Firms' moral hazard in sickness absences *

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Abstract

Sick workers in many countries receive sick pay during their illness-related absences from the workplace. In several countries, the social security system insures firms against their workers' sickness absences. However, this insurance may create moral hazard problems for firms, leading to the inefficient monitoring of absences or to an underinvestment in their prevention. In the present paper, we investigate firms' moral hazard problems in sickness absences by analyzing a legislative change that took place in Austria in 2000. In September 2000, an insurance fund that refunded firms for the costs of their blue-collar workers' sickness absences was abolished (firms did not receive a similar refund for their white-collar workers' sickness absences). Before that time, small firms were fully refunded for the wage costs of blue-collar workers' sickness absences. Large firms, by contrast, were refunded only 70% of the wages paid to sick blue-collar workers. Using a difference-in-differences-in-differences approach, we estimate the causal impact of refunding firms for their workers' sickness absences. Our results indicate that sickness incidences dropped by approximately 6.6% and sickness absences were almost 7.6% shorter following the removal of the refund. Several robustness checks confirm these results.

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1 Introduction

Sickness absences lead to significant losses in productivity for firms and, consequently, to lower incomes and profits (Barham and Begum, 2005; Brown and Sessions, 1996). Labor laws typically grant workers continued pay if they are unable to work because they are ill. However, guaranteed sick pay may induce workers to "adapt their work-absence behavior" (Johansson and Palme, 2005, p. 1880). In other words, workers may be absent from work without actually being sick. Johansson and Palme (2005) find such a moral hazard problem for Swedish workers. Similarly, Ziebarth (2009) estimate that a reduction in the replacement rate for sick workers in Germany from 100% to 80% led to some 12 fewer days of sick leave.

Countries differ with respect to how they share the costs of sickness absence between workers, firms, and the social security system. Firms in most OECD countries face direct and indirect costs because of absences, but they are, to some extent, insured against their workers' sicknesses. Insurance arises because either the amount or the period of sick pay is limited, or firms are refunded for their costs. For example, Norwegian firms need to pay the first 16 days of a worker's sickness absence at a replacement rate of 100%; however, the worker's wages are paid by social security thereafter (Markussen, Mykletun and Roed, 2010). In Germany, firms that have fewer than 30 employees receive 80% of their costs refunded by an insurance fund (Ziebarth, 2009). Depending on how the insurance system is organized, firms may pass their costs onto the public, for example by exerting too little effort in monitoring or preventing absences.

We use an exogenous change in the way Austrian firms are compensated for their workers' sickness absences in order to shed light on these firms' moral hazard problems. The Austrian social insurance system provides an excellent setting for the analysis of moral hazard and sickness absences because Austrian legislation guarantees each regular employee continued wage payments for at least six weeks, to be paid by the employer. Barmby, Ercolani and Treble (2002) argue that to understand fully the impact of regulations on absence rates, it is necessary to gather data that enable the analysis of a regime shift within a single jurisdiction.

Until September 2000, small firms received a full refund for the direct costs (gross wages and employers' social security contributions) of blue-collar workers' sickness absences (see 1. Indirect costs, such as replacements, restructuring, or down-time, were not refunded. Large firms, by contrast, received only 70% of paid wages. The definition of a small firm was based on the firm's wage bill (of month $t-2$), and refunds were paid automatically within three months. Between October and December 2000, only sickness absences that started before October were refunded, and no refunds were made after December 2000.[1] We use the end of the refund period in 2000 to investigate whether or not workers' absences were affected by different policy regimes. The 2000 reform abolished the reimbursement for blue-collar workers' absences, thereby increasing costs for firms.

The data used in the present study are from the Austrian Social Security Database (ASSD) and refer to one large Austrian state (Upper Austria). The

[1] A partial refund was reintroduced for small firms in October 2002; however, there is no differential treatment for blue- and white-collar workers under this regime.

ASSD is the administrative database for the calculation of pension benefits for private sector employees in Austria. We then linked these data to the administrative database of public health insurance, which covers all employees, in order to obtain information on workers' sickness absences.

Changes in the incidences or durations of sickness episodes may indicate that firms as well as workers react to the incentives built into the system. The literature on sickness absences, recently summarized in Ziebarth (2009), focuses almost entirely on workers' behavior. An exception is the recent contribution by Fevang, Markussen and Røed (2011), who analyze reform in the Norwegian sick leave insurance scheme, where employers are exempt from refunding sick pay for pregnancy-related absences. They show that this exemption from refunding sick pay led to approximately 5% more sickness absences. The present study estimates that, over a period of almost two years, the removal of the refund resulted in 10% fewer blue-collar workers' sicknesses and shortened sickness absences by 11.3%. As expected, the impact of the reform was stronger on small firms, where spell incidences and durations were reduced by 8.9% and 8.1% more than in large firms. We provide a series of robustness checks, which confirm the reliability of our results.

Our findings are important for the design of sickness insurance systems in many countries. According to Scheil-Adlung and Sandner (2010), as many as 145 countries within and outside the OECD provide paid sick leave. Sick pay regulation is a central component of modern welfare states, and provisions aimed at insuring workers against the loss of income because of illness date back to the very origins of the welfare state.

2 Institutional settings

Workers in Austria continue to receive their wages from their employers if they are unable to attend work because of sickness. A sick worker needs to see a medical doctor who certifies the sickness and informs the social security administration. The worker is required to inform his or her employer about the expected period of sickness leave. The length of continued wage payment by the employer depends on worker tenure and amounts to a maximum of 12 weeks. After that period, workers continue to receive their wages for another four weeks; however, during these four weeks, the wage is split equally between the firm and the social security. After these four weeks, the worker receives sick pay from the social security for up to one year, which is 50% of the wage until the 42nd day of sickness and 60% thereafter. According to Austrian employment protection legislation, being on sick leave does not provide any form of protection from dismissal. An employee who is dismissed while on sick leave does, however, retain the right to continued wage payments for the full period prescribed by law.

Private sector workers in Austria are employed either as blue-collar or as white-collar workers depending on the types of tasks that they carry out.[2] Although several legal reforms diminished the original relevance of this distinction, labor law provisions generally were and remain more favorable to white-collar workers than they are to blue-collar workers. In particular, for

[2] According to Austrian law, white-collar workers are employees who carry out commercial tasks (*kaufmännische Dienste*), non-commercial higher tasks (*höhere, nicht kaufmännische Dienste*) or clerical work (*Bürotätigkeiten*). Conversely, blue-collar are entrusted mainly with manual tasks.

blue-collar workers the maximal duration of continued wage payments in the case of sickness is fixed within a calendar year, regardless of how many times a worker falls ill, whereas white-collar workers may claim longer periods of wage payments within a calendar year if they fall ill repeatedly. Social security contributions are slightly higher (by 1 percentage point of gross wage) for blue-collar workers than they are for white-collar workers. In addition, blue-collar and white-collar workers also differ with respect to their periods of notice and their treatments in the case of dismissal (see Table 1).

Until September 2000, firms received a refund for the wages they had paid to sick blue-collar workers. The amount of the refund depended on a firm's total wage bill. If a firm's total monthly wages exceeded a threshold, namely 180 times the maximum daily social security contribution, it was considered a large firm. This classification was based on the wage bill in month t for sicknesses in month $t+2$. The threshold for this definition was €18,836.82 in 2000, which corresponded to approximately 10 full-time blue-collar workers if they were paid the monthly median wage (€1,822). Although the refund compensated firms only for blue-collar workers' absences, the definition of a small firm was based on the wages of both blue-collar and white-collar workers. The compensation was paid automatically within three months by the social security administration.

The compensation fund (*Entgeltfortzahlungsfond*) was financed by employers' contributions of 2.1% of their blue-collar workers' wages. This fund was managed by the Austrian social security administration. Upon its abolishment on September 30, 2000, only sickness episodes that started before this date were eligible for a refund and no refunds have been provided since January 2001.[3]

Small firms received 100% and large firms received 70% of the wages of sick blue-collar workers. The regulation intentionally favored smaller firms because they were assumed to have more problems covering sickness absences compared with larger firms. In particular, small firms may need to hire replacement workers if any of their permanent employees are sick. Large firms, by contrast, can often cover for sick workers by reallocating tasks within the firm.[4] There was no refund for the wages a firm had to pay white-collar workers when they were sick.

Contemporaneously to the abolition of the compensation fund, also the entitlement to continued wage payment of blue-collar workers was reformed. Until 2000, blue-collar workers had a less favourable regulation than white-collar workers. For equal tenure their maximum continued wage payment durations in case of sickness were two weeks shorter than they were for white-collar. Since January 2001, both blue-collar and white-collar employees are entitled to at least six weeks of continued wage payments in the case of sickness or accident. A worker that has a tenure of five years has an entitlement

[3] From October 2000 until September 2002, firms received no compensation for their sick workers' wages. Since September 2002, small firms have been receiving a compensation of 50% of the paid-out wages towards their sick workers' wages, if the sickness absences lasted longer than 10 days. Small firms are now defined as firms where the average employment is less than 51 employees per year. A firm is also considered to be small if the average number of employees is 53 or fewer because of the employment of apprentices or disabled workers. Because of the different definitions of firm size, we do not analyze firms' behavior in the later regime.

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

of up to eight weeks, a worker that has a tenure of 15 years has an entitlement of up to 10 weeks, and a worker that has a tenure of 25 years or more is entitled to up to 12 weeks of continued wage payments.

3 Theoretical motivation and empirical research design

The theoretical motivation for our analysis is rooted in the incentive structure which was inherent in the Austrian sickness absence regulation until September 2000. In addition to the moral hazard problem that exists because workers have an incentive to remain absent more often and longer than is necessary, a moral hazard problem arose because firms were insured against their blue-collar workers' sickness absences. The availability of this refund may have caused firms to monitor their absent workers less, which would result in higher absence rates.[5]

In fact, a firm will monitor its workers and try to detect shirkers only if the expected gains from monitoring (such as saved sickness payments) are higher than the monitoring costs. A refund of the wage payments incurred due to workers' sickness absence diminishes the potential gains of monitoring on the part of the firm. This implies that a sufficiently high refund of sickness wage payments will cause the firm to stop monitoring its absent workers for shirking. Depending on the costs of monitoring, even refund rates of less than 100% will result in inefficient levels of monitoring.[6]

[5]Firms might even encourage prolonged absences, for example if demand is low.

[6]It should be mentioned that, in addition to direct costs in form of continued wage

The inefficient monitoring of workers will lead to more frequent and longer sickness absences. Thus, the abolition of the refund in September 2000 and the change in sickness behavior around this date will provide evidence of the extent of firms' moral hazard problems. Large firms, because they received only partial compensation for their blue-collar workers' sicknesses, had a relatively stronger incentive to monitor their workers' absences and to encourage earlier returns to work.

We analyze how firms responded to the end of the refund for sickness absences by exploiting the differential impacts of the reform across different sized firms and workers that have different qualifications. Our estimation is a difference-in-differences-in-differences (DDD) specification that enables us to exploit the double variation in treatment across firms and worker groups. This empirical strategy gives us a precise causal estimate of the degree of moral hazard inherent in the full refund policy that existed prior to the reform for small firms in comparison with the partial refund for large firms. The DDD model is also adequate to quantify the total impact of the reform on sickness absence in Austria.

Our unit of observation is the firm, and we analyze the average number of sickness leaves per worker in the firm (the extensive margin) and the average duration of sickness leave per worker (the intensive margin). For each firm i in month t, we take the sum of sickness spells recorded for blue-collar workers

payments, workers' absences can cause indirect costs to the firm, such as the disruption of an assembly line. Theoretically, if these costs are substantial, they will provide a rationale for monitoring absences even in the presence of a large refund of direct costs. In most cases, however, it is unlikely that these indirect costs exceed a fraction of the direct costs represented by continued wage payments.

and divide it by the number of blue-collar workers. Similarly, we sum the sickness spells recorded by white-collar workers and divide it by the number of white-collar workers. The same is done to calculate sickness durations for each firm and month. Sickness days are always assigned to the month in which the spell began, also in cases where these spells extended to the following month.

We distinguish between sicknesses in small and large firms and before and after the reform. An indicator, blue-collar, is set to unity if the sickness absence was recorded by a blue-collar worker and to zero if it was recorded by a white-collar worker. We regress the sickness indicator, y_{itc}, of worker type c in firm i in month t on a set of explanatory variables:

$$\begin{aligned} y_{itc} \quad = \quad & \beta_0 + \tau(\text{blue-collar} \times \text{period} \times \text{small})_{itc} \\ & + \beta_1 \text{blue-collar}_{itc} + \beta_2 \text{period}_{itc} + \beta_3 \text{small}_{itc} \\ & + \beta_4 (\text{blue-collar} \times \text{period})_{itc} + \beta_5 (\text{blue-collar}_{itc} \times \text{small}) \qquad (1) \\ & + \beta_6 (\text{period} \times \text{small})_{itc} + X'_{it}\beta + \varepsilon_{itc}, \end{aligned}$$

where β are parameters to be estimated and τ, the coefficient on the triple interaction term, is the parameter of interest. (For the ease of the interpretation of the estimated coefficients as elasticities, we scale y_{itc} by the average for each group, $\bar{y}_c$.) The parameter τ gives the causal change in sicknesses for blue-collar workers in small firms because of the end of the refund of sickness costs. The vector X contains firm characteristics, for example the fraction of women in the firm, and a linear trend. Standard errors are clustered by firm.

We assess the overall impact of the reform by summing the effects for blue-collar workers in small and in large firms. All coefficients that are necessary to quantify these effects can be recovered from the DDD model.[7]

Our identification strategy has to deal with two issues. The first refers to the harmonization of blue-collar and white-collar workers' entitlements with respect to the maximum duration of wage payment in case of sickness, which was implemented in concomitance with the abolition of the compensation fund. After the reform, the maximum duration for blue-collar workers was extended by two weeks for every level of tenure, so that blue- and white-collar workers had the same continued wage payment entitlement. As a result we would expect an increase in the duration (although not necessarily in the incidence) of sickness spells for blue-collar workers. This effect runs contrary to the expected impact of the compensation fund abolition, which should have eliminated the moral hazard on the part of the employers and accordingly reduced blue-collar workers' absenteeism. The treatment coefficient in our DDD model might thus be biased downwards and will have to be interpreted as a lower bound of the effect of refund abolition on sickness absence levels.

The second issue concerns possible compositional shifts in firm size and workforce composition due to the abolition of the refund. Before the reform was implemented, firms at the margins of the size threshold had an incentive to be considered small in order to qualify for the full refund. These firms could have kept their wage bill deliberately below the size threshold to take advantage of the additional 30% refund. After the reform, they no longer

[7] For small firms, the impact of the reform is given by $\tau + \beta_4$ and the change in sickness absence in large firms corresponds to β_4.

had an incentive to stay small and their change in size category might bias the estimation of treatment effects. In addition, the reform could have induced firms to substitute away from blue-collar workers as their relative price increased (slightly) relative to white-collar workers.

In both cases, there are considerations that suggest these effects were of limited scope. Sorting into a firm size category was complicated by the fact that the wage bill and its development over time depends not only on the number of workers, but also on promotions, industry-wide wage bargaining and turnover. Substitution processes between blue- and white-collar workers, on the other hand, are unlikely in the short-term because workers are classified as blue-collar or white-collar workers depending on the tasks they perform. Such a substitution would require, for example, restructuring the production process.

The following evidence helps to assess empirically the relevance of compositional shifts that might jeopardize the identification of treatment effects.

Figure 2 plots the average monthly proportion of blue-collar workers in small and large firms between 1998 and 2002. According to this plot, we can observe a slow but steady decline in the share of blue-collar workers.[8] The plot suggests that there was no visible pattern of substitution between blue- and white-collar workers after the reform (over and above the long-term trend). Further evidence comes from Table 2, that shows how the decrease in blue-collar employment between 2000 and 2001 did not deviate from that in

[8]This reduction is part of a consolidated trend and can be linked to the long-term decline in the share of manual jobs in post-industrial economies. Between 1995 and 2003, for instance, the number of blue-collar workers in the Austrian economy fell by 6.2%, whereas white-collar workers increased by 7.8% Austria (2004).

previous years. There was a comparatively strong reduction in the share of blue-collar workers in January 2002 as compared to January 2001 - but this pattern can be observed for small and large firms alike and it was actually reversed in the course of the year (see October values).

Analogously, Table 3 indicates that the reform of sickness compensation did not coincide with an overproportional reduction in the share of small firms in the economy. The selection of data points shows a consistent pattern over time, with a higher number of small firms at the end and a lower number at the beginning of the year (as small firms typically exit with the end of the calendar year). In order to provide conclusive evidence on this issue, we will carry out robustness checks of our DDD estimations excluding firms that changed size category (from small to large) after the reform. The corresponding results, which are presented in section 6.2, corroborate the conclusion that compositional shifts should not be of concern for our identification strategy.

4 Data and descriptives

We use register data from the ASSD for Upper Austria, one large state that in 2000 accounted for approximately 17.5% of workers and 18% of firms (NACE C-E) in Austria (Austria, 2009).[9] These data provide information on all employees in dependent employment but do not include the self-employed or civil servants. Because each worker can be linked to a particular employer

[9] Zweimüller, Winter-Ebmer, Lalive, Kuhn, Wuellrich, Ruf and Büchi (2009) provide a detailed description of these data. The ASSD contains matched employer-employee data detailing the labor market history of private sector workers on a daily basis.

via a unique firm identifier, we can construct firm-level information, such as firm size and the number of sickness absences or their average durations, in a particular firm. We augment these data with information on statutory health insurance. The health insurance data provide information on sicknesses, in particular on days of paid sick leave.

Our sample consists of all firms with at least one employee in the period January 1998 to September 2002. We compare firms' sickness indicators for the period January 1998-September 2000 with those of January 2001-September 2002. We selected these two periods to exclude the period in which the compensation fund was phased out (October to December 2000) and to minimize variation in sicknesses owing to the seasonality of sicknesses. We provide estimates from different periods in our robustness checks in section 6.3.

A specific data issue concerns short sickness spells of up to three days. Labor legislation mandates that workers must provide a medical certificate for all absences of more than three days. Employers are also allowed to ask their employees to provide a certificate for sick leaves of shorter durations. Because not all firms request a medical certificate from the first day of absence, short absences are not fully covered by the administrative data. The abolition of the refund in 2000 may have influenced the way in which firms handle short absences of less than four days. Because firms receive no refunds after the reform, they have a lower incentive to require workers to supply a doctor's note for short sicknesses. By contrast, an increase in monitoring effort following the end of refunds might work exactly in the opposite direction and compensate for the disincentive to request a medical certificate. To avoid the possible distortions resulting from these effects we will restrict our analysis to spells of durations that are longer than three days. We also restrict the sample to those spells that last up to 42 days, i.e. the minimum duration for which companies have to pay wage compensation after the reform.[10] In total, the spells between 4 and 42 days days of length account for about three quarters of all sickness absence episodes recorded in the administrative statistics.

The final sample consists of 2.3 million firm-months observations. Table 4 shows the mean number of sickness episodes and their durations in small and large firms by blue-collar and white-collar workers and by period. Because it may be easier to extend a period of sick leave compared with obtaining sick leave when not actually sick, we expect a stronger reaction of the sickness durations than of the incidences. If the refund provided an incentive for firms to monitor their sick workers less, we expect to see that the average sickness incidences and durations of blue-collar workers decreased both in small and in large firms. Because the refund was larger for small firms, we expect the reaction in small firms to be greater than it was in large firms. The values for white-collar workers should have remained unchanged or have changed less than those for blue-collar workers.

These expectations are supported by the mean values in Table 4. We see that the average incidences and durations for blue-collar workers decreased

[10]Since the reform extended the minimum duration of wage compensation for blue-collar workers from 4 to 6 weeks, we will also carry out a robustness check restricting our sample to spells with a duration of up to 4 weeks (see section 6.3).

and that this reduction was significantly stronger in small firms than in large firms. Blue-collar workers in small firms recorded 17.9% less absence spells and 19.0% less sickness days after the reform than before, in large firms the drop amounted to 6.9% and 8.6%. The sicknesses of white-collar workers decreased too, although by much less than those of blue-collar workers.[11] It is interesting to note that the decrease in white-collar absenteeism was stronger in small firms than it was in large firms. This patterns suggests that in small firms stronger monitoring in the wake of the reform affected not only blue-collar, but also white-collar workers. Figures 3 to 6 show that blue-collar workers in small and large firms have similar sickness patterns, in particular an increase in sickness absences during winter months. Small firms have lower levels of absenteeism than large firms, this is true for both blue- and white-collar workers and is in line with theoretical and empirical findings from the absenteeism literature (see for instance Barmby and Stephan (2000); Winkelmann (1999).

Tables 5 presents the summary statistics of our sample by firm size and period. The average values for the incidences and durations of sicknesses mirror the results in Table 4. We see that there were fewer and shorter sicknesses after the reform and that this change was more pronounced in small than it was in large firms. The summary statistics for the other characteristics indicate that over time firms increased the fractions of women, white-collar employees and older workers in their workforce. These long-term trends concern small and large firms in equal measure.

[11]The reduction in sickness absence over the observed period across both worker groups is part of a long-term decline in sickness absence in Austria. In 1980 every Austrian worker recorded on average 17.4 sickness days per year, in 2011 only 13.2 days.

5 Estimation results

Table 7 shows the DDD estimates of average sickness durations. The specifications differ in the included covariates in that the first specification has no covariates other than the indicator variables. A set of firm characteristics is included in specification 2. Specification 3 additionally includes a linear trend, and specification 4, our preferred specification, also includes a group-specific linear trend for small firms. The causal effect is given by the estimated coefficient on the triple interaction term described above. (All listed coefficients can be interpreted as elasticities.)

In each of these specifications, the estimated causal effect is statistically significant at conventional error levels. This suggests that durations decreased on average by 8.1% more in small firms than they did in large firms following the removal of the refund. Note that the estimates are almost identical across specifications. Table 6 presents the results for the incidences of sickness. We find that the sickness incidences of blue-collar workers in small firms were significantly lower after the end of the refund period than they were in large firms. The effect is similar in magnitude to the effect found for sickness durations, the reduction amounted to 8.9%. These are large effects, and they imply that firms' moral hazard problems because of the refund were substantial and led to inflated sickness absences.

The overall effect of the reform is the total change in blue-collar workers' sickness absences caused by the abolition of the refund policy. For small firms, this effect is given by $\tau + \beta_4$ and corresponds to a reduction by 17.2% in incidences and by 17.8% in durations. The change in large firms (expressed by the coefficient β_4) corresponds to a decrease by 8.3% in incidences and by 9.7% in durations. Together these reductions account for 10% of blue-collar absence spells and 11.3% of blue-collar absence days across all firms. This corresponds to 6.6% of all absences and 7.6% of all absence days recorded by private sector employees (with the restriction that we take into account only absence spells between 4 and 42 days of duration).

6 Robustness

Our estimates indicate that the abolishment of the refund reduced total absenteeism in the economy significantly and resulted in almost 9% fewer and approximately 8% shorter blue-collar workers' sicknesses in small than in large firms. We provide a series of robustness checks to gauge the reliability of these results.

6.1 Spell duration

As previously stated, the abolition of the refund in 2000 may have influenced the way in which firms handle short absences of less than four days. Because firms receive no refunds after the reform, they have a lower incentive to require workers to supply a doctor's note for short sicknesses. By contrast, an increase in monitoring effort following the end of refunds might work exactly in the opposite direction and compensate for the disincentive to request a medical certificate. In our first robustness check, we thus include in our sample all absences between one and three days. The results in Table 8, column 1, show that the estimated treatment effects differ only marginally from those in the main specification.

In our main specification, we restricted the sample to absences of up to 42 days because this is the maximum duration a firm has to pay wages for sick workers that have tenures of up to five years. Arguably, workers who are absent longer are sick more severely. If we assume that severe sicknesses are more difficult to manipulate compared with shorter sicknesses, then we expect to find a lesser treatment effect if long sickness durations are included in the estimation. The estimation results for the sample with no restriction on the upper bound of sickness duration are presented in column 2 of Table 8. As can be seen, the differential impact of the reform on small firms as opposed to large firms does not change after inclusion of very long sickness spells. However, the β_4 coefficients (*blue* $\times$ *after*, not shown in the table) are smaller than for the sample restricted to spells with a maximum length of 42 days.[12] This clearly indicates that the inclusion of very long spells does indeed lessen the total impact of the reform, albeit not its asymmetric effect on small and large firms.

For completeness, we also estimate our model with a sample restricted to those spells that lasted between 4 and 28 days. Before the reform and the extension by two weeks, four weeks corresponded to the maximum duration of continued wage payment for blue-collar workers with less than five years of tenure. We find that the DDD coefficients for this sub-sample of spells

[12] As we would expect, the difference is particularly pronounced for durations, where β_4 amounts only to -4.1% as opposed to -9.7% in the main specification. With respect to incidences, the difference is smaller, 7.8% vs. 8.3%.

hardly differ from those in our main specification (see column 3 of Table 8).

6.2 Compositional effects and sample restrictions

The differential treatment of small and large firms within the compensation refund scheme may have triggered strategic behaviour. Under the old regime, firms had an incentive to remain deliberately small. As a result, faster growth and an overproportional number of size changes in the wake of the reform might have biased our previous treatment effect estimates. We therefore carry out a battery of analyses to test whether there was a significant compositional effect due to strategic firm behaviour and whether our estimation results are sensitive to sample restrictions.

First, we attempt to identify strategic behaviour by looking at firm growth and changes in size category before and after the reform. Table 12 indicates that the number of small firms that crossed the threshold to become a large firm was not affected by the reform. On average, roughly 0.5% of the small firms we observe in our sample at time t switch to being a large firm one month later. This share was equally high in the periods January 1998-September 2000 and January 2001-September 2002, as well as in the months immediately after the reform (September 2000-March 2001), suggesting that strategic adaption of firm size did not occur on a significant scale.

In a second step, we carry out additional estimations after eliminating "strategic" firms from our sample. We apply a broad definition of strategic behaviour, that encompasses firms that were prevalently (i.e. two thirds of the time) small before the reform and prevalently large after the reform.

Estimated treatment coefficients differ only negligibly from those presented in section 5. We then restrict the sample to firms that do not change their size category at all, namely those firms that are always small or always large. This removes 155,894 observations from our overall sample. The resulting sub-sample contains only firms observed both before and after the reform and thus eliminates also the potential bias due to the exit after the reform of firms that had high absence rates. As can be seen from column 2 in Table 8, the estimated DDD coefficients for this restricted sample are slightly higher than those for the full sample.

All above results were restricted to working age employees, i.e., men younger than 65 and women younger than 60 years of age. Table 8 reports also estimation results based on a sample excluding workers aged over 55 years. The aim of this check is to eliminate a potential source of bias because older workers' absence behaviour might be influenced by changes in pension legislation and in the handling of claims to disability pension. The treatment effects for this sample are almost identical to those for the full sample and confirm the robustness of our results.

Finally, we restrict the sample to non-seasonal sectors because firms in these sectors may differ in their degree of monitoring compared with seasonal firms. This reduces the number of observations to 1,466,997. In this sub-sample, the reform led to a reduction in incidences and durations that were sligthly stronger than those observed in the full sample (-10.1% and -9.4%, respectively). This suggests that prior to the reform the moral hazard problem was less accentuated in the seasonal sectors of the economy.

6.3 Anticipation effects and comparison of different periods

In the next step, we vary the observational window and compare the year 1999 with the year 2001, which provides us with a sample of 1,065,436 observations. This restriction of the observational period aims at avoiding potential announcement effects due to the media coverage of the reform. An analysis of the Austrian media shows that there was hardly any public discussion on the proposed law before April 2000. After April 2000, the Austrian media started to report on the proposed changes and, especially before the start of the new rules, reported on the abolishment of the refund. The ensuing anticipation effect could represent a source of bias for our analysis. This would occur if sickness absences increased while the reform was announced but the subsidy still in place. Conversely, announcement effects could also have led firms to implement monitoring procedures or to fire sick workers prior to the reform, which would lower its treatment effect. The estimation results on sickness incidence, which are presented in column 1 of Table 10, are very close to those based on the full period. The coefficient on durations is slightly lower and less statistically significant than in the main specification (6.9%), suggesting that in the run-up to the abolition of the refund an extension of sickness absence periods took place.

To test conclusively for the existence of anticipation effects, we also estimated a "placebo -treatment". The test aims to clarify whether the announcement of the reform in the media in April 2000 led to a change in sickness behavior. We therefore construct the DDD dummies as if the reform was introduced in April 2000. In this estimation, all observations before that month are in the pre-reform period, whereas all observations between April 2000 and December 2000 are in the post-treatment period. The estimation results clearly indicate that there was no treatment effect associated with the announcement of the reform, as both our incidence and duration coefficients are statistically insignificant (column 2 of Table 10).

If we select the same number of months before and after September 2000, we can compare the period of January 1999 to September 2000 with the period of January 2001 to September 2002 (N=1,781,552). In this case, too, the estimated DDD treatment effect for sickness incidence varies little with respect to the main specification, whereas the effect on durations is smaller (5.8%) and only statistically significant at the 5%-level (Table 10, column 3).

It is worthwhile to point out that in both instances where the DDD coefficient on sickness durations is comparatively low and less statistically significant than in the main results, the β_4 coefficients (*blue* × *after*, not shown in the table) are higher than those presented in section 5. Accordingly, although the restriction to different observational windows reduces the differential impact of the reform on sickness durations in small firms compared to large ones, there is virtually no difference with respect to the reforms' total impact on durations.

6.4 Difference-in-differences

If white-collar workers are not an appropriate control group for blue-collar workers, because the reform changed the monitoring levels of all types of workers, then our DDD estimates are biased. Note that if the reform led firms to more strictly monitor both their blue-collar and their white-collar workers, the DDD model underestimates the true reduction in sicknesses caused by the reform. (The underestimated treatment effect is, therefore, a lower bound of the true treatment effect.) A difference-in-differences comparison of blue-collar workers' absences in small and large firms, before and after the reform, will estimate the extent of the change in sickness behavior for blue-collar workers because of the end of the refund period.

The estimates presented in Table 11 indicate that blue-collar workers in small firms had fewer and shorter sickness spells after the reform in comparison with blue-collar workers in large firms. Although the estimated coefficients are somewhat smaller (in absolute value) than the effects we obtain from the DDD estimates, the results provide corroborating evidence of firms' moral hazard problems.

Because the refund was only available for the sicknesses of blue-collar workers, the end of the refund period should have had no effect on how firms treated white-collar workers; therefore, we should not be able to estimate statistically significant effects for the sicknesses of white-collar workers. Our estimates show that this expectation is fulfilled: The estimated treatment effects for sickness incidences and durations of white-collar employees are positive and not statistically different from zero. This indicates that the end of the refund period did not significantly affect the absenteeism of white-collar workers and that our estimates from the DDD model represent a good measure for the actual response to the reform.

7 Conclusion

We analyzed sickness absences in small and large firms that had received different compensations for the wages they paid to their sick workers. Small firms received more compensation than did large firms because of their presumed difficulties in covering the tasks usually carried out by sick workers. Using administrative data, we found robust evidence for firms' moral hazard problems using the differential treatments of small and large firms and of blue-collar and white-collar workers as sources of variation. We identified a causal effect by comparing sickness behavior in two different policy regimes, namely one with and one without compensation. We estimated that the incidences of blue-collar workers' sicknesses in small firms dropped by approximately 17% and that sickness durations were almost 18% shorter after the reform. As expected, the effect of the reform on large firms was less pronounced. Overall, the reform reduced the number and durations of sickness absences in the private sector by 6.6% and 7.6%, respectively. These calculations refer to sickness absence spells of 4–42 days length, which represent roughly three quarters of total absenteeism.

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 and, according to Scheil-Adlung and Sandner (2010), as many as 145 countries provide paid sick leave.[13] Similar settings to the one analyzed here exist, for example, in Germany, Denmark, and the UK. In Germany,

[13] In 1883, the German Chancellor Otto von Bismarck initiated sickness insurance legislation that included paid sick leave for workers in the case of illness for a period of 13 weeks.

firms that have fewer than 30 employees are eligible for a refund of 80% of the wages paid to sick workers. These examples could be expanded to all instances where the sick pay system fails to provide adequate incentives for firms to monitor their employees' absences. Clearly, the moral hazard problem is exacerbated in institutional settings—such as the Austrian case until 2000—where (some) firms have little incentive to monitor absenteeism, while at the same time workers benefit from high replacement rates during sickness.

In 2000, the Austrian social security administration counted approximately 25 million days of blue-collar workers' absenteeism that were caused by sickness spells of between 4 and 42 days. If, for simplicity, we assume that the distribution of workers and firms in our sample is representative of other Austrian provinces, our estimated reduction by 7.6% would correspond to approximately 1.9 million fewer sickness days for blue-collar workers.[14] If we approximate the economic costs of an absence day using the median daily gross wage of €60.4 in 2000, the estimated savings would be approximately €114.7 million (corresponding to approximately 7.2% of the total costs of continued wage payments for all absences (regardless of duration) in that year).[15] These figures represent reference points for situations that are comparable to the case investigated here. They strengthen the argument for legislation that calls on firms to carry a proportion of the costs of sick pay,

[14] Due to its strong manufacturing base, the Upper Austrian economy is characterised by a comparatively high share of blue-collar workers (45.3% of total dependent employment against a national average of 41.2%, in 2000) and by above-average firm size (10.1 workers per place of employment against a national average of 8.6).

[15] The Austrian Ministry for Social Affairs estimated the costs sustained by private companies to be €1.58 billion in 2000 (Ministry for Social Affairs, 2011).

especially with respect to short and medium absence periods.

It would, however, be wrong to conclude that shifting the burden to firms, especially if this shift were substantial, would necessarily increase overall welfare in an economy. A shift of sick pay costs—be it through the abolition of refunds or the extension of the employer's liability period—could result in a worker's health status playing an increased role in firms' hiring (and firing) decisions, which may lead to unintended effects on the employment of workers suffering from poor health.

Recent results by Fevang et al. (2011) confirm the expectation that firms respond to an increase in liability by employing fewer workers with a (presumed) propensity for absences, such as older workers or pregnant women. Fevang et al. (2011) corroborate the view that firms influence the absence behavior of their workers and that they react quickly to incentives. Discussions on sickness absence regulations have stressed the demand-side and supply-side aspects of absence behavior (Bonato and Lusinyan, 2007; Rae, 2005). The optimal design of sickness insurance legislation thus has to consider adequate risk coverage, while containing the moral hazard problems that affect workers and firms.

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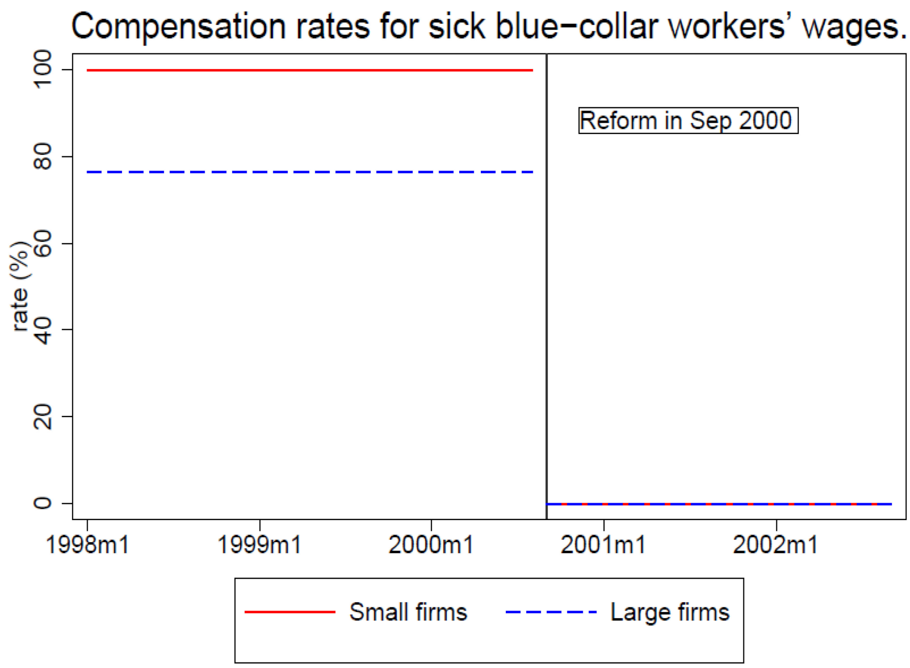
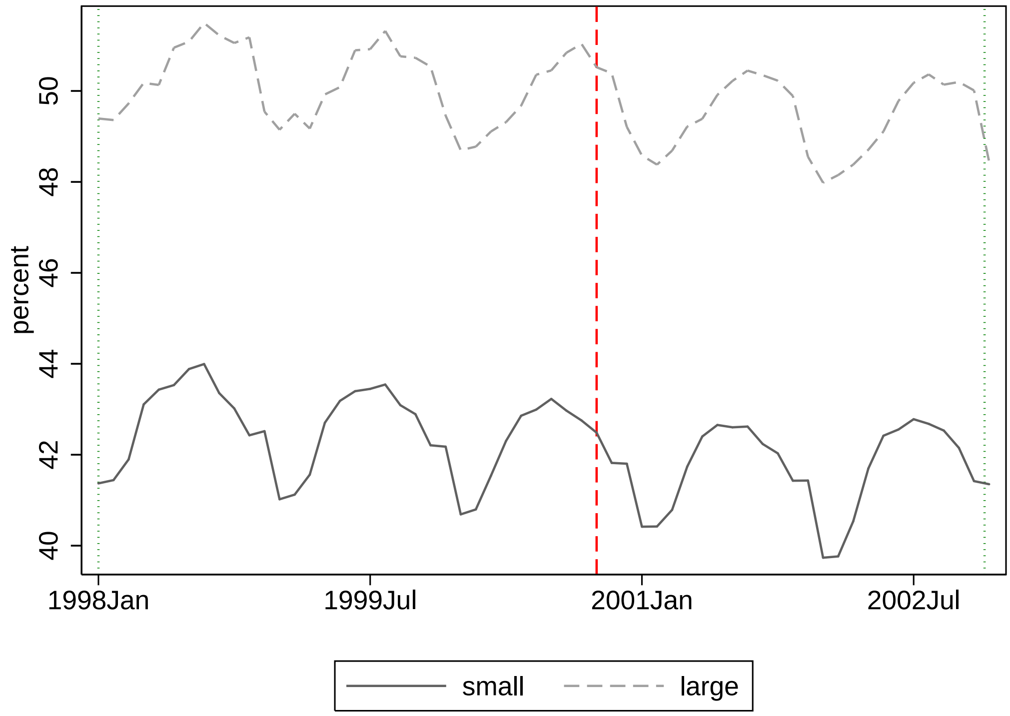
A Figures and Tables

Figure 1: Compensation regimes



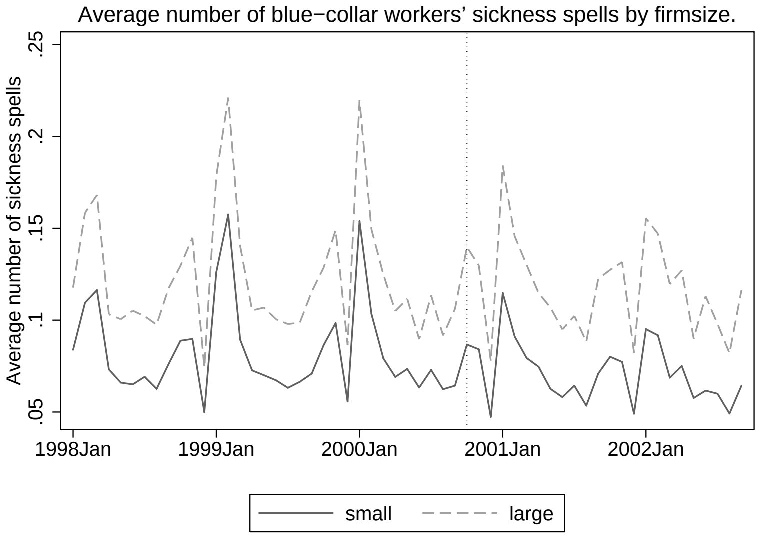
Note: Per cent of gross wages and employers social security contribution.

Figure 2: Blue-collar workers, by firmsize.



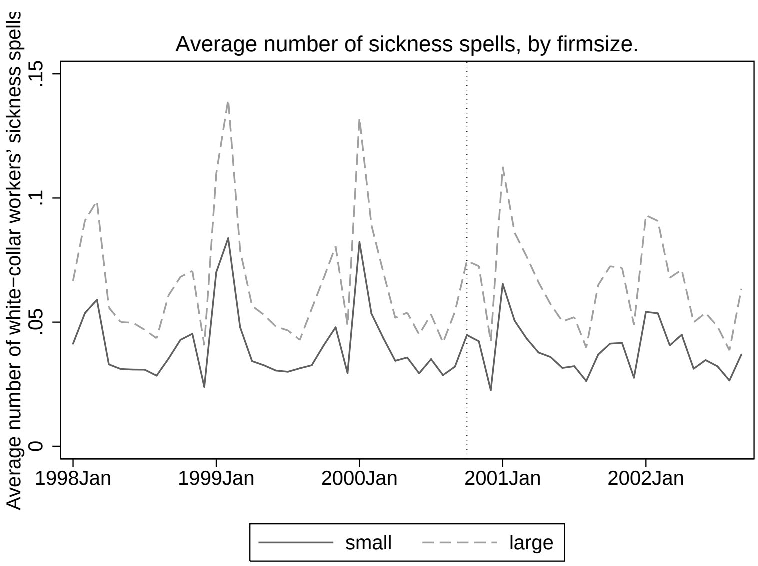
Note: Averge percentage of blue-collar workers in month, by firm type. Reform indicated by dashed line. Period for analysis indicated by dotted lines.

Figure 3: Sicknesses of blue-collar workers, by firm size.



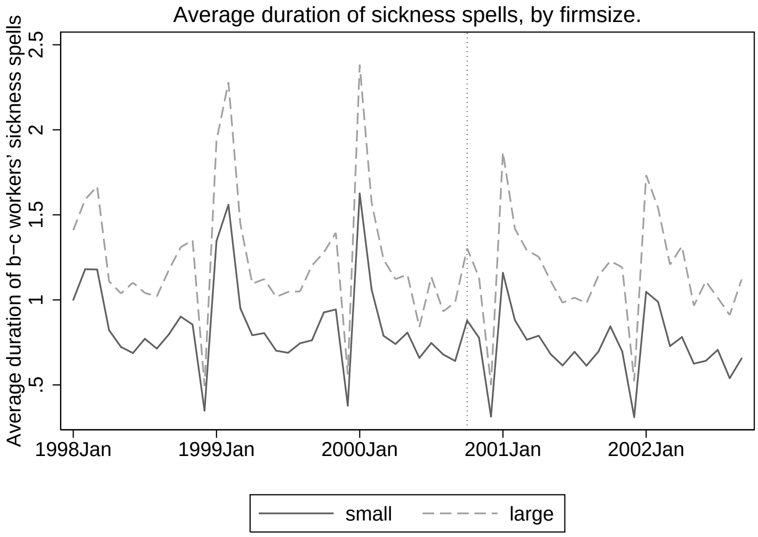
Note: Average of blue-collar workers' sickness in firms. Monthly observations. Only spells with length 4-42 days.

Figure 4: Sicknesses of white-collar workers, by firm size.



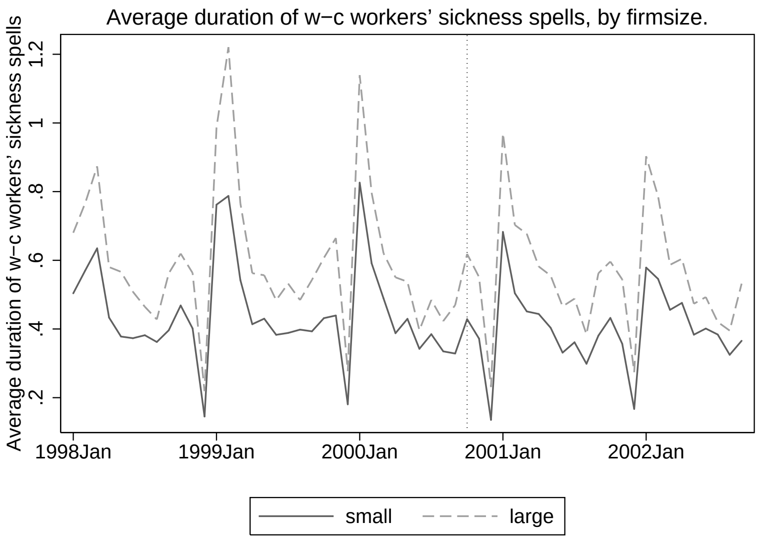
Note: Average of white-collar workers' sickness in firms. Monthly observations. Only spells with length 4-42 days.

Figure 5: Sickness durations of blue-collar workers, by firm size.



Note: Average of blue-collar workers' sickness duration in firms. Monthly observations. Only spells with length 4-42 days.

Figure 6: Sickness durations of white-collar workers, by firm size.



Note: Average of white-collar workers' sickness duration in firms. Monthly observations. Only spells with length 4-42 days.

Table 1: **Labor law differences between blue- and white-collar workers.**

	2000 (before reform)		2001 (after reform)	
	Blue-collar	White-collar	Blue-collar	White-collar
Health insurance contribution	7.9%	6.9%	7.6%	6.9%
Period of notice	min. 1 day	min. 6 weeks	min. 1 day	min. 6 weeks
Maximum wage payment in case of sickness				
Within calendar year	fixed	extendable	fixed	extendable
At tenure				
$<$ 5 years	4 weeks	6 weeks	6 weeks	
$>$ 5 years	6 weeks	8 weeks	8 weeks	
$>$ 15 years	8 weeks	10 weeks	10 weeks	
$>$ 25 years	10 weeks	12 weeks	12 weeks	

Note: Health insurance contribution: expressed as percentage of gross wage/salary. Period of notice: For blue-collar workers it is regulated by collective agreement and generally much shorter than for white-collar workers (who are subject to a general regulation). Maximum period of wage payment within a calendar year: For blue-collar workers it is fixed, regardless of how many times a worker falls ill, whereas white-collar workers may claim longer periods of wage payments within a calendar year if they fall ill repeatedly.

Table 2: **Fraction of blue collar workers.**

Year	All firms Mean values			Small firms Mean values		
	Jan	Oct	Dec	Jen	Oct	Dec.
1998	0.429	0.446	0.438	0.414	0.430	0.425
1999	0.426	0.444	0.435	0.410	0.429	0.422
2000	0.422	0.440	0.431	0.407	0.425	0.418
2001	0.420	0.436	0.427	0.404	0.420	0.414
2002	0.413	0.437	.	0.397	0.421	.

Note: Average values in January, October, and December.

Table 3: **Fraction of small firms in the economy.**

Year	Mean values		
	Jan	Oct	Dec
1998	0.810	0.809	0.823
1999	0.810	0.811	0.823
2000	0.808	0.806	0.821
2001	0.809	0.808	0.820
2002	0.808	0.805	.

Note: Average values in January, October, and December.

Table 4: **Sickness absences in small and large firms**

Firm size	White-collar		Blue-collar	
	Mean values			
	Before	After	Before	After
Large				
Incidence	0.049	0.047	0.101	0.094
Duration	0.459	0.428	1.008	0.927
Spell length	9.062	8.663	10.247	10.229
N	128,724	85,144	113,020	72,646
Small				
Incidence	0.031	0.029	0.066	0.055
Duration	0.326	0.297	0.687	0.558
Spell length	10.020	9.653	10.225	10.141
N	691,470	455,320	520,890	332,281

Note: Sample: 1/1998-9/2000 and 1/2001–9/2002. Sickness spells 4-42 days. Includes zeros.

Table 5: **Summary statistics**

	Small firms				Large firms			
Indicators	Before		After		Before		After	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Incidence (4-42)	0.058	0.194	0.053	0.182	0.092	0.128	0.090	0.123
Duration (4-42)	0.621	3.248	0.552	3.205	0.903	1.916	0.855	1.890
Incidence (all)	0.169	0.498	0.156	0.471	2.266	4.894	2.340	5.076
Duration (all)	1.694	6.877	1.527	6.698	22.182	52.258	22.626	53.648
Firmsize (ln)	3.832	3.532	3.826	3.591	42.887	59.183	44.668	61.358
Wage (ln)	40.817	20.727	43.505	22.149	54.162	13.041	57.579	14.004
Age	37.241	8.727	37.934	8.713	36.815	5.390	37.399	5.417
Fraction								
female workers	0.550	0.408	0.555	0.408	0.334	0.269	0.342	0.273
blue-collar workers	0.418	0.392	0.410	0.394	0.503	0.291	0.496	0.297
older workers (55+)	0.043	0.162	0.046	0.167	0.038	0.049	0.039	0.050
N	1,212,357		749,420		241,744		149,925	

Note: Before: 1/1998–9/2000. After: 1/2001–9/2002. No observations from 10 to 12/2000. Incidences and durations are per worker, unit is firm x month.

Table 6: **Estimated impact on the incidence of sickness absences, spells between 4 and 42 days length.**

Variable	(1)	(2)	(3)	(4)
Treatment	-0.092	-0.088	-0.088	-0.089
	(0.021)	(0.021)	(0.021)	(0.021)
Small	0.054	0.107	0.107	0.022
	(0.019)	(0.019)	(0.019)	(0.021)
After	-0.029	-0.044	-0.058	0.057
	(0.010)	(0.010)	(0.013)	(0.017)
Blue	1.087	1.077	1.077	1.076
	(0.017)	(0.017)	(0.017)	(0.017)
Small*after	0.010	0.007	(0.007)	-0.132
	(0.012)	(0.012)	(0.012)	(0.020)
Blue*after	-0.090	-0.083	-0.083	-0.083
	(0.017)	(0.017)	(0.017)	(0.017)
Small*blue	-0.204	-0.239	-0.239	-0.239
	(0.021)	(0.021)	(0.021)	(0.021)
Trend			0.000	-0.003
			(0.000)	(0.000)
Small*trend				0.005
				(0.001)
adjusted R^2	0.010	0.017	0.017	0.017
N	2,399,489			

Note: The table summarizes the estimation results of the effect the abolishment of the refund had on the incidence of sickness absences. Method of estimation is OLS. Listed coefficients multiplied by 100 give the percent change in the sickness incidence due to the abolishment of the refund. Robust standard errors clustered by firm (allowing for heteroskedasticity of unknown form) are in parentheses. 2,399,489 firm-month observations. Other covariates in specifications (2)–(4) are ln(average wage), ln(firm size), fraction of women, fraction of blue-collar workers, fraction of workers aged 55+, average age, indicators for industry and for quarter.

Table 7: **Estimated impact on the incidence of sickness durations, spells between 4 and 42 days length.**

Variable	(1)	(2)	(3)	(4)
Treatment	-0.085	-0.080	-0.080	-0.081
	(0.024)	(0.024)	(0.024)	(0.024)
Small	0.104	0.128	0.128	0.050
	(0.021)	(0.022)	(0.022)	(0.025)
After	-0.037	-0.066	-0.052	0.053
	(0.011)	(0.011)	(0.015)	(0.018)
Blue	1.118	1.119	1.119	1.118
	(0.017)	(0.017)	(0.017)	(0.017)
Small*after	0.017	0.008	(0.008)	-0.120
	(0.014)	(0.014)	(0.014)	(0.023)
Blue*after	-0.099	-0.097	-0.097	-0.097
	(0.019)	(0.019)	(0.019)	(0.019)
Small*blue	-0.309	-0.302	-0.301	-0.301
	(0.023)	(0.023)	(0.023)	(0.023)
Trend			0.000	-0.004
			(0.000)	(0.001)
Small*trend				0.004
				(0.001)
adjusted R^2	0.010	0.017	0.017	0.017
N		2,399,489		

Note: The table summarizes the estimation results of the effect the abolishment of the refund had on sickness durations. Method of estimation is OLS. Listed coefficients multiplied by 100 give the percent change in the sickness incidence due to the abolishment of the refund. Robust standard errors clustered by firm (allowing for heteroskedasticity of unknown form) are in parentheses. 2,399,489 firm-month observations. Other covariates in specifications (2)–(4) are ln(average wage), ln(firm size), fraction of women, fraction of blue-collar workers, fraction of workers aged 55+, average age, indicators for industry and for quarter.

Table 8: **Robustness: Spell duration.**

Variable	(1)	(2)	(3)
Incidence			
Treatment	-0.067	-0.087	-0.076
	(0.019)	(0.021)	(0.020)
N	2,399,489	2,353,446	2,399,489
adjusted R^2	0.019	0.017	..
Duration			
Treatment	-0.075	-0.082	-0.086
	(0.023)	(0.029)	(0.023)
N	2,399,489	2,353,446	2,353,446
adjusted R^2	0.010	0.006	..

Note: All specifications as in Tables 6 and 7, column 4. (1) Durations between 1 and 42 days. (2) Durations longer than three days (no upper limit). (3) Durations between 4 and 28 days.

Table 9: **Robustness: Sample restrictions.**

Variable	(1)	(2)	(3)	(4)
Incidence				
Treatment	-0.089	-0.110	-0.081	-0.101
	(0.021)	(0.023)	(0.022)	(0.027)
N	2,399,489	2,243,595	2,252,824	1,466,997
adjusted R^2	0.017	0.015	0.018	0.019
Duration				
Treatment	-0.081	-0.097	-0.072	-0.094
	(0.024)	(0.026)	(0.025)	(0.031)
N	2,399,489	2,243,595	2,252,824	1,466,997
adjusted R^2	0.009	0.008	0.008	0.010

Note: All specifications as in Tables 6 and 7, column 4; sickness spells with durations 4–42 days. (1) Without firms that changed "‘strategically"’ from small to large size. (2) Only firms that did not change size category at all. (3) Without workers aged over 55. (4) Without seasonal sectors (tourism and construction).

Table 10: **Robustness: Sampling periods.**

Variable	(1)	(2)	(3)
Incidence			
Treatment	-0.087	-0.009	-0.082
	(0.029)	(0.030)	(0.023)
N	1,065,436	1,409,054	1,781,552
adjusted R^2	0.020	..0	018.
Duration			
Treatment	-0.069[a]	0.040	-0.058[b]
	(0.034)	(0.036)	(0.026)
N	1,065,436	1,409,054	1,781,552
adjusted R^2	0.011	..0	010.

Note: All specifications as in Tables 6 and 7, column 4. All spells with durations 4-42 days. (1) Pre-period: 1999; post-period: 2001. (2) "Placebo treatment". Pre-period: 1/1999–3/2000; post-period: 4/2000–12/2000. (3) Pre-period: 1/1999–9/2000; post-period: 1/2001–9/2002. [a] p-value of 0.041. [b] p-value of 0.025.

Table 11: **Robustness: Difference in difference estimates.**

Variable	Blue-collar	White-collar
Incidence		
Treatment	-0.055	0.023
	(0.013)	(0.017)
N	999,211	1,309,188
adjusted R^2	0.009	0.006
Duration		
Treatment	-0.042	0.025
	(0.014)	(0.020)
N	999,211	1,309,188
adjusted R^2	0.005	0.003

Note: Method of estimation is OLS. Listed coefficients multiplied by 100 give the percent change in the sickness incidence due to the abolishment of the refund. Robust standard errors clustered by firm (allowing for heteroskedasticity of unknown form) are in parentheses. Other covariates are ln(average wage), ln(firm size), fraction of women, fraction of blue-collar workers, fraction of workers aged 55+, average age, indicators for industry and for quarters as well as a linear trend. Spells with durations between 4 and 42 days.

Table 12: **Firms changing size category.**

Firm size at t-1	Before			After			Around reform		
	Large	Small	Total	Large	Small	Total	Large	Small	Total
Large	315,119	6,530	321,649	261,417	5,663	267,080	51,954	1,345	53,299
	97.97	2.03	100.00	97.88	2.12	100.00	97.48	2.52	100.00
Small	7,561	1,363,038	1,370,599	6,441	1,127,781	1,134,222	1,031	223,117	224,148
	0.55	99.45	100.00	0.57	99.43	100.00	0.46	99.54	100.00
Total	322,680	1,369,568	1,692,248	267,858	1,133,444	1,401,302	52,985	224,462	277,447
	19.07	80.93	100.00	19.11	80.89	100.00	19.1	80.9	100.00

Firm size at t: Before / After / Around reform.

Note: Absolute number and fraction of firms, according to firm size at times t and t-1. Before: 1/1998-9/2000. After: 1/2001-9/2002. Around reform: 9/2000-3/2001.