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Do Employees' Sickness Absences React to a Change in Costs for Firms? Evidence from a Natural Experiment*

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Abstract

We analyse the impact of a social security reform that changed the costs incurred by firms due to sickness absences. The reform abolished a compulsory insurance for firms, which insured them against the wages paid to sick blue-collar workers. During the first year after its introduction, we estimate that the reform resulted in about 6.3 percent fewer sickness absences, and in about 8.6 percent fewer absence days. We do not find evidence for changes in hiring or firing, and we find only limited workforce composition changes. We do not find spillover effects on the absences of white-collar workers. Robustness checks confirm these results.

Keywords: Absenteeism; moral hazard; sickness insurance

JEL classification: *I*38; *J*22

I. Introduction

Sickness absences lead to significant productivity losses, and consequently reduce incomes and profits (Allen, 1983; Coles and Treble, 1993; Brown and Sessions, 1996; Barham and Begum, 2005; Osterkamp and Röhn, 2007). Firms in most OECD countries are at least partially insured against the direct costs arising from their workers' sickness absences. In some countries, the amount or period of statutory sick pay is limited. For example,

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Norwegian firms need to pay workers their full wages during the first 16 days of a sickness absence, and social security pays the wages thereafter (Markussen *et al.*, 2012). In other countries, firms are refunded for the costs incurred when their workers are sick. For example, in Germany, firms that have fewer than 30 employees have part of their costs refunded by an insurance fund (Ziebarth and Karlsson, 2010). Depending on how such insurance is organized, firms might have an incentive to exert very little effort in monitoring or in preventing absences, which in turn might lead to more sickness absences.

Economic research has devoted considerable effort to determine if sick pay leads employees to "adapt their work-absence behavior" (Johansson and Palme, 2005, p. 1880), and to remain absent from work without actually being sick.[1] The growing body of literature on absenteeism has identified several other determinants of sickness absence.[2]

However, there is little evidence on how changes in the costs incurred by firms affect sickness absences. Westergaard-Nielsen and Pertold (2012) find that a voluntary insurance scheme for small companies in Denmark led to more short-term sickness absences, possibly from lower levels of monitoring. Fevang *et al*. (2014) analyse a Norwegian reform that exempted employers from refunding sick pay only for pregnancy-related absences. They show that this exemption led to approximately 5 percent more sickness absence spells of pregnant women.

We exploit a reform of the social security system in Austria to provide causal evidence for the effect of employers' incurred costs on their workers' absences. The reform provides a unique situation that allows us to study whether employees' absences changed after an exogenous variation in firms' costs. The mandatory insurance insured firms against the direct costs incurred because of their blue-collar workers' sickness absences until

[1]There is substantial evidence that both the availability and the level of sickness benefits influence the extent of absenteeism. Johansson (1996), Johansson and Palme (2005), and Hall and Hartman (2010) find such a moral hazard for Swedish workers. Similarly, Ziebarth and Karlsson (2010) and Puhani and Sonderhof (2010) show that a reduction in the replacement rate for sick workers in Germany substantially decreased sickness absences and hospitalization days. In the United States, where workers are not universally covered by sickness insurance, numerous studies investigate the moral hazard associated with worker compensation schemes. Krueger (1990) and Hirsch *et al*. (1997), among others, find that these insurance schemes have large incentive effects on workers' behaviour, although more recently, Bronchetti and McInerney (2012) have challenged the prevailing wisdom that workers are highly responsive to changes in benefit levels.

[2]These include: workers' health status (Delgado and Kniesner, 1997) and gender differences (Ichino and Moretti, 2009); social norms, peer-group dynamics, and infection effects on the workplace (Drago and Wooden, 1992; Ichino and Maggi, 2000; Barmby and Larguem, 2009); working conditions, work arrangements, and work contracts (Darr and Johns, 2008; Riphahn, 2004; Ichino and Riphahn, 2005; Dionne and Dostie, 2008); business cycle effects on labour force composition (Askildsen *et al*., 2005); and doctors' behaviour (Markussen *et al*., 2011).

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September 2000. The insurance was abolished in 2000 and firms no longer receive a refund. This reform changed the firms' costs for sickness absences, but it did not affect workers' entitlements to continued wage payments when sick.

Our findings are helpful for designing sickness insurance systems worldwide, because as many as 145 countries provide paid sickness absences (Scheil-Adlung and Sandner, 2010). The institutional arrangements that govern the roles of firms and public entities (e.g., social insurance agencies) in providing insurance are heterogeneous and rarely studied. Our results are particularly relevant for analysing institutional settings similar to the one that we observe in Austria until 2000. Examples of (at least partial) reimbursements of firms' wage payments during sickness absences can be observed in Germany, Denmark, the United Kingdom, and South Africa. Moreover, concerns about firms' behaviour towards sickness absences might also exist in countries such as Switzerland, where private insurance companies offer to insure employers against the wage payments they incur when their workers are sick.

We use register data from the Austrian Social Security database (ASSD) on 33,892 firms for the period from January 1998 to September 2002. We combine these data with data from the statutory health insurance, which provide information on workers' days of paid sickness absences. We distinguish between the extensive margin of sickness absences, the number of sickness absences per worker in a firm, and the intensive margin, the number of sickness absence days per worker. We calculate for each firm the ratio of refunds for wages paid to insurance premiums to obtain a variable for the intensity of the treatment. We estimate the effect of the reform by comparing firms that were not treated (i.e., those that employed only white-collar or blue-collar workers who were never on sickness absence) with treated firms (i.e., those that received refunds in the pre-reform period). According to our estimates, the removal of insurance significantly reduced sickness absences. We estimate that the reform lowered both the number of sickness spells (extensive margin) and the number of sickness days (intensive margin). Thus, we estimate that in the first year after the reform was introduced, it resulted in about 6.3 percent fewer sickness absences and about 8.6 percent fewer absence days.

The available data do not allow us to examine the behavioural changes of firms and employees in detail; however, we show that the reform did not change the hiring and firing practices of firms in the short term. In addition, changes in workforce composition were limited. This suggests that other factors (e.g., stricter monitoring or adapted worker behaviour) were the major forces behind the observed reduction in sickness absences.

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II. Institutional Settings

In Austria, a sick worker needs to see a medical doctor who certifies the sickness and informs the social security administration. The worker has to inform the employer about the expected period of the sickness absence. A worker who is still sick at the end of the period needs to see a doctor again to obtain an extension. Similarly, if a worker wishes to return to work before the end of the initially expected period, he or she will need to see a doctor to obtain a revised certificate. Employers who allow workers to return to work without the doctor's approval risk a fine and might lose insurance coverage, for instance, if the worker is involved in a work accident.

Employers have to pay sick workers their full wages. The length of continued wage payment depends on worker tenure, and ranges from six weeks for tenures shorter than five years to a maximum of 12 weeks for tenures longer than 25 years. After this period, social security pays half of the wages for another four weeks, while the other half has to be paid by the firm. After these four weeks, a firm's obligation to continued payments ends.[3] According to labour law, a sickness absence does not provide any additional protection from dismissal than those regulated by the law. An employee who is dismissed during a sickness absence does, however, retain the right to receive continued wage payments for the full period prescribed by the law.

Sickness absences cause substantial direct costs to firms. A reform of the Austrian social insurance system provides a natural experiment to investigate the effect of changes in sickness costs on firms' sickness absences. Until September 2000, a mandatory insurance refunded firms for wages paid to sick blue-collar workers. This insurance was abolished in September 2000 and no refunds were made after December 2000.[4]

The insurance was financed by firms' contributions and it was managed by the Austrian social security administration. There was no experience rating and firms had to pay 2.1 percent of their blue-collar workers' wages as contribution.[5] The refund differed between small and large firms. Small firms received 100 percent and large firms received 70 percent of the wages

[3]The worker might receive sick pay from the social security for up to one year if still unfit for work.

[4]Between October and December 2000, only sickness absences that had started before October were refunded. In September 2002, a minor reform was introduced. For absences longer than 10 days, small firms receive a 50 percent refund for the wages paid to sick workers after the 10th day of the sickness absence.

[5]Private-sector workers in Austria are employed either as blue- or white-collar workers depending on the types of tasks they perform. According to Austrian law, white-collar workers are employees who perform commercial tasks (*kaufmännische Dienste*), non-commercial higher tasks (*höhere, nicht kaufmännische Dienste*), or clerical work (*Bürotätigkeiten*). Conversely, blue-collar workers are entrusted mainly with manual tasks. Several legal reforms diminished the relevance of this

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paid to sick blue-collar workers. A firm was considered large if its total monthly wage bill of month $t-2$ was above a certain threshold, 180 times the maximum daily social security contribution, for all sickness absences that started in month t.[6]

Although the insurance refunded firms only for blue-collar workers' absences, the definition of firm size was based on the wages of both blue- and white-collar workers. The regulation intentionally favoured smaller firms because they were assumed to have more problems covering sickness absences than larger firms. In particular, small firms might need to hire replacement workers if any of their workers are sick. Large firms, by contrast, can often cover for sick workers by reallocating tasks within the firm.[7]

From a worker's perspective, the regulation of sickness absences changed very little during the period. At the same time as the abolition of the insurance scheme, the maximum period for continued wage payments of blue-collar workers was extended to that of white-collar workers. Until 2000, the entitlement for blue-collar workers was, for every tenure category, two weeks shorter than that of white-collar workers.

III. Hypotheses

The abolition of the insurance changed firms' incentives. Firms can influence the level of sickness absences of their employees primarily by investing in health promotion (Aldana and Pronk, 2001) or by enforcing stricter monitoring (Heywood and Jirjahn, 2004). When firms are (partly) insured against the absences of their blue-collar workers, the insurance might lead to inefficient levels of monitoring and prevention. A refund reduces the potential gains from monitoring and prevention, and could lead firms to refrain from monitoring their workers if the expected gains from monitoring are less than the costs incurred. This also implies that a sufficiently high refund will cause the firm to stop monitoring its absent workers for shirking.

The abolition of the insurance increased the potential net gains from prevention and monitoring activities, which is why we expect the reform

distinction. Table A1 in the Appendix provides an overview of the differences in regulations affecting blue- and white-collar workers, both before and after 2000.

[6]The social security contribution is regulated by law and changes each year. The threshold was 18,836.82 euros in 2000, which corresponded to approximately 10 full-time blue-collar workers if they were paid the monthly median wage (1,822 euros).

[7]This assumption is supported by the economic literature on absenteeism. For instance, Barmby and Stephan (2000) provide theoretical arguments and empirical evidence showing how firm size is inversely related to the costs resulting from absences.

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to result in fewer sickness absences, if there was adjustment along the extensive margin. We expect shorter sickness absences, if there was adjustment along the intensive margin. Because workers have to pass a medical examination by a medical doctor to be able to be absent from work, we expect to see a stronger reaction in the durations rather than the number of spells.

However, monitoring and prevention activities are not the only transmission channels through which the reform might have affected absences. Firms might have reacted to the reform with changes in their hiring practices and in their selection of workers, perhaps even terminating the contracts of sick workers. Also, behavioural changes of employees could account for changes in the number or duration of absences. Given that, in Austria, workers are not protected from dismissal during sickness absences, it is possible that they reacted to the reform by avoiding sickness absences altogether (i.e., with increased presenteeism).

The abolition of the insurance affected firms with different intensities. Firms that employed only white-collar workers were not affected. The treatment intensity of firms with blue-collar workers varied, depending on the actual or potential changes in sickness absence costs resulting from the reform. Firms that, on average, paid more premiums than they received in refunds gained from the reform, and firms that received more refunds than they paid in premiums lost from the reform. Therefore, we expect to see a (larger) reduction in sickness absences in firms that received more refunds than the premiums they paid. Treatment intensity might also depend on the potential rather than the realized additional costs resulting from the reform for the individual firm. Firms with a high share of blue-collar workers in their workforce were affected more strongly by the reform than those with a low share. Accordingly, we expect to see a (larger) reduction in sickness absences in firms with a predominantly blue-collar workforce.

The reform increased blue-collar workers' entitlement to sickness absences to the duration of that of white-collar workers. The extension of the maximum duration by two weeks might have increased blue-collar workers' incentives for prolonged sickness absences. If these changes result in such behavioural responses, then our estimates cannot be interpreted as the true causal effect of the reform. However, as the responses are likely to increase rather than decrease sickness absences, the estimates could be interpreted as a lower bound of the true causal effect.

IV. Data and Initial Results

We use register data from the ASSD for Upper Austria, one large state that in 2000 accounted for approximately 17.5 percent of workers and 18 percent

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of firms (in NACE sectors C–E) in Austria (Statistik Austria, 2009).[8] The data provide information on all employees in dependent employment but do not include the self-employed or civil servants. We observe all firms for the period January 1998 to September 2002, and a unique firm identifier permits the construction of firm-level information, such as firm size and the number of sickness absences or their average duration, for each firm. We augment these data with data from the statutory health insurance, which provide information on sicknesses, particularly on the days of paid sickness absences. The sample consists of about 1.65 million (firm × months) observations on 33,892 firms.[9]

We distinguish between the extensive margin of sickness absences (number of sickness absences per worker in firm i in month t) and the intensive margin (number of days per worker on sickness absences). Sicknesses are assigned to the month in which the spell began, and – in case the spells extended to the following month – are not reset at the beginning of the next month. We also calculate the number of absence days divided by the number of spells to obtain a variable that informs us about the average length of a sickness spell.

To analyse if the reform had an impact on sickness absences, we construct a binary indicator, which is one if firm i received any refunds in the pre-reform period, and zero if it did not. Table 1 tabulates the means of the sickness variables for the two types of firms, for the pre- and post-reform periods. The numbers indicate that sicknesses decreased in firms that received refunds. On average, treated firms had about 7.8 percent fewer sickness absences and about 8.8 percent fewer sickness days. Sickness absences were, on average, about 11.1 percent shorter. In contrast, sickness absences increased in firms that did not receive refunds: they had about 11.1 percent more sickness absences, 9.6 percent more sickness days, and absences were, on average, about 8 percent longer. Taking these different developments into account, tabulated in the DiD column of Table 1, this suggests that treated firms had about 12 percent fewer sickness spells and about 13 percent fewer sickness days, and that sickness spells were about 16 percent shorter in the post-reform period.

Labour legislation mandates that workers must provide a medical certificate for all absences of more than three days. Employers can request that their employees provide a certificate for sickness absences of shorter durations; however, not all firms request a certificate from the first day of

[8]Zweimüller *et al*. (2009) provide a detailed description of these data. The ASSD contains matched employer–employee data detailing the labour market history of private-sector workers on a daily basis.

[9]Firms that do not have at least one employee in a month are dropped from the sample for that particular month, but are included in the sample in other months if they employ workers.

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Table 1. *Sickness absences, by treatment status*

	Before	After	Difference	DiD
Treated firms				
Spells	0.089	0.082	−0.007	−0.011
Days	0.933	0.850	−0.083	−0.122
Days per spell	0.623	0.554	−0.069	−0.098
Spells, bounded	0.074	0.067	−0.007	−0.010
Days, bounded	0.696	0.606	−0.090	−0.112
Observations	528,629	359,113		
Control firms				
Spells	0.036	0.040	0.004	
Days	0.406	0.445	0.039	
Days per spell	0.356	0.385	0.029	
Spells, bounded	0.029	0.032	0.003	
Days, bounded	0.271	0.293	0.022	
Observations	442,464	322,291		

Notes: 1,652,497 monthly observations of 33,892 firms. The treatment indicator is 1 if a firm received refunds in the pre-reform period, whereas it is 0 if it did not receive any refunds. "Spells" refers to the number of sickness absences per worker in a firm that started in a month. "Days" refers to the number of sickness days per worker in a firm of all spells that started in the month. (Both variables are not reset at the beginning of the next month if an absence lasts until the next month.) "Before" indicates the pre-reform period (i.e., January 1998 to September 2000) and "After" indicates the post-reform period (i.e., October 2000 to September 2002). "Difference" indicates the difference between the value in the After column and the Before column; "DiD" indicates the difference-in-differences (i.e., the difference for treated firms − the difference for control firms). "Spell, bounded" and "Days, bounded" are based on sickness absences of 4–28 days durations only.

absence. Consequently, short absences might not be fully documented by the administrative data that are based on doctors' notifications to the health insurance. The abolition of the insurance in 2000 might have influenced how firms handle absences of fewer than four days. Because firms do not receive refunds after the reform, they have a lower incentive to require workers to supply a doctor's certificate for short sicknesses. In contrast, an increase in monitoring after the abolition of the insurance might have led firms to request medical certificates for short sickness spells as well. At the same time, the reform extended the maximum duration of wage payments by two weeks and affected spells of more than four weeks differently. (A maximum of four weeks' wage payments was the shortest entitlement; see Table A1.) Therefore, we also use additional measures of sickness absences, which are based on spells that were between 4 and 28 days long. However, the differences are minor.

If both types of firms shared a common trend during the pre-reform period, the difference-in-differences of Table 1 provides a causal estimate

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of the effect of the reform. This causal effect corresponds to θ in the following equation,

$$\begin{aligned}\text{sick}_{it} &= \alpha_0 + \theta(\text{treatment indicator} \times \text{period})_{it} \\ &\quad + \alpha_1 \text{treatment indicator}_i + \alpha_2 \text{period}_t + \epsilon_{it}, \qquad (1)\end{aligned}$$

where $period_t$ has a value of zero for the pre-reform period, January 1998 to September 2000, and a value of one for the post-reform period, October 2000 to September 2002.

To assess the common trend assumption more formally, we follow Autor (2003) and use monthly indicators, m_t, and their interactions with the treatment indicator to estimate k leads and q lags of the reform:

$$\begin{aligned}\text{sick}_{it} &= \beta_0 + \beta_1 \text{treatment indicator}_i + m_t \\ &\quad + \sum_{j=-k}^{q} \beta_j (m_{t+j} \times \text{treatment indicator}_{i,t+j}) + X'_{it}\delta + \epsilon_{it}. \qquad (2)\end{aligned}$$

We use the last pre-reform month, September 2000, as the omitted category. The estimated β_j for the pre-reform months indicate any (statistically significant) differences between the treated and control observations that existed before the reform, and therefore serve to assess the common trend assumption. The vector X_{it} contains covariates, which control for firm characteristics that have been reported as being related to firms' sickness absences. These are the fraction of women in the firm (Ichino and Moretti, 2009), the fraction of workers older than 55 years (Barmby *et al*., 2002), and the log of the average wage in firm i in month t (Winkelmann, 1999). We also control for the fraction of workers younger than 25 years, the fraction of apprentices, the fraction of foreign workers, and the workers' average age. We use indicator variables for the industry and indicators for the quarter to account for seasonality in sicknesses.

The estimated β_j and their 95 percent confidence intervals from fixed-effects panel regressions are plotted in Figure 1. The top panel plots the estimated treatment effects for spells and the bottom panel plots the estimated treatment effects for days.[10] The reference month for both specifications is September 2000 (i.e., the last pre-reform month), indicated by the vertical dashed line. Although some of the estimated pre-reform coefficients are different from zero, no pattern is observed to suggest different trends for treated and control firms. The estimates indicate that firms reduced sickness absences at the onset of the reform and the estimated treatment effects are consistently lower after the reform, indicating a reduction in both sickness spells and sickness days.

[10]We also estimated ordinary least-squares (OLS) regressions. The patterns are similar and the coefficients are plotted in Figure W.1 in the Online Appendix.

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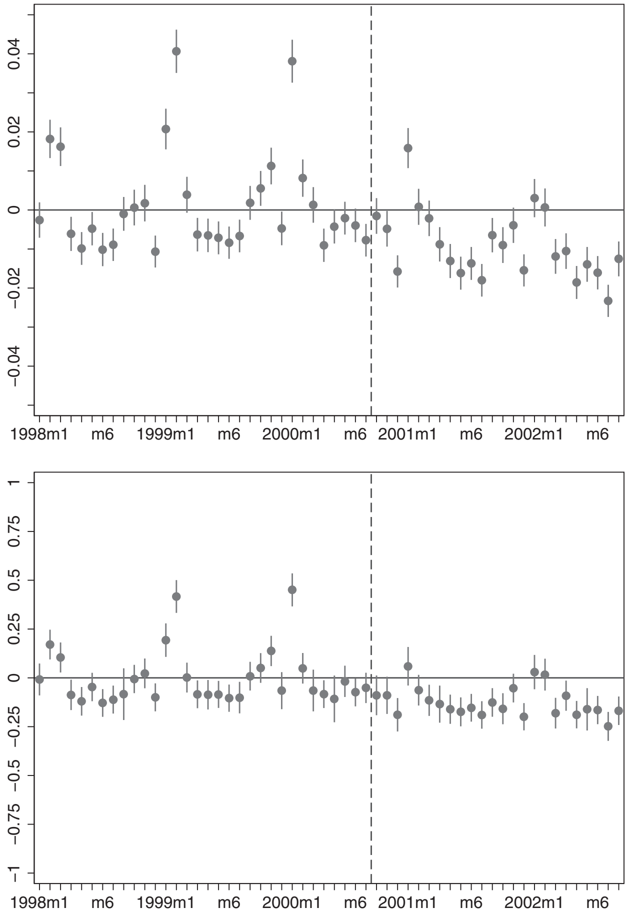

Fig. 1. Leads and lags, fixed-effects panel regressions

Notes: $N = 1{,}652{,}497$ firm × month observations. The diagram plots estimated treatment effects, β_j, from estimating equation (2). Each dot is an estimated treatment effect from fixed-effects panel regressions. The top panel plots the estimated treatment effects for spells and the bottom panel plots the estimated treatment effects for days. The reference month is September 2000 (i.e., the last pre-reform month), which is indicated by the vertical dashed line.

This result could also be caused by regressions towards the mean rather than by causality. If sickness absences are random, firms that had high absence levels in a period are then unlikely to experience high absence levels in the next period. If this were the case, we would see that firms that had low absence levels experienced an increase in their absences, and those with a high absence level would have experienced a decline. To address this concern, we calculate for each firm the refunds-to-premiums ratio in each

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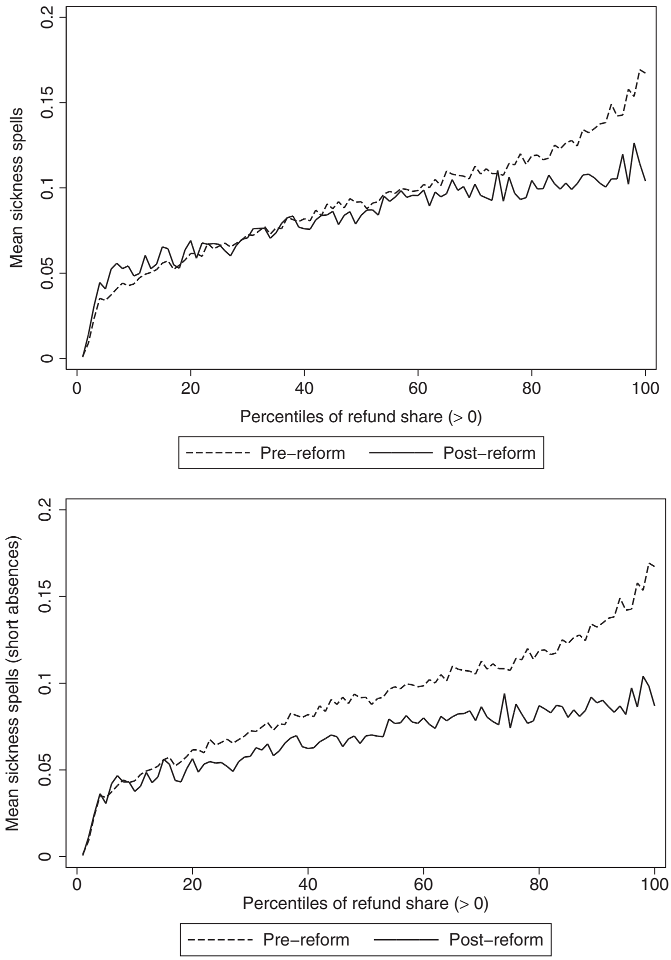

Fig. 2. Sickness spells by intensity of treatment, before/after
Notes: The figure plots the mean sickness spells based on all absences (top) and on absences between 4 and 28 days duration (bottom) by the percentiles of the refund share distribution, where we exclude firms with a treatment intensity of 0. N = 17,465 firms.

pre-reform month. We average these monthly ratios over the pre-reform period and interpret this "refund share" as a proxy for how much a firm would have lost from the reform. A firm that did not employ blue-collar workers, or whose blue-collar workers were never sick, has a value of zero. About 48.5 percent of the firms (16,427) did not receive any refund in the pre-reform period. The other 17,465 firms had an average of about 1.13, suggesting that – conditional on receipt – refunds exceeded premiums. For

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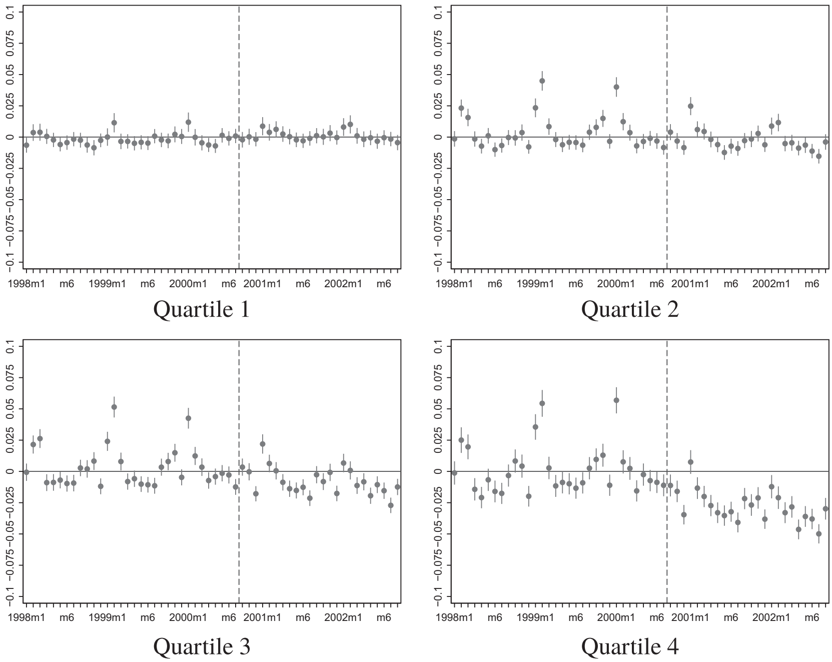

Fig. 3. Leads and lags, by quartiles of treatment intensity: spells

Notes: The figure plots the estimated treatment effects, β_j, for spells from estimating equation (2). Each dot is an estimated treatment effect from a fixed-effects panel regression. The treatment indicator is 0, if the firm did not receive refunds during the pre-reform period. The treatment indicator is 1 if the firms belongs to the qth-quartile of the refund share distribution, excluding zeros. Each plot is based on a regression using the control group (N = 764,755 firm–month observations) and a quartile of the treatment intensity with values strictly greater than 0 (N = 221,962, 221,953, 221,905, and 221,922 firm–month observations). The reference month is September 2000 (i.e., the last pre-reform month), which is indicated by the vertical dashed line.

each percentile of the strictly positive refund share distribution, we calculate the average sickness spells and days for the pre- and post-reform periods.

The top panel of Figure 2 plots the mean sickness spells over the percentiles of the treatment intensity for treated firms (i.e., those with a treatment intensity > 0) by the pre- and post-reform periods. The top panel is based on all absences, while the bottom panel is based on absences that lasted between 4 and 28 days. Figure 2 suggests that although there was some increase in sickness absences for firms with a low treatment intensity, those that had a higher treatment intensity had fewer spells over most of the treatment intensity distribution. We see a similar pattern for days; however, firms that had a low treatment intensity had, on average, slightly more days post-reform than pre-reform. (This plot is shown in Figure W.2 in the Online Appendix.)

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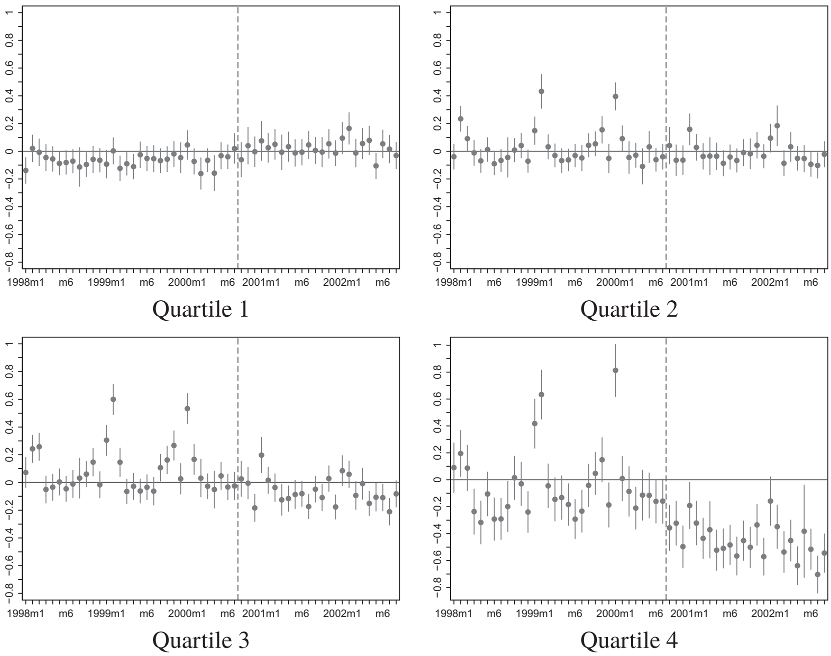

Fig. 4. Leads and lags, by quartiles of treatment intensity: days

Notes: The figure plots the estimated treatment effects, β_j, for days from estimating equation (2). Each dot is an estimated treatment effect from a fixed-effects panel regression. The treatment indicator is 0, if the firm did not receive refunds during the pre-reform period. The treatment indicator is 1 if the firms belongs to the qth-quartile of the refund share distribution, excluding zeros. Each plot is based on a regression using the control group (N = 764,755 firm–month observations) and a quartile of the treatment intensity with values strictly greater than 0 (N = 221,962, 221,953, 221,905, and 221,922 firm–month observations). The reference month is September 2000 (i.e., the last pre-reform month), which is indicated by the vertical dashed line.

For values greater than zero, we split the distribution of the refund share into its quartiles. We compare firms from each quartile with the control firms by re-estimating equation (2). The results from fixed-effects panel regressions are plotted in Figures 3 and 4. These plots indicate that firms that had a low refund share in the pre-reform period were not more likely to experience an increase in sickness absences than firms in the control group. In addition, we see that firms which that had a greater refund share reacted more strongly to the reform than firms with lower values did. These plots do not suggest that the common trend assumption was violated. We re-estimate equation (2) but consider only absences of more than three and less than 28 days. The pattern of leads and lags is similar to that presented in Figure 1, and the diagrams are presented in Figure W.3 in the Online Appendix.

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V. Main Results

In our main econometric specification, we model the effect of the reform on sickness absences using the refund share to indicate the treatment intensity of the insurance (e.g., Acemoglu *et al*., 2004):

$$\begin{aligned} \text{sick}_{it} \quad = \quad & \pi_0 + \lambda(\text{refund share} \times \text{period})_{it} \\ & + \pi_1 \text{refund share}_i + \pi_2 \text{period}_t + X'_{it}\delta + a_i + \nu_{it}, \qquad (3) \end{aligned}$$

where the refund share is each firm's pre-reform average of the monthly refunds-to-premiums ratios. The main parameter of interest, λ, states the marginal effect of the reform for given treatment intensities. The period indicator has a value of zero for the pre-reform period (January 1998 to September 2000), and a value of one for the post-reform period (October 2000 to September 2002). To account for differences between firms, we use a set of firm characteristics, X_{it}, as control variables, which serve to improve the precision of the estimated effects. We estimate equation (3) using fixed-effects panel regression to account for unobserved heterogeneity, a_i, among firms. Standard errors are clustered on firms. (We also estimate this equation using OLS, to demonstrate the robustness of the results.)

Table 2 tabulates the results from regressions where we estimate the impact of the reform for the sickness outcomes. We use OLS specifications, with and without additional covariates, and firm-level fixed-effects regressions. The variables that control for firms' characteristics are the fraction of female workers, the fraction of workers younger than 25, the fraction of workers older than 55, the fraction of apprentices, the fraction of non-Austrian workers, workers' mean wage, workers' mean age, firms' industry classification, and seasonal indicators.

Our preferred specifications are the fixed-effects panel regressions, but there are only minor differences between the specifications. The results indicate that the reform led to fewer sickness spells per worker, fewer sickness days, and shorter spells. We also find that firms that were treated more intensively had, on average, more and longer absences, and that there were only minor differences between the pre- and post-reform periods. The estimates indicate that a one-unit increase in the treatment intensity led to, on average, 0.013 fewer spells per worker per month, 0.19 fewer absence days per worker per month, and shorter durations of 0.14 days per spell. These numbers imply elasticities of −12.2 percent for sickness spells, −17.5 percent for sickness days, and −17.1 percent for days per spell.[11] We also present the results from standardized regressions, which indicate that a one standard deviation (SD) increase in the treatment intensity (0.766) led to

[11] Elasticities are calculated as $\hat{\lambda} * \bar{X}/\bar{y}$, where a bar indicates the mean of a variable.

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Table 2. *Estimated effects of the reform on sickness absences*

	Spells			Days			Days per spell		
	(OLS 1)	(OLS 2)	(FE)	(OLS 1)	(OLS 2)	(FE)	(OLS 1)	(OLS 2)	(FE)
Treatment intensity × after									
λ	−0.014	−0.013	−0.013	−0.198	−0.198	−0.190	−0.154	−0.151	−0.140
	(0.000)	(0.000)	(0.000)	(0.007)	(0.007)	(0.007)	(0.006)	(0.006)	(0.007)
Standardized λ	−0.052	−0.050	−0.049	−0.041	−0.041	−0.039	−0.034	−0.033	−0.031
	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Treatment intensity									
π_1	0.046	0.046	(omitted)	0.529	0.549	(omitted)	0.323	0.336	(omitted)
	(0.000)	(0.000)		(0.004)	(0.005)		(0.005)	(0.005)	
Post-reform period									
π_2	0.006	0.006	0.007	0.092	0.081	0.094	0.069	0.056	0.066
	(0.000)	(0.000)	(0.000)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Covariates	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes

Notes: N = 1,652,497 firm × month observations. The table presents coefficients, and their standard errors (in parentheses), from separate estimations of equation (3) of the effect of the reform on the number of sickness spells, the number of sickness days, and the number of days per sickness spell. The specifications (OLS 1) and (OLS 2) differ only by the use of additional covariates. Covariates used in the OLS specifications are the fraction of workers aged 18–25, the fraction of workers aged 55+, the fraction of female workers, the fraction of apprentices, the fraction of foreign workers, average workers' age, average workers' wage, industry classifications, and seasonal indicators. Firm-level fixed-effects panel regressions control for unobserved firm heterogeneity over time and for the time-varying covariates used in the OLS specifications. Robust standard errors are clustered on firms. Standardized coefficients are from regressions where the dependent and independent variables are standardized; the coefficient can thus be interpreted as changes in the SD of the dependent variable.

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0.05 SD fewer sickness spells, 0.04 SD fewer sickness days, and 0.03 SD shorter sickness spells.

For 2001, the first year after the reform, based on these estimates, we calculate that the reform resulted in about 6.3 percent fewer sickness absences.[12] A similar calculation for absence days suggests that the adjustment along the intensive margin resulted in about 8.6 percent fewer absence days. Consequently, the average spell in 2001 was shorter by about one day.

Alternative Treatment Variable

Blue-Collar Worker Share. We use the blue-collar worker share as an alternative treatment variable to analyse the effects of the reform. The share of blue-collar workers in a firm is perhaps more indicative of future costs from sickness absences than past refunds are. We calculate a firm's mean share of blue-collar workers in the pre-reform period as a measure of how strongly a firm could be affected by the reform. A comparison of sickness absences along the distribution of this variable shows that both spells and days are lower in the post-reform period (see Figures W.4 and W.5 in the Online Appendix). Unlike the pattern shown in Figure 2, there is no evidence that firms that had a greater share of blue-collar workers had a greater reduction. The relative decrease in absences is fairly stable across all percentiles of the blue-collar worker share distribution. The results from these estimates are tabulated in Table 3.[13]

The results also indicate an effect of the reform on firms' sickness absences, and firms that had more blue-collar workers during the pre-reform period had fewer and shorter sickness absences after the reform. However, the estimated effect is smaller than in our previous estimates.

Blue-Collar Workers' Wage Share. We also use the blue-collar workers' wage share as an alternative measure for expected sickness costs. For each firm, we calculate the monthly share of the wages paid to blue-collar workers. Similar to the blue-collar worker share measure used above, we estimate that the reform led to fewer and shorter absences; however, the results do not differ from those obtained from using the blue-collar worker share.

[12]We calculate the predicted number of sickness absences had there been no reform, using the estimated parameters of the fixed-effects panel regression, and we compare this with the observed number of sickness days.

[13]The estimated leads and lags of the reform are plotted in Figures W.6 and W.7 in the Online Appendix, and patterns are similar to those in Figure 1.

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Table 3. *Estimated effects of the reform on sickness absences, using alternative treatment intensity measures*

	Spells			Days			Days per spell		
	(OLS 1)	(OLS 2)	(FE)	(OLS 1)	(OLS 2)	(FE)	(OLS 1)	(OLS 2)	(FE)
Panel A: blue-collar worker share × after									
λ	−0.008	−0.007	−0.007	−0.065	−0.063	−0.072	−0.028	−0.029	−0.036
	(0.001)	(0.001)	(0.001)	(0.013)	(0.013)	(0.013)	(0.012)	(0.012)	(0.012)
Standardized λ	−0.020	−0.017	−0.018	−0.009	−0.008	−0.010	−0.004	−0.004	−0.005
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Panel B: blue-collar workers' wage share × after									
λ	−0.008	−0.007	−0.007	−0.064	−0.063	−0.071	−0.028	−0.029	−0.035
	(0.001)	(0.001)	(0.001)	(0.012)	(0.012)	(0.013)	(0.012)	(0.012)	(0.012)
Standardized λ	−0.020	−0.017	−0.018	−0.008	−0.008	−0.009	−0.004	−0.004	−0.005
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Covariates	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes

Notes: N = 1,652,497 firm × month observations. The table presents coefficients, and their standard errors (in parentheses), from separate estimations of equation (3) of the effect of the reform on the number of sickness spells, the number of sickness days, and the number of days per sickness spell. The specifications (OLS 1) and (OLS 2) differ only by the use of additional covariates. Panel A uses the average of a firm's blue-collar worker share in the pre-reform period as a proxy for exposure to the reform; Panel B uses the average of the blue-collar workers' wage share over the pre-reform period as a proxy for exposure to the reform. Covariates used in the OLS specifications are the fraction of workers aged 18–25, the fraction of workers aged 55+, the fraction of female workers, the fraction of apprentices, the fraction of foreign workers, average workers' age, average workers' wage, industry classifications, and seasonal indicators. Firm-level fixed-effects panel regressions control for unobserved firm heterogeneity over time, and for the time-varying covariates used in the OLS specifications. Robust standard errors are clustered on firms.

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Table 4. *Workforce compositions, by treatment status*

	Before	After	Difference	DiD
Treated firms				
Share blue-collar workers	0.684	0.671	−0.013	−0.031
Share workers 55+	0.045	0.052	0.007	0.001
Hiring rate	0.119	0.119	0.000	−0.002
Separation rate	0.118	0.123	0.005	−0.002
Recall rate	0.120	0.121	0.001	−0.002
Blue-collar hiring rate	0.244	0.244	0.000	−0.006
Blue-collar separation rate	0.270	0.275	0.005	−0.003
Blue-collar recall rate	0.684	0.671	−0.013	−0.031
N	528,629	359,113		
Control firms				
Share blue-collar workers	0.247	0.265	0.018	
Share workers 55+	0.066	0.072	0.006	
Hiring rate	0.123	0.125	0.002	
Separation rate	0.119	0.126	0.007	
Recall rate	0.128	0.131	0.003	
Blue-collar hiring rate	0.086	0.092	0.006	
Blue-collar separation rate	0.103	0.111	0.008	
Blue-collar recall rate	0.247	0.265	0.018	
N	442,464	322,291		

Notes: 1,652,497 monthly observations of 33,892 firms. The treatment indicator is 1 if a firm received refunds in the pre-reform period, whereas it is 0 if it did not receive any refunds. "Before" indicates the pre-reform period (January 1998 to September 2000) and "After" indicates the post-reform period (October 2000 to September 2002). The hiring rate is the ratio of new workers to existing workers, for each month. The separation rate is the ratio of workers who are not employed in the next month to the number of workers employed in current month. The recall rate is the ratio of workers who are not employed in the next month, but who are re-employed within the following 12 months, to those who are not employed in the next month. The corresponding blue-collar rates give the fraction of blue-collar workers in the rate. "Difference" indicates the difference between the value in the After column and the Before column; "DiD" indicates the difference-in-differences (i.e., the difference for treated firms − the difference for control firms).

Workforce Composition

Because the reform might have changed the relative cost of blue-collar and white-collar workers, firms might have systematically different workforces after the reform. In Table 4, we present several measures of workforce changes, by treatment status and period. For the tabulation, we use the binary treatment indicator, which is zero if the firm did not receive refunds, and one if it did. We calculate the hiring rate (i.e., the ratio of new workers to existing workers) for each month. Similarly, we calculate the separation rate (i.e., the ratio of the number of workers who are not employed in the next month to the number of workers employed in the current month).

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Table 5. *Estimated changes to firms' workforces*

	λ	Standardized λ	Mean (SD) dependent variable
(1) Fraction of blue-collar workers	−0.008	−0.020	0.483
	(0.001)	(0.002)	(0.395)
(2) Fraction of workers 55+	−0.007	−0.017	0.057
	(0.001)	(0.002)	(0.150)
Fraction of blue-collar workers:			
(3) among new workers	−0.006	−0.011	0.172
	(0.001)	(0.002)	(0.336)
(4) among separations	−0.005	−0.008	0.195
	(0.002)	(0.002)	(0.358)
(5) among recalled workers	−0.001	−0.001	0.450
	(0.002)	(0.002)	(0.426)
(6) Fraction of new workers	−0.001	−0.004	0.121
	(0.001)	(0.002)	(0.221)
(7) Fraction of separations	−0.0002	−0.001	0.121
	(0.002)	(0.001)	(0.222)
(8) Fraction of recalled workers	−0.002	−0.004	0.125
	(0.001)	(0.002)	(0.222)

Notes: N = 1,652,497 monthly observations of 33,892 firms. The table presents coefficients, and their standard errors (in parentheses), from separate estimations of equation (3) of the effect of the reform on workforce composition, using fixed-effects panel regressions. Workforce composition is measured by: (1) the fraction of blue-collar workers; (2) the fraction of older workers (workers older than 55); (3) the fraction of workers who started to work in the firm in month t as a fraction of the number of workers in month t; (4) the number of workers who will stop working in the firm in month $t + 1$ as a fraction of the number of workers in month t; (5) the fraction of workers who are not with the firm in month $t + 1$ and who are re-employed within the following 12 months as a fraction of all workers in month t; (6) the fraction of blue-collar workers among all new workers; (7) the fraction of blue-collar workers among all separations; and (8) the fraction of blue-collar workers among recalled workers. Firm-level fixed-effects panel regressions control for unobserved firm heterogeneity over time and for the time-varying covariates used in the OLS specifications. Robust standard errors are clustered on firms.

We also calculate the recall rate (i.e., the fraction of workers who are not employed in the next month, but are re-employed within the following 12 months) among all workers who are not employed in the next month. For each of the three rates, we also calculate the fraction of blue-collar workers in that rate. The results in Table 4 suggest that the reform had little impact on the hiring or separation behaviour of firms, except for the recall of blue-collar workers in treated firms, which decreased by about three percentage points.

In Table 5, we tabulate the estimation results to detail the effect of the reform on workforce composition. We use the same econometric specifications as for our main results but we focus on whether firms changed the hiring and workforce selection processes or not. In the first step, we

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regress firms' share of blue-collar workers on the treatment intensity, and other explanatory variables, to estimate if the reform led to differential changes in firms' workforce compositions. In the next step, we calculate the share of blue-collar workers among newly hired workers and among those who left the firm. We then regress these rates on the treatment intensity to analyse whether the (perceived) change in relative prices for both blue- and white-collar workers led firms to adapt their hiring.

We estimate that the reform caused firms that were treated more intensively to lower the fraction of blue-collar workers; however, the estimated effect is small. A one-unit increase in treatment intensity is estimated to decrease the fraction of blue-collar workers in the firm by 0.8 percentage points (pre-reform mean of 0.485). Substitution between blue- and white-collar workers is costly in the short term because workers are classified as blue- or white-collar workers depending on the tasks they perform, and any significant substitution would thus require restructuring the production process. We examine the possible channel of such an adjustment by estimating the fraction of blue-collar worker among new hires, workers who leave the firm, and among recalls. We estimate that the fraction of blue-collar workers among new hires declined. The share of blue-collar workers among the workers who leave the firm also declined (i.e., blue-collar worker retention increased). However, both effects are relatively small.

Older workers are more often sick than younger workers; for example, in 2000, Austrian workers aged 55–64 had more than twice the average number of sickness days (*Hauptverband der österreichischen Sozialversicherungsträger*, 2001). Firms could have reacted to the reform by changing the age structure of their workforce. We estimate that firms employed fewer older workers post-reform, although the magnitude of this change was small.

In addition, we calculate a hiring rate and a separation rate to see if the reform led firms to change their hiring or separation patterns. The estimated results do not suggest that firms changed their behaviour along these dimensions. Similarly, we do not find evidence for a change in how firms use recalls (i.e., lay off workers who are rehired within the next 12 months).[14]

Sample Restrictions

Our estimates indicate that the abolition of the insurance significantly reduced sickness absences. We provide a series of robustness checks to

[14]Because our data are right-censored, we use different cut-offs for the periods within which a worker is recalled (3, 6, and 12 months). The results do not differ.

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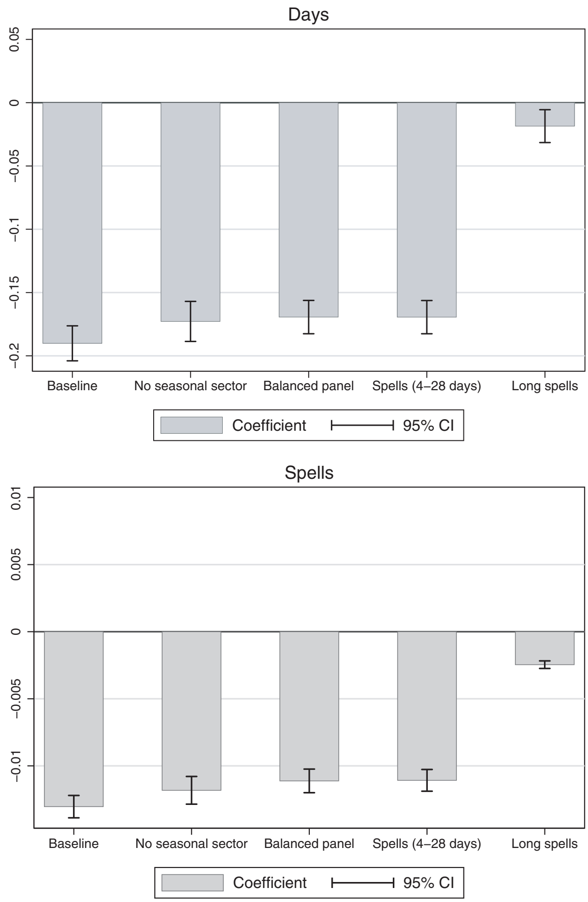

Fig. 5. Robustness: different definitions

Notes: Bars present the estimated effects of abolishing the refund, λ, and the whiskers show the 95 percent confidence intervals from estimations of equation (3) using fixed-effects panel regressions. The first bars are the estimated effects as tabulated in Table 2. The second bars are from estimations where we exclude all firms in seasonal sectors. The third bars give the estimated effects limiting the observations to a balanced panel. The fourth bars are from estimations where the sickness variables are based on sickness absences of 4–28 days only. The estimations that are the basis for the fourth bars use only spells where the firms' liability to pay continued wages ended during the spell.

gauge the reliability of these results. We plot the estimated coefficients, and their confidence intervals, from firm-specific fixed-effects panel regressions in Figure 5. The first bars plot the estimated λ, with standard errors in parentheses, from the panel regressions, which are tabulated in Table 2.

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Seasonal Sectors. Austria has a considerable seasonal sector, particularly in the tourism and in the building sectors (Del Bono and Weber, 2008). Firms that operate seasonally might monitor their workers differently or provide their workers with different incentives than firms that operate throughout the year. While we control for a firm's sector using indicator variables, or firm-fixed effects, in our main specifications, we re-estimate the effect of the reform on a reduced sample to investigate the robustness of the results. Eliminating firms that operate in the construction and tourism sectors from our sample does not result in a different estimate of the treatment effect, which can be seen from the second bars in Figure 5.

Balanced Panel. Our main estimating sample is an unbalanced panel and consists of all firms with employees in any month during January 1998 to September 2002. This sample could systematically bias the results, if, for example, the insurance allowed firms with a sicker workforce to remain in the market longer than without an insurance. Therefore, we restrict our sample to a balanced panel of those firms that are observed in each of the 57 months of our observation period. (Note that this creates a survival bias as exiting firms are no longer in the sample.) These coefficients are plotted as the third bar in Figure 5. There are only small differences between these estimates and our main results.

Short Spells. All employees must visit a medical doctor if their sickness absence is longer than three days. Firms can request that employees visit doctors even for shorter sickness absences. Because not all firms compel their workers in the same way, we construct alternative measures of sicknesses to limit any measurement error arising from this. We use bounded versions of our main sickness indicator that are based on sickness absences of a duration of 4–28 days. The estimated treatment effect is plotted as the fourth bar in Figure 5. The estimates are virtually identical and do not change our interpretation of the causal effects of the reform.

Very Long Spells. If a worker is sick for more than his or her legal entitlement to continued wage payments, the worker receives sick pay from the social security. It is likely that firms and employees cannot influence very long sickness absences as easily as shorter sickness absences. We estimate the effect of the reform on both the number and the days of very long sickness absences. We plot the estimated coefficients as the fifth bar in Figure 5. We estimate that the reform lowered the number of very long sickness spells, but it had virtually no effect on the number of days of such very long spells.

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White-Collar Workers. We also estimate sickness absences separately for blue- and white-collar workers. The estimated λ (and standard error) for the sickness spells of white-collar workers is 0.0001 (0.0002) and for sickness days −0.010 (0.007). The estimates are available in Table W.1 in the Online Appendix. These estimates indicate that the reduction in sickness absences was caused by a reduction of blue-collar workers' absences. We do not find evidence for spillover effects of the reform on white-collar workers' absences.

VI. Overall Effect of the Reform

The reform lowered the number of absence days of blue-collar workers and thus led to savings in terms of continued wage payments. Based on the panel fixed-effect estimates, we estimate that firms would have had about 8.58 percent fewer absence days in 2001. Based on blue-collar workers' average wages, this reduction implies a saving of about 8.25 percent in continued wage payments. Alternatively, in 2001, each firm in our sample saved, on average, about 1,931 euros, almost 20 percent more than one monthly average wage (1,633 euros).

However, if we consider the net cost effects of the reform on firms, we need to take into account that in a counterfactual scenario, without abolition of the insurance scheme, firms would have paid insurance premiums but would have received wage payment refunds. Prior to the reform, a large share of the firms in our sample received more refunds than they paid premiums. For example, in 1999, the year prior to that of the reform, about 37.3 percent of the firms in our sample received more in refunds than they paid in premiums.[15] For this reason and in spite of the substantial reduction in sickness days, for many firms the direct net costs of sickness absences were greater in 2001 than they would have been if the insurance scheme were still in place. According to our estimates, 77.9 percent firms had higher costs than under the counterfactual scenario. For them, absence costs in 2001 were, on average, about 1,066 euros greater than with insurance.

Ideally, we would want to compare the total costs for sickness absence borne by firms and social security in the aftermath of the reform with the counterfactual costs of all stakeholders, under the assumption that the insurance scheme was still in place. We do not possess sufficient

[15]This does not necessarily imply that the insurance performed negatively overall. A negative financial performance in one period could have been counterbalanced by a positive performance in another period. We only observe sicknesses in one region, and in other regions the pattern could have been reversed, resulting in an overall positive financial performance. Unfortunately, we do not have any information on the insurance's financial performance.

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information to carry out this comparison, mainly because we do not know much about the financial performance of the insurance fund. The quantification of potential savings for public funds depends at least partially on whether the reform reduced the sick pay days paid by the social insurance agency. Our results indicate that, in the short term, the reform did not affect long-term sickness absences, and therefore the number of absence days covered by sick pay.[16] This is consistent with the expectation that, at least in the short to medium term, the reform did not result in changes that affected the causes for long absence spells (i.e., severe illnesses and accidents); in the long term, changes in prevention could potentially reduce long absences. We conclude that the reform had a sizeable effect on absences and reduced the overall expenditure for continued wage payment, but that it did not directly affect the monetary sickness absence costs borne by social insurance.

VII. Conclusion and Discussion

We analysed sickness absences in an institutional setting where a reform led to the abolition of an insurance mechanism that refunded firms for the wages they paid their sick blue-collar workers. Using administrative data and the variation in firms' reliance on the insurance before the reform, we found robust evidence for a reduction in firm-level absenteeism in reaction to an exogenous change in sickness costs.

Consistent with our expectations, we found that sickness absences decreased more in firms that had benefited most from the insurance mechanism (i.e., those that potentially faced the greatest increase in costs). According to our estimates, the reform led, on average, to fewer and shorter sickness absences. Absences started to fall immediately after the policy change, indicating that the reform had an immediate impact. The drop in sickness absences was confined to blue-collar workers' absences and we did not detect any effect on white-collar workers' absences. This reaction to the reform lowered continued wage payments for firms.

We interpret our results as a clear finding that sickness absences react to changes in firms' sickness costs. Different transmission mechanisms might explain this result. Firms might have reacted to the reform by investing in health promotion (Aldana and Pronk, 2001) or by enforcing stricter monitoring (Heywood and Jirjahn, 2004). They might also have changed their hiring policies to select a healthier workforce, but it is also

[16]If the reform resulted in more presenteeism (e.g., if workers fear losing their job due to greater sickness costs for firms), then this could result in more severe sickness absences in the medium term.

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possible that changes in workers' behaviour were the source of the observed reduction in absences. Given that, in Austria, workers are not protected from dismissal during sickness absences, it is possible that part of the reduction was due to workers who avoided sickness absences after the reform (presenteeism). However, it is also possible that absences before the reform were inflated and the reduction is due to a correction of firms' moral hazard.

While our data do not allow us to investigate all potential transmission mechanisms, the available information does provide partial evidence. Our analyses show that in the two years after the reform, firms did not change their hiring and separation rates, and did not make increased use of temporary lay-offs. We did find a small reduction in the workforce share of blue-collar workers because of the reform. This suggests that changes in workforce composition and stricter selection processes played at most a limited role. Because we observe that the effects emerged immediately after the reform, it is unlikely that the reduction in sickness absence can be explained by increased prevention. Overall, our findings suggest that other factors are more likely the reason for the reaction to the reform. These include stricter monitoring of absences and behavioural responses by employees.

Our findings are of interest for the design of social insurance policies and sick pay systems. Sick pay regulation is a central component of modern welfare states.[17] Our results strengthen the argument that calls on firms to carry a portion of the costs of sick pay. Clearly, an insurance for costs related to sickness absences might have negative effects on absenteeism and (some) firms have little incentive to monitor or prevent absenteeism.

However, this does not imply that a large shift of the burden to firms necessarily increases the overall welfare. An increase in costs borne by firms (e.g., through experience-rated sick pay schemes) could lead firms to place more weight on workers' health status during their hiring procedure. This might create unintended difficulties for workers who suffer from poor health and their integration in the labour market. Although we do not find such responses in the short term, evidence for the Netherlands, for instance, where the introduction of the Gatekeeper protocol extended firms' liability for continued wage payments of sick workers to two years, suggests a stronger sorting of workers with poor health into flexible and non-permanent employment contracts (Koning and Lindeboom, 2015).

[17] In 1883, the German Chancellor Otto von Bismarck laid the foundation of the modern welfare state with sickness insurance legislation that included paid sickness absences for workers in the case of illness for a period of 13 weeks.

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Appendix: Background and Descriptive Statistics

Table A1. *Legal differences between blue- and white-collar workers*

	2000 (before reform)		2001 (after reform)	
	Blue-collar	White-collar	Blue-collar	White-collar
Maximum duration of wage payments in case of sickness (weeks)				
At tenure:				
< 5 years	4	6	6	6
5–15 years	6	8	8	8
15–25 years	8	10	10	10
≥ 25 years	10	12	12	12
Within calendar year	Fixed	Unlimited	Fixed	Unlimited
Health insurance contributions				
Employers	3.95	3.40	3.65	3.40
Workers	3.95	3.50	3.95	3.50
Minimum period of notice	One day	Six weeks	One day	Six weeks

Notes: Health insurance contributions are expressed as percentage of gross wage (salary). Period of notice: the minimum period can be extended by collective bargaining. Maximum period of wage payment within a calendar year: for blue-collar workers this is fixed, regardless of how many times a worker falls ill; white-collar workers can claim longer periods of wage payments within a calendar year if they fall ill repeatedly.

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Table A2. *Summary statistics*

	All		Treatment indicator			
			Control		Treated	
	Mean	SD	Mean	SD	Mean	SD
Days	0.679	2.440	0.422	3.080	0.899	2.440
Days, bounded	0.484	1.506	0.281	1.453	0.660	1.527
Spells	0.064	0.152	0.038	0.146	0.086	0.153
Spells, bounded	0.053	0.138	0.030	0.132	0.072	0.141
Refunds	292.799	3,470.664	0	0	544.899	4,720.689
Premiums	489.204	3,413.106	480.930	3,370.715	496.333	3,449.193
Refund share	0.600	0.765	0	0	1.117	0.715
Small firm in pre-reform	0.744	0.416	0.769	0.399	0.722	0.430
Fraction						
apprentices	0.069	0.148	0.040	0.120	0.095	0.164
blue-collar workers	0.483	0.395	0.255	0.375	0.679	0.293
foreigners	0.075	0.168	0.070	0.167	0.079	0.169
women	0.490	0.371	0.511	0.390	0.472	0.354
workers 18–24	0.185	0.233	0.142	0.216	0.221	0.240
workers 55+	0.058	0.150	0.069	0.180	0.048	0.117
Average workers' age	35.995	7.651	37.313	8.037	34.859	7.109
Average workers' wage	53.037	24.388	58.591	28.618	48.251	18.764
N	1,652,497		764,755		887,742	

Notes: A firm is considered treated (untreated) if it received (did not receive) any refunds for its blue-collar workers' sickness absences during the pre-reform period, January 1998 to September 2000. "Days, bounded" and "Spells, bounded" refer to sickness days per worker and spells per worker in month t based on sickness absences of durations of 4–28 days only. A firm is considered small if its monthly wage sum is less than 180 times the maximum daily social security contribution. Wages are deflated to 2000 using the Harmonized Consumer Prize Index (Statistik Austria, 2013).

Supporting Information

Additional supporting information may be found online in the Supporting Information section at the end of the article.

Online Appendix

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