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*The health externalities of downsizing**

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Abstract

We show that downsizing has substantial externalities on the health of workers who remain in the firm. To this end, we study mass layoff (ML) survivors in Austria, using workers who survive a ML themselves, but a few years in the future, as a control group. Based on high-quality administrative data, we find evidence that downsizing has persistent effects on mental and physical health, and that these effects can be explained by workers fearing for their own jobs. We also show that health externalities due to downsizing imply non-negligible cost for firms, and that wage cuts may have similar effects.

JEL Classification: J63, I12, J23
Keywords: Downsizing, survivors, mass layoffs, health, job insecurity

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I. Introduction

The global economy is in crisis. With consumption and investment plunging, Europe and the United States have witnessed a wave of massive layoffs in 2020.[1] Job loss often leads to mental and physical health problems in affected workers.[2] This is particularly concerning in pandemic times, because unemployment may exacerbate problems in the already struggling healthcare sector. While most of the literature has focused on laid-off workers, we show that even those who *remain* in downsizing firms suffer from persistent health problems. Such spillovers can arise if layoffs trigger psychological stress in the remaining workforce, especially when survivors fear for their own jobs or are being pushed into new tasks and responsibilities.[3] This is particularly relevant for firms in distress that need to decide whether to cut wages or workers (see the discussion in Bewley 1999). If there are significant health externalities, the price of dismissing workers may be higher than previously thought.

Job stress is believed to be the most common source of work-related illness, plaguing roughly 40 million workers across the European Union (Eurofound 2017). From the medical literature we know that physical health often worsens after periods of stress. The literature finds strong correlations with a variety of adverse outcomes, especially cardiovascular, musculoskeletal, and infectious disease (Cohen et al. 2012, Dimsdale 2008, Lang et al. 2012, Steptoe & Kivimäki 2012). Females are particularly prone to stress-induced ischemia, which are episodes of reduced blood flow to the heart (Bacon 2018, Vaccarino et al. 2018). Ischemia is associated with increased risk of heart attack. In addition, stress may affect physical health also indirectly through promoting poor nutrition (e.g., Klatzkin et al. 2018), less exercise (e.g., Stults-Kolehmainen & Sinha 2014), and risky behaviors (e.g., Porcelli & Delgado 2009).

[1]In the US, Disney has recently laid off 28,000 workers, Ralph Lauren 3,600; Allstate 3,800; and American Airlines and United Airlines a combined 31,000. Source: Time (2020), https://time.com/5895669/pandemic-layoffs/ (last accessed December 5, 2020).

[2]See, e.g., Black, Devereux & Salvanes (2015), Browning & Heinesen (2012), Cygan-Rehm, Kuehnle & Oberfichtner (2017), Eliason & Storrie (2009), Kuhn, Lalive & Zweimüller (2009), Schaller & Stevens (2015), Schiele & Schmitz (2016), Sullivan & von Wachter (2009).

[3]Note that we use the terms 'externalities' and 'spillovers' interchangeably in this paper. Implicitly, we assume that downsizing-induced health costs cannot be anticipated and are, therefore, not ex-ante priced into labor contracts.

To test for the health externalities of downsizing, we study workers who remain in firms that underwent a mass layoff (ML) between 1998 and 2014 in Upper Austria. We have access to high-quality administrative records from the main public health insurance provider, covering the universe of inpatient and outpatient claims of all Upper Austrian workers. To avoid selection problems, we construct counterfactuals by using workers who survive a ML a few years in the future as a control group, observing them before they experience a ML themselves.[4] This gives rise to a difference-in-differences (DD) framework. On average, workers in the control group should be similar in time-varying unobserved characteristics to those in the treatment group and only differ in the timing of the MLs they are exposed to.

We find that surviving a ML leads to persistent health issues. Our DD estimates suggest that drug prescriptions and hospital admissions increase substantially after MLs.[5] Within one and a half years, prescriptions increase by 2.4 percent and hospital days by 12.4 percent in the remaining workforce. Downsizing triggers a variety of health problems. Our estimates indicate upticks in mental, cardiovascular, and to a lesser degree also musculoskeletal disease. Workers with preexisting conditions are more strongly affected, and female workers face particularly fierce consequences. Their probability of having a cardiac event temporarily increases by up to 0.05 percentage points, or 58 percent, due to the ML. This is consistent with the recent medical literature suggesting that women are more likely to experience stress-related cardiac events than men (Vaccarino et al. 2018). Several robustness checks confirm these results. Most importantly, we can show that using an alternative control group composed of workers who are not affected by MLs themselves does not affect our conclusions.

We find that workers from poor socioeconomic background are particularly susceptible to adverse health consequences, and that effects are strongest in large firms where turnover had been low prior to the ML. Moreover, we provide suggestive evidence that job insecurity, and not changes in income or workload, is an important mechanism that may explain our findings. This is supported

[4]This is in the spirit of Fadlon & Nielsen (2019), who compare health behaviors in families that experience similar health shocks but a few years apart.

[5]We use healthcare utilization as a proxy for general health status.

by the fact that our estimates are stronger in areas with high unemployment and for workers with low-income spouses, where the potential cost of unemployment are likely higher. Finally, we find that changes in worker health impose non-negligible costs for firms, as sick days in the retained workforce increase by up to 18 percent per quarter. For an average firm, this translates to additional direct labor cost of about €94,000 per year. We then compare this to changes in sick leave takeup in a separate sample of workers in firms that cut wages of at least one coworker by more than 10 percent. We find some evidence that downsizing and wage cuts lead to similar health spillovers.

This paper builds on the existing literature studying the health status of victims of mass layoffs and plant closures, showing that job loss can have negative effects on workers (e.g., Black, Devereux & Salvanes 2015, Browning & Heinesen 2012, Cygan-Rehm, Kuehnle & Oberfichtner 2017, Eliason & Storrie 2009, Kuhn, Lalive & Zweimüller 2009, Schaller & Stevens 2015, Schiele & Schmitz 2016, Sullivan & von Wachter 2009), their spouses (Marcus 2013), and their children (Schaller & Zerpa 2017). However, these papers focus on workers that actually lost their jobs, while we study effects on those who remain employed with the downsizing firm. This offers a new angle on the overall welfare effects of downsizing and allows us to estimate firm-level health externalities for the first time.[6]

While other studies have considered survivor health, these papers are mostly descriptive in nature, using survey data to study correlations with self-reported sick days and mental conditions. Østhus (2012) regress self-reported psychological distress on whether the worker's employer reduced its workforce by at least five percent between survey waves, Reichert & Tauchmann (2017) regress self-reported mental health on whether a respondent's firm had laid off any workers in the past year, Le Clainche & Lengagne (2019) compare antidepressant use between French workers who report working in firms that underwent a ML in the last year with other workers not reporting firm MLs, and Østhus & Mastekaasa (2010), Sigursteinsdóttir & Rafnsdóttirr (2015), and Vahtera et al. (2004) study correlations of firm turnover rates with sick days. Although these papers provide valuable insights, there are reasonable concerns that impede a causal interpretation of their findings. In

[6]A related paper is also Gathmann, Helm & Schönberg (2020), who show that MLs can have local multiplier effects that cause entire regions to suffer employment losses.

particular, they cannot account for unobserved differences between workers in downsizing and non-downsizing firms. If the former are negatively selected, part of the difference in healthcare takeup would wrongly be attributed to downsizing. We therefore avoid comparing workers in downsizing and non-downsizing firms altogether—instead, we focus solely on survivors and exploit temporal variation in their exposure to downsizing events. Moreover, while the existing literature has focused on a few select self-reported outcomes, our rich administrative data covering the universe of medical claims for affected workers allow us to study different health responses in a comprehensive manner. We are also among the first to offer suggestive evidence on different mechanisms governing ML effects, and to study implications for firm decision making more broadly.

Our paper also speaks to the literature on the health effects of job stress. It consists mostly of epidemiological studies, which tend to find strong correlations between job stress and health (for meta-analyses, see, e.g., Jamison et al. 2004, Nixon et al. 2011). However, these papers are typically cross-sectional and based on surveys with few observations and self-reported health measures. Especially the former raises questions about endogeneity, and in particular reverse causality, in the sense that ill health may cause stress as well. Well-identified studies are comparably scarce. Often they focus on different types of stress, in particular shocks during childhood, such as famines (Lindeboom, Portrait & van den Berg 2010, van den Berg, Pinger & Schoch 2016) or the death of a family member (Persson & Rossin-Slater 2018, Schmidpeter 2019). Another strand of the literature interprets bad economic conditions or import competition as stress and estimates effects on health, both during childhood (van den Berg, Lindeboom & Portrait 2006) and adulthood (e.g., Adda & Fawaz 2020, Johnston et al. 2020, Kronenberg & Boehnke 2019, Pierce & Schott 2020, Ruhm 2000, 2016). We also speak to the literature showing that unemployment leads to increases in 'deaths of despair' (i.e., deaths due to drug and alcohol abuse and suicides) using aggregate data for the US (e.g., Case & Deaton 2017, Hollingsworth et al. 2017). Our results suggest that these aggregate effects may partly arise because even workers who are only indirectly affected by job loss suffer from serious health problems too.

Our paper is also related to the personnel and management literature that studies whether a firm in distress should cut wages across the board or downsize its workforce (Lazear & Gibbs 2014). Early theoretical work by Weiss (1980) argues that general wage cuts will lead to adverse selection, because the most productive workers leave the firm. In line with this, a variety of studies show that wage cuts hurt worker morale, lead to productivity losses, increase absenteeism, and lead to premature job terminations (Cohn et al. 2014, Coviello et al. 2018, Kube et al. 2013). We extend this literature by showing that there are considerable side effects from downsizing too, and that they are comparable to health spillovers associated with wage cuts.

Our findings have several implications for management and policy. We document that MLs emit substantial externalities on the remaining workforce, which puts a burden on society if firms do not fully internalize the resulting cost. This is likely the case, because health consequences are not immediately salient, so it is difficult for firms to price them into labor contracts. Thus, there is scope for government intervention. A potential tool to incentivize firms to internalize the health cost of MLs could be to impose a layoff tax, along the lines of Blanchard & Tirole (2008). This would incentivize firms to take more efficient layoff decisions. It is important to note here that the adverse health effects we document appear in spite of Austria's universal healthcare system. In countries with less generous safety net programs, such effects may be even more pronounced. Also, since our results suggest that job insecurity is the main reason workers experience increased stress during downsizing periods, management may consider providing reassurance or, in extreme cases, even short-term job guarantees to help cushion these effects.

The paper proceeds as follows. In section II we discuss the institutional setting. In section III we present our data and empirical methodology in more depth. In section IV we discuss our main results on health and stress-related outcomes and provide several robustness checks. In section V we test for different mechanisms. In section VI we discuss the consequences of our findings for firms and management. Finally, in section VII we conclude.

II. Setting

II.1. Social security

Austria has a *Bismarckian* social security system with universal access to healthcare, pension, disability, and unemployment benefits. Enrollment into the system is automatic and linked to employment, but insurance is also extended to spouses and children, unemployed people, pensioners, and individuals with disabilities. In fact, 99.9% of the population is covered by health insurance (Hofmarcher-Holzhacker & Quentin 2013). It covers a wide range of services, including visits to outpatient general practitioners (GPs) and specialists, inpatient care, and prescription drugs with no or only minor copayments. Although there is no mandatory gatekeeping system, GPs are traditionally the first point of access to the healthcare system. Workers may take sick leaves with full wage compensation for at least 6 weeks, provided they produce a medical certificate. These certificates are almost exclusively issued by GPs (Ahammer 2018). In this paper we focus on Upper Austria, which is one of the nine Austrian federal states and has 1.5 million residents or around one-fifth of the Austrian population.[7]

II.2. Labor market

Austria's labor market is characterized by strong industrial relations and a system of centralized bargaining for wages and working conditions. Per capita GDP is among the highest worldwide; unemployment is low but has been increasing steadily in recent years. Female labor market participation has traditionally been low, and almost 50 percent of females that participate in the labor market work part-time (StatAUT 2020). This is significantly higher than the OECD average. The labor market is flexible, with relatively weak protection against dismissal and high turnover rates compared to other European countries (Böheim 2017). The law distinguishes whether labor

[7]We focus on Upper Austria because this is the only Austrian federal state we have medical claims data for. The Austrian population is generally quite homogeneous. For example, the difference in average dispensable income between the top and bottom 20 percent of the regional income distribution is only € 1,909 (OECD 2015). Hence, we have no reason to believe that Upper Austria is not representative for the entire country. Austria, in turn, is a typical Continental European country with similar institutions to other countries with generous welfare systems.

contracts are terminated unilaterally or in mutual agreement. Unilateral terminations generally do not require a reason to be specified, but a statutory notice period has to be observed.[8]

The Austrian unemployment office mandates a system of advance layoff reporting. Firms have to consult the unemployment office at least 30 days in advance prior to laying off a large share of their workforce. The law defines a ML as a layoff of 5 or more employees in firms with 10 to 99 employees, at least 5 percent of employees in firms with 100 to 599 employees, and at least 30 in firms with 600 or more employees. Notification is also required for layoffs of at least 5 employees older than 50 years of age. We use these thresholds—apart from the last one—to identify mass layoff events. In firms with a works council, management has to consult the works council prior to the notification as well.

In Austria, workers are automatically enrolled into the public unemployment insurance (UI) system, which is financed through a 6 percent payroll tax. UI benefit length depends on work experience and age, and varies between 20 weeks and one year.[9] A necessary condition to receive UI benefits is that claimants are willing to accept reasonable employment or undergo retraining. The replacement rate is 55 percent of pre-unemployment earnings. After benefit exhaustion, workers are eligible for means-tested income support. Due to the short benefit period, the Austrian UI system is more comparable to the US and the United Kingdom than other European countries (Halla et al. 2020). Disability benefits are available as a form of retirement in Austria.

III. Empirical Strategy

Estimating the effect of surviving a ML on health presents us with an important empirical challenge. Workers do not survive MLs exogenously, because there is both selection into being exposed to a ML and, conditional on working in a ML firm, selection into surviving the ML. Hence, we cannot compare ML survivors with non-survivors or the general population, as this would most likely lead

[8]An exception are fixed-term contracts, which can only end by expiration, unless the employee agrees to terminate the contract. Pregnant women before and after childbirth and workers on parental leave are generally protected from dismissal.

[9]Longer UI duration in Austria has been shown to affect reemployment wages (Nekoei & Weber 2017) and health for those laid off (Ahammer & Packham 2020).

to biased estimates. Instead, we use workers who survive a ML sufficiently far in the future as a control group. These workers should, on average, have similar unobserved characteristics to the treated workers and only differ in the timing they experience their ML, mitigating concerns about omitted variables bias. This is similar in spirit to Fadlon & Nielsen (2019), who compare health behaviors in families that experience the same health shocks a few years apart.

Figure A.1 sketches how we select the treatment and the control group. For a ML in quarter t, we observe worker health between $t-6$ and $t+6$. The control group is comprised of all MLs occurring between $t+7$ and $t+10$, but the relevant window to observe survivor health in these firms is also between $t-6$ and $t+6$, with the 'placebo' ML occurring in $t=0$. Since we always compare the treatment and control group in the same calendar quarter, the business cycle will not affect our estimations. Workers in the control group have to be continuously employed for at least 20 quarters with the same firm to qualify as survivors (e.g., for a control ML in $t=7$, the worker has to be employed at least between $t-6$ and $t+13$, counting also $t=0$). We impose the same tenure requirement to the treatment group to obtain a consistent sample definition. A potential concern related to the tenure requirement is that we underestimate the true ML effect if vulnerable workers leave the firm prior to the end of the 6-quarter post-ML period. Even though we are particularly interested in workers in stable employment—which is the most relevant population from the firm's perspective—we address this issue by relaxing the tenure requirement in section IV.3.

Note that the pre-treatment period for control MLs partly coincides with the post-treatment period of the treatment MLs, although a ML can never appear simultaneously in the treatment and the control group. This is similar to the treatment and control group definition in Fadlon & Nielsen (2019). We adopt this strategy to maximize the set of possible control MLs to draw from. It means, however, that we would overestimate the effect of MLs if health in the control group systematically became worse prior to their ML. However, this is not what we see in the raw data (see Figure A.3).[10] In fact, health status seems to be remarkably stable prior to MLs, both in the treatment and the control group. Nevertheless, we show a robustness check below which indicates that our results

[10]Below we will see that our control group is thrice as large as our treatment group, which explains why the treatment group trend is somewhat noisier than the control group's.

replicate if we draw the control group from a different set of workers who never experience a ML.

Based on this sample, we estimate the effect of surviving a ML on worker health using a difference-in-differences (DD) model,

$$y_{it} = \varphi_i + \theta_t + \gamma X_{it} + \beta S_{it} + \varepsilon_{it}, \tag{1}$$

where y_{it} is health of worker i in quarter t, φ_i are worker $\times$ ML fixed effects that control for systematic time-invariant heterogeneity between workers and MLs, θ_t are calendar quarter fixed effects that account for shocks common to all workers in a specific quarter, X_{it} contains worker age and tenure in linear form, S_{it} is an indicator variable equal to one for all workers who survive a ML in t, and zero for the group of future survivors who experience a ML between $t+7$ and $t+10$.[11] Our coefficient of interest is $\hat{\beta}$, which is the average treatment effect of downsizing on survivor health. Because health status may be correlated among workers in the same firm, we cluster our standard errors on the firm level.

To explore dynamic treatment effects, we additionally estimate a generalized DD model where we extend equation (1) to allow for the effect of downsizing on survivor health to vary non-parametrically in a period spanning 6 quarters before and after the ML,

$$y_{it} = \varphi_i + \theta_t + \gamma X_{it} + \sum_{k=-6 \mid k \neq 0}^{6} \beta_k (\tau_k \times S_{it}) + \varepsilon_{it}, \tag{2}$$

where $\tau_k = 1\{t = k\}$, $k = -6, \dots, -1, 1, \dots, 6$ indicates quarters relative to the ML in $t = 0$, and the post-ML coefficients $(\beta_1, \dots, \beta_6)$ is the series of period-specific treatment effects on health.

The crucial assumption for identification is that the difference in health between the treatment and control group would continue along the same trend absent the ML. We present evidence in support of this assumption in a number of ways. First, we show that the estimated leading coefficients $(\hat{\beta}_{-6}, \dots, \hat{\beta}_{-1})$ are insignificantly different from zero across all our outcomes. This indicates that

[11] In preliminary analyses, we ran regressions based on data aggregated at the ML level, which is the actual level of treatment. This gave almost identical results. The reason we use worker-level data is because they allow more flexibility to study heterogeneous effects and mechanisms.

workers do not systematically change their health behavior anticipating a ML. Second, we use workers drawn from the general population as an alternative control group. Third, we show that our results are stable when we reweight the sample so that the treatment and control group have the same covariate distribution, and, alternatively, when we give more weight to larger MLs. Fourth, we perform a placebo test using cancer as an outcome that cannot be affected by MLs in the short-run.

Another potential issue is that death as a result of the ML may act as a confounder in our analysis, namely if control group workers die at a higher rate than those in the treatment group. There is some evidence that MLs increase mortality (e.g., Sullivan & von Wachter 2009), but so far the literature has considered only ML victims and not survivors. As an informal test, we can check how many workers that had been employed in a ML firm at $t = 0$ die during the post-treatment period $t = [1, 6]$ (see Figure A.1).[12] The unconditional death rates in this period are 0.00186 among control group workers and 0.00193 among treatment group workers. A two-sided t-test reveals that the difference in death rates is not statistically significant ($t = -0.43$, $p = 0.664$). We are therefore convinced that our results are not driven by differential death rates between treatment and control workers.

III.1. Data

We use high-quality administrative data from the *Austrian Social Security Database* (ASSD, Zweimüller et al. 2009) linked with health records from the *Upper Austrian Sickness Fund* (UASF). The ASSD is structured as a linked employer-employee panel and covers the universe of Austrian workers from the 1970s onward. It contains detailed administrative records originally used to verify pension claims. We use the ASSD to obtain information on MLs and employment histories, wages, and certain demographics for affected workers. The limitations of the ASSD data are top-coded wages and a lack of information on working hours.

The UASF database comprises individual-level information on healthcare service utilization

[12] To calculate death rates, we drop the tenure requirement we impose for our other analyses. In the treatment group, we consider all workers that had been employed at $t = 0$ in the ML firm and who were not dismissed during the ML. For the control group, we take all workers who had been employed at $t = 0$ in a control group firm (that is, a firm experiencing a ML between $t = 7$ and $t = 10$). This gives us a population of workers that were potentially eligible to be drawn for either the treatment or the control group, regardless of how long they eventually survived.

in both the inpatient and outpatient sector for members of the sickness fund. We have data on drug prescriptions, sick leaves, hospital stays, and physician visits. Diagnoses are recorded as *International Classification of Diseases, 2010 revision* (ICD-10) codes, and drugs using the *Anatomical Therapeutic Chemical* (ATC) classification system. The UASF covers around one million members representing roughly 75% of the population in Upper Austria. Except for workers in the railway and mining industries, all private-sector employees in Upper Austria are insured with the UASF. Farmers, self-employed persons, and civil servants are covered by other institutions.

For our empirical analysis we consider the universe of all MLs between 1998 and 2014 in Upper Austria. To identify MLs we use the so-called *worker flow* approach as outlined in Fink et al. (2010). In short, we use the ASSD to build a quarterly panel measuring the number of employees in each plant. Drops in firm sizes between two quarters are considered MLs whenever they exceed the thresholds stipulated by the system of advance layoff reporting described in section II.2. Events in which a larger group of employees moves to the same plant identifier are excluded (this indicates a change in plant identifiers or a corporate spinoff instead of a true ML). Similar to other papers in the literature (e.g., Del Bono et al. 2012) we drop seasonal industries, such as farming, construction, mining, and hospitality from the sample.

Our final sample is comprised of 42,703 worker-ML dyads in the treatment group and 126,775 worker-ML dyads in the control group. In the treatment group, these stem from a total of 1,131 MLs in 1,021 firms. Most firms in our data, therefore, have only one ML event. This is a consequence of our strict sample definition, which posits that workers be continuously employed for at least 20 quarters to qualify as survivors. By construction, workers in struggling firms that undergo multiple MLs are therefore less likely to enter our sample. This has implications for the interpretation of our estimates. If stress levels are higher in struggling firms, our estimates would understate the effect of downsizing on health. In the control group—where we draw eligible workers with replacement—we have 3,618 in 913 firms.[13] Observing all workers over 13 quarters, this gives us a total sample size of 2,203,214.

[13]Using the stylized example in Figure A.1, a ML in $t+10$ can serve as a control group for MLs in t, $t+1$, $t+2$, and $t+3$. At most, eligible MLs can serve 4 times as controls.

Descriptive statistics can be found in Table 1. Columns (1) and (2) are means and standard deviations for the full sample. In columns (3) and (4) we show means for all variables for the treatment and control group, respectively. It is reassuring that the means between the two groups are fairly similar for most variables, which suggests that our sample is well-balanced.[14] Workers who survive a ML are on average 40.7 years old, have 10 years of tenure, are more likely to be male and blue collar workers, work in firms with around 300 employees, and experience MLs where roughly 8 percent of the workforce is laid off. The turnover rate one year prior to the ML, measured by the number of worker exits relative to the firm size, is around 4 percent, on average, and the local unemployment rate in the firms' zip code area is 11 percent.

It is worthwhile to point out two statistics here. First, average tenure in our sample is 10 years, which is rather long. The reason is that we require both the treatment and the control group to be employed 13 quarters prior to the ML. This is important when thinking about external validity, as our design can only speak to workers in stable employment. Even after MLs, our surviving workers remain in their firms for a remarkably long time. In Figure A.2, we plot Kaplan-Meier survival estimates, which give the share of workers that are still employed with the same firm a certain number of years after the ML. The first workers leave 1.5 years after the ML, which is the end of our observation window. After 2.5 years, 88 percent are still with their employer. After 5 and 10 years, the shares are 62 percent and 22 percent, respectively.

Second, the relative ML size appears low at first sight. However, compared to the average firm size, an 8 percent reduction corresponds to 25 workers being laid off, the standard deviation amounts to another 15 workers. While we are confident that seeing 25 colleagues fired at once can indeed trigger stress in most people, we note that effects may be even stronger if we were able to focus on larger MLs.[15] However, we show below that our estimates are not affected if we weight the data by

[14] In section 5, we additionally show that reweighting the data so that the treatment and control group have the same covariate distribution does not affect our estimates. To generate these unit weights we use the entropy balancing approach suggested by Hainmueller (2012).

[15] Focusing on larger MLs is difficult in our design. Since we consider *surviving* workers, firms that undergo large MLs are, by construction, much less likely to appear in our sample. This is further complicated by our 20-quarter tenure requirement (which is necessary to achieve as clean identification as possible). Only firms with workers in stable employment before and after the ML are included. This disqualifies many struggling firms that undergo multiple MLs in a row, and firms that do not survive the 1.5 years after an ML disappear altogether. Again, we want to emphasize

ML size prior to running our DD regressions.

III.2. Health measures

We use several proxies for worker health. Our primary measures are physician visits, drug prescriptions, and inpatient stays. Physician visits comprise all consultations with general practitioners and specialists which are paid at least in part by the UASF.[16] We measure both the number of visits as well as the log sum of expenditures billed to the UASF for these visits (these are essentially doctors' fees). For both we include also observations with no expenditures. This gives an average 5.2 physician visits with €56 in fees per quarter (see Table 1). For the latter we take the log to give less weight to extreme outliers,[17] and we add €1 prior to taking the log in order not to lose observations with zero expenditures.[18]

Prescription data include the names and ATC codes of every medication that requires a prescription in Austria. We have no information on over-the-counter drugs. As outcomes we use the total number of drug packages prescribed and the log of the sum of expenditures for these drugs. On average, workers have 1.17 prescriptions per quarter worth €25. Hospital data are only available for inpatient stays, we do not observe ambulance visits. Again, we consider both the number of days spent in hospital (on average 0.2 days per quarter) as well as the log of the sum of expenditures for these hospital stays (with a mean before taking logs of €29). Here the means are rather low, again because we include also observations with zero hospital days and expenditures.

In a second step we consider three specific medical conditions that are proposed as potential consequences of job stress in the literature: mental or psychological conditions, cardiovascular

that any effects we find are, therefore, lower bounds.

[16]This may exclude visits to private physicians if patients do not claim those expenses from the insurance. Every treatment provided by a private physician is eligible for a refund by the UASF up to the amount the health insurance would have reimbursed a contracted physician for the same treatment. We are therefore confident that a vast majority of such visits will in fact be claimed.

[17]Extreme outliers are not uncommon in health data, because severe illness can trigger exorbitant healthcare spending. Also in our sample maximum outpatient expenditures are more than 112 times larger than the mean. However, only few observations have such large values. The 99$^{\text{th}}$ percentile of the expenditure distribution is €439, which is only about 4 standard deviations larger than the sample mean of €56 (see Table 1).

[18]This could potentially be problematic due to the lack of scale invariance of the $\log(1 + y)$ function. Below we show, however, that our results are qualitatively similar if we do not logarithmize expenditures at all or we use the inverse hyperbolic sine transformation.

disease, and musculoskeletal disease. For mental conditions we consider anxiety (ICD-10 codes F40 and F41), depression and persistent mood disorders (F32, F33, F34, F38, and F39), and burnout (F43 and Z73). We construct binary variables indicating whether any of these conditions is diagnosed in a quarter. Additionally, we observe psychotherapies, which are covered by public health insurance in Austria. We also consider log expenditures for antidepressants (N06A), which are the most important drugs used to treat mental conditions.

Cardiovascular disease we define as having either a stroke or a heart attack, but we also consider expenditures for cardiovascular system drugs (ATC category C), which primarily include antihypertensives, diuretics, beta blockers, and anti-cholesterol drugs. Moreover, any condition in ICD-10 chapter M is considered a musculoskeletal disease. For related drugs, we take the log of aggregate expenses for both opioid (ATC codes N01AH and N02A) and non-opioid painkillers (M01, M02, and N02B). Finally, we consider drugs approved for addiction treatment in ATC categories N07BB (alcohol) and N07BC (opioids), as well as diagnoses indicating alcohol or drug dependence to test for effects on risky health behaviors. Alcohol and drug diagnoses are usually recorded for psychotherapies or rehabilitations, both are covered by universal healthcare in Austria. Sample means for these outcomes can also be found in Table 1.

IV. Results

IV.1. Effects on worker health

The main results are in Table 2. We present both the average DD effects from the model in equation (1) in panel (a) and the dynamic DD effects from equation (2) in panel (b). We test the pre-ML coefficients for joint significance instead of reporting them individually, but full event studies can be found in Figure 1. Indeed, we find no evidence for significant pretrends in any of our health outcomes.

A consistent finding is that downsizing leads to increases in healthcare utilization for the remaining workforce, and that this effect becomes stronger over time. First, however, we find that workers see physicians at the same rate before and after the ML. The estimate for physician visits is small and insignificant. This is an important finding, because it suggests that any negative health effects do not arise owing to failures to seek medical help or workers simply postponing necessary treatments in anticipation of the ML. We do find, however, a sizable impact on drug prescriptions and inpatient days. The average number of drug prescriptions increases by 0.03 or 2.4 percent of the sample mean (column 3), and inpatient days increase by 0.014 or 12.4 percent of the sample mean (column 5). These effects increase up to the sixth quarter after the ML, where the increase in drug prescriptions amounts to 6.8 percent and the increase in hospital days amounts to 47 percent. Hence, even though we find that outpatient physician visits remain constant, we find that more drugs are prescribed per visit and hospital days increase too.

We also consider log expenditures for both drugs and inpatient stays to get a sense of how effects compare at the intensive and extensive margin of healthcare utilization. We find that drug expenditure increase, on average, by 1.2 percent, and inpatient expenditure increase by 1.4 percent. Again, these estimates are largest in the 6$^{\text{th}}$ quarter after the ML. At this stage the estimated effects are remarkably similar; we find that expenditures for physician visits, drugs, and hospital days increase by around 4–5 percent. For hospital expenditures, this would amount to an increase from €28.9 to €30.3, which is relatively small.[19] This suggests that effects are stronger at the extensive than at the intensive margin.

A recurring pattern is that coefficient magnitudes increase over time, and effects are strongest towards the end of the observation period.[20] This is perhaps unsurprising, as stress exposure likely takes time to burgeon its full impact on the body. Also, workers may develop comorbidities after an initial health shock, for example if cardiac conditions lead to other physical or mental problems.[21]

[19]These estimates are qualitatively similar if we do not logarithmize our expenditure outcome variables or if we use the inverse hyperbolic sine transformation instead (Table A.1). Using expenditures in linear form, we find much larger coefficients that would suggest a 12 percent increase in hospital expenditures relative to the sample mean. This may be because high-expenditure workers have some leverage on our estimates. Our baseline results based on log transformed outcomes are the most conservative ones.

[20]The fact that healthcare utilization remains persistently high after a small initial dip in the ML quarter makes us confident that we do not just measure a postponement effect, in the sense that workers delay necessary treatments as a result of the ML. If this were the case, we would likely see an uptick in utilization a few quarters after the ML which quickly fades back to zero.

[21]If we look at accidents and injuries as one-shot health shocks that are less likely to entail comorbidities in the

Together with the fact that health status is state-dependent at least to some degree (Contoyannis et al. 2004, Finkelstein et al. 2009, Halliday 2008), we expect healthcare spending to remain high or even increase after a large enough stress shock. Recent medical studies show that even minor stress events can lead to rather severe health problems that linger for 10 years or more (Korkeila et al. 2010, Leger et al. 2018).

Given these patterns, it is natural to ask how these estimates would behave if we had a longer observation period. This is not a straightforward exercise, because extending the observation window means that we have to adapt also the tenure requirement we impose when constructing the treatment and control group. If we want to add an additional two quarters, for example, we have to require all workers to be employed for at least 25 (instead of 20) quarters with the same firm.[22] This reduces our sample size by almost half—we now have 22,348 worker-ML dyads in the treatment group and 74,649 worker-ML dyads in the control group. In Figure A.4, we show DD results for this new sample. Our conclusions from above remain the same, despite estimates being a little noisier than before. In fact, while drug prescriptions continue to rise after 6 quarters, the increase in inpatient days appears to level off but remain at a high level.

IV.2. Effects on stress

Next, we ask whether these effects can be explained by hikes in psychological stress levels. While we cannot measure stress directly, we consider a set of outcomes that have been suggested as potential consequences of job stress in the literature. In particular, we estimate effects on mental, cardiovascular, and musculoskeletal health (Dimsdale 2008, Lang et al. 2012, Steptoe & Kivimäki 2012). Incidentally, these three conditions are also responsible for a vast majority of inpatient days in our sample.[23] The estimates are summarized in Table 3. In short, we find some evidence for all three mechanisms, but the major drivers seem to be upticks in mental and cardiovascular disease.

future, we find a completely flat effect profile with estimates being close to zero (Figure A.5).

[22]This is also the reason why we cannot look at longer-term outcomes, such as mortality.

[23]In Figure A.6, we report the 10 three-digit ICD-10 codes responsible for most hospital days in our sample. The most common categories are M (musculoskeletal conditions), F (mental conditions), and I (heart conditions). These diseases are often tied to workplace conditions. For example, coxarthrosis (hip pain) has been linked to poor posture and physically demanding work (Tüchsen et al. 2003).

We first consider mental conditions. While we do not see effects on new mental diagnoses, workers are more likely to be in psychotherapy and have higher expenses on antidepressants. In particular, MLs increase the probability of psychotherapy on average by about 20 percent, and this effect sets in soon after the layoff. The effect on antidepressants is more modest; two quarters after the layoff, we estimate that expenditures increase by 0.5 percent.[24] Since new diagnoses remain unaffected at first, we interpret our results as evidence that workers with preexisting mental conditions are more strongly affected by MLs.

We do not find effects on cardiac conditions in the form of heart attacks and strokes. We note, however, that these are rare events, hence we may not have enough power to detect meaningful effects here. In contrast, expenses for cardiovascular system drugs, such as beta blockers or cholesterol drugs, increase by up to 0.7 percent. This effect accumulates over time; in the 6$^{\text{th}}$ quarter it amounts to as much as 2 percent. Musculoskeletal conditions and pain killer prescriptions increase too, but these effects kick in relatively late as well. The increase in both new diagnoses and expenditures for pain killers amounts to roughly 10 percent.

Finally, we test for effects on risky behaviors. This is important, as job stress may drive workers into alcohol or drug abuse. These behaviors are difficult to measure in administrative data, but we can leverage information on alcohol and drug treatment in our registers. The entry barriers to such treatments are generally low in Austria, because expenses are covered fully by public health insurance. Since these treatments are usually considered to be an *ultima ratio*, however, we can only identify severe cases of addiction. Estimates are provided in Table 4. We find no effects of MLs on alcohol abuse, and the point estimates on drugs are imprecisely estimated despite being relatively large compared to the sample mean. However, severe addiction may take too long to develop for us to find meaningful effects in a 1.5-year window after the ML. Furthermore, the sample means in Table 4 suggest that particularly drug abuse is a very rare outcome, hence our design may simply be too underpowered to detect any effects. We therefore find that job loss affects the health of

[24]We note that, for antidepressant expenditure, the joint F-test for pretrends is 2.2, which is significant at the 5 percent level. In Figure A.7 (web appendix), we plot each pre- and post-ML coefficient graphically. We can show that the F-test fails because of a single significant coefficient for $t = -2$. The flat trend prior to the ML makes us confident, however, that there is no systematic pattern we have to worry about.

workers who remain in the company primarily through mental and cardiac conditions, and that this contributes to an increase in inpatient days and drug prescriptions. There is some evidence that stress affects workers with preexisting conditions more strongly, but we also find an impact on new diagnoses.

IV.3. Robustness

Throughout this paper we argue that workers in stable employment—according to our definition these are people with at least 20 quarters of firm tenure, 13 quarters before and 6 quarters after the ML—are the most relevant population from a firm's perspective. If workers in bad health, however, leave the firm as a result of the ML, our sample would be positively selected, and we would underestimate the full extent of health effects of the ML. A comparison between surviving workers and those that leave during the ML is beyond the scope of this paper, especially because there is already evidence available for health effects of plant closures in Austria (Kuhn et al. 2009). To see how conservative our estimates are, however, it is worthwhile to check how our results change if we relax the post-ML tenure criterion and keep all workers in our sample that survive the ML but leave within the 6 quarters afterwards.[25]

We present these estimates in Figure A.8. Note that only few workers that had been employed for at least 13 quarters prior to the ML and survive the ML leave the firm in the 6 quarters afterwards. After imposing this pre-ML tenure requirement, which we need to obtain comparable estimates, we can add 8,005 workers to our sample. Including these workers hardly affects our estimates, but we do see that the estimated post-ML coefficients become slightly larger. This supports our conjecture that our estimates are lower bounds of the overall effect of MLs on the health of surviving workers.

An important empirical challenge for our paper is the choice of an appropriate control group. We think that future survivors are the optimal fit, because we can reasonably assume that they share similar unobserved characteristics with current survivors. We can show, however, that other control

[25]The workers that survive MLs but leave the firm afterwards are generally quite similar in observables compared to our treated workers in the original sample. However, we do see that they have, on average, slightly shorter tenure (10.4 instead of 11.1 years) and earn less (€ 25,500 vs. € 29,400 p.a.).

group choices lead to similar conclusions. In particular, we present results using a control group that is drawn from the general population of workers in Upper Austria. To make this a fair comparison, however, we first estimate a very simple propensity score that measures the probability of surviving a ML conditional on a set of predetermined variables for every worker in the population, and then select only those workers who have the largest propensities but are not already in our main sample. To this end, we first estimate a logit model that regresses the probability a worker survives a ML on age, tenure, and year fixed effects, all fully interacted with sex. We then predict conditional ML survival probabilities for each worker based on this logit model, and select only the 75,000 workers with the highest predicted probabilities (which yields a reasonable sample size). This ensures that the new control group has similar balancing properties with regard to age, tenure, and sex as our main control group.

We present estimates for this alternative control group in Table 5, panel (a). For comparison, we report the baseline results from Table 2 in panel (b). We can see that the control group choice does not affect our estimates. Coefficients are slightly smaller, but generally well within the range of our baseline specification. This is perhaps to be expected, given that our propensity score balancing procedure ensures that the two control groups we compare here share a similar age and tenure composition. Nevertheless, it is reassuring that using a completely different set of workers, notably ones that are not struck by a ML themselves in the future, does not affect our conclusions whatsoever.

Despite our preferred treatment and control group being very similar in terms of observables (see Table 1), in a next step we show that, even if the groups were perfectly balanced, our results would remain unchanged. We use the entropy balancing approach proposed by Hainmueller (2012), which generates unit weights that ensure that the reweighted treatment and control group have the same covariate distribution.[26] We present our DD estimates based on these reweighted samples in Figure A.9. For reference, we also plot the baseline estimates from Table 2 and their 95 percent confidence intervals. The reweighted estimates, indicated by the green line, are close to the baseline

[26]We calibrate the entropy weights such that the age, tenure, sex, and occupation distribution is the same in the treatment and control group. An advantage of this approach is that it not only balances the means across groups but also the second and third moments.

across outcomes, we therefore conclude that differences in observables are unlikely to explain our treatment effects.

Moreover, we test whether there are effects on health outcomes that cannot possibly be affected by job stress due to surviving a ML. An obvious candidate for such a placebo test is cancer. Even though stress may increase the risk of certain cancers in the longer run, there should not be any effects within six quarters after the ML, in particular if workers are not more likely to seek medical help in the first place due to the ML (as we have shown above). We therefore match information on malignant neoplasms (ICD category C) to our data, using a binary indicator for whether a neoplasm is diagnosed as the outcome variable in our DD model. We report the results of this test in Figure 4. The estimated coefficients are close to zero and statistically insignificant in all periods.[27]

Another potential concern is that we pool MLs regardless of size in our regressions, and these MLs can be relatively small (the Austrian UI office stipulates that a 5 percent reduction in the workforce constitutes a ML). If small MLs drove our results, we would potentially estimate only a lower bound of the true effect of downsizing on survivor health. In Figure A.10, we therefore present our DD estimations where observations are weighted by the relative ML size (i.e., the number of laid-off workers relative to the pre-ML firm size). The green lines represent the point estimates from these weighted regressions, and these lie well within the confidence interval of our baseline estimates (the black line). This suggests that our results are practically unchanged if we attach more weight to workers exposed to larger MLs.

Lastly, an important question is whether we in fact capture job stress by looking at ML survivors. If, for example, workers retaliated against their employers by obtaining bogus sick leave certificates, the interpretation of our findings would be different. In such a case, we would expect health effects only to be present in the outpatient sector, and for typical shirking diagnoses such as low back pain. This is not what we find. In fact, our results are mostly driven by hospital stays, which are almost impossible to fake. Moreover, shirking cannot explain the fact that cardiac conditions go up by more than 50 percent for women in response to the ML.

[27]Perhaps due to cancer being a rare outcome, these estimates are rather noisy. However, we can rule out more than a 0.004 percentage point increase in the probability of being diagnosed with cancer.

V. Effect heterogeneity and mechanisms

V.1. Which workers are most affected?

For management and policy to effectively counteract the negative health effects of downsizing, it is vital to understand the mechanisms governing these effects. In a first step, we therefore split our sample by several baseline socioeconomic characteristics to determine which workers suffer most from downsizing. This will help us to think about potential channels in the sections below. We present these results in Table 6. We report average treatment effects from equation (1) for each subsample, where age is split at 40 years and tenure and wages are split at their sample medians. We consider only count outcomes (columns 1, 3, and 5 from Table 2), but results for log expenditures are similar.

A consistent finding is that our results are entirely driven by older, blue collar, and especially low-wage workers. This is perhaps unsurprising, as these groups are generally more susceptible to adverse health shocks. While old age is related to ill-health, poor socioeconomic background is also correlated with risky behavior and morbidity (Case & Deaton 2017). For sex, the pattern is less clear. While drug prescriptions react stronger for males, effects on inpatient days are more pronounced for females. Hospitalizations are typically necessary to treat more severe conditions, a naïve interpretation of these estimates would therefore be that females face fiercer consequences of stress than men. To validate this, we investigate cardiac events by sex.[28]

These estimates are in Figure 2. They suggest that females indeed face a higher risk of cardiac events (panel a), and higher expenditure for cardiovascular drugs (panel b) due to the ML. Although the estimated coefficients appear small at face value (4 quarters after the ML, the effect on cardiac events is 0.05 percentage points), compared to the sample mean this corresponds to an increase of 58 percent. This partly explains why females have more hospital days after the ML, and is consistent with the recent medical literature suggesting that women are more susceptible to heart problems following stressful events than men (Bacon 2018). In fact, women's risk to develop

[28]We also estimated the effects of stress on mental and musculoskeletal health by sex, but found no significant heterogeneity in these outcomes.

stress–induced myocardial ischemia is twice as high than men's, and this difference cannot be explained by psychosocial or clinical risk factors (Vaccarino et al. 2018).[29] We conclude that vulnerable workers deserve special attention during downsizing periods, while younger, high-wage, white collar workers are hardly affected.

V.2. The role of wages

Above we have shown that low-wage workers are particularly susceptible to adverse health effects after downsizing periods. While the literature is divided as to whether income in itself can cause health to decline,[30] it may be correlated with other variables that make it more difficult for the worker to cope with negative health shocks, such as poor education, nutrition, or housing. A natural exercise is therefore to check whether wage levels have changed for survivors before and after the downsizing period. It is possible that managers see wage cuts as a complement to downsizing, rather than a substitute. If this were the case, we would perhaps not measure stress but a mechanical income effect, in the sense that workers have less money to invest in their health stock after the ML.

In Figure 3, we plot wage trends relative to the ML over all survivors. The graph depicts average daily wages that are regression-adjusted for age, sex, tenure, occupation, and quarter-year fixed effects. We see pretty much a flat profile before and after a ML. This confirms that income effects cannot explain our findings. Also, if we assume that workers would bargain for higher wages if their workload increased substantially after a ML, it is unlikely that our effects can be explained by systematic changes in job requirements given the wage pattern we observe. We will explore this mechanism further below.

[29] Stress-induced ischemia is found to result mostly from constriction of tiny arteries. This results in greater resistance which requires the heart to use more force in pumping blood.

[30] Lindahl (2005), for example, documents positive effects of lottery earnings on health, while Ahammer et al. (2017) show that there is no causal relationship between labor income and mortality. Snyder & Evans (2006) even find that higher social security payments lead to higher mortality.

V.3. The role of the firm

Next we test whether our results mask important heterogeneity by firm characteristics. In Table 7, we split the sample by firm size (more or less than 200 workers, which is one of the UI office ML thresholds), relative ML size (number of laid-off workers relative to the pre-ML firm size, split at the median), and the pre-ML turnover rate (number of exiting workers one year prior to the ML over the firm size one year prior to the ML, also split at the median). We again report average treatment effects from equation (1) for each subsample.

We find that effects tend to be stronger in larger firms with low turnover rates. This suggests that workers with little previous exposure to coworker fluctuation are driving our results, which is consistent with the idea that stress is most intense when workers did not expect a large layoff and perhaps considered their job to be secure prior to the layoff. For ML size, the pattern is ambiguous. While severe conditions that require hospitalization increase by the same margin regardless of ML size, mild conditions proxied by drug prescriptions go up more for smaller MLs. This suggests that, for the latter, exposure to a ML itself is more important than the actual number of colleagues laid off. This also explains why our estimates are practically unchanged if we weight each observation by the fraction of workers laid off (Figure A.10). Moreover, since it is less likely that workload is redistributed among remaining workers to a significant extent if the company is large and only few colleagues are laid off, changes in job requirements are unlikely to drive our results.

V.4. The role of job insecurity

The management literature identifies the fear of losing one's own job as the main source of stress in workers during downsizing periods (e.g., Klehe et al. 2011), and there is some empirical evidence that job insecurity can affect mental health (Cottini & Ghinetti 2018, Johnston et al. 2020, Reichert & Tauchmann 2017).[31] Isolating this mechanism can be difficult absent survey data, but we believe that singling out workers with characteristics that are correlated with higher potential cost of

[31] There is also plenty of epidemiological literature estimating correlations between self-reported job insecurity and health, e.g., from the Whitehall II study (Ferrie et al. 2005).

unemployment helps us to assess the relative importance of job insecurity versus other mechanisms (e.g., an increased workload) in explaining our results.[32]

Many of the characteristics that make workers more vulnerable to adverse health shocks also raise the cost of unemployment. In particular, the 'scarring' effect of unemployment—i.e., persistent long-term losses in wage and employment opportunities following unemployment—is particularly pronounced for low-wage workers (Jarosch 2015, Pinheiro & Visschers 2015) and to some degree also older workers (Ichino et al. 2017). Moreover, low-wage blue collar jobs may be perceived as bad signals on the job market, making job search more difficult (McCormick 1990). In section IV.1 we have seen that these groups are driving our results, which is consistent with fear of job loss being an important mechanism.

In columns (7) and (8) of Table 7, we additionally compare health responses in regions with low and high unemployment. When outside options are scarce, and when the chances of finding employment again are perceived as being small because of high competition among job seekers, we would naturally expect fear of job loss to be more prevalent in the working population. If we split the zip code-level unemployment rate at the sample median, we find that our results are indeed driven exclusively by MLs in areas with high unemployment. We consider this as suggestive evidence that fear of job loss is the main mechanism behind our findings.

In a similar vein, we can check how our estimates differ by spousal wage. The idea is that workers with high-earning spouses are less likely to experience job insecurity, because the relative potential drop in household income following job loss would be smaller. We therefore draw a subsample of workers we know are married in a given year and reestimate our DD model from equation (1).[33] We present these estimates for both sexes separately in Table A.2. Indeed, we find that point estimates are larger for workers with low-wage spouses. This is especially the case for drug prescriptions in females and hospital days in males.[34] This lends support to the notion that job

[32]We note that these mechanisms are not mutually exclusive and may, in fact, correlate with each other. This makes it more difficult to give a definitive answer as to what mechanism dominates.

[33]We do not have a perfect measure of marital status, but we can infer marriages from different tax deductions and (child care) subsidies that only apply to married people, and from the birth register, given that parents were married at the time of birth. Hence, we only observe a subset of all marriages.

[34]These estimates are practically unchanged if we control for the worker's own wage. The coefficient on drug

insecurity is the main driver behind our health effects.

VI. The cost for firms

For firms, perhaps the most relevant parameter when it comes to internalizing possible health externalities is productivity. Since productivity is difficult for us to observe, the second-best measure we have to analyze the impact on firms is sick leaves. Sick leaves are costly (Ziebarth & Karlsson 2010), even in economies without universal sick pay mandates, such as the United States. In Figure 5, we therefore regress our dynamic DD model on worker sick days. The pattern we find is similar to the outcomes we have studied before. There is an immediate uptick in sick days after the ML, and the effect increases in magnitude over time. If we estimate a single post-ML coefficient, we obtain an average effect of 0.14 sick days per quarter. Compared to the sample mean of 2.25 days per quarter, this corresponds to a 6.5 percent increase. This is well within the range of our other estimates. After six quarters, sick days already increase by 18 percent, which is substantial.[35]

To get a sense of how important this effect is, it is useful to translate the increase in sick days into monetary cost. The average daily wage in our sample is €94; the total labor cost, including social security contributions, are approximately double that, namely €188 per worker and day. Workers take on average 9 days of sick leave per year, which yields total yearly labor cost of €1,692. For a firm of 308, which is the average firm size in our sample, the direct expenditures for sick leaves are therefore €521,136 per year, a 6.5% increase of which equals €33,874. These expenditures become increasingly large over time. After six quarters, the additional sick pay cost are already up to roughly €94,000 per year. This is likely a lower bound of the true economic impact of ML-induced job stress, because it does not capture productivity losses that go beyond sick days or the potential cost of losing workers altogether due to severe illness. We can therefore assume that

prescriptions for females with low wage spouses (column 1) becomes 0.083 (0.030) instead of 0.089, the coefficient on inpatient days for males with low wage spouses becomes 0.024 (0.014) instead of 0.035, all other coefficients remain insignificant.

[35]Since we know that workers do not visit physicians more often after MLs, this indicates that the ML effect is primarily driven by longer sick leaves. This is consistent with our results from sections IV.1 and IV.2, which suggest that workers with preexisting conditions are more strongly affected.

the true effect is likely an order of magnitude larger.

Given these results, a central question for managers is whether to cut workers or wages if their firm is in distress. We cannot give a definitive answer to that. To get at least some idea of how the health spillovers of wage cuts and dismissals compare, however, we can look at sick leave takeup in firms with workers who experienced significant wage cuts. We therefore build a separate dataset based on firms where the daily wage of at least one employee is reduced by more than 10 percent from one year to another, provided that they did not switch from full-time to part-time status.[36] We then calculate, for a period of three years around this wage cut, the average number of sick days of coworkers in the firm that did *not* also experience a wage cut.[37]

In Figure 6 we plot event study estimates for changes in coworker sick days, controlling for calendar year fixed effects. Sick days appear to remain stable before the wage cut event and increase significantly afterwards. If we estimate a model with a single post-treatment dummy, we obtain a coefficient of 1.02 ($p < 0.001$), suggesting that coworkers increase sick days by roughly 1 day per year due to the wage cut. Compared to average pre-treatment yearly sick days, this amounts to an increase of 8.4 percent. Albeit being slightly larger, this coefficient is not economically different from the downsizing effect we have found above. This supports the idea that downsizing and wage cuts appear to emit similar negative health spillovers. We note that, while this may seem at odds with the narrative in Bewley (1999), this comparison is not nuanced enough to take a definitive stance on that. Even if sick days increase to a similar degree, other responses to either measure, such as changes in motivation or commitment, are difficult to tease out in our setting.

A factor we leave out of this discussion—but one that is indeed also important when it comes to the potential impact on firms—is presenteeism. While some workers may have to take sick leaves due to severe illness, others with more minor conditions may refrain to take leave and go to work instead. This may be true especially in times where workers fear for their own job. This

[36]Part-time status is defined as working 30 hours per week or less. Unfortunately we do not observe smaller reductions in working hours in our data.

[37]This gives us a sample of 316,728 wage cut events. Importantly, this is a different sample of workers compared to the one we use for our main analysis. The average number of coworker sick days per year is calculated for all workers that are employed in the same firm but did not experience a wage cut. We do not impose a tenure requirement for coworkers; for every year we calculate average sick days for all workers who were employed in the firm.

has wide-ranging effects that are well-documented in the literature, for example through fostering infectious disease transmission (Pichler & Ziebarth 2017).

VII. Conclusion

We show that downsizing leads to ill health in the firm's retained workforce. Using high-quality administrative data from Austria, we find that drug prescriptions and hospital stays increase significantly after MLs, and that this is partly driven by upticks in mental and cardiac disease. Older, low-wage, and blue collar workers respond particularly strongly to MLs. The main mechanism we have in mind to explain these findings is stress, which likely arises because workers fear for their own job after downsizing periods. In contrast, there is little evidence that income effects or changes in workload can explain our results. Finally, we find that health externalities imply non-negligible costs for firms, as sick leave takeup increases significantly after MLs. Wage cuts, as another potential cost reduction measure for firms in financial distress, appear to have similar spillover effects as downsizing.

Our results suggest that there are hidden health cost of MLs that have not previously been considered in the literature. This is particularly interesting because papers that study layoff *victims* often fail to find significant health effects. Using data from Austria, for example, Kuhn et al. (2009) find no evidence that plant closures affect the health of displaced workers. Also in Germany—the country most similar in terms of norms and welfare policies to Austria—Schmitz (2011) finds no effect of plant closures on worker health, while Schiele & Schmitz (2016) find adverse effects only for workers with bad initial health status.[38] This is perhaps because job loss-induced health problems can immediately be dampened or even reversed once workers reenter employment (Huber et al. 2011), while ML survivors are continuously exposed to the same source of stress.

We therefore think that our findings have important implications for policy and management. Health problems of surviving workers are a negative externality if they are not compensated by

[38]This is, of course, only a selected extract of the large literature on job loss and health. A number of earlier papers document rather large adverse health effects, in particular those using data from the United States and Scandinavia (Browning & Heinesen 2012, Eliason & Storrie 2009, Sullivan & von Wachter 2009).

higher wages. In light of the discussion in Blanchard & Tirole (2008), a layoff tax could be an appropriate remedy that incentivizes firms to internalize the costs of MLs. In Austria, health insurance is partly financed via payroll taxes—if a ML leads to an increase in overall public health care costs, layoff taxes would give firms the right incentives to make more efficient layoff decisions. Furthermore, management may consider offering short-term employment protection in order to reassure workers and potentially dampen these effects.

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A. Tables and figures

Table 1 — Descriptive statistics

	Mean	Std. dev.	Treatment Mean	Control Mean
	(1)	(2)	(3)	(4)
(a) Covariates and sample split variables				
Age (years)	40.7	9.2	41.8	40.3
Tenure (years)	9.9	7.6	11.1	9.6
Female	0.33	0.47	0.33	0.33
Blue collar	0.39	0.49	0.40	0.38
Wage p. a. (1,000 €)	28.6	11.0	29.4	28.4
Firm size	308.0	338.5	316.2	305.2
Relative ML size[a]	0.08	0.05	0.08	0.08
Turnover prev. year	0.04	0.04	0.04	0.03
Local unemployment	0.11	0.03	0.11	0.11
(b) Main outcomes				
Physician fees				
Count	5.20	14.25	5.59	5.07
Expenditure (€)	55.9	109.3	59.5	54.7
Drug prescriptions				
Count	1.17	2.51	1.26	1.13
Expenditure (€)	25.0	182.0	26.9	24.3
Inpatient days				
Count	0.17	1.48	0.20	0.17
Expenditure (€)	28.9	249.1	34.1	27.2
Mental conditions				
Diagnosed	0.002	0.043	0.002	0.002
Psychotherapy	0.002	0.048	0.003	0.002
Drug expenditure (€)[b]	0.9	8.1	1.0	0.9
Cardiac conditions				
Event[c]	0.001	0.030	0.001	0.001
Drug expenditure (€)[d]	3.7	18.5	4.3	3.5
Musculoskeletal conditions				
Diagnosed	0.032	0.176	0.034	0.031
Drug expenditure (€)[e]	1.0	6.0	1.1	1.0
(d) Other outcomes				
Risky health behaviors				
Alcohol	0.00055	0.02353	0.00060	0.00054
Drugs	0.00008	0.00873	0.00008	0.00008
Sick days	2.3	7.4	2.4	2.2

Notes: Quarterly means, N = 2,203,214; [a] laid-off workers relative to pre-ML firm size, [b] antidepressants (ATC code N06A), [c] heart attacks and strokes, [d] cardiovascular system drugs (ATC category C), e.g., beta blockers, [e] opioid (ATC codes N01AH and N02A) and non-opioid (M01, M02, and N02B) painkillers,

Table 2 — Effects of surviving MLs on health

	Physician visits		Drug prescriptions		Inpatient days	
	Count (1)	Log exp. (2)	Count (3)	Log exp. (4)	Count (5)	Log exp. (6)
(a) Average effects						
Average effect	0.015	0.005	0.028***	0.012**	0.021***	0.014***
	(0.053)	(0.008)	(0.008)	(0.006)	(0.007)	(0.005)
(b) Dynamic effects						
$t = 1$	–0.005	0.006	0.014	0.004	0.027**	0.015*
	(0.094)	(0.013)	(0.011)	(0.008)	(0.011)	(0.008)
$t = 2$	0.039	0.020	0.032***	0.010	0.025**	0.018**
	(0.121)	(0.013)	(0.012)	(0.009)	(0.012)	(0.008)
$t = 3$	–0.018	0.014	0.030**	0.014	0.019	0.008
	(0.094)	(0.014)	(0.012)	(0.009)	(0.012)	(0.009)
$t = 4$	0.051	0.024*	0.039***	0.024***	0.010	0.014
	(0.104)	(0.013)	(0.013)	(0.009)	(0.012)	(0.009)
$t = 5$	–0.002	0.018	0.050***	0.030***	0.044***	0.024***
	(0.109)	(0.014)	(0.014)	(0.010)	(0.011)	(0.008)
$t = 6$	0.134	0.044***	0.079***	0.037***	0.080***	0.048***
	(0.114)	(0.015)	(0.014)	(0.010)	(0.014)	(0.009)
Sample mean	5.20	2.49	1.17	1.22	0.17	0.20
F-test for pretrends[†]	0.1 (0.996)	0.8 (0.541)	1.0 (0.447)	0.9 (0.483)	0.7 (0.620)	0.5 (0.781)

Notes: This table displays DD estimates for the effect of surviving a ML on health. The sample is based on 169,478 worker × ML dyads observed over 13 quarters, which gives a total number of observations in each column of 2,203,214. Panel (a) is a static DD model based on equation (1), where the coefficients can be interpreted as average treatment effects on the respective outcome. Panel (b) is a dynamic DD model based on equation (2); the 6 pre-ML time dummies are not reported explicitly but we test for their joint significance below. In columns (1) and (2) the outcome is outpatient physician visits, in columns (3) and (4) drug prescriptions, and in columns (5) and (6) days spent in hospital. We measure the number of physician visits (1), drug packages prescribed (3), and days spent in hospital (5) along with log expenditure arising from each outcome. Each regression controls for worker age, tenure, and a set of worker × ML fixed effects. Standard errors are clustered at the firm level, stars indicate significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

[†] We test for pretrends using the (non-reported) pre-ML time dummies from the models in panel (b); $(\hat{\beta}_6, \ldots, \hat{\beta}_{-1})$. The F-test is against the null of *no* pretrend in the respective outcome. Corresponding p-values are reported in parentheses.

Table 3 — Mechanisms for the effects of surviving MLs on health

	Mental conditions			Cardiac conditions		Musculoskeletal conditions	
	Diagnosed	Psychotherapy	Drug exp.	Event	Drug exp.	Diagnosed	Drug exp.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
(a) Average effects							
Average effect	0.0003	0.0004*	0.003	0.0001	0.007**	0.0009	0.002
	(0.0002)	(0.0003)	(0.002)	(0.0001)	(0.003)	(0.0009)	(0.003)
(b) Dynamic effects							
$t = 1$	0.0001	0.0001	0.003	0.0002	0.006**	0.0009	0.001
	(0.0003)	(0.0003)	(0.002)	(0.0002)	(0.003)	(0.001)	(0.004)
$t = 2$	0.0002	0.0006**	0.005**	0.0003	0.002	0.0003	0.006
	(0.0003)	(0.0003)	(0.002)	(0.0002)	(0.003)	(0.001)	(0.004)
$t = 3$	0.0003	0.0007**	0.005**	0.0002	0.007*	0.0004	0.004
	(0.0003)	(0.0003)	(0.002)	(0.0002)	(0.004)	(0.001)	(0.005)
$t = 4$	0.0002	0.0004	0.003	0.0003	0.01**	0.0006	0.006
	(0.0004)	(0.0003)	(0.003)	(0.0002)	(0.004)	(0.001)	(0.004)
$t = 5$	0.0004	0.0007*	0.004	0.0002	0.01***	0.003**	0.01***
	(0.0004)	(0.0004)	(0.003)	(0.0002)	(0.004)	(0.002)	(0.005)
$t = 6$	0.001***	0.0009**	0.007**	0.0002	0.02***	0.003**	0.009*
	(0.0004)	(0.0004)	(0.003)	(0.0002)	(0.005)	(0.002)	(0.005)
Sample mean	0.002	0.002	0.08	0.0009	0.3	0.03	0.2
F-test for pretrends†	1.6 (0.150)	0.9 (0.458)	2.2 (0.044)	0.9 (0.469)	0.5 (0.836)	0.3 (0.935)	1.6 (0.148)

Notes: This table displays DD estimates for the effect of surviving a ML on additional health outcomes. The sample is based on 169,478 worker × ML dyads observed over 13 quarters, which gives a total number of observations in each column of 2,203,214. Panel (a) is a static DD model based on equation (1), where the coefficients can be interpreted as average treatment effects on the respective outcome. Panel (b) is a dynamic DD model based on equation (2); the 6 pre-ML time dummies are not reported explicitly but we test for their joint significance below. In column (1) the outcome is an indicator for whether anxiety, burnout, or depression is diagnosed in a worker; (2) is whether the worker is in psychotherapy, (3) is log expenditure for antidepressants and benzodiazepines; (3) is whether a stroke or a heart attack occurs, (4) is log expenditure for cardiovascular system drugs, (5) is whether a musculoskeletal condition is diagnosed, and (6) is log expenditure for both opioid and non-opioid painkillers. Each regression controls for worker age, tenure, and a set of worker × ML fixed effects. Standard errors are clustered at the firm level, stars indicate significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

† We test for pretrends using the (non-reported) pre-ML time dummies from the models in panel (b); $(\hat{\beta}_6, \ldots, \hat{\beta}_{-1})$. The F-test is against the null of *no* pretrend in the respective outcome. Corresponding p-values are reported in parentheses.

TABLE 4 — Effects of surviving MLs on measures of alcohol and drug use

	Alcohol (1)	Drugs (2)
(a) Average effects		
Average effect	–0.00005	0.00002
	(0.0001)	(0.00004)
(b) Dynamic effects		
$t = 1$	0.00002	0.00002
	(0.0001)	(0.00006)
$t = 2$	–0.00007	–0.00001
	(0.0001)	(0.00005)
$t = 3$	–0.0001	–0.00001
	(0.0001)	(0.00005)
$t = 4$	0.00009	–0.00001
	(0.0002)	(0.00005)
$t = 5$	0.00003	–0.00001
	(0.0002)	(0.00005)
$t = 6$	0.0003	–0.00004
	(0.0002)	(0.00006)
Sample mean	0.00055	0.00008
F-test for pretrends†	0.9 (0.507)	0.8 (0.602)

Notes: This table displays DD estimates for the effect of surviving a ML on measures of alcohol and drug abuse. The sample is based on 169,478 worker × ML dyads observed over 13 quarters, which gives a total number of observations in each column of 2,203,214. Panel (a) is a static DD model based on equation (1), where the coefficients can be interpreted as average treatment effects on the respective outcome. Panel (b) is a dynamic DD model based on equation (2); the 6 pre-ML time dummies are not reported explicitly but we test for their joint significance below. In column (1) the outcome is a binary variable indicating whether the worker was treated for alcohol dependence, in column (2) the outcome is whether the worker is treated for drug addiction. Each regression controls for worker age, tenure, and a set of worker × ML fixed effects. Standard errors are clustered at the firm level, stars indicate significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

† We test for pretrends using the (non-reported) pre-ML time dummies from the models in panel (b); $(\hat{\beta}_6, \ldots, \hat{\beta}_{-1})$. The F-test is against the null of *no* pretrend in the respective outcome. Corresponding p-values are reported in parentheses.

Table 5 — Robustness to choosing a different control group

	Physician visits (1)	Drug prescriptions (2)	Inpatient days (3)
(a) Alternative control group			
$t = 1$	0.012	0.010	0.024**
	(0.097)	(0.011)	(0.011)
$t = 2$	0.054	0.025**	0.020*
	(0.120)	(0.012)	(0.012)
$t = 3$	−0.016	0.019	0.012
	(0.093)	(0.012)	(0.012)
$t = 4$	0.066	0.025**	0.001
	(0.108)	(0.013)	(0.012)
$t = 5$	0.015	0.036***	0.032***
	(0.113)	(0.014)	(0.012)
$t = 6$	0.141	0.059***	0.066***
	(0.116)	(0.013)	(0.014)
Observations	1,503,567	1,503,567	1,503,567
Sample mean	4.76	1.12	0.19
F-test for pretrends†	0.1 (0.999)	1.4 (0.205)	1.8 (0.097)
(b) Baseline			
$t = 1$	−0.005	0.014	0.027**
	(0.094)	(0.011)	(0.011)
$t = 2$	0.039	0.032***	0.025**
	(0.121)	(0.012)	(0.012)
$t = 3$	−0.018	0.030**	0.019
	(0.094)	(0.012)	(0.012)
$t = 4$	0.051	0.039***	0.010
	(0.104)	(0.013)	(0.012)
$t = 5$	−0.002	0.050***	0.044***
	(0.109)	(0.014)	(0.011)
$t = 6$	0.134	0.079***	0.080***
	(0.114)	(0.014)	(0.014)
Observations	2,203,214	2,203,214	2,203,214
Sample mean	5.20	1.17	0.17
F-test for pretrends†	0.1 (0.996)	1.0 (0.447)	0.7 (0.620)

Notes: This table displays DD estimates for the effect of surviving a ML on additional health outcomes for a control group matched from firms that do not experience a ML, see the notes of Table 2. We use only the count measures from Table 2 as outcomes. Each regression controls for worker age, tenure, and a set of worker × ML fixed effects. Standard errors are clustered at the firm level, stars indicate significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

† We test for pretrends using the (non-reported) pre-ML time dummies from the models in panel (b); $(\hat{\beta}_6, \ldots, \hat{\beta}_{-1})$. The F-test is against the null of *no* pretrend in the respective outcome. Corresponding p-values are reported in parentheses.

Table 6 — Worker-level heterogeneity in the effects of surviving MLs on health

	Sex		Age		Tenure		Collar		Wage	
	Female	Male	< 40	≥ 40	Short	Long	Blue	White	Low	High
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Physician visits	–0.100	0.063	0.003	0.004	0.030	0.003	0.096	–0.027	0.155**	–0.113
	(0.100)	(0.062)	(0.071)	(0.075)	(0.081)	(0.071)	(0.077)	(0.069)	(0.071)	(0.079)
Sample mean	7.18	4.22	4.11	6.03	4.96	5.44	5.03	5.31	5.49	4.92
Pretrends†	0.392	0.897	0.720	0.800	0.548	0.555	0.600	0.713	0.655	0.991
Drug prescriptions	0.019	0.030***	0.012	0.030**	0.022*	0.026**	0.035**	0.027**	0.046***	0.010
	(0.016)	(0.009)	(0.010)	(0.012)	(0.012)	(0.012)	(0.014)	(0.011)	(0.013)	(0.011)
Sample mean	1.38	1.06	0.75	1.49	1.03	1.31	1.24	1.12	1.23	1.10
Pretrends†	0.100	0.990	0.843	0.305	0.121	0.531	0.432	0.065	0.204	0.393
Inpatient days	0.027***	0.018**	–0.001	0.034***	0.013*	0.024**	0.029***	0.016*	0.020*	0.017*
	(0.010)	(0.009)	(0.008)	(0.011)	(0.008)	(0.011)	(0.011)	(0.009)	(0.010)	(0.010)
Sample mean	0.18	0.17	0.12	0.22	0.15	0.20	0.18	0.17	0.18	0.17
Pretrends†	0.705	0.420	0.990	0.413	0.530	0.310	0.662	0.972	0.753	0.493

Notes: This table displays subgroup DD estimates for the effect of surviving a ML on health by worker socioeconomic characteristics. Tenure and wage are split at the sample median (7.5 years and €28,182 p.a., respectively). We report average treatment effects from the static DD model in equation (1). For details on the outcome variables, please consult the notes of Table 2. Each regression controls for worker age, tenure, and a set of worker × ML fixed effects. Standard errors are clustered at the firm level, stars indicate significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

† To test for pretrends we run an auxilliary regression similar to the dynamic DD model in equation (2) and perform an F-test for the joint significance of the estimated pre-ML coefficients $(\hat{\beta}_6, \ldots, \hat{\beta}_{-1})$. Here we only report the corresponding p-value of this test.

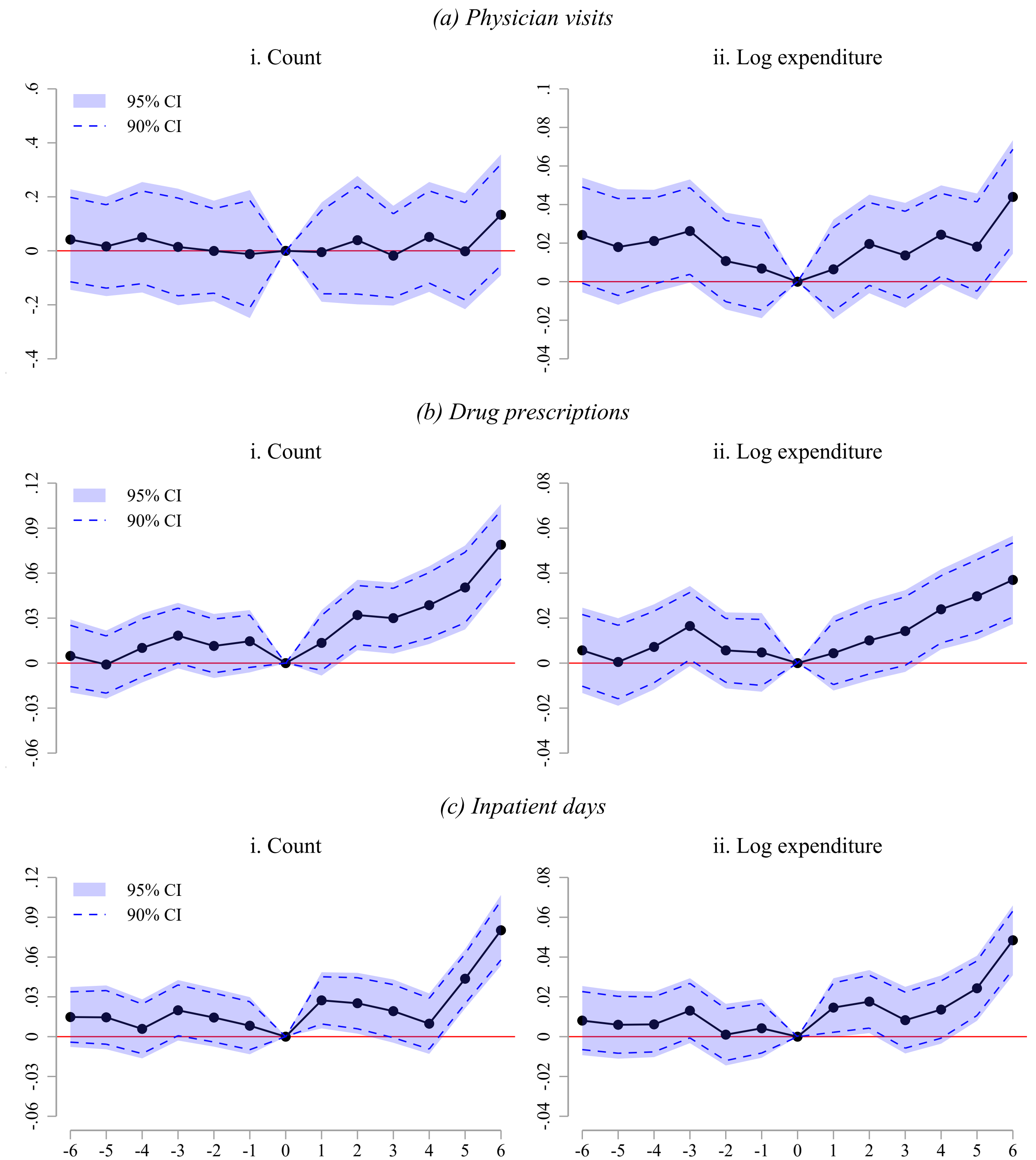
Table 7 — Heterogeneity by firm characteristics and local unemployment rate in the effects of surviving MLs on health

	Firm characteristics						**Local unemployment**	
	Firm size		Relative ML size		Turnover rate			
	< 200	≥ 200	Low	High	Low	High	Low	High
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Physician visits	0.059	–0.027	0.002	0.019	–0.017	0.002	0.065	0.046
	(0.074)	(0.073)	(0.075)	(0.076)	(0.122)	(0.074)	(0.214)	(0.058)
Sample mean	5.27	5.12	5.42	4.98	5.17	5.23	4.10	5.26
Pretrends†	0.974	0.998	0.665	0.573	0.992	0.820	0.532	0.998
Drug prescriptions	0.024**	0.037***	0.038***	0.017	0.056***	0.026**	0.008	0.029***
	(0.012)	(0.013)	(0.013)	(0.012)	(0.016)	(0.011)	(0.040)	(0.009)
Sample mean	1.14	1.20	1.22	1.11	1.16	1.17	1.10	1.17
Pretrends†	0.686	0.525	0.155	0.791	0.830	0.631	0.300	0.553
Inpatient days	0.007	0.034***	0.021*	0.020**	0.060***	0.007	0.007	0.020***
	(0.008)	(0.011)	(0.011)	(0.009)	(0.012)	(0.009)	(0.033)	(0.007)
Sample mean	0.17	0.18	0.19	0.16	0.18	0.17	0.16	0.18
Pretrends†	0.676	0.142	0.258	0.621	0.561	0.752	0.210	0.547

Notes: This table displays subgroup DD estimates for the effect of surviving a ML on health by firm and local economy characteristics. We report average treatment effects from the static DD model in equation (1). Relative ML size, turnover, and unemployment rate are split at the sample median (8.6%, 9.9%, 10%, respectively). For details on the outcome variables please consult the notes of Table 2. Each regression controls for worker age, tenure, and a set of worker × ML fixed effects. Standard errors are clustered at the firm level, stars indicate significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

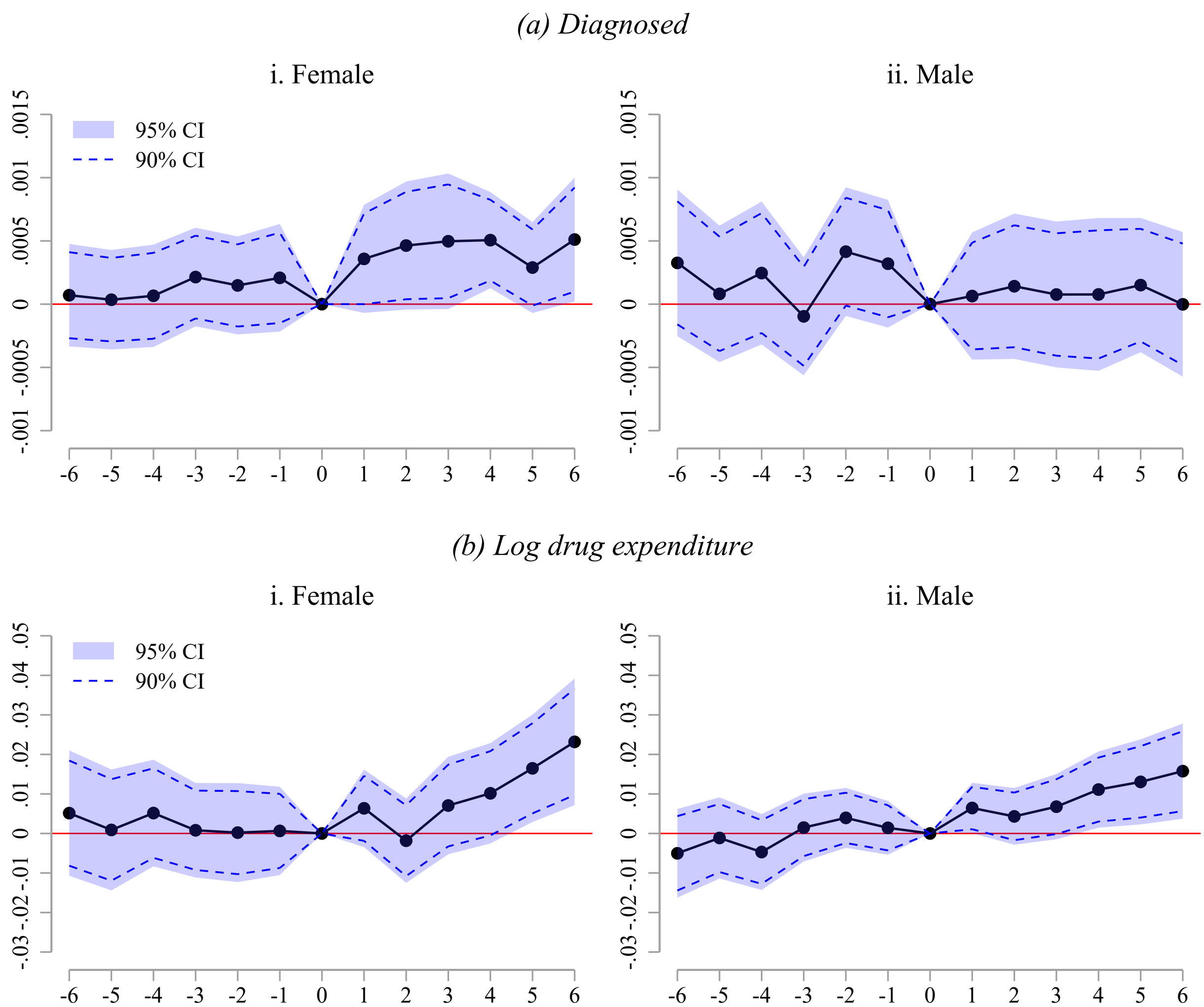
† To test for pretrends we run an auxilliary regression similar to the dynamic DD model in equation (2) and perform an F-test for the joint significance of the estimated pre-ML coefficients $(\hat{\beta}_6, \ldots, \hat{\beta}_{-1})$. Here we only report the corresponding p-value of this test.

Figure 1 — Dynamic treatment effects



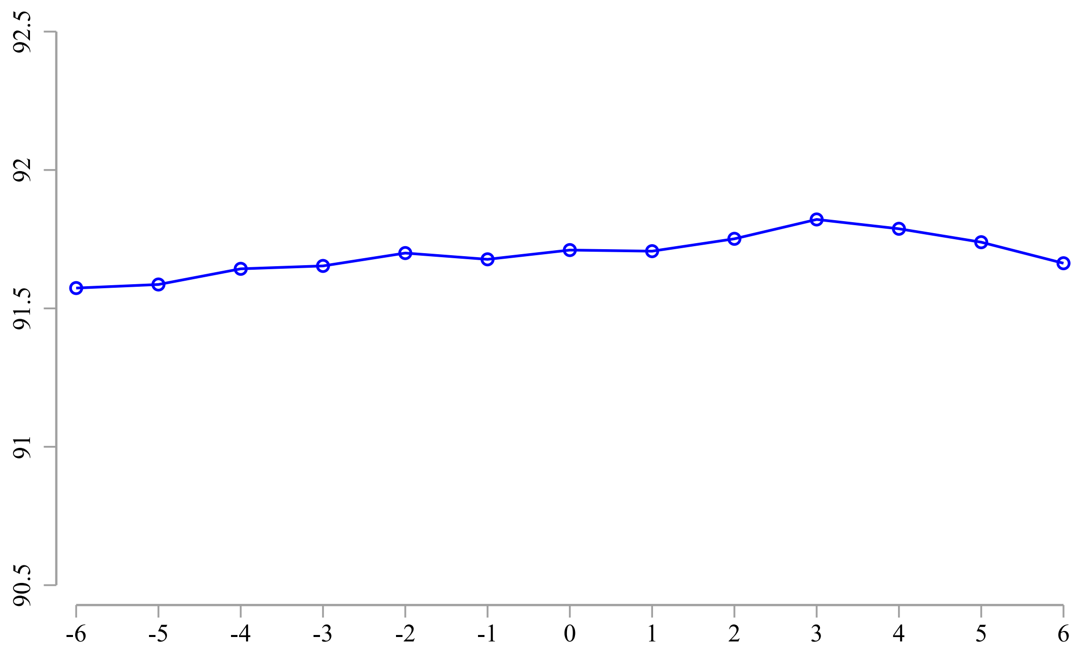
Notes: These graphs depict dynamic DD estimates for effects of surviving a ML on health, 6 quarters before until 6 quarters after the mass layoff. The post-ML coefficients are taken from Table 2. Scatters represent point estimates. The blue-shaded area is a 95% confidence band, the dashed line a 90% confidence band, both based on firm-level clustered standard errors. In each regression we control for worker age, tenure, and a set of worker × ML fixed effects.

Figure 2 — Sex heterogeneity in the effects of surviving MLs on cardiovascular health



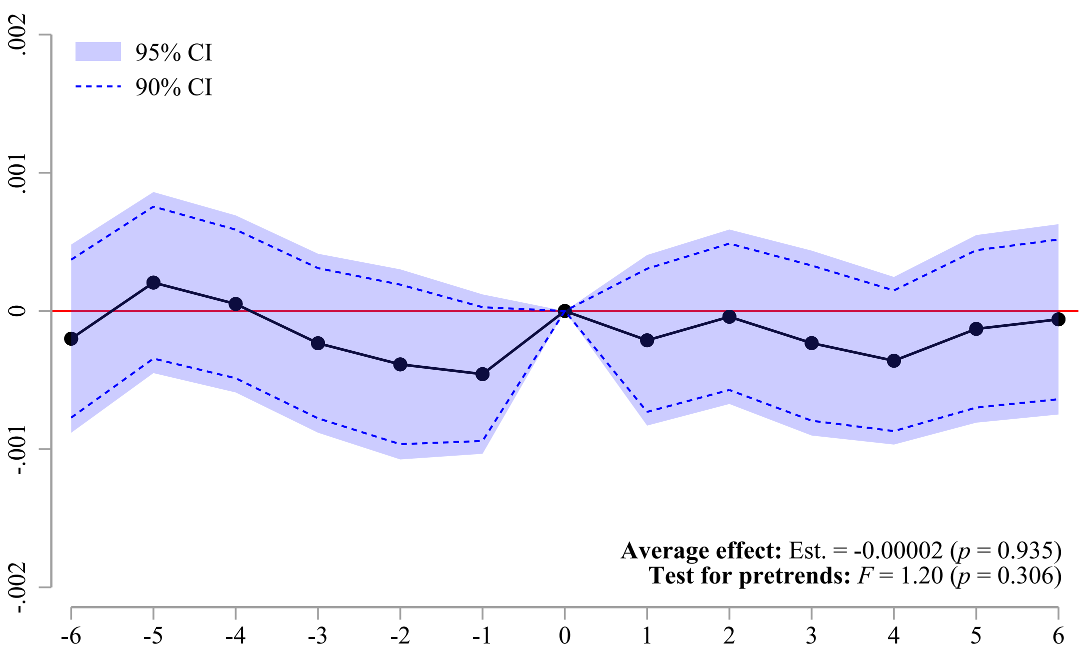
Notes: These graphs depict dynamic DD estimates for effects of surviving a ML on the probability of having a stroke or a heart attack (panel a) and log expenditure for cardiovascular drugs (panel b), 6 quarters before until 6 quarters after the mass layoff. Scatters represent point estimates. The blue-shaded area is a 95% confidence band, the dashed line a 90% confidence band, both based on firm-level clustered standard errors. In each regression we control for worker age, tenure, and a set of worker-level fixed effects.

FIGURE 3 — Evolution of survivor wages



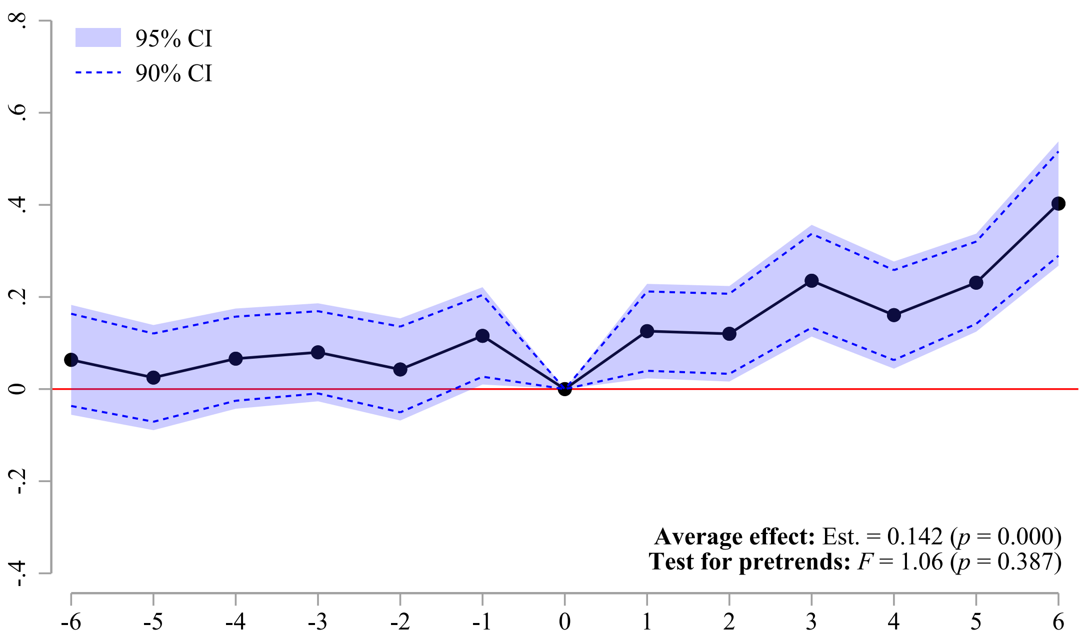
Notes: This graph plots average daily wages for ML survivors relative to their ML quarter, with wages being regression-adjusted for age, sex, tenure, occupation, and calendar quarter fixed effects (we regress wages on these covariates, recover the residuals from this regression, and add those to the sample mean of daily wages).

FIGURE 4 — Effects on neoplasms



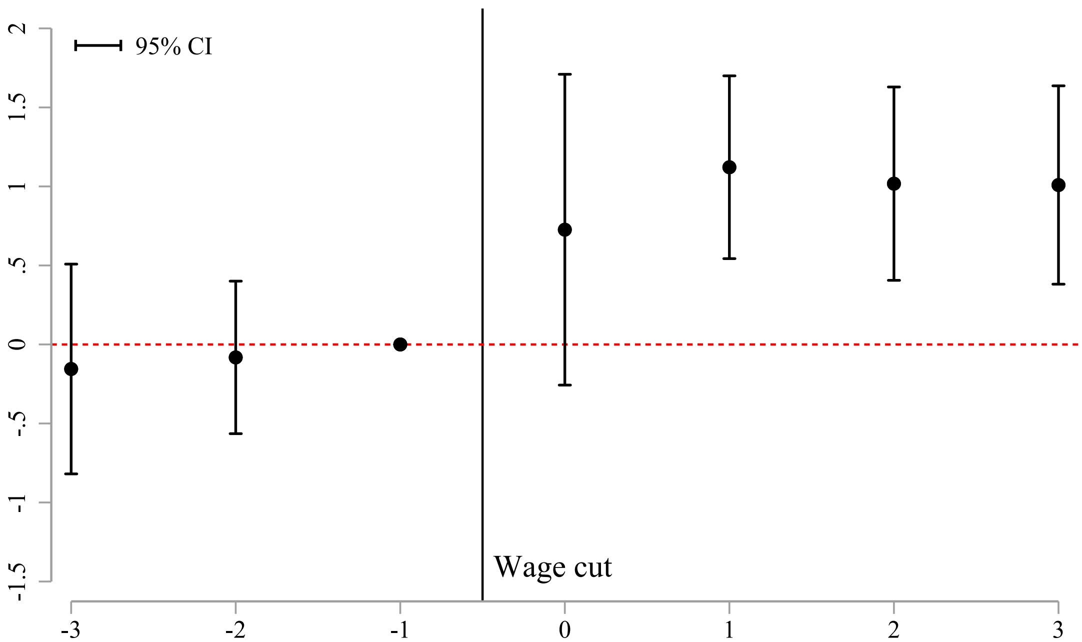
Notes: This graph depicts dynamic DD estimates for the effect of surviving a ML on the probability of being diagnosed with a malign neoplasm, 6 quarters before until 6 quarters after the mass layoff. Scatters represent point estimates. The blue-shaded area is a 95% confidence band, the dashed line a 90% confidence band, both based on firm-level clustered standard errors. In each regression we control for worker age, tenure, and a set of worker × ML fixed effects..

Figure 5 — Effects on sick days



Notes: This graph depicts dynamic DD estimates for the effect of surviving a ML on the number of sick days, 6 quarters before until 6 quarters after the mass layoff. Scatters represent point estimates. The blue-shaded area is a 95% confidence band, the dashed line a 90% confidence band, both based on firm-level clustered standard errors. In each regression we control for worker age, tenure, and a set of worker × ML fixed effects.

FIGURE 6 — Change in average sick days when coworkers experience wage cuts



Notes: For this graph, we first identify workers who experience at least a 10 percent annual wage cut between 1998 and 2014 in Upper Austria, provided that the worker did not switch from full-time to part-time status (this is different from our main data set). In total, we have 411,925 such events (note that this is a different sample of workers compared to our main analysis). We then calculate the average number of sick days per year for all coworkers in the same firm that did not also experience a wage cut. We then plot the change in the average number of these coworker sick days three years before and three years after the wage cut. If a worker experiences multiple wage cuts, we only include the first. If we estimate this model with a single post wage cut dummy, we obtain an increase in average sick days of $\hat{\beta} = 1.02$ ($p < 0.001$) relative to year -1, the pre-treatment sample mean is 12.1 days.

A. Web appendix

This web appendix contains additional tables and figures for the paper *"The health externalities of downsizing"* by Alexander Ahammer, Dominik Grübl, and Rudolf Winter-Ebmer.

A.1. Additional tables and figures

Table A.1 — Effects for different transformations of the expenditure outcomes

	Physician visits (1)	Drug prescriptions (2)	Inpatient days (3)
(a) Baseline: $\log(1+y)$			
Average effect	0.005	0.012**	0.014***
	(0.008)	(0.006)	(0.005)
Sample mean	2.49	1.22	0.20
(b) Linear form: y			
Average effect	0.343	1.298**	3.583***
	(0.504)	(0.551)	(1.152)
Sample mean	55.94	24.96	28.93
(c) Inverse hyperbolic sine: $\operatorname{asinh}(y)$			
Average effect	0.006	0.014**	0.016***
	(0.010)	(0.007)	(0.006)
Sample mean	2.91	1.45	0.22

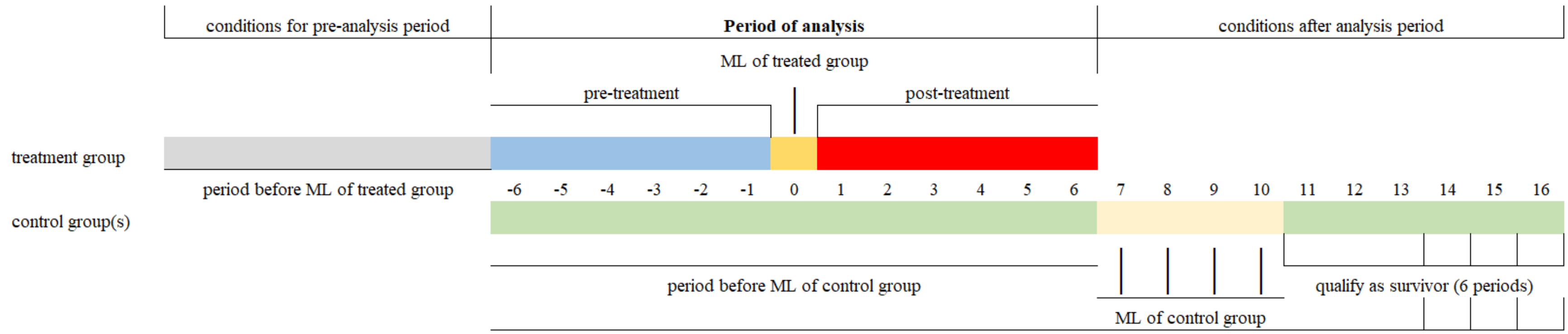
Notes: This table replicates the DD estimates for the effect of surviving a ML on healthcare spending from Table 2, columns (2), (4), and (6). The sample is based on 169,478 worker × ML dyads observed over 13 quarters, which gives a total number of observations in each column of 2,203,214. Panel (a) is the baseline, where expenditures on physician visits, drugs, and inpatient stays are measured in logs. In Panel (b), we measure each outcome in Euros and do not apply any transformations. In Panel (c), we use the inverse hyperbolic sine transformation. Each regression controls for worker age, tenure, and a set of worker × ML fixed effects. Standard errors are clustered at the firm level, stars indicate significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.2 — Effects by spousal wage

	Females		Males	
	Spouse wage low	Spouse wage high	Spouse wage low	Spouse wage high
	(1)	(2)	(3)	(4)
Physician fees	–0.045	0.207	0.051	–0.089
	(0.171)	(0.146)	(0.110)	(0.149)
Sample mean	7.37	6.67	4.33	4.12
Observations	71,032	95,056	221,195	406,328
Drug prescriptions	0.089***	–0.014	0.013	0.008
	(0.030)	(0.025)	(0.015)	(0.010)
Sample mean	1.56	1.47	1.14	1.00
Observations	71,032	95,056	221,195	406,328
Inpatient days	0.029	0.027	0.035***	0.008
	(0.023)	(0.017)	(0.014)	(0.010)
Sample mean	0.19	0.17	0.17	0.16
Observations	71,032	95,056	221,195	406,328

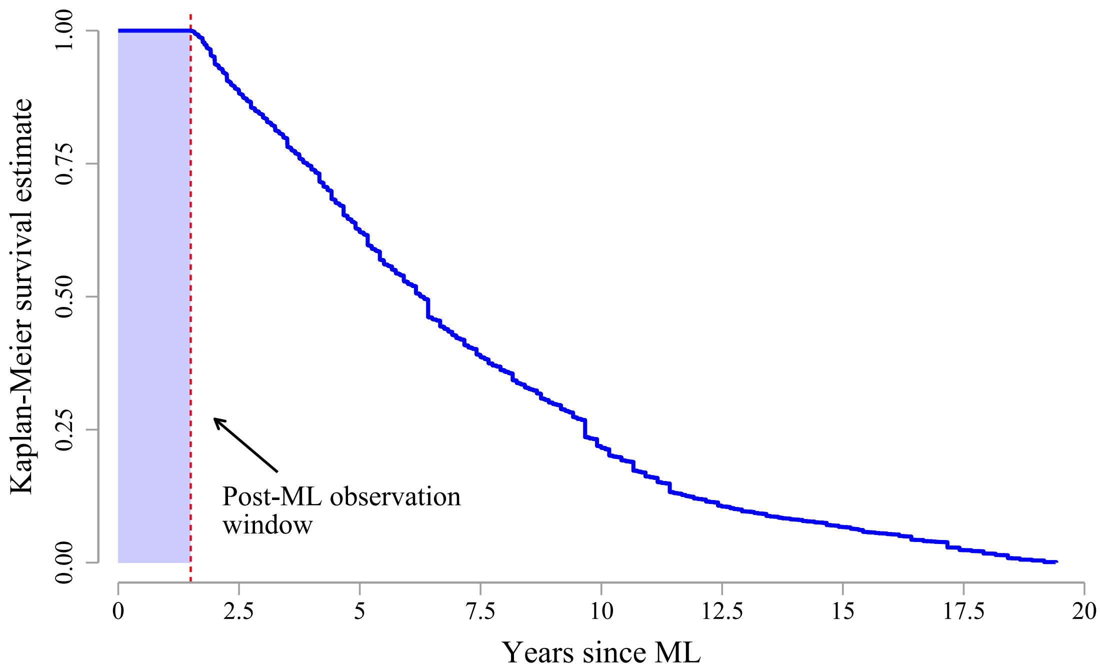
Notes: This table compares the average treatment effect estimates for our three count outcomes from Table 2 (columns 1, 3, and 5) by spousal wage, split at the median wage distribution across all spouses. Since we do not observe marital status directly, this is based on a subset of married workers we can identify based on a specific set of tax deductions and (child care) subsidies. We can confirm marriage status for 61,047 worker-ML dyads in our data; this would correspond to a marriage rate of 36 percent, The overall number of observations is 793,611. Each regression controls for worker age, tenure, and a set of worker × ML fixed effects. Stars indicate significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure A.1 — Treatment and control group definition



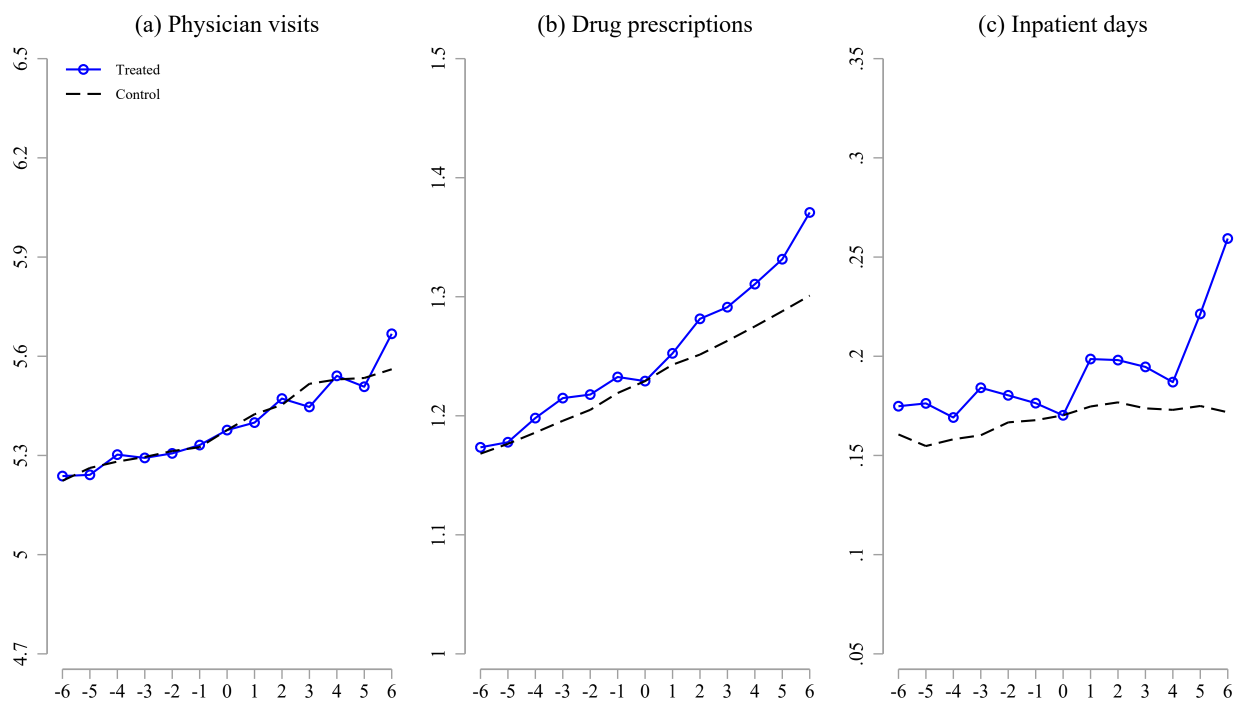
Notes: This sketch shows how we define the treatment and control group for our baseline regressions. Importantly, the control group is chosen from the set of firms that have their MLs 7 to 10 quarters after the treatment group ML in $t = 0$. We then compare health status in both the treatment and control group between $t = -6$ and $t = 6$. Because we require the control group to be employed with the same firm already 13 quarters prior to their ML, we impose a similar tenure condition to the treatment group.

FIGURE A.2 — Worker survival in firms after MLs



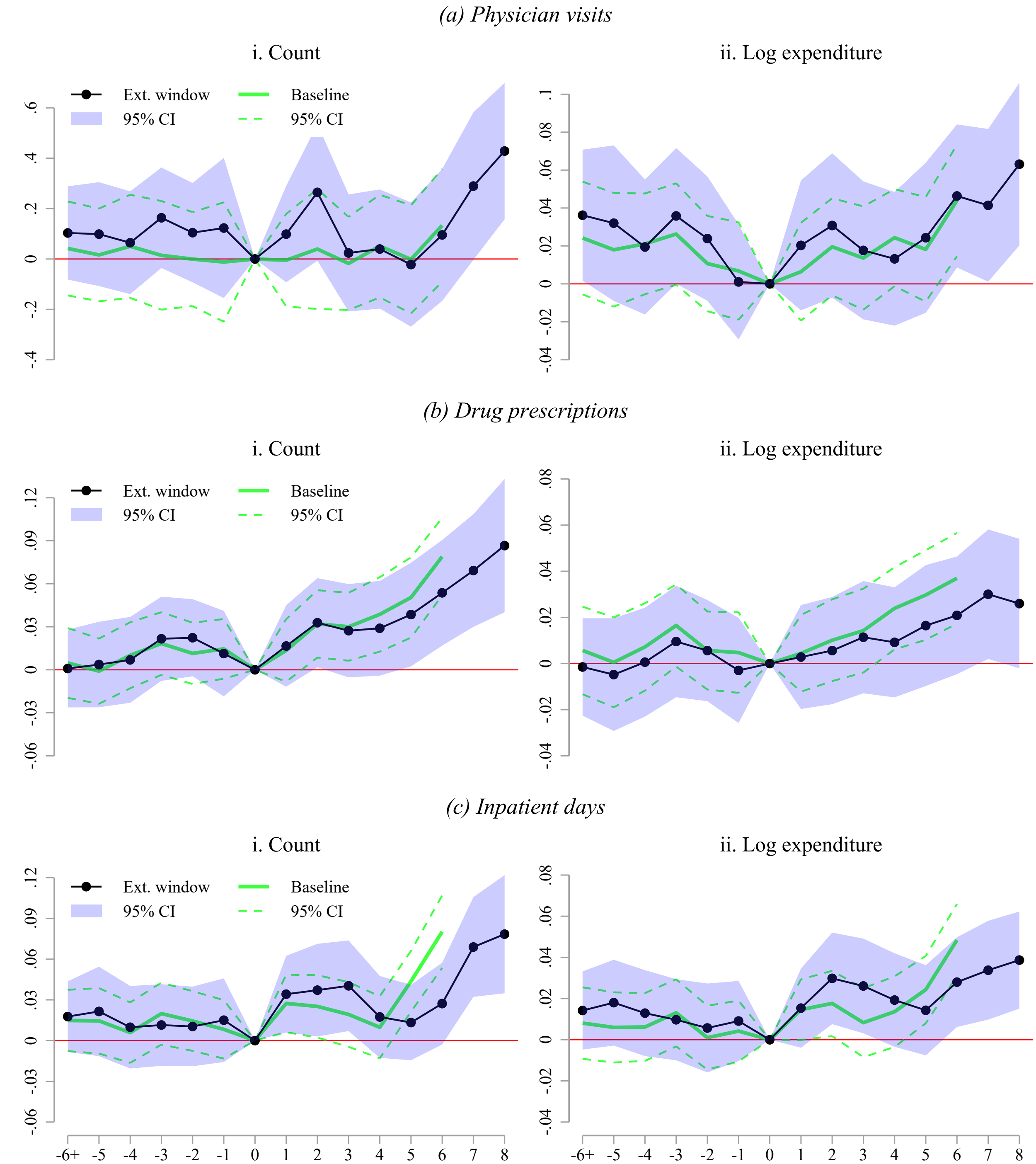
Notes: This figure provides Kaplan-Meier estimates for the shares of workers still employed with the same firm a certain number of years after the ML. By construction, no workers can leave within the first 6 quarters after the ML (see section III for a detailed discussion), this is indicated by the blue area. We restrict the sample to all treated workers observed in the quarter of their ML, meaning we do not match control workers for this exercise. N = 42,703.

FIGURE A.3 — Trends in raw health outcomes



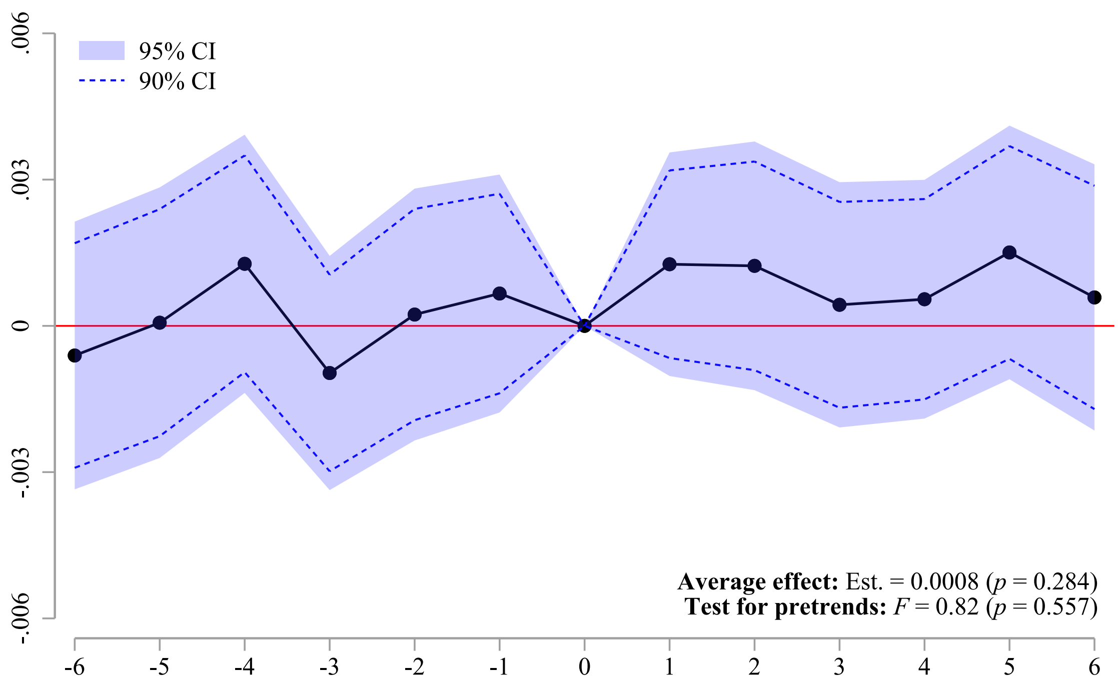
Notes: This figures plots raw data means for the number of physician visits, drug prescriptions, and inpatient days, relative to the ML quarter in both the treatment group and the control group. For the treatment group, $t = 0$ is the period of the actual ML, for the control group, $t = 0$ is when a 'placebo' shock occurs, with their actual shock occurring between $t = 7$ and $t = 10$. Similar to Fadlon & Nielsen (2019), we normalize the level of the control group's outcome to the treatment group's level at $t = 0$.

Figure A.4 — Main DD regressions with extended sample window



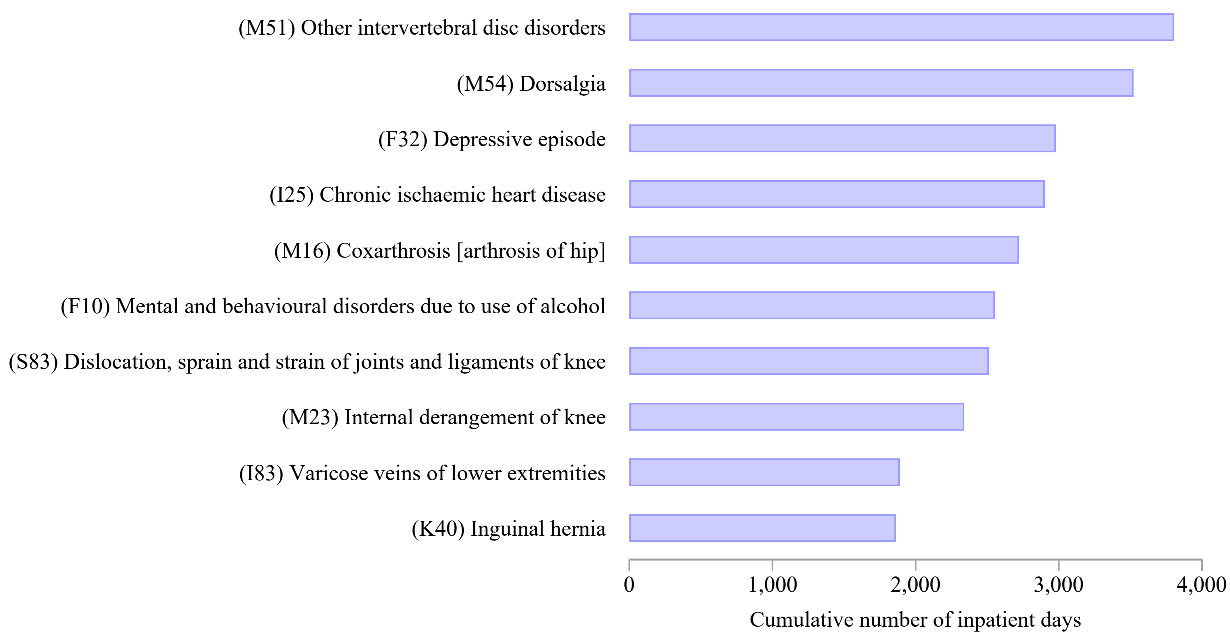
Notes: These graphs depict dynamic DD estimates for effects of surviving a ML on health, 8 quarters before until 8 quarters after the mass layoff. To plot the event graphs, we bin the pre-treatment quarters $t = -8, -7, -6$ into a single $t = -6$ endpoint. These estimates are based on a different sample compared to the baseline—which we plot for reference as well—because adding the additional two quarters requires drawing a new treatment and control group. We explain this in detail in section IV; must importantly, we require that workers be employed continuously for 25 instead of 20 quarters. Scatters represent point estimates. The blue-shaded area is a 95% confidence band for the extended sample, based on firm-level clustered standard errors. The lime line represents the baseline estimates from Figure 1. In each regression we control for worker age, tenure, and a set of worker × ML fixed effects.

FIGURE A.5 — Effects on accidents and injuries



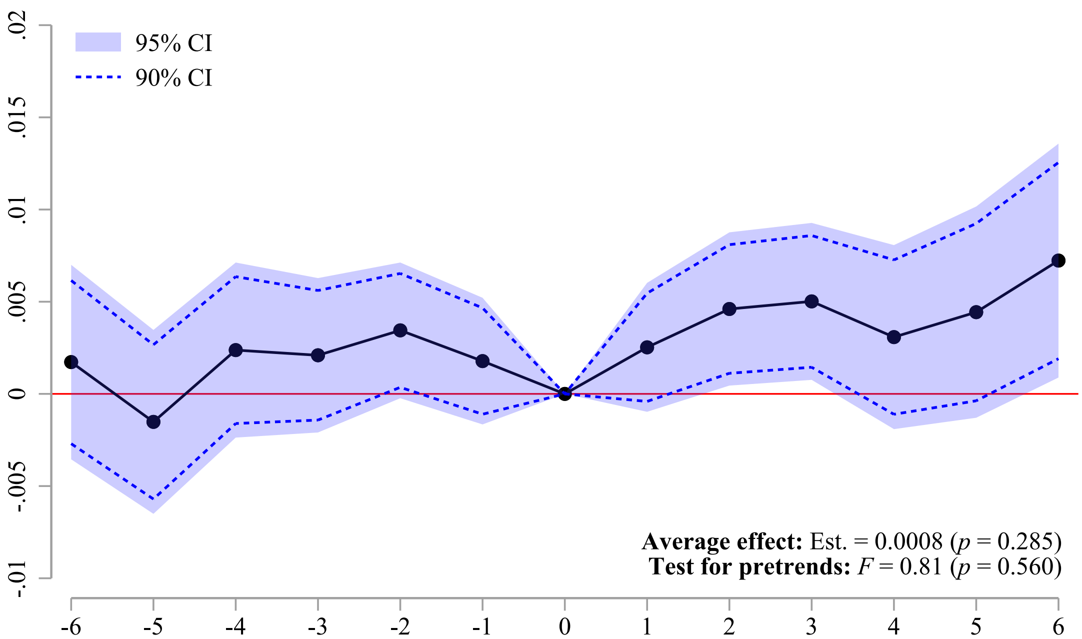
Notes: These graphs depict dynamic DD estimates for effects of surviving a ML on accidents and injuries, 6 quarters before until 6 quarters after the mass layoff. Scatters represent point estimates. The blue-shaded area is a 95% confidence band, the dashed line a 90% confidence band, both based on firm-level clustered standard errors. In each regression we control for worker age, tenure, and a set of worker × ML fixed effects.

FIGURE A.6 — Most common diagnoses among survivors by cumulative inpatient days



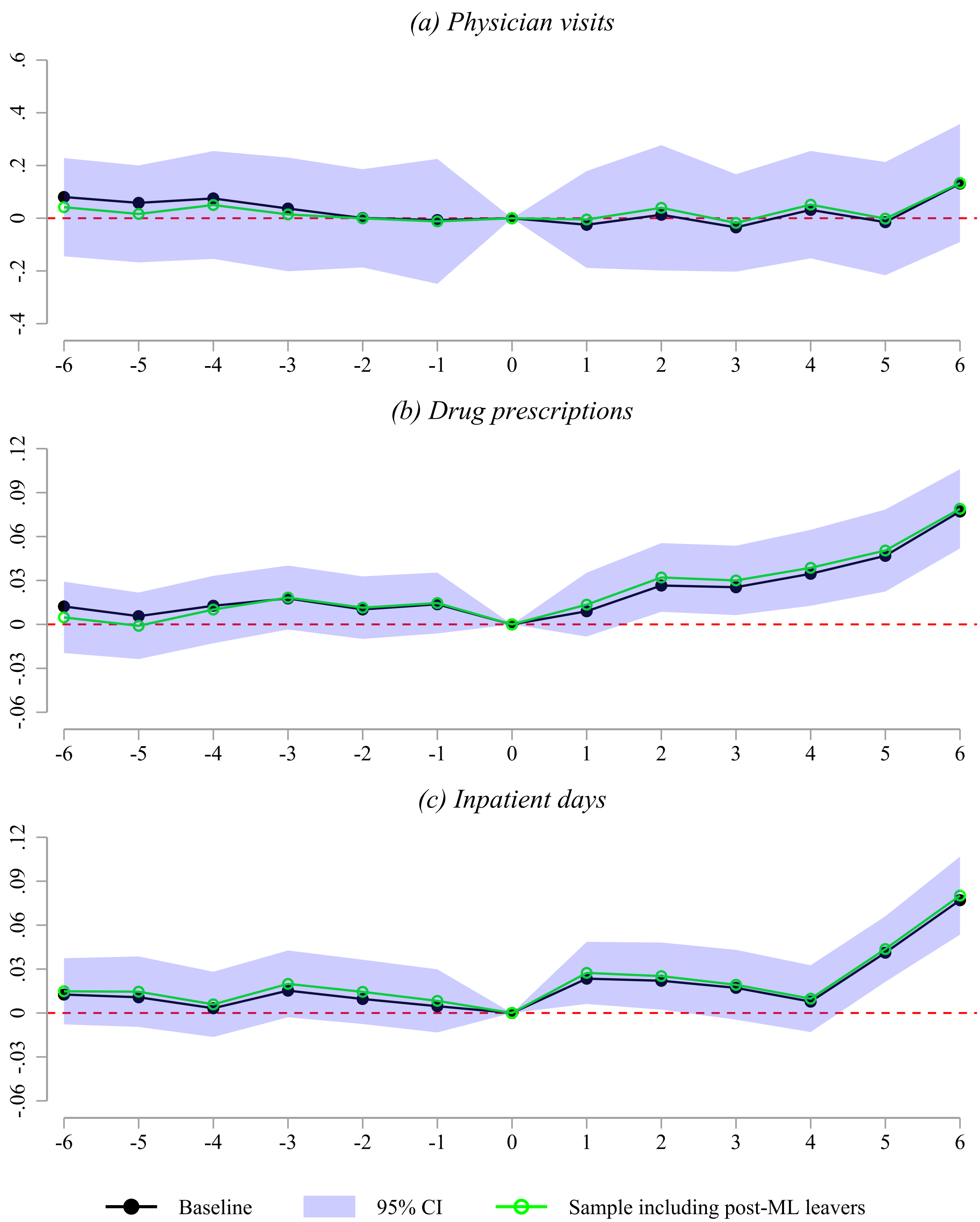
Notes: This figure summarizes the 10 three-digit ICD-10 codes with the largest cumulative number of inpatient days for ML survivors over our sample period (1998–2014).

FIGURE A.7 — Effect on log antidepressant expenditure



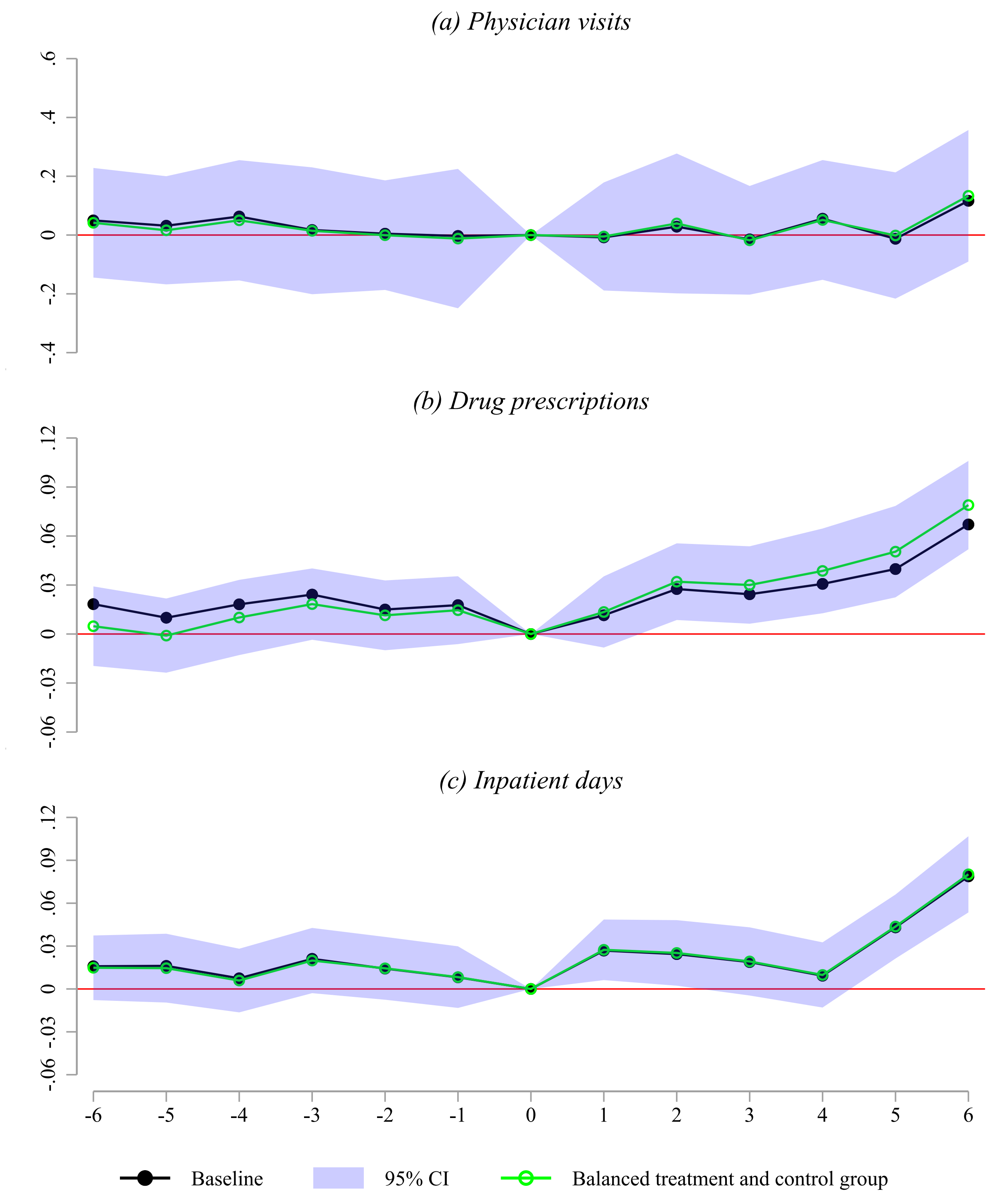
Notes: These graphs depict dynamic DD estimates for effects of surviving a ML on log antidepressant expenditure, 6 quarters before until 6 quarters after the ML. Scatters represent estimates which can be interpreted as the change in log expenditure compared to the quarter of the ML between present and future survivors. The blue-shaded area represents a 90% confidence band, the dashed line represents a 95% confidence band.

FIGURE A.8 — DD regressions on sample including post-ML leavers



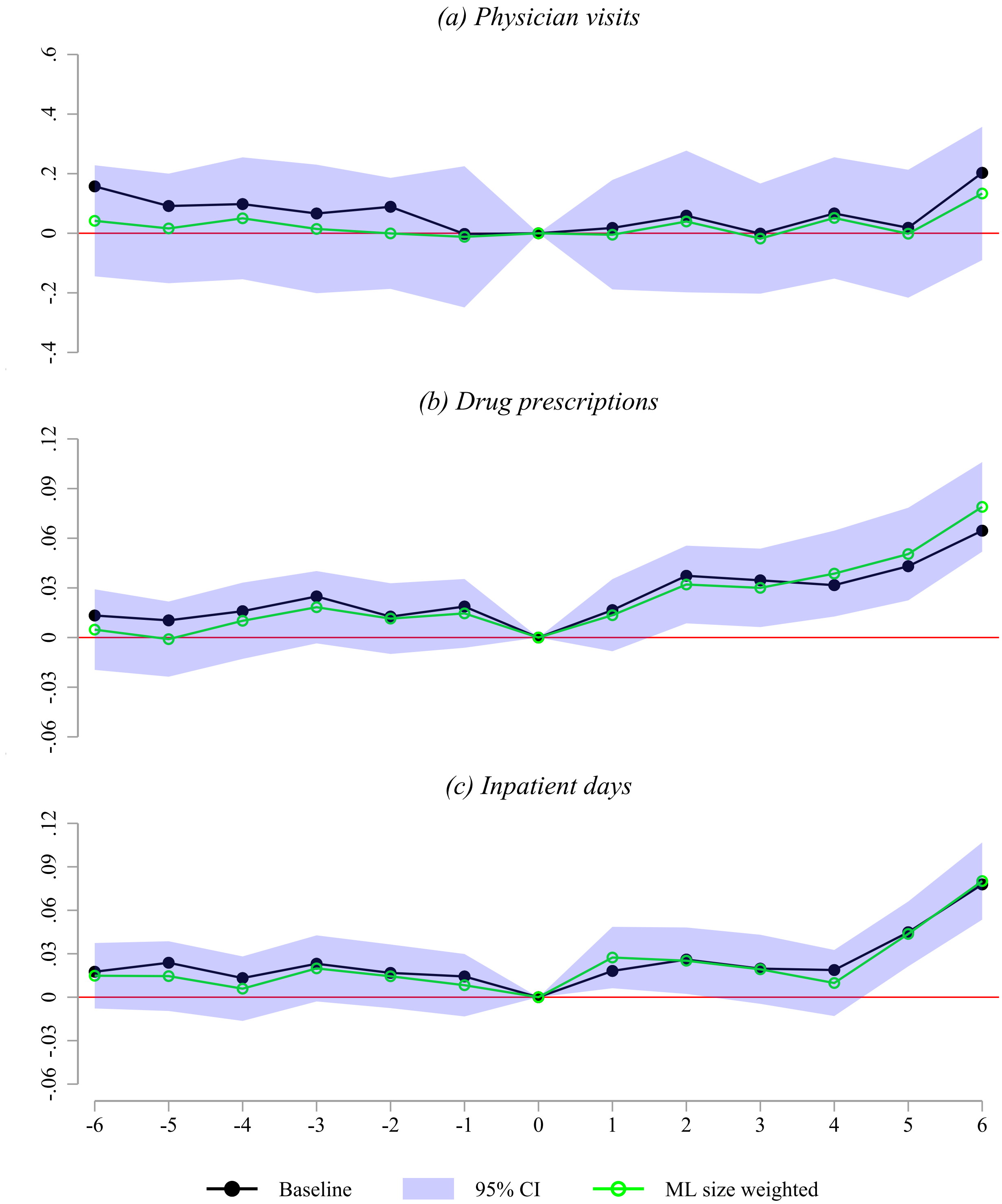
Notes: These graphs depict dynamic DD estimates for effects of surviving a ML on health, 6 quarters before until 6 quarters after the mass layoff. The sample is extended with 8,005 workers leaving the firm between $t = 1$ and $t = 6$. In each regression we control for worker age, tenure, and a set of worker × ML fixed effects.

Figure A.9 — DD regressions with balanced treatment and control groups



Notes: These graphs depict dynamic DD estimates for effects of surviving a ML on health, 6 quarters before until 6 quarters after the mass layoff. In each regression we control for worker age, tenure, and a set of worker × ML fixed effects.

Figure A.10 — DD regressions weighted by relative ML size



Notes: These graphs depict dynamic DD estimates for effects of surviving a ML on health, 6 quarters before until 6 quarters after the mass layoff. In each regression we control for worker age, tenure, and a set of worker × ML fixed effects.