*The hidden cost of mass layoffs — Do workers react with absenteeism when coworkers are fired?**

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Abstract

We evaluate the effect of surviving a mass layoff (ML) on shirking behavior and health using high-quality administrative data. Both outcome dimensions are highly correlated with productivity, thus they give us insights into the extent firm performance suffers from MLs. Comparing survivors with other workers in firms unaffected by MLs, we perform difference-in-differences analyses to track the full adjustment path of various shirking measures sixteen quarters around the ML. To account for the endogeneity of MLs, we use radius matching to find firms for the control group that are highly similar in terms of observable characteristics to our treated firms. Using the universe of mass layoffs between 1996 and 2014, we do not find any changes in shirking behavior due to surviving a mass layoff. In line with the existing literature, we also do not find any effects on workers' health status. Because ML survivors are generally positively selected, we additionally provide results from a robustness check where the control group is comprised of workers experiencing a ML themselves in the near future. This essentially allows us to compare survivors with future survivors, yet we still find no statistically or economically significant effects on shirking.

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I. Introduction

Downsizing is a popular strategy of firms to reduce labor cost in times of crises. Personnel economists typically prefer this strategy in lieu of a general wage cut (e.g., Lazear and Gibbs 2014),[1] because a restricted layoff is supposed to cause less turbulence towards the remaining workforce. Psychological and management studies do show, however, that survivors of downsizing firms perform worse in terms of work ethic, turnover intention, and organizational commitment (Datta et al. 2010). Recent experimental studies seem to confirm this view; they report 43% (Drzensky and Heinz 2016) or 12% (Heinz et al. 2017) lower productivity of workers whose coworkers had just been fired. These are massive effects, which would make the continuation of such a firm essentially impossible after downsizing.

Since all these studies are either based on very small samples and/or a very short observational period after the mass layoff (ML), we perform a related analysis using administrative data from the universe of surviving workers in all MLs in Austria between 1994 and 2015. In a first step, we use statistical matching procedures to find comparable firms with and without MLs. As productivity is not directly observable in administrative data, we resort to an easily measurable — but important — dimension of work effort: working at all. We focus on absenteeism in general, but provide also a more detailed analysis of absence days due to less verifiable reasons, such as common cold or back pains. In addition, absenteeism due to shirking is likely to be more common on days following or leading to a weekend, or on bridge days (days which are in-between a public holiday and a weekend). Finally, we also check effects on standard health measures (such as doctor's visits or use of psychotropic medication) in order to distinguish shirking from true health conditions. Our difference-in-difference (DiD) results show that downsizing does not lead to increased shirking, absenteeism, or lower health status. Nevertheless, it is still possible that there could be an effect on productivity: Workers could reduce their work effort, work pace or concentration after a downsizing episode.

A ML may have conflicting effects on the productivity of the surviving workforce. Organizational theory claims that fairness concerns may cause a "survivor syndrome" (Van Dierendonck and Jacobs 2012): Motivation of ML survivors may decrease as the implicit contract between the firm and the worker is broken. This is precisely the theory which is tested in most experimental studies. In a real firm, another factor may be important as well: fear of future job losses. Since the firm is revealed to be in trouble economically, the remaining workers may fear that they might be laid-off as well in the future. While these workers may be angry about the downsizing, the fear of a future job loss may lead them to work harder,[2] or at least work harder in dimensions which are observable to the management.

Most of the related literature on MLs and plant closures so far has focused on dismissed workers instead of survivors. Studies generally find negative effects of MLs on earnings and

[1] See Kube et al. (2013) for a productivity study on wage cuts.

[2] This is a variant of the shirking efficiency wage argument (Shapiro and Stiglitz 1984).

future employment (Stevens 1997, Huttunen et al. 2011, Ichino et al. 2016), health and mortality (Sullivan and Von Wachter 2009, Browning and Heinesen 2012), as well as fertility of career-oriented women (Del Bono et al. 2012). The management and experimental economics literature primarily considers market-based outcomes of the downsizing company, like market capitalization or profits, as well as individual outcomes of the remaining workers after a downsizing episode. They tend to find lower effort, performance, and commitment (Travaglione and Cross 2006, Goesaert et al. 2015, van Dick et al. 2016) as well as increased turnover intentions (Travaglione and Cross 2006, Allen et al. 2001) of downsizing survivors.[3] Other empirical studies find no or even positive effects of downsizing on the productivity of workers (Østhus and Mastekaasa 2010, Østhus 2012, Sigursteinsdóttir and Rafnsdóttirr 2015). Many of these studies rely on surveys before and after downsizing periods, or to case studies with relatively small sample sizes, and are therefore not generalizable. Closest to our study is Travaglione and Cross (2006), who find a 36% increase of absenteeism after a ML. Due to their use of a survey design, sample selection issues cannot be ruled out.

Our empirical design allows us to consider both short-run and long-run effects, thus we are able to distinguish the mechanisms discussed above (survivor syndrome or threat of further dismissal), which might go in different directions. This explains why we do not find these strong negative effects on productivity as in the experimental literature: Drzensky and Heinz (2016), for example, look at two 10 minutes experimental interactions between worker and firm in their laboratory experiment, while Heinz et al. (2017) look at two 3.5 hour interactions in their field experiment. After these interactions, the "firm" is officially closed. Due to this short period, immediate anger or frustration about inequity effects may be overwhelming to the agents, thus producing strong survivor effects. As we will look in detail at eight consecutive quarters after the downsizing episode, the importance of instantaneous reactions to the layoff will be reduced and long-term effects will prevail.

II. Empirical Strategy

In this section we discuss our empirical strategy. We describe the data in section II.1, which contains a detailed discussion on the definition of MLs as well as the sample we use as a control group in section II.1.1, and an overview on our outcome variables in section II.1.2. The matching procedure, the difference-in-differences strategy we apply to estimate treatment effects, and identification issues are discussed in section II.2.

II.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*

[3] Vahtera et al. (2004) find a five-times higher risk to suffer from cardiovascular death in the first six months after downsizing.

(UASF). The ASSD is structured as a linked employer-employee dataset, and covers the universe of Austrian workers from the 1970s onward. It contains detailed administrative records which are used to verify pension claims. We use the ASSD to obtain information on MLs and employment histories, wages, and certain demographics for affected workers.[4] The UASF database comprises individual-level information on health care service utilization in both the inpatient and outpatient sector for members of the sickness fund. The UASF covers around one million members representing roughly 75% of the population in Upper Austria, one of the nine Austrian provinces. Except for workers in the railway and mining industries, all employed individuals in Upper Austria are insured with the UASF. Farmers, self-employed persons, and civil servants are insured with other institutions. We extract information on sick leaves, diagnoses, and drug expenditures from these data. Finally, we augment our main data with industry-level import and export penetration data drawing from the *United Nations Comtrade Database*.[5]

II.1.1. Mass layoffs

For our empirical analysis we consider the universe of all MLs between 1996 and 2014 in Upper Austria. MLs are defined according to the system of advance layoff reporting mandated by the *Austrian Unemployment Office*. Employers planning to layoff a large number of workers have to give advance notice to the unemployment office, if the number of layoffs exceeds a certain threshold. Depending on the firm size, different thresholds apply for mass layoffs to be present (see Table 1). For firms with less than 100 employees in total, a ML is considered as such only if at least five employees are laid-off. For medium-sized firms with less than 600 employees, a layoff of at least five percent of the work force is considered a ML. In big firms, every layoff which involves at least 30 workers at a given time is considered a ML.

In order 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 time series measuring the number of employees in each plant. Drops in firm sizes between two quarters are considered as MLs whenever they comply with the thresholds discussed above. Events in which a large group of employees moves to the same plant identifier are excluded (this indicates a change in plant identifiers, not a true ML). Similar to other papers in the literature (e.g., Ichino et al. 2016), we drop seasonal industries such as the farming, construction, mining, and hospitality industry from the sample. Civil servants are excluded from the sample as well.

II.1.2. Outcome variables

We are particularly interested in the extent to which workers respond to colleagues being dismissed with changes in their shirking behavior. Shirking is fundamentally difficult to measure in administrative data, thus we have to construct proxies which are likely to correlate highly

[4] The limitations of the ASSD data are top-coded wages and the lack of information on (contracted) working hours.

[5] These data are available at `https://comtrade.un.org/`.

with work absence despite being healthy. First, we draw information on *doctor's visits* from the UASF data. This outcome is not only interesting by itself (because it also captures actual health problems induced by the coworkers being fired), but it is also a necessary condition for shirking and thus important to analyze. Second, we consider *health spa stays.* Austria is one of the few countries with a long tradition in health spas, which offer stays of around 20 days involving both rehabilitative and preventive measures. The purpose of spa stays is to prolong the duration patients can participate in the labor market and to reduce costs of retirement and care for the elderly.[6] Spa stays can be applied for twice every five years and are financed by the health insurance with minor co-payments (Hofmarcher and Quentin 2013). They are relatively common in Austria: In 2012, for example, 108,032 stays were recorded for the total population.[7] A spa stay typically lasts around 20 days, and is legally similar to a sick leave (with full wage reimbursement). Anecdotally, a spa stay is often regarded as shirking.

Third, we consider psychotropic drug expenditures, which help us to distinguish shirking from actual mental health conditions. Fourth, we look at effects on various sick leave measures. We distinguish between overall days of sick leave, days of sick leave which include mondays (irrespective of their length), sick leaves which include bridge days, and the aggregate number of sick leaves which last only one day and fall on a monday, a friday, or on a bridgeday.[8] Especially those sick leaves which are taken at the beginning or end of the week clearly correlate highly with shirking behavior.

II.2. Empirical setting, estimation, and identification

In our empirical analysis we aim to estimate the effect of surviving a ML on individual shirking behavior. This presents us with several challenges: First, firms do not enter MLs exogenously. In order to establish causal effects, we therefore require a control group comprised of firms which are highly similar in characteristics to the treated firms, namely those that experienced MLs. Second, ML survivors may be positively selected, and simply comparing them with non-survivors potentially poses another endogeneity problem if we fail to account for the inherent

[6]There is an exhaustive list of medical conditions a patient must fulfill in order to be eligible for a spa stay, which covers relatively mild conditions ranging from muscosceletal diseases such as back pain, metabolic disorders, respiratory problems, skin diseases such as exczemas, diseases of the vascular system, and lately also mental conditions such as burnouts.

[7]This figure is drawn from an official report issued by the *Hauptverband der österreichischen Sozialversicherungsträger,* which is the umbrella organization for the 22 Austria health insurance institutions. See https://www.parlament.gv.at/PAKT/VHG/XXV/AB/AB_06224/imfname_480546.pdf (accessed January 22, 2018).

[8]A restriction in the data is that we observe sick leaves which are shorter than four days only for some firms in the data. Workers are legally not obliged to produce certificates for absences of less than four days, unless the firm explicitly requires it. According to Ahammer (forthcoming), approximately 20% of all sick leaves in Austria fall into this category, and around 62% firms have required a certificate for short sick leaves at least once between 2005 and 2012. These firms are on average older, bigger, pay higher wages, and have a slightly lower intra-firm wage inequality. Industry distributions are highly similar, but firms that do not require certificates for short leaves are more often in the construction sector. This may induce measurement error in our estimations, but should not affect consistency of our point estimates.

differences between the two groups.

Let (ij) be worker-firm pairs with $i = 1, \dots, N$ denoting workers and $j = 1, \dots, J$ denoting firms. Furthermore, define the set of surviving workers in firm j, $S_j \subset N_j$, $s_j = 1, \dots, S_j$. Ideally, we want to estimate the following DiD model which captures the effect of surviving a ML on the outcome y_{ijq} in quarter $q = 1, \dots, Q$;

$$y_{ijq} = \alpha_1 t_{ijq} + \alpha_2 p_{jq} + \varphi \cdot \left[t_{ijq} \times p_{jq} \right] + \mathbf{x}_{ijq}\gamma' + u_{ijq}, \tag{1}$$

where the treatment indicator $t_{ijq} = \mathbf{1}\{i \in S_j\}$ is equal to one if individual i survived a mass layoff in firm j in quarter q, and zero else, p_{jq} is a binary variable equal to unity in the period after the mass layoff in firm j, and zero else, $\mathbf{x}$ is a vector of control variables comprising firm and individual-level information as well as industry fixed effects, and u_{ijt} is a stochastic disturbance term. The model in (1) can easily be extended to account for treatment group-specific trends of polynomial P,

$$y_{ijq} = \alpha_1 t_{ijq} + \alpha_2 p_{jq} + \varphi \cdot \left[t_{ijq} \times p_{jq} \right] + \mathbf{x}_{ijq}\gamma' + \sum_{p=1}^{P} \left[\mathbf{1}\{i \in S_j\} \times \tau^p \right] + u_{ijq}, \tag{2}$$

where τ is a linear time trend. Furthermore, we present also results for which we allow for up to 8 leads and lags of the treatment effects in (2) in order to trace out the full adjustment path of the respective outcome before and after a ML. Inference across specifications is based on industry-level clustered and robust standard errors in order to account for serial correlation.

Crucial for our coefficient of interest φ to have a causal interpretation is the selection of a proper control group (i.e., the sample of employees which have $t_{ijq} = 0$). Although the literature typically considers MLs as exogenous shocks on employees, firms who experience MLs are not randomly selected. We therefore use radius matching to find a sample of control firms which are similar in characteristics to our treated. Under a conditional independence assumption — which implies that we can control for all factors which determine a firm's selection into ML — any differences in behavioral responses due to surviving a ML cannot be caused by differences in firm characteristics.

The second assumption which is necessary for our DiD model to be identified is the so-called parallel trends assumption. The parameter φ has a causal interpretation only if all outcome variables y_{ijq} follow common trends before MLs. We test this assumption in Figure 1, where we provide estimates of φ for 8 lags before a ML, conditional on a full set of control variables, industry-level fixed effects, and treatment group-specific trends. For almost all of the outcomes we find significant lags before MLs. The probability of using psychotropic drugs, however, is slightly smaller in the treatment group 2 quarters before the ML, these results should therefore be interpreted with caution.

For our main results, we use the full population of firms in the data to match a control group for our ML firms. The only restriction is that the control firm must not had suffered a ML itself 8 quarters after the mass layoff of the treated firm. We adapt the radius matching protocol of Huber et al. (2013, 2015), which we describe in more detail in appendix A.1. Because it is possible to compare survivors with non-survivors using this framework, we also apply another matching strategy where we match only out of the pool of firms that experienced a ML themselves *in the future*. This allows us to compare our survivors with (1) workers from other mass layoff firms, and (2) other — future — survivors in these firms. Our results are robust to these two other specifications of the control group.

III. Results

Our main results are presented in Table 2. For each outcome we provide four different model specifications. Model (1) is the basic model without any control variables. Model (2) includes individual and firm level control variables as well as industry-level fixed effects. Models (3) and (4) additionally include a linear time trend and quarter-specific cohort trends, respectively. In the last column we provide the sample mean along with its standard deviation in parentheses for each outcome. Coefficients can be interpreted as average treatment effects on the treated of surviving a ML.

We find very small and statistically insignificant results across almost all specifications, suggesting that surviving a ML has no effects on shirking and health *per se.* There is, however, a small negative effect on single-day sick leaves which fall on Mondays, Fridays, or bridge days; which is significant at the 5% level in all model specifications. This indicates that survivors reduce the amount of sick leaves on days which are close to weekends and holidays after they experience a ML. The results are robust across all four specifications. Although statistically significant, the size of this effect is extremely small. We find that surviving a ML leads to a reduction of 0.001 single-day sick leaves on Mondays, Fridays, or bridgedays; which amounts to only about 0.01 standard deviations.

Apart from checking pre-treatment trends, we can use Figure 1 also to study the full adjustment path of each outcome after a ML. Indeed it is possible that workers react differently immediately after the ML and in the long-run, thus aggregating the entire post-treatment period in one coefficient (as in Table 2) may not tell the full story. The coefficients plotted in Figure 1 use the same set of controls as column (4) discussed above. Across outcomes and time, we continue to find no significant results of MLs.

III.1. Robustness

As outlined above, we use workers from firms who experience a ML in the future as an additional control group in order to account for unobserved heterogeneity among survivors. We replicate the estimations from Table 2 with different control groups in Tables 3 and 4. In Table 3, the control group is comprised of all workers in firms who experienced a ML in the future. In Table 4, we further restrict the control group to workers in these firms who survived the future

ML. The results in Table 3 are remarkably similar to our baseline. For single-day sick leaves we do find a significant negative effect, which vanishes once we add our control variables. The coefficient on health spa stays, however, is now significantly positive across specifications, albeit of small economic magnitude. We find a 0.001 increase in spa stays, which amounts to 0.02 standard deviations. In Table 4 we now compare survivors only with future survivors. Using this control group, we find robust positive coefficients on health spa stays and the probability of psychiatric drug use. However, both amount only to an increase in 0.02 standard deviations compared to their sample means, which are economically negligible effects. For single-day sick leaves, the negative coefficient from before is significant in Table 4, but not in Table 3. Since magnitudes are again very small, these effects should not be overinterpreted.

III.2. Heterogeneous effects

So far, the presented results are aggregated over all individuals. In order to refine our findings we split the sample by observable characteristics available. Figure 2 depicts the estimated coefficients of our DiD estimator for all outcomes using our standard control group. Each sample is split into blue and white collar workers, union membership, gender, and migration status. The results clearly support the findings of the pooled sample, since all coefficients are insignificant. At most, there is some positive significance for natives for the use of psychiatric drugs. Notably, also single day sick leaves on Mondays, Fridays, and bridge days inherit no significant effects in the split samples.

Extending our analysis of heterogeneous effects, we define new samples for each outcome variable by drawing a line at the mean of the previous outcome intensity. More precisely, we identify two samples for each outcome by calculating the mean previous to the mass layoff and rerun our estimations for individuals above and below the mean. The idea is to separate individuals into groups which might be predetermined for shirking and non-shirking. Since we cannot clearly identify shirking, we proxy it by previous intensity of our observable outcomes at hand. One possible interpretation, for example, would be that those individuals which are on single day sick leaves on Mondays, Fridays, or bridge days over-average, are more probable of being shirkers beforehand. One can think of similar interpretations for the other outcomes. Table 5 reports the estimated coefficients split by previous outcome intensity. The estimated coefficients uniformly suggest that those who had low previous outcome intensity increase the number of doctor visits, health spa stays, psychiatric drug use and expenditure, and sick leave days, while those with high previous outcome intensity decrease theirs. In case of causal effects the interpretation can be that those who were most probably shirkers previous to the mass layoff, are experiencing strong disciplinary effects after the mass layoff. They reduce absenteeism and psychiatric drug use. It is here to mention, that one has to be careful in interpreting these results as causal effects of the mass layoff, since it might well be possible that this is the result of a convergence to the mean. Since the number of observations for those previously above the mean are considerably smaller than for the ones previously below the mean, and the sample means of the high previous outcome intensity individuals are by far further apart from the pooled sample means, it is plausible that the ones above the mean were just having health problems before the mass layoff and are now returning to their usual state. This would also explain, why the negative effects for those above the mean are much larger. So there might be selection bias by just splitting at the mean.

IV. Discussion

Using a large data set of high-quality administrative data from Austria through the years 1996 to 2014, we show that survivors of mass layoffs do not fall into shirking behavior, nor show significant decrements in health status. Both of these outcomes are related to productivity measures of a firm, which are found to be decreasing after a mass layoff, according to the majority of the prevailing literature. Most importantly, we focus on long term rather than instantaneous effects and rely on observational data, which might explain differences. Using four different model specifications and three different control groups, we are able to show that our results are robust, since the few significant estimates are vanishingly small in magnitude. Additionally, we provide evidence for the homogeneity of our findings across multiple sub samples split by occupational collar, union membership, gender, and migration status.

Another aspect worth mentioning concerns the effects estimated by sample splits according to previous outcome intensity. The desired goal of this analysis is to distinguish between the shirking-prone individuals and all the others. A simple split by the mean before the ML – as we did in our analysis – is a simple approach to reach this goal, however, in order to better identify which individuals are more likely to be shirkers beforehand, we would need additional information. A more elaborate source could be psychological surveys, which are not available in our data.

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A. Appendix

A.1. Matching protocol

First, we perform nearest neighbor matching with replacement based on a logistic regression, where we use firm size, age, turnovers, tenure structures, wage, share of employees with respect to working class, education, age, migration on the firm-level and the share of market entries and exits, unemployment rates, and import and export growth on the industry-level as control variables. We also use lags for the entire pre-treatment period for all these variables to estimate propensity scores in order to increase reliability of our matches. Second, we truncate our matches. We exclude all matches for which the propensity scores in the group of control firms are larger than the 95$^{\text{th}}$ percentile of the propensity scores in the treatment group. Then, we define a radius as the 95$^{\text{th}}$ percentile of the propensity score differences between treated and control firms in order to perform radius matching. Within the defined radius we connect all control firms to corresponding treatment firms and construct firm-level weights. Where the firm-level weights of control firms are proportionally to the inverse of their distance to the respective treated firm they are matched to. In addition, the firm-level weights are normalized such that all control firm weights add up to one with respect to each treated firm. This sample represents the final firm level data. However, we merge individual employees to the firm level data to increase inference. To obtain results on firm level the firm level weights are normalized to one within each firm after the merge. We do not apply the bias adjustment of Huber et al. (2015) because we apply DiD after matching.

We obtain a final sample with very similar treated and control firms. In particular we compare propensity scores of being treated for firms with and without suffering a ML. It is crucial that the propensity scores of treated and control overlap such that the common support assumption is fulfilled. Figure 3 presents the propensity score overlap of our treated and control observations. The first column shows that the propensity score density of treated and control differ before matching as well as with simple nearest neighbour matching (NN). However, after truncating the matched NN sample, the propensity score densities become more alike. The fourth graph ”RM only” illustrates how radius matching improved the matching quality. The propensity score distributions of control and treated overlap very well after performing radius matching. This property is crucial for our identification.

In table 6 we show that radius matching improved the balance of observed firm characteristics. Treated and control firms differ significantly in most characteristics before matching. On the contrary, after matching most observed firm characteristics do not differ significantly between treated and control firms.

Our final t-test on the sample balance ensures our matching quality (see table 7). Each column of this table reports whether an observed variable significantly differs between treated and control with respect to each pre-treatment quarter. It is clear that treated and control firms do not differ significantly in almost any pre-treatment quarter after matching. All three analyses show that our matching protocol performs well on our sample.

A.2. Tables and Figures

Table 1 — The definition of mass layoffs by the *Austria Unemployment Office* (AMS).

Firm size	Mass layoff is given if . . .
20 < size < 100	at least five employees are laid-off
100 ≤ size ≤ 600	at least five percent of employees are laid-off
600 < size	at least 30 employees are laid-off

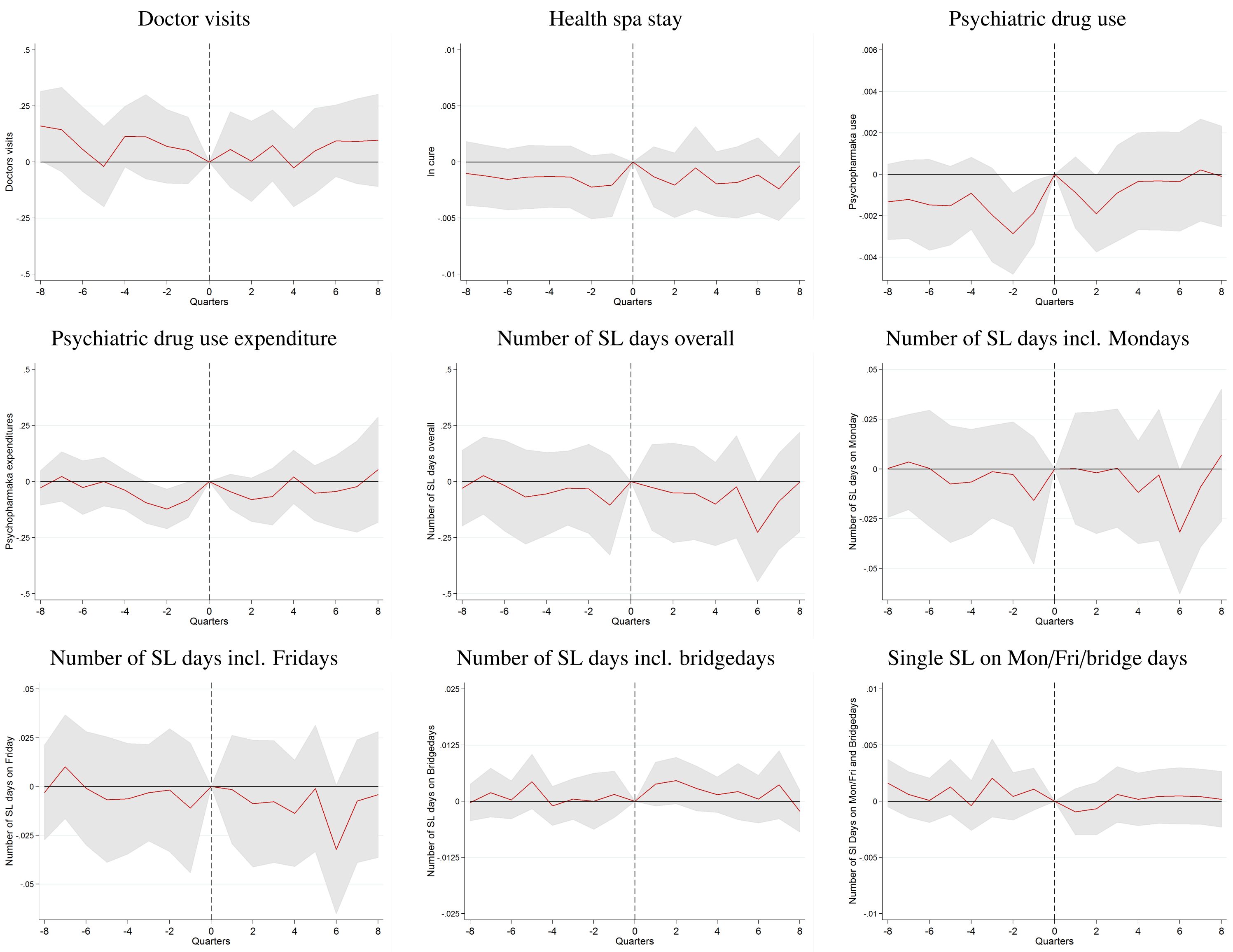
Notes: This table presents the official thresholds according to which mass layoffs are defined, as mandated by the *Austrian Unemployment Office*. Source: http://www.ams.at/service-unternehmen/fruehwarnsystem (in German).

TABLE 2 — DiD results

	(1)	(2)	(3)	(4)	Sample mean
Doctor visits	-0.041	-0.043	-0.051	-0.060	4.658
	(0.042)	(0.041)	(0.044)	(0.045)	(7.885)
Health spa stay	0.000	0.000	0.000	0.000	0.002
	(0.000)	(0.000)	(0.000)	(0.000)	(0.049)
Psychiatric drug use	0.001	0.001	0.001	0.001	0.015
	(0.001)	(0.001)	(0.001)	(0.001)	(0.121)
Psychiatric drug expenditures	0.039	0.044	0.006	0.015	0.581
	(0.039)	(0.051)	(0.046)	(0.063)	(7.299)
Number of SL days overall	0.003	-0.003	0.010	-0.004	2.170
	(0.036)	(0.036)	(0.038)	(0.043)	(6.355)
Number of SL days incl. Monday	0.003	0.002	0.003	0.001	0.292
	(0.005)	(0.005)	(0.005)	(0.006)	(0.903)
Number of SL days incl. Friday	-0.002	-0.003	-0.001	-0.003	0.328
	(0.005)	(0.006)	(0.006)	(0.007)	(0.955)
Number of SL incl. bridge days	0.001	0.001	0.001	0.002	0.025
	(0.001)	(0.001)	(0.001)	(0.001)	(0.179)
Single day SL on Mon/Fri and bridge days	-0.001**	-0.001**	-0.001**	-0.001**	0.007
	(0.000)	(0.001)	(0.001)	(0.001)	(0.088)
Individual level controls		✓	✓	✓	
Firm level controls		✓	✓	✓	
Industry level fixed effects		✓	✓	✓	
Linear time trend			✓	✓	
Quarter specific cohort trends				✓	
Observations			14,057,550		

Notes: In this table we present DiD estimates for the effect of surviving a ML on various outcomes. Each cell represents a separate regression on the outcome of the respective row. Model (1) is the basic model without any control variables. Model (2) includes individual and firm level controls, as well as industry level fixed effects. Models (3) and (4) include a linear time trend and quarter specific cohort trends, respectively. Doctor visits, health spa stays, and sick leaves are measured in days per quarter. Psychiatric drug use is reported as 1 if they were consumed during a quarter and 0 otherwise. Psychiatric drug expenditures are measured in Euro per quarter. Standard errors in parentheses are clustered on the industry level, $^{*}p < 0.10$,** $p < 0.05$,*** $p < 0.01$.

FIGURE 1 — Event plots



Note: Model specification (4), using control group 1. Shaded area: 95% confidence interval. Reference category is treatment period (0).

TABLE 3 — DiD results - Control group 2

	(1)	(2)	(3)	(4)	sample mean
Doctor visits	-0.080*	-0.057	-0.049	-0.065	4.686
	(0.048)	(0.057)	(0.049)	(0.067)	(7.990)
Health spa stay	0.001**	0.001**	0.001**	0.001**	0.002
	(0.000)	(0.000)	(0.000)	(0.001)	(0.050)
Psychiatric drug use	0.000	0.001	0.000	0.001	0.016
	(0.001)	(0.001)	(0.001)	(0.001)	(0.127)
Psychiatric drug expenditures	0.086*	0.071	0.062	0.081	0.646
	(0.044)	(0.088)	(0.091)	(0.092)	(6.974)
Number of SL days overall	-0.023	-0.027	-0.008	-0.036	2.245
	(0.037)	(0.061)	(0.053)	(0.061)	(6.354)
Number of SL days inkl. Monday	-0.001	-0.001	0.001	-0.003	0.300
	(0.005)	(0.009)	(0.008)	(0.009)	(0.901)
Number of SL days inkl. Friday	-0.006	-0.006	-0.003	-0.006	0.341
	(0.006)	(0.009)	(0.008)	(0.009)	(0.958)
Number of SL inkl. bridge days	-0.001	-0.001	-0.000	0.000	0.025
	(0.001)	(0.001)	(0.001)	(0.001)	(0.180)
Single day SL on Mon/Fri and	-0.001**	-0.001	-0.001	-0.001	0.007
bridge days	(0.001)	(0.001)	(0.001)	(0.001)	(0.088)
Individual level controls		✓	✓	✓	
Firm level controls		✓	✓	✓	
Industry level fixed effects		✓	✓	✓	
Linear time trend			✓	✓	
Quarter specific cohort trends				✓	
Observations			16,220,451		

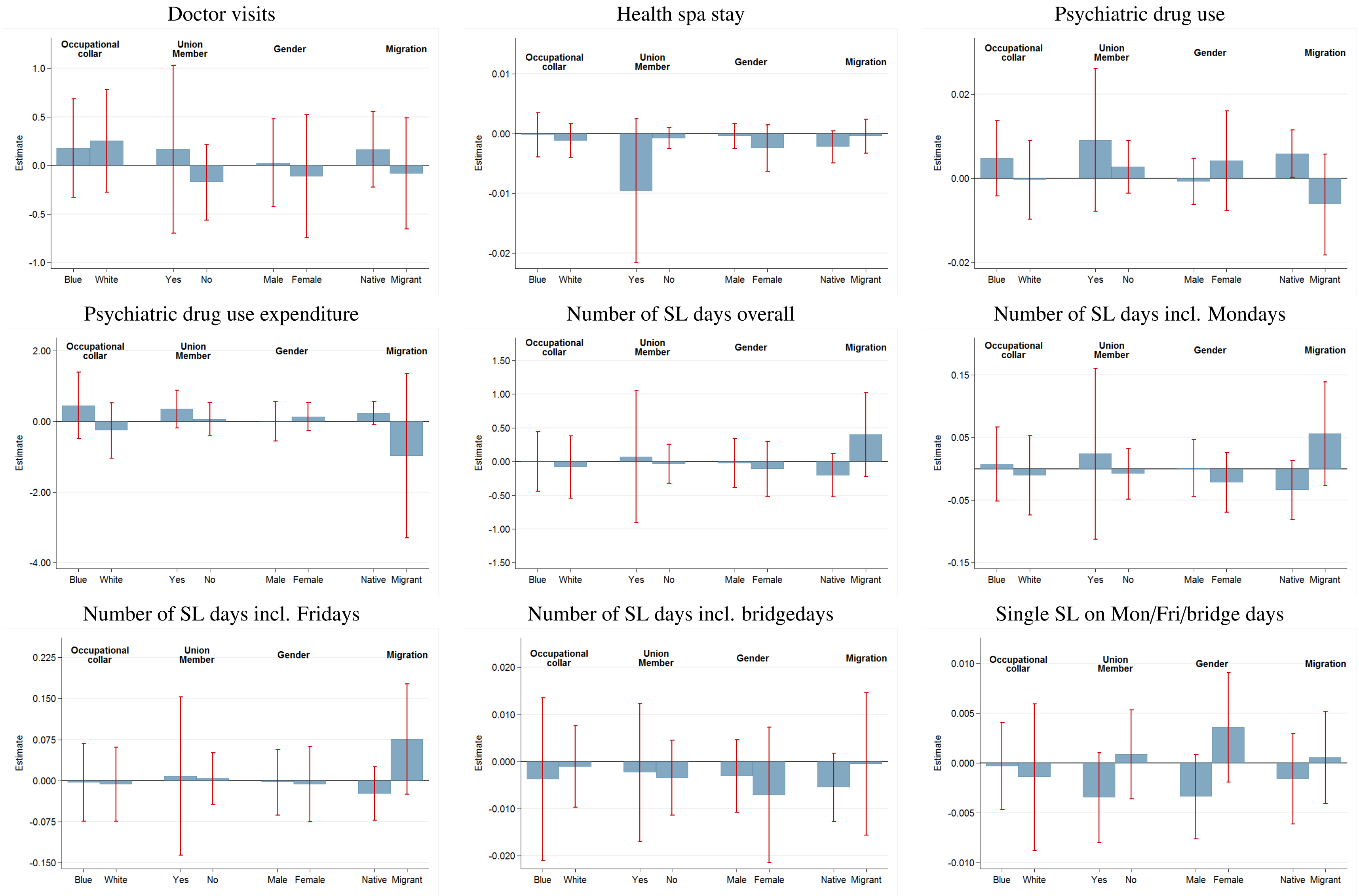
Note: Doctor visits, health spa stays, and sick leaves are measured in days per quarter. Psychiatric drug use is reported as 1 if they were consumed during a quarter and 0 otherwise. Psychiatric drug expenditures are measured in Euro per quarter. Standard errors in parentheses are clustered on industry level, , $^{*}p < 0.10,^{**}\ p < 0.05,^{***}\ p < 0.01$.

Table 4 — DiD results - Control group 3

	(1)	(2)	(3)	(4)	sample mean
Doctors visits	-0.035	-0.016	-0.000	-0.015	4.627
	(0.057)	(0.107)	(0.097)	(0.122)	(7.920)
Health spa stay	0.001***	0.001***	0.001**	0.001**	0.002
	(0.000)	(0.000)	(0.001)	(0.001)	(0.046)
Psychiatric drug use	0.002***	0.003**	0.003**	0.003**	0.015
	(0.001)	(0.001)	(0.001)	(0.001)	(0.122)
Psychiatric drug expenditures	0.162***	0.146	0.146	0.152	0.609
	(0.047)	(0.104)	(0.108)	(0.122)	(6.982)
Number of SL days overall	0.079*	0.077	0.090	0.040	2.135
	(0.042)	(0.088)	(0.080)	(0.090)	(6.177)
Number of SL days inkl. Monday	0.008	0.009	0.010	0.002	0.285
	(0.006)	(0.012)	(0.011)	(0.012)	(0.875)
Number of SL days inkl. Friday	0.011*	0.011	0.014	0.006	0.324
	(0.006)	(0.012)	(0.011)	(0.013)	(0.932)
Number of SL inkl. bridge days	0.002*	0.002	0.002	0.003	0.024
	(0.001)	(0.002)	(0.002)	(0.003)	(0.177)
Single day SL on Mon/Fri and	-0.003***	-0.003	-0.003*	-0.003*	0.007
bridge days	(0.001)	(0.002)	(0.002)	(0.001)	(0.087)
Individual level controls		✓	✓	✓	
Firm level controls		✓	✓	✓	
Industry level fixed effects		✓	✓	✓	
Linear time trend			✓	✓	
Quarter specific cohort trends				✓	
Observations			11,749,205		

Note: Doctor visits, health spa stays, and sick leaves are measured in days per quarter. Psychiatric drug use is reported as 1 if they were consumed during a quarter and 0 otherwise. Psychiatric drug expenditures are measured in Euro per quarter. Standard errors in parentheses are clustered on industry level, , $^{*}p < 0.10$,** $p < 0.05$,*** $p < 0.01$.

Figure 2 — Sample split by observable characteristics



Note: Estimated DiD coefficients for split samples, using control group 1. 95% confidence intervals (red).

Figure 3 — Propensity score density of treated and control

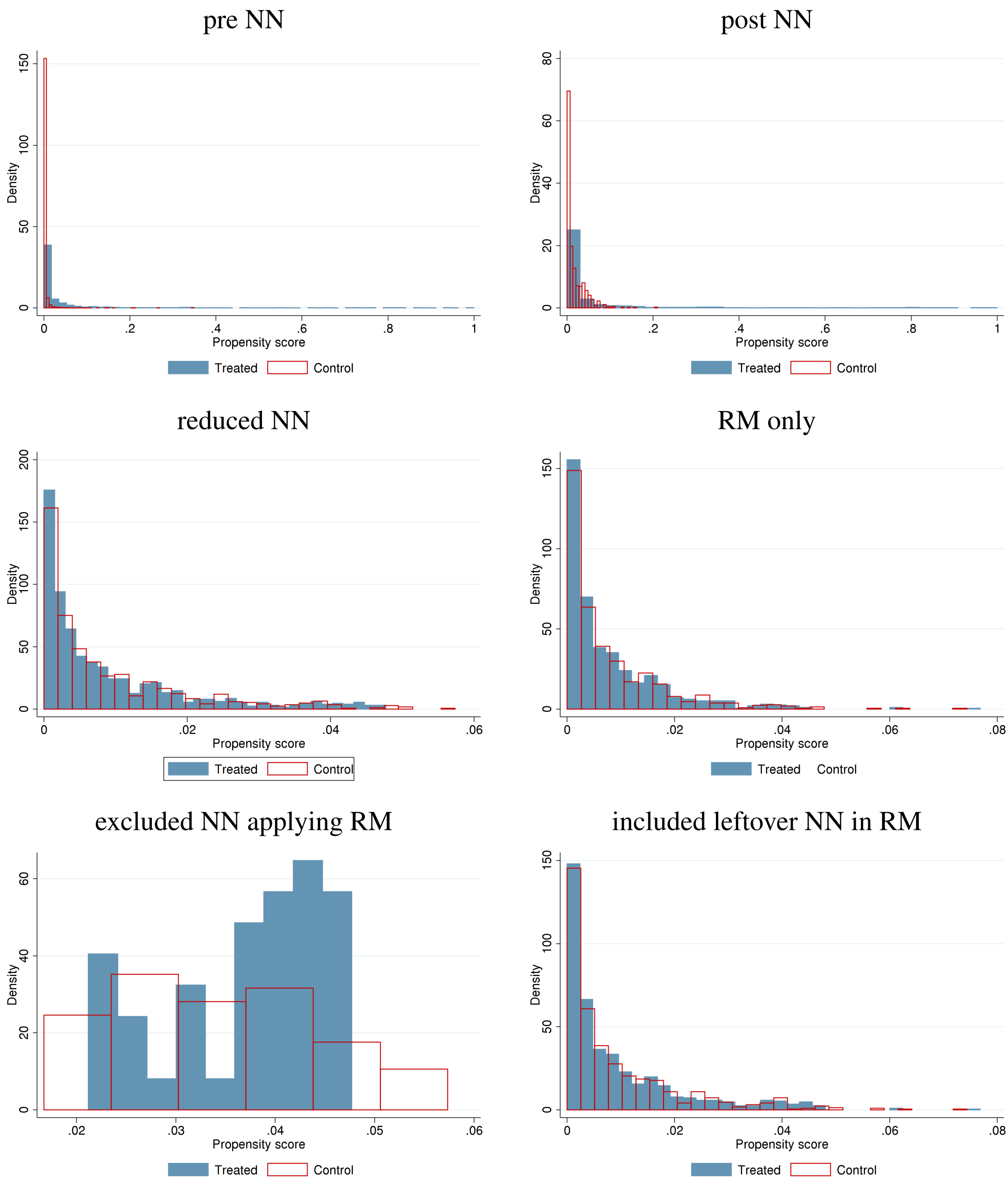


TABLE 5 — Control group 1: split by previous outcome intensity

	Low previous intensity		High previous intensity	
	Estimate	Sample Mean	Estimate	Sample mean
Doctor visits	0.622***	2.438	-0.801***	8.770
	(0.055)	(4.737)	(0.105)	(10.469)
	N=8,525,218		*N*=5,282,147	
Health spa stay	0.002***	0.001	-0.082***	0.047
	(0.000)	(0.028)	(0.005)	(0.211)
	N=12,652,835		*N*=556,049	
Psychiatric drug use	0.004***	0.002	-0.047***	0.252
	(0.000)	(0.040)	(0.009)	(0.434)
	N=12,640,953		*N*=874,029	
Psychiatric drug expenditures	0.131***	0.053	-1.943**	13.262
	(0.018)	(1.684)	(0.938)	(32.393)
	N=13,103,948		*N*=580,879	
Number of SL days overall	0.618***	0.994	-1.216***	4.773
	(0.048)	(3.711)	(0.111)	(9.464)
	N=8,967,205		*N*=4,737,954	
Number of SL days inkl. Monday	0.090***	0.132	-0.171***	0.652
	(0.007)	(0.537)	(0.015)	(1.348)
	N=8,988,291		*N*=4,713,384	
Number of SL days inkl. Friday	0.096***	0.144	-0.159***	0.704
	(0.007)	(0.562)	(0.016)	(1.388)
	N=8,928,842		*N*=4,850,859	
Number of SL inkl. bridge days	0.018***	0.007	-0.081***	0.082
	(0.001)	(0.094)	(0.005)	(0.322)
	N=9,534,655		*N*=3,962,517	
Single day SL on Mon/Fri and bridge days	0.004***	0.002	-0.093***	0.056
	(0.000)	(0.041)	(0.004)	(0.245)
	N=11,760,892		*N*=1,600,807	

Note: Doctor visits, health spa stays, and sick leaves are measured in days per quarter. Psychiatric drug use is reported as 1 if they were consumed during a quarter and 0 otherwise. Psychiatric drug expenditures are measured in Euro per quarter. Standard errors in parentheses are clustered on industry level, , $^{*}p < 0.10$,** $p < 0.05$,*** $p < 0.01$.

TABLE 6 — T-test over the pre treatment period before and after matching

	Unmatched sample					Matched sample				
Variable Names	T	C	T-C	t-val		T	C	T-C	t-val	
Firm characteristics										
Firm age	24.302	26.159	-1.856	13.800	***	25.616	25.663	-0.047	0.303	
Firm size	95.773	93.723	2.049	1.006		94.285	97.358	-3.073	1.618	
Firm wage structure										
Sd of female wage	21.868	22.948	-1.080	6.620	***	22.385	22.572	-0.187	1.031	
Sd of male wage	22.229	21.216	1.013	6.570	***	22.412	22.063	0.348	2.617	***
Average yearly wage	24579.191	26590.872	-2011.681	14.989	***	25523.104	25532.983	-9.879	0.066	
Sd of yearly wage	13807.584	14739.210	-931.626	6.537	***	14254.137	14259.618	-5.481	0.035	
Tenure structure										
Tenure < 1*year*	0.203	0.138	0.065	36.811	***	0.174	0.177	-0.003	1.692	**
Tenure < 5*years*	0.565	0.461	0.105	35.840	***	0.526	0.523	0.003	0.775	
Firm quarterly fluctuations (share)										
New male employment	0.047	0.035	0.012	1.023		0.033	0.034	-0.001	0.296	
New female employment	0.044	0.028	0.016	2.159	*	0.029	0.032	-0.003	2.020	*
New employment age < 25	0.025	0.020	0.005	2.936	***	0.021	0.022	-0.001	0.926	
New employment age < 50	0.057	0.039	0.018	1.342		0.038	0.040	-0.002	1.044	
New employment age 50+	0.010	0.004	0.005	1.248		0.004	0.004	-0.000	0.441	
Male lay-offs	0.032	0.020	0.012	21.141	***	0.027	0.027	0.000	0.463	
Female lay-offs	0.028	0.019	0.008	16.260	***	0.024	0.025	-0.001	2.040	*
Lay-offs age < 25	0.016	0.012	0.004	10.893	***	***4	***4	-0.001	1.347	
Lay-offs age < 50	0.037	0.023	0.014	26.289	***	0.031	0.032	-0.001	1.064	
Lay-offs age 50+	0.007	0.005	0.002	7.900	***	0.006	0.006	0.000	1.070	
Share of Employees by Education										
University degree	0.099	0.088	0.011	6.907	***	0.092	0.095	-0.003	1.479	
High school degree	0.326	0.369	-0.043	19.587	***	0.339	0.345	-0.005	2.325	*
Apprenticeship examination	0.421	0.421	0.000	0.005		0.426	0.420	0.006	2.260	*
Compulsory school	0.154	0.122	0.032	18.548	***	0.143	0.140	0.002	1.226	
Share of employees by sex and age										
Female	0.445	0.454	-0.009	2.303	*	0.443	0.447	-0.003	0.652	
Male average age	37.829	38.640	-0.811	10.126	***	38.162	38.257	-0.095	1.065	
Female average age	37.342	38.337	-0.995	13.432	***	37.653	37.828	-0.175	2.136	*
Male and age < 25	0.086	0.082	0.003	2.262	*	0.085	0.086	-0.001	0.474	
Female and age < 25	0.070	0.063	0.007	5.672	***	0.067	0.068	-0.001	1.026	
Male and age < 50	0.426	0.420	0.006	1.858	**	0.427	0.423	0.004	1.071	
Female and age < 50	0.351	0.364	-0.013	3.830	***	0.352	0.354	-0.002	0.445	
Male and age 50+	0.043	0.043	-0.000	0.679		0.044	0.044	-0.000	0.207	
Female and age 50+	0.024	0.027	-0.003	5.165	***	0.025	0.025	0.000	0.022	
Share of employees by working class										
Female blue collar	0.144	0.132	0.012	4.946	***	0.136	0.135	0.000	0.146	
Male blue collar	0.297	0.252	0.046	12.224	***	0.290	0.286	0.004	0.920	
Female white collar	0.248	0.269	-0.021	6.838	***	0.258	0.259	-0.001	0.334	
Male white collar	0.214	0.247	-0.033	11.420	***	0.223	0.222	0.001	0.362	
Share of employees by migration status										
Native / Migrants	21.795	23.855	-2.060	5.975	***	22.831	23.204	-0.372	0.911	
Female natives	0.399	0.414	-0.015	3.859	***	0.403	0.406	-0.003	0.601	
Male natives	0.506	0.530	-0.023	6.379	***	0.515	0.512	0.003	0.769	
Male migrants	0.062	0.036	0.026	22.053	***	0.055	0.055	-0.000	0.040	

Note: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 7 — Sample balance t-Test of treated and control with respect to each pre treatment quarter

			Quarters before ML							
Variable Names	T-C	p	1	2	3	4	5	6	7	8
Firm characteristics										
Firm age	-0.047	0.76								
Firm size	-3.073	0.11								
Firm wage structure										
Sd of female wage	-0.187	0.30								
Sd of male wage	0.348	0.01								
Average yearly wage	-9.879	0.95								
Sd of yearly wage	-5.481	0.97								
Tenure structure										
Tenure < 1*year*	-0.003	0.09								
Tenure < 5*year*	0.003	0.44								
Firm quarterly fluctuations (share)										
New male employment	-0.001	0.77								
New female employment	-0.003	0.04				*				
New employment age < 25	-0.001	0.35								
New employment age < 50	-0.002	0.30				*				
New employment age 50+	-0	0.66				**				
Male lay-offs	0	0.64								
Female lay-offs	-0.001	0.04								
Lay-offs age < 25	-0.001	0.18								
Lay-offs age < 50	-0.001	0.29								
Lay-offsage 50+	0	0.28								
Share of Employees by Education										
University degree	-0.003	0.14								
High school degree	-0.005	0.02								
Apprenticeship examination	0.006	0.02								
Compulsory school	0.002	0.22								
Male average age	-0.095	0.29								
Female average age	-0.175	0.03								
Share of employees by sex and age										
Female	-0.003	0.51								
Male age < 25	-0.001	0.64								
Female age < 25	-0.001	0.30								
Male age < 50	0.004	0.28								
Female age < 50	-0.002	0.66								
Male age 50+	-0	0.84								
Female age 50+	0	0.98								
Share of employees by working class										
Female blue collar	0	0.88								
Male blue collar	0.004	0.36								
Female white collar	-0.001	0.74								
Male white collar	0.001	0.72								
Share of employees by migration status										
Native / Migrants	-0.372	0.36								
Female natives	-0.003	0.55								
Male natives	0.003	0.44								
Male migrants	-0	0.97								

Note: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.