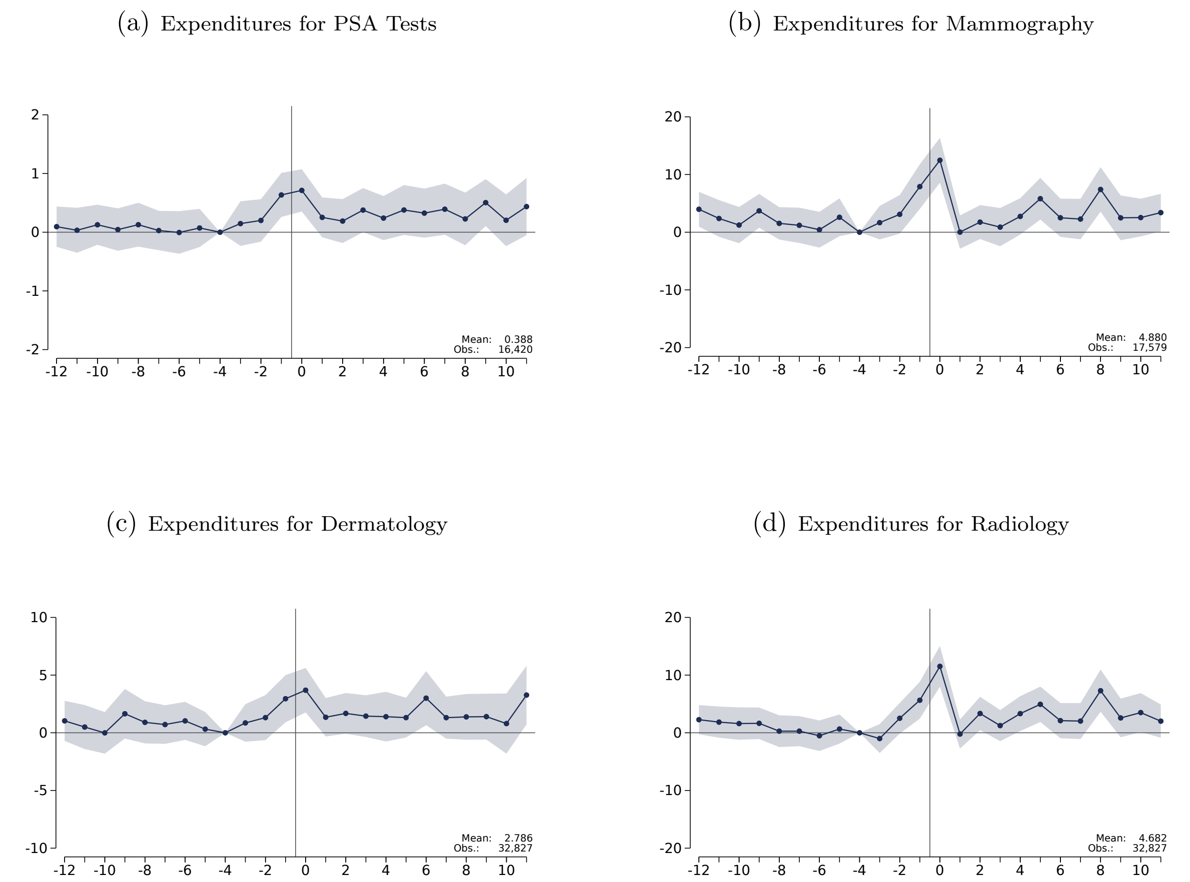
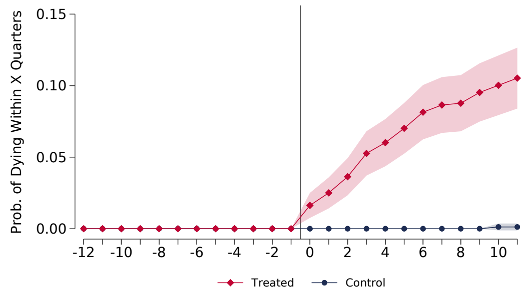


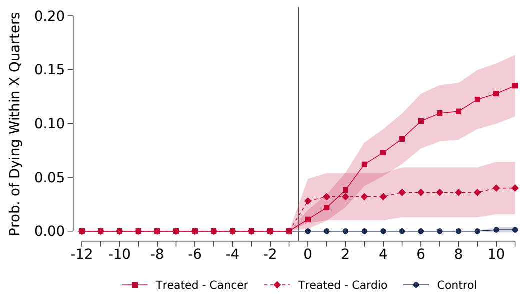

**Figure A.1** Shock Workers – Preventive Healthcare

*Note* — The figure shows the estimated coefficients with 95 percent confidence intervals for different types of shock workers’ preventive healthcare expenditures. The estimates are based on Equation (2). Panel (a) considers the amount of outpatient expenditures for prostate-specific antigen (PSA) tests in a given quarter. Panel (b) uses expenditures for mammography as an outcome. The outcomes in Panels (a) and (b) are analyzed only for men and women, respectively. Expenditures for dermatologists are considered in Panel (c). Panel (d) shows the change in the amount of expenditures for radiologists. The estimates can be interpreted as the change in expenditures in a given quarter in Euros. Estimates are shown for 12 quarters before and 12 quarters (including the quarter of the health shock) after the health shock. Zero indicates the quarter of the health shock. The relative period -4 has been chosen as a reference period. The number of observations and the mean of the outcome variable for control shock workers in the reference period are reported in the bottom right corner. Standard errors are clustered at the shock firm level.

(a) Overall




(b) By Type of Health Shock




(c) By Age of Shock Worker

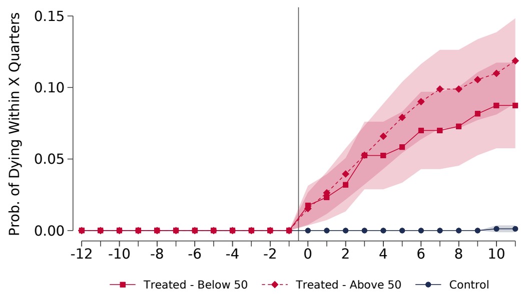



**Figure A.2** Shock Workers – Mortality

*Note* — The figure shows sample averages with 95 percent confidence intervals for shock workers' mortality. Mortality is defined as the probability of dying within a given number of quarters, i.e., the cumulative probability of dying before a given quarter (including the respective quarter). The averages are shown separately for the treatment (red diamonds) and the control (blue dots) group. Panel (a) shows the overall probabilities for the treatment and control group. In Panel (b), probabilities for the treatment group are shown separately for severe health shocks due to cancer (red squares) and health shocks due to acute cardiovascular events (red diamonds). Panel (c) differentiates between treated shock workers below the age of 50 at the time of the health shock (red squares) and those above the age of 50 (red diamonds). Averages are shown for 12 quarters before and 12 quarters (including the quarter of the health shock) after the health shock. Zero indicates the quarter of the health shock.

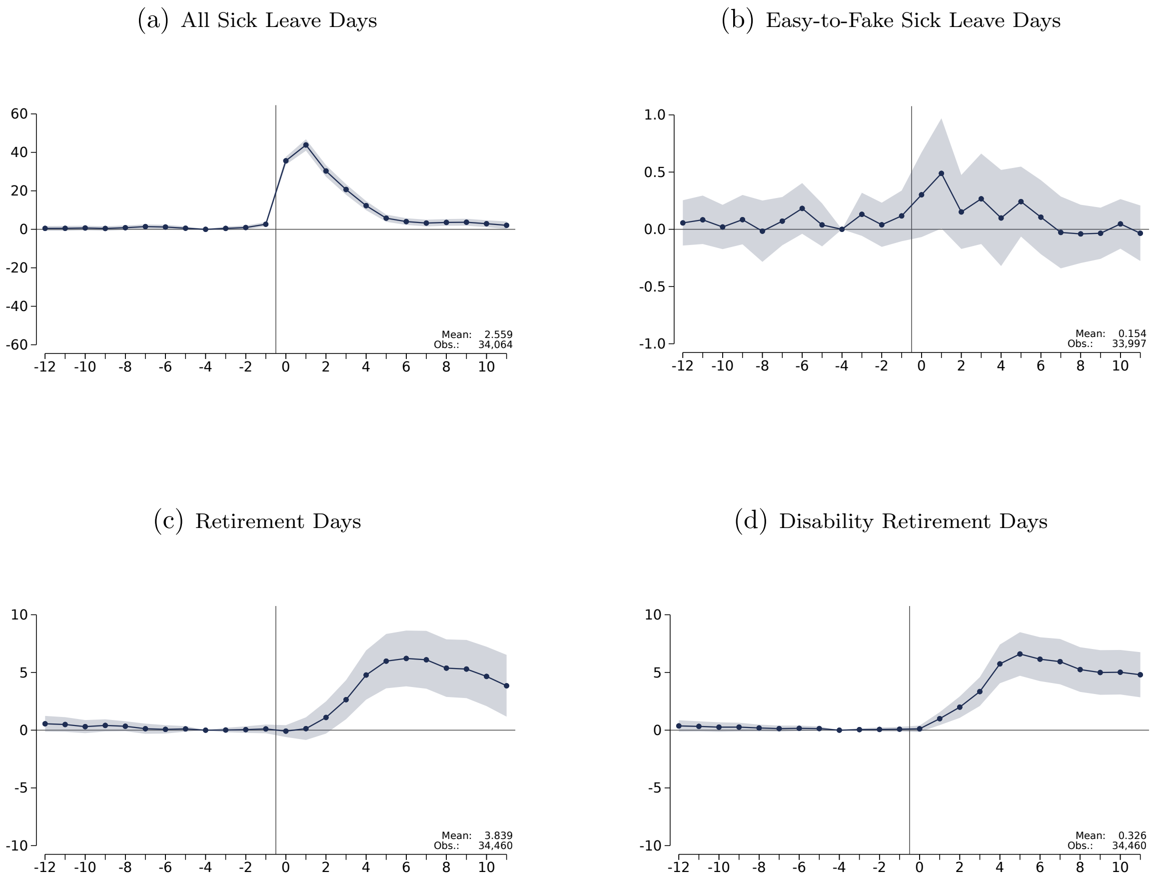



**Figure A.3** Shock Workers – Sick Leave Days and Retirement

*Note* — The figure shows the estimated coefficients with 95 percent confidence intervals for shock workers' sick leave days and retirement days. The estimates are based on Equation (2). Panel (a) shows the results for the number of sick leave days in a given quarter. Panel (b) includes only sick leaves associated with easy-to-fake diagnoses, including the common cold (ICD-10 J04 and J06), lower back pain (ICD-10 M54.5), and headache (ICD-10 R51). Panel (c) shows the change in the quarterly number of retirement days. Panel (d) considers only days associated with disability retirement. The estimates can be interpreted as the change in the number of days in a given quarter. Estimates are shown for 12 quarters before and 12 quarters (including the quarter of the health shock) after the health shock. Zero indicates the quarter of the health shock. The relative period -4 has been chosen as a reference period. The number of observations and the mean of the outcome variable for control shock workers in the reference period are reported in the bottom right corner. Standard errors are clustered at the shock firm level.

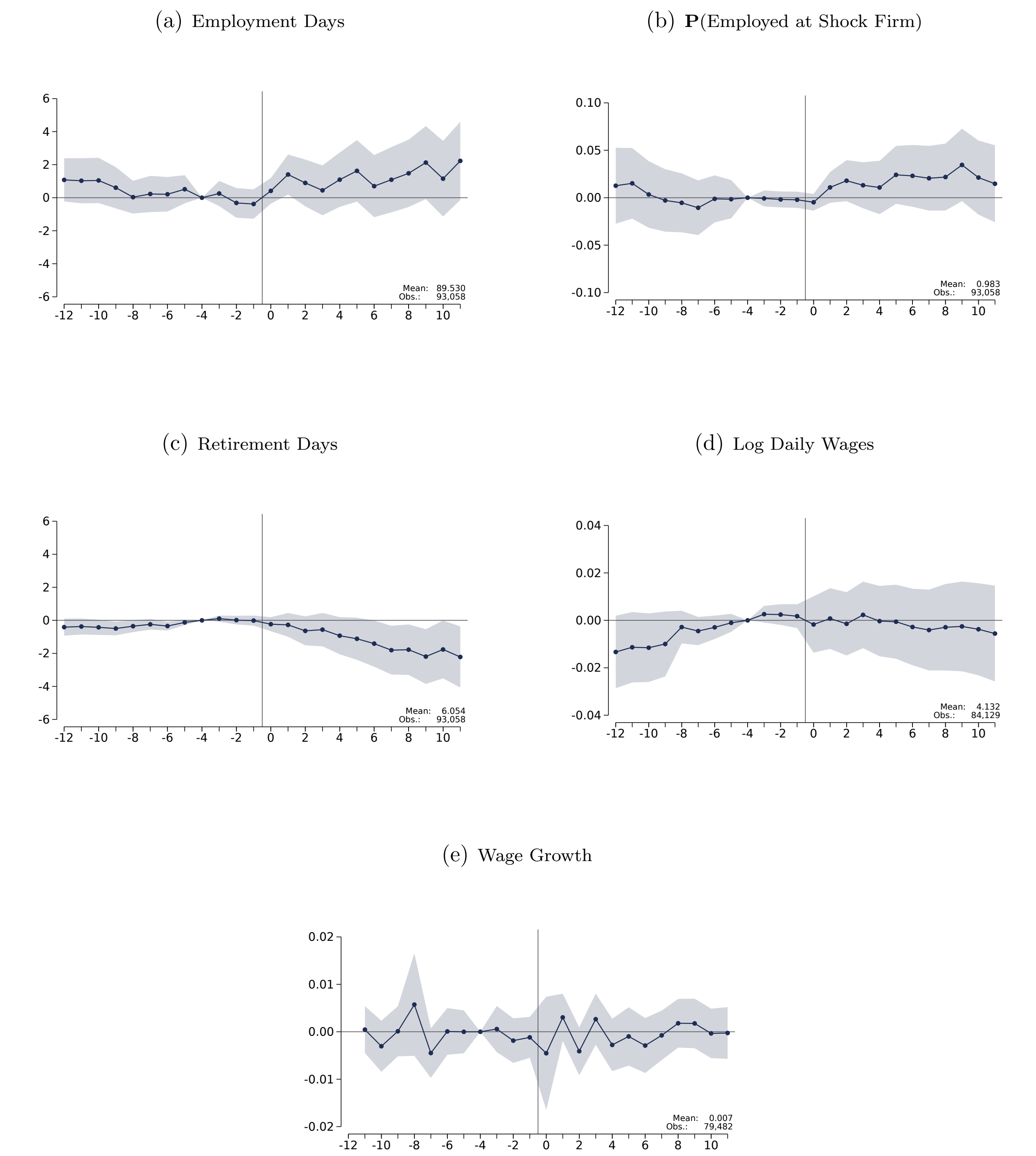
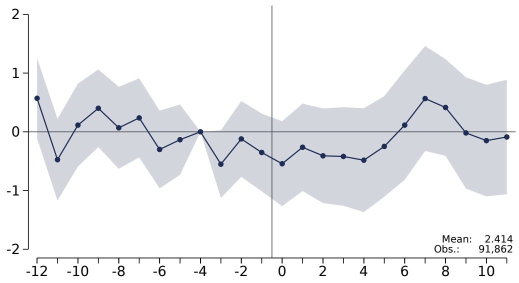


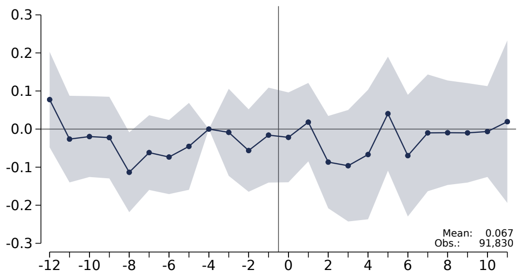

**Figure A.4** Dynamic Effects of Health Shocks on Coworkers' Labor Market Outcomes

*Note* — The figure shows the estimated coefficients with 95 percent confidence intervals for coworkers' labor market outcomes. The estimates are based on Equation (2). Panel (a) shows the results for the number of employment days in a given quarter. Panel (b) uses a binary indicator that equals one if a coworker is employed at the firm where the (placebo) health shock occurred and zero otherwise. Panel (c) shows the change in the quarterly number of retirement days. Panel (d) uses the log daily wage as an outcome. Panel (e) considers wage growth, which is defined ans the difference in log daily wages between two consecutive quarters, as an outcome. Estimates are shown for 12 quarters before and 12 quarters (including the quarter of the health shock) after the health shock. Zero indicates the quarter of the (placebo) health shock. The relative period -4 has been chosen as a reference period. The number of observations and the mean of the outcome variable for the control group are reported in the bottom right corner. For all outcomes except retirement, the outcome mean is based on observations from the relative period -4. The outcome mean for retirement is based on the relative period 4. Standard errors are clustered at the shock firm level.

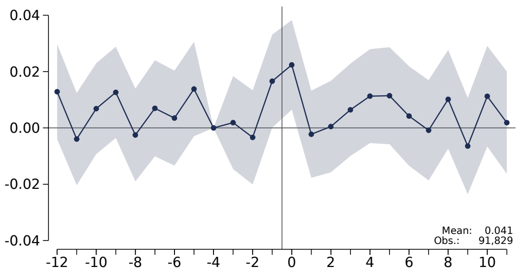
(a) All Sick Leave Days



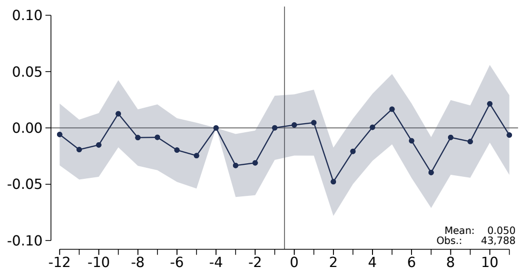

(b) Easy-to-Fake Sick Leave Days




(c) **P**(Exp. General Health Check-Up > 0)




(d) **P**(Exp. PSA Screening > 0)




(e) **P**(Exp. Mammography > 0)

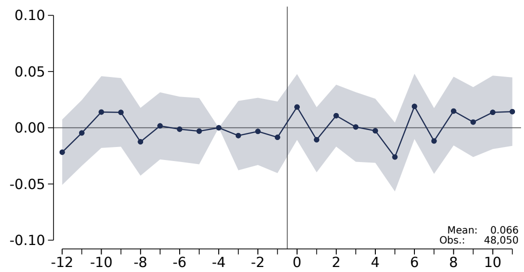



**Figure A.5** Dynamic Effects of Health Shocks on Coworkers' Health Outcomes

*Note* — The figure shows the estimated coefficients with 95 percent confidence intervals for coworkers' health outcomes. The estimates are based on Equation (2). Panel (a) shows the results for the number of sick leave days in a given quarter. Panel (b) considers only sick leaves associated with easy-to-fake diagnoses, including the common cold (ICD-10 J04 and J06), lower back pain (ICD-10 M54.5), and headache (ICD-10 R51). In Panel (c), we define a binary indicator that equals one if the outpatient expenditures for the general health screening program in a given quarter are larger than zero and zero otherwise. Panels (d) and (e) use analogous binary indicators for expenditures related to PSA tests and mammography. These outcomes are analyzed only for men and women, respectively. Estimates are shown for 12 quarters before and 12 quarters (including the quarter of the health shock) after the health shock. Zero indicates the quarter of the (placebo) health shock. The relative period -4 has been chosen as a reference period. The number of observations and the mean of the outcome variable for the control group in the reference period are reported in the bottom right corner. Standard errors are clustered at the shock firm level.

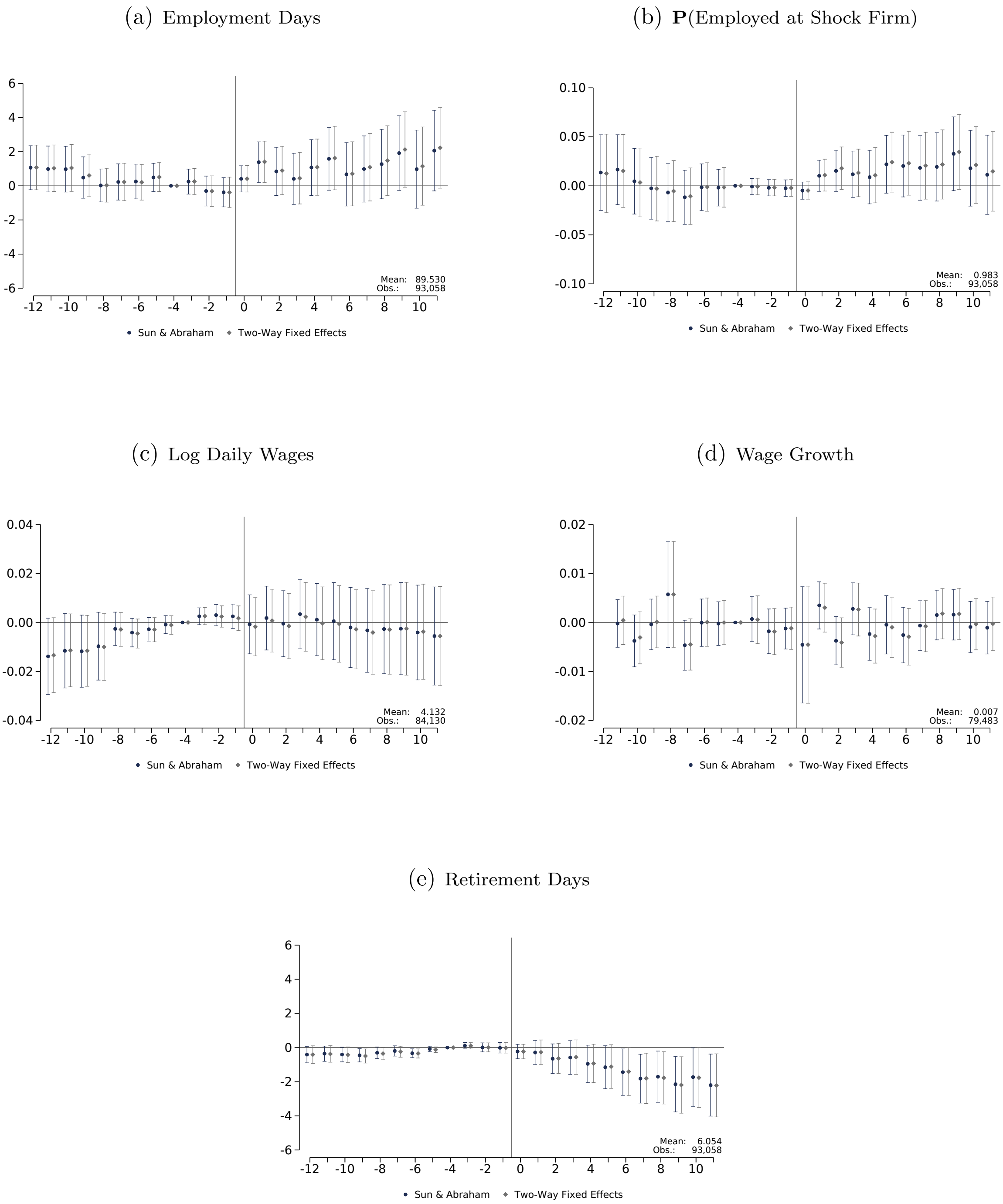
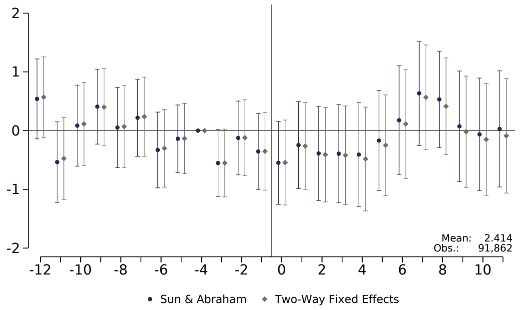


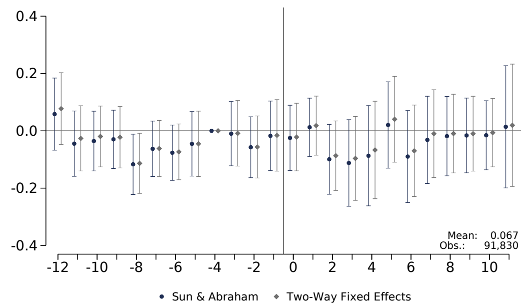

**Figure A.6** Dynamic Effects of Health Shocks on Coworkers' Labor Market Outcomes – Alternative Estimation Method

*Note* — The figure shows the estimated coefficients with 95 percent confidence intervals for coworkers' labor market outcomes for different estimation methods. The estimates are based on Equation (2). Estimates represented by blue dots are based on the estimation method by Sun and Abraham (2021). Gray diamonds represent the baseline two-way fixed effects estimates. Panel (a) shows the results for the number of employment days in a given quarter. Panel (b) uses a binary indicator that equals one if a coworker is employed at the firm where the (placebo) health shock occurred and zero otherwise. Panel (c) shows the change in the quarterly number of retirement days. Panel (d) uses the log daily wage as an outcome. Panel (e) considers wage growth, which is defined ans the difference in log daily wages between two consecutive quarters, as an outcome. Estimates are shown for 12 quarters before and 12 quarters (including the quarter of the health shock) after the health shock. Zero indicates the quarter of the (placebo) health shock. The relative period -4 has been chosen as a reference period. The number of observations and the mean of the outcome variable for the control group are reported in the bottom right corner. For all outcomes except retirement, the outcome mean is based on observations from the relative period -4. The outcome mean for retirement is based on the relative period 4. Standard errors are clustered at the shock firm level.

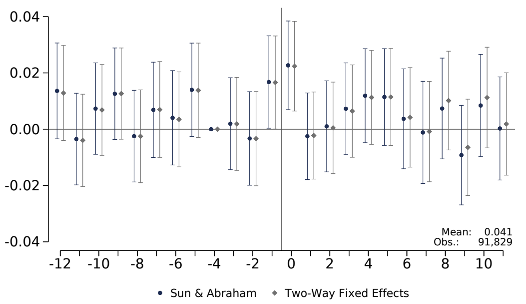
(a) All Sick Leave Days



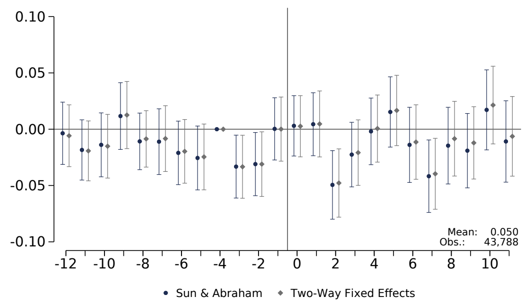

(b) Easy-to-Fake Sick Leave Days




(c) $\mathbf{P}$(Exp. General Health Check-Up > 0)




(d) $\mathbf{P}$(Exp. PSA Screening > 0)




(e) $\mathbf{P}$(Exp. Mammography > 0)

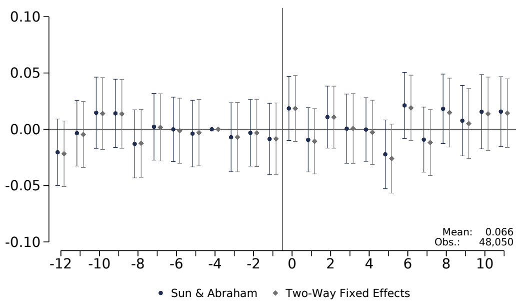



**Figure A.7** Dynamic Effects of Health Shocks on Coworkers' Health Outcomes – Alternative Estimation Method

*Note* — The figure shows the estimated coefficients with 95 percent confidence intervals for coworkers' health outcomes for different estimation methods. The estimates are based on Equation (2). Estimates represented by blue dots are based on the estimation method by Sun and Abraham (2021). Gray diamonds represent the baseline two-way fixed effects estimates. The estimates are based on Equation (2). Panel (a) shows the results for the number of sick leave days in a given quarter. Panel (b) considers only sick leaves associated with easy-to-fake diagnoses, including the common cold (ICD-10 J04 and J06), lower back pain (ICD-10 M54.5), and headache (ICD-10 R51). In Panel (c), we define a binary indicator that equals one if the outpatient expenditures for the general health screening program in a given quarter are larger than zero and zero otherwise. Panels (d) and (e) use analogous binary indicators for expenditures related to PSA tests and mammography. These outcomes are analyzed only for men and women, respectively. Estimates are shown for 12 quarters before and 12 quarters (including the quarter of the health shock) after the health shock. Zero indicates the quarter of the (placebo) health shock. The relative period -4 has been chosen as a reference period. The number of observations and the mean of the outcome variable for the control group in the reference period are reported in the bottom right corner. Standard errors are clustered at the shock firm level.

## A.2 Additional Tables

**Table A.1** Comparing Matched and Non-Matched Worker-Firm Pairs

| | Ø All Obs. | Ø Matched | Ø Non-Matched | Diff. | Stand. Diff. in % | N |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| **Firm Characteristics** | | | | | | |
| Firm Size | 16.996 | 11.897 | 21.153 | $-9.256^{***}$ | −92.45 | 2,302 |
| % Women | 46.470 | 52.265 | 41.744 | $10.521^{***}$ | 32.53 | 2,302 |
| % Blue Collar | 40.473 | 36.112 | 44.028 | $-7.916^{***}$ | −24.74 | 2,302 |
| % Non-Austrian Citizens | 8.460 | 6.243 | 10.269 | $-4.026^{***}$ | −30.39 | 2,302 |
| Average Worker Age | 40.681 | 40.793 | 40.590 | 0.203 | 3.61 | 2,302 |
| Avgerage Daily Wage | 79.277 | 75.148 | 82.645 | $-7.496^{***}$ | −28.21 | 2,302 |
| Firm Age | 17.534 | 18.343 | 16.874 | $1.470^{***}$ | 11.08 | 2,302 |
| **Worker Characteristics** | | | | | | |
| Matched | 0.449 | 1.000 | 0.000 | $1.000^{***}$ | 0.00 | 2,302 |
| *Demographic Characteristics* | | | | | | |
| Age | 48.017 | 47.648 | 48.317 | $-0.669^{*}$ | −7.36 | 2,302 |
| Female | 0.438 | 0.512 | 0.378 | $0.134^{***}$ | 27.17 | 2,302 |
| Non-Austrian Citizen | 0.079 | 0.025 | 0.124 | $-0.099^{***}$ | −38.25 | 2,302 |
| *Labor Market Outcomes* | | | | | | |
| Employed at Shock Firm | 0.878 | 0.952 | 0.818 | $0.134^{***}$ | 42.84 | 2,302 |
| Employment Days | 89.819 | 90.044 | 89.608 | 0.435 | 5.14 | 2,137 |
| Blue Collar | 0.453 | 0.416 | 0.488 | $-0.072^{***}$ | −14.48 | 2,137 |
| Daily Wage | 73.556 | 69.136 | 77.703 | $-8.566^{***}$ | −24.95 | 2,136 |
| Unemployment Days | 1.405 | 1.145 | 1.648 | −0.503 | −5.67 | 2,137 |
| Retirement Days | 0.556 | 0.355 | 0.745 | −0.390 | −5.52 | 2,137 |
| *Health Outcomes* | | | | | | |
| Sick Leave Days (All Causes) | 2.053 | 2.049 | 2.056 | −0.007 | −0.11 | 2,114 |
| Hospital Days | 0.156 | 0.173 | 0.141 | 0.032 | 3.13 | 1,977 |
| Outpatient Expenditures | 90.827 | 87.797 | 93.665 | −5.868 | −3.22 | 1,977 |
| Medication Expenditures | 44.490 | 38.514 | 50.086 | −11572 | −4.80 | 1,977 |
| GP Expenditures | 23.147 | 23.271 | 23.030 | 0.241 | 0.68 | 1,977 |

*Note* — The table reports descriptive statistics for firm characteristics as well as the demographic, labor market, and health characteristics separately for worker-firm pairs with a health shock that could and could not be matched with worker-firm pairs from the pool of worker-firm pairs without a health shock. The sample includes all worker-firms pairs for which a health shock was identified as described in Section 3.1. We include only firms that have fewer than 50 workers four quarters before the health shock. Note that health characteristics can only be observed for coworkers insured in Upper Austria. Column (1) reports the mean for the full sample. The means for matched and non-matched worker-firm pairs are reported in Columns (2) and (3). Column (4) reports the difference in means between the treatment and control group. Column (5) reports the standardized difference in means. The standardized difference is defined as the difference in means between the treated and control group for a given variable $(\mu_{treated} - \mu_{control})$ divided by the average standard deviation $\left(\sqrt{0.5 \cdot (\sigma^2_{treated} + \sigma^2_{control})}\right)$ and multiplied by 100. The number of observations is given in Column (6). Standard errors are clustered at the shock firm level. $^{*}$ $p < 0.1$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.001$.

**Table A.2** Comparing Treatment and Control Shock Workers

| | Ø Full | Ø Treatment | Ø Control | Diff. | Stand. Diff. in % | N |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| **Shock Worker Characteristics** | | | | | | |
| Treated | 0.496 | 1.000 | 0.000 | 1.000*** | 0.00 | 18,221 |
| *Demographic Characteristics* | | | | | | |
| Female | 0.510 | 0.501 | 0.519 | −0.018 | −3.67 | 18,221 |
| Age in Years | 47.625 | 47.853 | 47.401 | 0.452 | 5.56 | 18,221 |
| Age at Shock in Years | 49.201 | 49.435 | 48.971 | 0.464 | 5.72 | 18,221 |
| Non-Austrian Citizen | 0.026 | 0.025 | 0.027 | −0.002 | −1.42 | 18,221 |
| *Labor Market Outcomes* | | | | | | |
| Employment Days | 88.573 | 88.643 | 88.505 | 0.138 | 0.98 | 18,221 |
| Employed at Shock Firm | 0.915 | 0.919 | 0.912 | 0.007 | 2.68 | 18,221 |
| Blue Collar | 0.422 | 0.414 | 0.430 | −0.015 | −3.05 | 17,906 |
| Daily Wage | 69.724 | 69.944 | 69.507 | 0.438 | 1.30 | 17,707 |
| Tenure in Quarters | 32.442 | 31.850 | 33.026 | −1.176 | −3.90 | 17,906 |
| *Health Outcomes* | | | | | | |
| Sick Leave Days (All Causes) | 2.260 | 2.326 | 2.195 | 0.131 | 1.68 | 18,067 |
| Easy-to-Fake Sick Leave Days | 0.100 | 0.081 | 0.118 | −0.037 | −2.73 | 18,046 |
| Inpatient Exp. | 110.687 | 125.878 | 96.666 | 29.211* | 2.96 | 17,259 |
| Outpatient Exp. | 87.760 | 91.070 | 84.704 | 6.366 | 3.87 | 17,259 |
| Medication Exp. | 35.831 | 43.893 | 28.389 | 15.504 | 7.27 | 17,259 |
| Exp. Preventative Health Check-Up | 2.431 | 2.456 | 2.406 | 0.050 | 0.37 | 18,046 |
| Exp. PSA Screening | 0.383 | 0.436 | 0.328 | 0.108* | 5.78 | 8,810 |
| Exp. Mammography | 4.008 | 4.025 | 3.992 | 0.032 | 0.20 | 9,237 |
| Dermatology Exp. | 2.010 | 2.238 | 1.800 | 0.438 | 3.52 | 17,259 |
| Radiology Exp. | 4.916 | 5.039 | 4.802 | 0.237 | 1.19 | 17,259 |

*Note* — The table reports descriptive statistics for the demographic, labor market, and health characteristics of the shock workers included in our main estimation sample. The definition of the main estimation sample and the construction of the treatment and control group are described in Section 3.1. The sample period comprises the 12 quarters preceding the (placebo) health shock. Note that health characteristics can only be observed for shock workers insured in Upper Austria. Prostate-specific antigen (PSA) tests are analyzed only for men, while mammographies are analyzed only for women. Column (1) reports the mean for the full sample. The means for treated and control shock workers are reported in Columns (2) and (3). Column (4) reports the difference in means between the treatment and control group. Column (5) reports the standardized difference in means. The standardized difference is defined as the difference in means between the treated and control group for a given variable $(\mu_{treated} - \mu_{control})$ divided by the average standard deviation $\left(\sqrt{0.5 \cdot (\sigma^2_{treated} + \sigma^2_{control})}\right)$ and multiplied by 100. The number of observations is given in Column (6). Standard errors are clustered at the shock firm level. $^{*}$ $p < 0.1$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.001$.

**Table A.3** Distribution of Shock Diagnoses

| | N | % Overall | % Within Category |
|---|---|---|---|
| | (1) | (2) | (3) |
| **Malignant Neoplasms** | | | |
| C01 – C14 | 15 | 1.88 | 2.74 |
| C15 – C25 | 59 | 7.39 | 10.77 |
| C31 – C34 | 33 | 4.14 | 6.02 |
| C40 – C41 | 3 | 0.38 | 0.55 |
| C43 – C44 | 90 | 11.28 | 16.42 |
| C48 – C49 | 8 | 1.00 | 1.46 |
| C50 | 124 | 15.54 | 22.63 |
| C51 – C57 | 44 | 5.51 | 8.03 |
| C60 – C62 | 62 | 7.77 | 11.31 |
| C64 – C67 | 24 | 3.01 | 4.38 |
| C69 – C71 | 14 | 1.75 | 2.55 |
| C73 – C75 | 20 | 2.51 | 3.65 |
| C76 – C80 | 18 | 2.26 | 3.28 |
| C81 – C96 | 34 | 4.26 | 6.20 |
| **Cerebrovascular Diseases** | | | |
| I60 – I65 | 147 | 18.42 | 100.00 |
| **Myocardial Infarction** | | | |
| I21 | 80 | 10.03 | 98.77 |
| I22 | 1 | 0.13 | 1.23 |
| **Heart Failure** | | | |
| I50 | 22 | 2.76 | 100.00 |

*Note* — The table reports the distribution of the different hospital admission diagnosis groups used to identify severe health shocks. Column (1) reports the absolute number of health shocks for each diagnosis group. Column (2) shows the percentage share of each diagnosis group in the total number of health shocks. Column (3) reports the percentage share of each diagnosis group within the four different types of health shocks (malignant neoplasms, cerebrovascular diseases, myocardial infarction, and heart failure). The ICD-10 codes represent the following diagnoses: C01 – C14 – malignant neoplasms of lip, oral cavity, and pharynx, C15 – C25 – malignant neoplasms of digestive organ, C31 – C34 – malignant neoplasms of respiratory and intrathoracic organs, C40 – C41 – malignant neoplasms of bone and articular cartilage, C43 – C44 – melanoma and other malignant neoplasms of skin, C48 – C49 – malignant neoplasms of mesothelial and soft tissue, C50 – malignant neoplasm of breast, C51 – C57 – malignant neoplasms of female genital organs, C60 – C62 – malignant neoplasms of male genital organs, C64 – C67 – malignant neoplasms of urinary tract, C69 – C71 – malignant neoplasms of eye, brain and other parts of central nervous system, C73 – C75 – malignant neoplasms of thyroid and other endocrine glands, C76 – C80 – malignant neoplasms of ill-defined, secondary and unspecified sites, C81 – C96 – malignant neoplasms, stated or presumed to be primary, of lymphoid, haematopoietic and related tissue, I60 – I65 – cerebrovascular diseases, I21 – acute myocardial infarction, I22 – subsequent myocardial infarction, I50 – heart failure.

**Table A.4** Effect of Health Shocks on Coworkers' Probability of Receiving a Golden Handshake

| | All Observations | | Coworkers Leaving the Labor Market | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Health Shock | 0.0101** | 0.0105** | 0.0349** | 0.0300* |
| | (0.0047) | (0.0046) | (0.0162) | (0.0173) |
| **Coworker Characteristics** | | | | |
| Non-Austrian Citizen | | 0.0096 | | 0.0269 |
| | | (0.0140) | | (0.0354) |
| Female | | -0.0002 | | -0.0146 |
| | | (0.0050) | | (0.0225) |
| Blue Collar | | -0.0005 | | -0.0020 |
| | | (0.0059) | | (0.0196) |
| **Shock Worker Characteristics** | | | | |
| Non-Austrian Citizen | | -0.0186* | | -0.0378 |
| | | (0.0106) | | (0.0292) |
| Female | | -0.0060 | | -0.0319 |
| | | (0.0064) | | (0.0285) |
| Blue Collar | | -0.0065 | | -0.0322 |
| | | (0.0070) | | (0.0266) |
| Shock Year FE | | ✓ | | ✓ |
| Coworker Age at Shock FE | | ✓ | | ✓ |
| Shock Worker Age at Shock FE | | ✓ | | ✓ |
| N | 1,564 | 1,564 | 435 | 435 |
| # Clusters | 632 | 632 | 299 | 299 |
| Outcome Mean | 0.0026 | 0.0026 | 0.0095 | 0.0095 |

*Note* — The table reports the estimated coefficients for the effect of a health shock on coworkers' probability of receiving a golden handshake after the health shock. The results are based on a cross-section of all coworkers included in the main estimation sample. The outcome variable is a binary indicator that is one if a coworker receives a special severance payment that is at least 8 times as large as their monthly earnings at some point after the health shock and zero otherwise. Columns (1) and (2) include all coworkers in the main estimation sample, while Columns (3) and (4) include only those coworkers whose last employment spell before retirement is at the shock firm. As the information on severance pay is only available until 2012, the sample only includes health shocks before 2010. The regressions for Columns (1) and (3) only include a binary treatment indicator as an explanatory variable. In Columns (2) and (4) we additionally control for coworkers' and shock workers' age, citizenship, gender, and collar. We also add a fixed effect for the year of the shock. The number of observations, the number of clusters, and the mean of the outcome variable for the control group are reported at the bottom of the table. Standard errors are clustered at the shock firm level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.001$.

**Table A.5** Average Effects of Health Shocks on Young Coworkers' Labor Market Outcomes

| | Employment | | Retirement | Wages | |
|---|---|---|---|---|---|
| | Days | **P**(Employed at Shock Firm) | Days | Log Daily Wage | Wage Growth |
| | (1) | (2) | (3) | (4) | (5) |
| Health Shock × Post | 0.7235* | 0.0101 | 0.1233*** | 0.0052 | 0.0004 |
| | (0.4122) | (0.0128) | (0.0401) | (0.0040) | (0.0009) |
| Individual FE | ✓ | ✓ | ✓ | ✓ | ✓ |
| Year × Birth Year FE | ✓ | ✓ | ✓ | ✓ | ✓ |
| N | 263,205 | 263,205 | 263,205 | 238,613 | 224,619 |
| # Clusters | 1,949 | 1,949 | 1,949 | 1,949 | 1,949 |
| p-Value of Pre-Coefficients | 0.400 | 0.656 | 0.630 | 0.146 | 0.202 |
| Outcome Mean | 83.014 | 0.829 | 0.027 | 4.092 | 0.013 |
| Effect in % of Mean | 0.872 | 1.216 | 459.269 | 0.128 | 2.817 |

*Note* — The table reports the estimated coefficients for the effect of a health shock on young coworkers' labor market outcomes. The estimation sample includes only coworkers who are below the age of 50 at the time of the (placebo) health shock. The results are based on Equation (1). Column (1) shows the results for the number of employment days in a given quarter. Column (2) uses a binary indicator that equals one if a coworker is employed at the firm where the (placebo) health shock occurred and zero otherwise. Column (3) reports the results for the quarterly number of retirement days. Column (4) uses the log daily wage as an outcome. Column (5) considers wage growth, which is defined as the difference in log daily wages between two consecutive quarters, as an outcome. The sample period comprises the 12 quarters preceding and the 12 quarters (including the quarter of the health shock) following the (placebo) health shock. The number of observations, the number of clusters, the p-value of an F-test for the joint significance of all pre-treatment coefficients of Equation (2), the mean of the outcome variable for the control group, and the coefficient in percent of the outcome mean are reported at the bottom of the table. For all outcomes except retirement, the outcome mean is based on observations from the pre-treatment period. The outcome mean for retirement is based on the post-treatment period. Standard errors are clustered at the shock firm level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.001$.

**Table A.6** Average Effects of Health Shocks on Young Coworkers' Health Outcomes

| | Sick Leave Days | | **P**(Expenditures > 0) | | |
|---|---|---|---|---|---|
| | All Causes | Easy-to-Fake Diagnoses | General Health Check-Up | PSA Screening | Mammography |
| | (1) | (2) | (3) | (4) | (5) |
| Health Shock × Post | -0.0544 | 0.0054 | 0.0017 | -0.0012 | -0.0002 |
| | (0.0866) | (0.0097) | (0.0011) | (0.0010) | (0.0016) |
| Individual FE | ✓ | ✓ | ✓ | ✓ | ✓ |
| Year × Birth Year FE | ✓ | ✓ | ✓ | ✓ | ✓ |
| N | 255,274 | 254,931 | 254,951 | 139,702 | 115,230 |
| # Clusters | 1,949 | 1,949 | 1,949 | 1,398 | 1,639 |
| p-Value of Pre-Coefficients | 0.252 | 0.171 | 0.764 | 0.363 | 0.997 |
| Outcome Mean | 1.921 | 0.073 | 0.025 | 0.007 | 0.027 |
| Effect in % of Mean | -2.830 | 7.374 | 6.916 | -16.909 | -0.581 |

*Note* — The table reports the estimated coefficients for the effect of a health shock on young coworkers' health outcomes. The estimation sample includes only coworkers who are below the age of 50 at the time of the (placebo) health shock. The results are based on Equation (1). Column (1) shows the results for the number of sick leave days in a given quarter. Column (2) includes only sick leaves associated with easy-to-fake diagnoses, including the common cold (ICD-10 J04 and J06), lower back pain (ICD-10 M54.5), and headache (ICD-10 R51). In Column (3), we define a binary indicator that equals one if the outpatient expenditures for the general health screening program in a given quarter are larger than zero and zero otherwise. Columns (4) and (5) use analogous binary indicators for expenditures related to PSA tests and mammography. These outcomes are analyzed only for men and women, respectively. The sample period comprises the 12 quarters preceding and the 12 quarters (including the quarter of the health shock) following the (placebo) health shock. The number of observations, the number of clusters, the p-value of an F-test for the joint significance of all pre-treatment coefficients of Equation (2), the mean of the outcome variable for the control group in the pre-treatment period, and the coefficient in percent of the outcome mean are reported at the bottom of the table. Standard errors are clustered at the shock firm level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.001$.

**Table A.7** Average Effects of Health Shocks on Coworkers' Outcomes by Age of Shock Worker

| | Employment | | | | Retirement | | **P**(Expenditures > 0) | |
|---|---|---|---|---|---|---|---|---|
| | Days | | **P**(Employed at Shock Firm) | | Days | | Mammography | |
| Age of Shock Worker | < 50 | ≥ 50 | < 50 | ≥ 50 | < 50 | ≥ 50 | < 50 | ≥ 50 |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Health Shock × Post | 1.9076** | 0.7358 | 0.0332* | 0.0202 | -1.7430*** | -0.6019 | 0.0045 | 0.0095** |
| | (0.9002) | (0.8489) | (0.0191) | (0.0196) | (0.6467) | (0.6393) | (0.0054) | (0.0044) |
| Individual FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Year × Birth Year FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| N | 40,743 | 52,315 | 40,743 | 52,315 | 40,743 | 52,315 | 20,799 | 27,251 |
| # Clusters | 722 | 884 | 722 | 884 | 722 | 884 | 499 | 630 |
| p-Value of Pre-Coefficients | 0.754 | 0.083 | 0.921 | 0.474 | 0.507 | 0.827 | 0.751 | 0.848 |
| Outcome Mean | 88.505 | 88.882 | 0.917 | 0.927 | 8.839 | 6.883 | 0.070 | 0.062 |
| Effect in % of Mean | 2.155 | 0.828 | 3.626 | 2.181 | -19.719 | -8.745 | 6.512 | 15.400 |

*Note* — The table reports the estimated coefficients for the effect of a health shock on coworkers' outcomes by the age of the shock worker at the time of the (placebo) health shock. We estimate Equation (1) separately for coworkers of shock workers below the age of 50 and for coworkers of shock workers above the age of 50. Columns (1), (3), (5), and (7) report the results for the first group, Columns (2), (4), (6), and (8) those for the second group. Columns (1) and (2) show the results for the number of employment days in a given quarter. Columns (3) and (4) use a binary indicator that equals one if a coworker is employed at the firm where the (placebo) health shock occurred and zero otherwise. Columns (5) and (6) report the results for the quarterly number of retirement days. In Columns (7) and (8), we define a binary indicator that equals one if the outpatient expenditures for mammography in a given quarter are larger than zero and zero otherwise. This outcome is analyzed only for women. The sample period comprises the 12 quarters preceding and the 12 quarters (including the quarter of the health shock) following the (placebo) health shock. The number of observations, the number of clusters, the p-value of an F-test for the joint significance of all pre-treatment coefficients of Equation (2), the mean of the outcome variable for the control group, and the coefficient in percent of the outcome mean are reported at the bottom of the table. For all outcomes except retirement, the outcome mean is based on observations from the pre-treatment period. The outcome mean for retirement is based on the post-treatment period. Standard errors are clustered at the shock firm level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.001$.

**Table A.8** Average Effects of Health Shocks on Coworkers' Outcomes by Age of Coworker

| | Employment | | | | Retirement | | **P**(Expenditures > 0) | |
|---|---|---|---|---|---|---|---|---|
| | Days | | **P**(Employed at Shock Firm) | | Days | | Mammography | |
| Age of Coworker | $< 55$ | $\geq 55$ | $< 55$ | $\geq 55$ | $< 55$ | $\geq 55$ | $< 55$ | $\geq 55$ |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Health Shock × Post | 0.5673 | 1.6420 | 0.0113 | 0.0341* | -0.2006 | -2.1280** | 0.0076* | 0.0049 |
| | (0.5708) | (1.1715) | (0.0155) | (0.0192) | (0.1623) | (1.0156) | (0.0046) | (0.0051) |
| Individual FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Year × Birth Year FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| N | 51,097 | 41,961 | 51,097 | 41,961 | 51,097 | 41,961 | 27,041 | 21,009 |
| # Clusters | 1,243 | 1,123 | 1,243 | 1,123 | 1,243 | 1,123 | 802 | 668 |
| p-Value of Pre-Coefficients | 0.699 | 0.275 | 0.512 | 0.703 | 0.709 | 0.158 | 0.131 | 0.656 |
| Outcome Mean | 88.696 | 88.717 | 0.920 | 0.925 | 0.434 | 16.963 | 0.069 | 0.061 |
| Effect in % of Mean | 0.640 | 1.851 | 1.232 | 3.682 | -46.198 | -12.545 | 11.010 | 8.159 |

*Note* — The table reports the estimated coefficients for the effect of a health shock on coworkers' outcomes by the age of the coworker at the time of the (placebo) health shock. We estimate Equation (1) separately for coworkers aged between 50 and 54 and for coworkers above the age of 55. Columns (1), (3), (5), and (7) report the results for the first group, Columns (2), (4), (6), and (8) those for the second group. Columns (1) and (2) show the results for the number of employment days in a given quarter. Columns (3) and (4) use a binary indicator that equals one if a coworker is employed at the firm where the (placebo) health shock occurred and zero otherwise. Columns (5) and (6) report the results for the quarterly number of retirement days. In Columns (7) and (8), we define a binary indicator that equals one if the outpatient expenditures for mammography in a given quarter are larger than zero and zero otherwise. This outcome is analyzed only for women. The sample period comprises the 12 quarters preceding and the 12 quarters (including the quarter of the health shock) following the (placebo) health shock. The number of observations, the number of clusters, the p-value of an F-test for the joint significance of all pre-treatment coefficients of Equation (2), the mean of the outcome variable for the control group, and the coefficient in percent of the outcome mean are reported at the bottom of the table. For all outcomes except retirement, the outcome mean is based on observations from the pre-treatment period. The outcome mean for retirement is based on the post-treatment period. Standard errors are clustered at the shock firm level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.001$.

**Table A.9** Average Effects of Health Shocks on Coworkers' Outcomes by Gender of Coworker

| | Employment | | | | Retirement | |
|---|---|---|---|---|---|---|
| | Days | | **P**(Employed at Shock Firm) | | Days | |
| Gender of Coworker | Women | Men | Women | Men | Women | Men |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Health Shock × Post | 0.4150 | 1.6689** | 0.0074 | 0.0434** | -1.4157** | -0.2086 |
| | (0.8440) | (0.8251) | (0.0160) | (0.0205) | (0.5970) | (0.5059) |
| Individual FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Year × Birth Year FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| N | 48,436 | 44,622 | 48,436 | 44,622 | 48,436 | 44,622 |
| # Clusters | 1,128 | 958 | 1,128 | 958 | 1,128 | 958 |
| p-Value of Pre-Coefficients | 0.043 | 0.135 | 0.647 | 0.326 | 0.233 | 0.106 |
| Outcome Mean | 88.669 | 88.746 | 0.925 | 0.919 | 9.766 | 5.600 |
| Effect in % of Mean | 0.468 | 1.881 | 0.796 | 4.723 | -14.496 | -3.725 |

*Note* — The table reports the estimated coefficients for the effect of a health shock on coworkers' outcomes by the the gender of the coworker. We estimate Equation (1) separately for women and men. Columns (1), (3), and (5) report the results for the first group, Columns (2), (4), and (6) those for the second group. Columns (1) and (2) show the results for the number of employment days in a given quarter. Columns (3) and (4) use a binary indicator that equals one if a coworker is employed at the firm where the (placebo) health shock occurred and zero otherwise. Columns (5) and (6) report the results for the quarterly number of retirement days. Since the probability of having a mammography is analyzed only for women, this outcomes is omitted from this table. The sample period comprises the 12 quarters preceding and the 12 quarters (including the quarter of the health shock) following the (placebo) health shock. The number of observations, the number of clusters, the p-value of an F-test for the joint significance of all pre-treatment coefficients of Equation (2), the mean of the outcome variable for the control group, and the coefficient in percent of the outcome mean are reported at the bottom of the table. For all outcomes except retirement, the outcome mean is based on observations from the pre-treatment period. The outcome mean for retirement is based on the post-treatment period. Standard errors are clustered at the shock firm level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.001$.

**Table A.10** Average Effects of Health Shocks on Coworkers' Outcomes by Gender of Coworker & Shock Worker

| | Employment | | | | Retirement | | **P**(Expenditures > 0) | |
|---|---|---|---|---|---|---|---|---|
| | Days | | **P**(Employed at Shock Firm) | | Days | | Mammography | |
| Gender of Coworker & Shock Worker | Same | Different | Same | Different | Same | Different | Same | Different |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Health Shock × Post | 0.8302 | 1.7573 | 0.0282* | 0.0111 | -0.8885 | -1.2507 | 0.0091** | 0.0010 |
| | (0.7355) | (1.0697) | (0.0158) | (0.0225) | (0.5477) | (0.7975) | (0.0039) | (0.0067) |
| Individual FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Year × Birth Year FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| N | 67,109 | 25,949 | 67,109 | 25,949 | 67,109 | 25,949 | 34,285 | 13,765 |
| # Clusters | 1,338 | 749 | 1,338 | 749 | 1,338 | 749 | 692 | 436 |
| p-Value of Pre-Coefficients | 0.258 | 0.217 | 0.698 | 0.348 | 0.189 | 0.674 | 0.368 | 0.625 |
| Outcome Mean | 88.459 | 89.304 | 0.920 | 0.928 | 7.851 | 7.775 | 0.068 | 0.060 |
| Effect in % of Mean | 0.938 | 1.968 | 3.068 | 1.200 | -11.316 | -16.086 | 13.393 | 1.629 |

*Note* — The table reports the estimated coefficients for the effect of a health shock on coworkers' outcomes for combinations of the gender of the coworkers and the shock worker. We estimate Equation (1) separately for coworkers with the same gender as the shock worker and coworkers with a different gender as the shock worker. Columns (1), (3), (5), and (7) report the results for the first group, Columns (2), (4), (6), and (8) those for the second group. Columns (1) and (2) show the results for the number of employment days in a given quarter. Columns (3) and (4) use a binary indicator that equals one if a coworker is employed at the firm where the (placebo) health shock occurred and zero otherwise. Columns (5) and (6) report the results for the quarterly number of retirement days. In Columns (7) and (8), we define a binary indicator that equals one if the outpatient expenditures for mammography in a given quarter are larger than zero and zero otherwise. This outcome is analyzed only for women. The sample period comprises the 12 quarters preceding and the 12 quarters (including the quarter of the health shock) following the (placebo) health shock. The number of observations, the number of clusters, the p-value of an F-test for the joint significance of all pre-treatment coefficients of Equation (2), the mean of the outcome variable for the control group, and the coefficient in percent of the outcome mean are reported at the bottom of the table. For all outcomes except retirement, the outcome mean is based on observations from the pre-treatment period. The outcome mean for retirement is based on the post-treatment period. Standard errors are clustered at the shock firm level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.001$.

**Table A.11** Average Effects of Health Shocks on Coworkers' Outcomes by Type of Shock

| | Employment | | | | Retirement | | **P**(Expenditures > 0) | |
|---|---|---|---|---|---|---|---|---|
| | Days | | **P**(Employed at Shock Firm) | | Days | | Mammography | |
| Type of Shock | Cancer | Cardio | Cancer | Cardio | Cancer | Cardio | Cancer | Cardio |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Health Shock × Post | 1.2147 | 0.7000 | 0.0164 | 0.0305 | -1.3900** | 0.0182 | 0.0088** | -0.0022 |
| | (0.7458) | (1.0749) | (0.0167) | (0.0235) | (0.5736) | (0.7021) | (0.0040) | (0.0066) |
| Individual FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Year × Birth Year FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| N | 63,236 | 29,821 | 63,236 | 29,821 | 63,236 | 29,821 | 35,679 | 12,370 |
| # Clusters | 1,105 | 504 | 1,105 | 504 | 1,105 | 504 | 809 | 322 |
| p-Value of Pre-Coefficients | 0.050 | 0.523 | 0.617 | 0.800 | 0.584 | 0.795 | 0.468 | 0.892 |
| Outcome Mean | 88.591 | 88.948 | 0.916 | 0.934 | 8.504 | 6.361 | 0.065 | 0.066 |
| Effect in % of Mean | 1.371 | 0.787 | 1.792 | 3.264 | -16.346 | 0.286 | 13.486 | -3.382 |

*Note* — The table reports the estimated coefficients for the effect of a health shock on coworkers' outcomes by the type of severe health shock. We estimate Equation (1) separately for health shocks due to a cancer diagnosis and health shocks due to a cardiovascular disease. Columns (1), (3), (5), and (7) report the results for the first group, Columns (2), (4), (6), and (8) those for the second group. Columns (1) and (2) show the results for the number of employment days in a given quarter. Columns (3) and (4) use a binary indicator that equals one if a coworker is employed at the firm where the (placebo) health shock occurred and zero otherwise. Columns (5) and (6) report the results for the quarterly number of retirement days. In Columns (7) and (8), we define a binary indicator that equals one if the outpatient expenditures for mammography in a given quarter are larger than zero and zero otherwise. This outcome is analyzed only for women. The sample period comprises the 12 quarters preceding and the 12 quarters (including the quarter of the health shock) following the (placebo) health shock. The number of observations, the number of clusters, the p-value of an F-test for the joint significance of all pre-treatment coefficients of Equation (2), the mean of the outcome variable for the control group, and the coefficient in percent of the outcome mean are reported at the bottom of the table. For all outcomes except retirement, the outcome mean is based on observations from the pre-treatment period. The outcome mean for retirement is based on the post-treatment period. Standard errors are clustered at the shock firm level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.001$.

**Table A.12** Average Effects of Health Shocks on Coworkers' Labor Market Outcomes by Whether Shock Worker Leaves the Firm

| | Employment | | | | Retirement | | Wages | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | Days | | **P**(Employed at Shock Firm) | | Days | | Log Daily Wage | | Wage Growth | |
| Shock Worker Leaves Firm? | Yes | No | Yes | No | Yes | No | Yes | No | Yes | No |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
| Health Shock × Post | 2.0715** | 0.0172 | 0.0441* | -0.0014 | -1.4503** | -0.5479 | -0.0040 | 0.0066 | -0.0015 | 0.0005 |
| | (0.9329) | (0.7916) | (0.0231) | (0.0153) | (0.6566) | (0.6267) | (0.0105) | (0.0055) | (0.0029) | (0.0010) |
| Individual FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Year × Birth Year FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| N | 45,785 | 47,273 | 45,785 | 47,273 | 45,785 | 47,273 | 41,362 | 42,766 | 39,037 | 40,444 |
| # Clusters | 827 | 782 | 827 | 782 | 827 | 782 | 826 | 782 | 826 | 782 |
| p-Value of Pre-Coefficients | 0.440 | 0.497 | 0.816 | 0.433 | 0.073 | 0.797 | 0.932 | 0.199 | 0.779 | 0.685 |
| Outcome Mean | 88.025 | 89.379 | 0.911 | 0.933 | 8.275 | 7.371 | 4.115 | 4.145 | 0.005 | 0.007 |
| Effect in % of Mean | 2.353 | 0.019 | 4.837 | -0.148 | -17.528 | -7.433 | -0.096 | 0.160 | -27.676 | 7.478 |

*Note* — The table reports the estimated coefficients for the effect of a health shock on coworkers' labor market outcomes by whether the shock worker leaves the shock firm after the health shock. We estimate Equation (1) separately for coworkers of shock workers who leave the shock firm within two quarters after the health shock and for coworkers of shock workers who do not leave the shock firm in this time period. Columns (1), (3), (5), (7), and (9) report the results for the first group, Columns (2), (4), (6), (8), and (10) those for the second group. Columns (1) and (2) show the results for the number of employment days in a given quarter. Columns (3) and (4) use a binary indicator that equals one if a coworker is employed at the firm where the (placebo) health shock occurred and zero otherwise. Columns (5) and (6) report the results for the quarterly number of retirement days. Columns (7) and (8) use the log daily wage as an outcome. Columns (9) and (10) consider wage growth, which is defined as the difference in log daily wages between two consecutive quarters, as an outcome. The sample period comprises the 12 quarters preceding and the 12 quarters (including the quarter of the health shock) following the (placebo) health shock. The number of observations, the number of clusters, the p-value of an F-test for the joint significance of all pre-treatment coefficients of Equation (2), the mean of the outcome variable for the control group, and the coefficient in percent of the outcome mean are reported at the bottom of the table. For all outcomes except retirement, the outcome mean is based on observations from the pre-treatment period. The outcome mean for retirement is based on the post-treatment period. Standard errors are clustered at the shock firm level. $^{*}$ $p < 0.1$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.001$.

**Table A.13** Average Effects of Health Shocks on Coworkers' Health Outcomes by Whether Shock Worker Leaves the Firm

| | Sick Leave Days | | | | **P**(Expenditures > 0) | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | All Causes | | Easy-to-Fake Diagnoses | | General Health Check-Up | | PSA Screening | | Mammography | |
| Shock Worker Leaves Firm? | Yes | No | Yes | No | Yes | No | Yes | No | Yes | No |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
| Health Shock × Post | -0.1164 | -0.0732 | -0.0278 | 0.0375 | 0.0014 | 0.0007 | 0.0066 | 0.0032 | 0.0058 | 0.0072 |
| | (0.2566) | (0.2777) | (0.0409) | (0.0380) | (0.0030) | (0.0031) | (0.0061) | (0.0062) | (0.0048) | (0.0047) |
| Individual FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Year × Birth Year FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| N | 45,013 | 46,849 | 44,992 | 46,838 | 44,989 | 46,840 | 22,395 | 21,393 | 22,598 | 25,451 |
| # Clusters | 827 | 782 | 827 | 782 | 827 | 782 | 492 | 467 | 565 | 567 |
| p-Value of Pre-Coefficients | 0.052 | 0.270 | 0.715 | 0.728 | 0.399 | 0.328 | 0.126 | 0.753 | 0.597 | 0.485 |
| Outcome Mean | 2.424 | 2.459 | 0.134 | 0.103 | 0.039 | 0.042 | 0.058 | 0.062 | 0.063 | 0.067 |
| Effect in % of Mean | -4.802 | -2.979 | -20.778 | 36.365 | 3.736 | 1.604 | 11.467 | 5.201 | 9.062 | 10.745 |

*Note* — The table reports the estimated coefficients for the effect of a health shock on coworkers' health outcomes by whether the shock worker leaves the shock firm after the health shock. We estimate Equation (1) separately for coworkers of shock workers who leave the shock firm within two quarters after the health shock and for coworkers of shock workers who do not leave the shock firm in this time period. Columns (1), (3), (5), (7), and (9) report the results for the first group, Columns (2), (4), (6), (8), and (10) those for the second group. Columns (1) and (2) show the results for the number of sick leave days in a given quarter. Columns (3) and (4) include only sick leaves associated with easy-to-fake diagnoses, including the common cold (ICD-10 J04 and J06), lower back pain (ICD-10 M54.5), and headache (ICD-10 R51). In Columns (5) and (6), we define a binary indicator that equals one if the outpatient expenditures for the general health screening program in a given quarter are larger than zero and zero otherwise. Columns (7) and (8) as well as (9) and (10) use analogous binary indicators for expenditures related to PSA tests and mammography. These outcomes are analyzed only for men and women, respectively. The sample period comprises the 12 quarters preceding and the 12 quarters (including the quarter of the health shock) following the (placebo) health shock. The number of observations, the number of clusters, the p-value of an F-test for the joint significance of all pre-treatment coefficients of Equation (2), the mean of the outcome variable for the control group, and the coefficient in percent of the outcome mean are reported at the bottom of the table. Standard errors are clustered at the shock firm level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.001$.

**Table A.14** Average Effects of Health Shocks on Coworkers' Outcomes – Weighted Regressions

| | Labor Market Outcomes | | | | | Health Outcomes | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | Employment | | Retirement | Wages | | Sick Leave Days | | **P**(Expenditures > 0) | | |
| | Days | **P**(Employed at Shock Firm) | Days | Log Daily Wage | Wage Growth | All Causes | Easy-to-Fake Diagnoses | General Health Check-Up | PSA Screening | Mammography |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
| Health Shock × Post | 1.2434* | 0.0263* | -1.1454** | 0.0084 | 0.0015 | -0.0768 | 0.0265 | 0.0005 | 0.0061 | 0.0055 |
| | (0.7472) | (0.0135) | (0.5561) | (0.0056) | (0.0012) | (0.2181) | (0.0311) | (0.0026) | (0.0052) | (0.0039) |
| Individual FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Year × Birth Year FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| N | 93,058 | 93,058 | 93,058 | 84,129 | 79,482 | 91,862 | 91,830 | 91,829 | 43,788 | 48,050 |
| # Clusters | 1,605 | 1,605 | 1,605 | 1,604 | 1,604 | 1,605 | 1,605 | 1,605 | 958 | 1,128 |
| p-Value of Pre-Coefficients | 0.388 | 0.583 | 0.522 | 0.885 | 0.881 | 0.244 | 0.508 | 0.207 | 0.219 | 0.522 |
| Outcome Mean | 88.613 | 0.920 | 8.263 | 4.100 | 0.006 | 2.253 | 0.111 | 0.041 | 0.060 | 0.064 |
| Effect in % of Mean | 1.403 | 2.859 | -13.862 | 0.206 | 23.641 | -3.407 | 23.895 | 1.241 | 10.216 | 8.502 |

*Note* — The table reports the estimated coefficients for the effect of a health shock on coworkers' labor market and health outcomes. The results are based on Equation (1) where the regression is weighted by the inverse number of workers employed at the shock firm in the quarter of the (placebo) health shock. Column (1) shows the results for the number of employment days in a given quarter. Column (2) uses a binary indicator that equals one if a coworker is employed at the firm where the (placebo) health shock occurred and zero otherwise. Column (3) reports the results for the quarterly number of retirement days. Column (4) uses the log daily wage as an outcome. Column (5) considers wage growth, which is defined as the difference in log daily wages between two consecutive quarters, as an outcome. Column (6) shows the results for the number of sick leave days in a given quarter. Column (7) includes only sick leaves associated with easy-to-fake diagnoses, including the common cold (ICD-10 J04 and J06), lower back pain (ICD-10 M54.5), and headache (ICD-10 R51). In Column (8), we define a binary indicator that equals one if the outpatient expenditures for the general health screening program in a given quarter are larger than zero and zero otherwise. Columns (9) and (10) use analogous binary indicators for expenditures related to PSA tests and mammography. These outcomes are analyzed only for men and women, respectively. The sample period comprises the 12 quarters preceding and the 12 quarters (including the quarter of the health shock) following the (placebo) health shock. The number of observations, the number of clusters, the p-value of an F-test for the joint significance of all pre-treatment coefficients of Equation (2), the mean of the outcome variable for the control group, and the coefficient in percent of the outcome mean are reported at the bottom of the table. For all outcomes except retirement, the outcome mean is based on observations from the pre-treatment period. The outcome mean for retirement is based on the post-treatment period. Standard errors are clustered at the shock firm level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.001$.

**Table A.15** Average Effects of Health Shocks on Coworkers' Outcomes – Relaxed Employment Constraint

| | Labor Market Outcomes | | | | | Health Outcomes | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | Employment | | Retirement | Wages | | Sick Leave Days | | **P**(Expenditures > 0) | | |
| | Days | **P**(Employed at Shock Firm) | Days | Log Daily Wage | Wage Growth | All Causes | Easy-to-Fake Diagnoses | General Health Check-Up | PSA Screening | Mammography |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
| Health Shock × Post | 1.0480* | 0.0163 | -1.0249** | 0.0017 | -0.0003 | -0.1227 | 0.0115 | 0.0001 | 0.0039 | 0.0063* |
| | (0.6356) | (0.0145) | (0.4394) | (0.0057) | (0.0014) | (0.1884) | (0.0281) | (0.0021) | (0.0043) | (0.0033) |
| Individual FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Year × Birth Year FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| N | 99,412 | 99,412 | 99,412 | 89,155 | 84,047 | 97,541 | 97,481 | 97,479 | 46,829 | 50,663 |
| # Clusters | 1,662 | 1,662 | 1,662 | 1,661 | 1,661 | 1,662 | 1,662 | 1,662 | 997 | 1,177 |
| p-Value of Pre-Coefficients | 0.024 | 0.057 | 0.280 | 0.328 | 0.469 | 0.026 | 0.256 | 0.275 | 0.144 | 0.600 |
| Outcome Mean | 87.283 | 0.882 | 7.685 | 4.126 | 0.006 | 2.546 | 0.128 | 0.040 | 0.059 | 0.066 |
| Effect in % of Mean | 1.201 | 1.851 | -13.336 | 0.041 | -4.224 | -4.820 | 8.955 | 0.241 | 6.608 | 9.529 |

*Note* — The table reports the estimated coefficients for the effect of a health shock on coworkers' labor market and health outcomes. The results are based on Equation (1). In contrast to the main estimation sample, where coworkers and shock workers have to be employed for at least four quarters before the health shock (including the quarter of the health shock), we only require coworkers and shock workers to be employed in the shock firm in the quarter of the shock for this estimation sample. Column (1) shows the results for the number of employment days in a given quarter. Column (2) uses a binary indicator that equals one if a coworker is employed at the firm where the (placebo) health shock occurred and zero otherwise. Column (3) reports the results for the quarterly number of retirement days. Column (4) uses the log daily wage as an outcome. Column (5) considers wage growth, which is defined as the difference in log daily wages between two consecutive quarters, as an outcome. Column (6) shows the results for the number of sick leave days in a given quarter. Column (7) includes only sick leaves associated with easy-to-fake diagnoses, including the common cold (ICD-10 J04 and J06), lower back pain (ICD-10 M54.5), and headache (ICD-10 R51). In Column (8), we define a binary indicator that equals one if the outpatient expenditures for the general health screening program in a given quarter are larger than zero and zero otherwise. Columns (9) and (10) use analogous binary indicators for expenditures related to PSA tests and mammography. These outcomes are analyzed only for men and women, respectively. The sample period comprises the 12 quarters preceding and the 12 quarters (including the quarter of the health shock) following the (placebo) health shock. The number of observations, the number of clusters, the p-value of an F-test for the joint significance of all pre-treatment coefficients of Equation (2), the mean of the outcome variable for the control group, and the coefficient in percent of the outcome mean are reported at the bottom of the table. For all outcomes except retirement, the outcome mean is based on observations from the pre-treatment period. The outcome mean for retirement is based on the post-treatment period. Standard errors are clustered at the shock firm level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.001$.