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 the incidences of blue-collar workers' sicknesses dropped by approximately 8% and sickness absences were almost 11% shorter following the removal of the refund. Several robustness checks confirm these results.

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Keywords: absenteeism, moral hazard, sickness insurance.

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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. 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 September 2000 and January 2001, only sickness absences that started before September were refunded, and no refunds were made after January 2001.[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 2001 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). The ASSD is the administrative database for the calcu-

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

lation 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 the removal of the refund resulted in approximately 8% fewer blue-collar workers' sicknesses and shortened sickness absences by approximately 11%. 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 for a period of up to 12 weeks, depending on length of tenure. 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. Although this distinction has lost most of its original relevance through recent legal reform, labor law provisions generally remain more favorable to white-collar workers than they are to blue-collar workers. In particular, for 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. (Blue-collar and white-collar workers also differ with respect to their periods of notice and their treatments in the case of dismissal.)

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. 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. A worker that has a tenure of five years has an entitlement 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.[2] After this 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.

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.

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

[2]Before 2001, the maximum durations for blue-collar workers were two weeks shorter than they were for white-collar workers.

within the firm.[3] There was no refund for the wages a firm had to pay white-collar workers when they were sick.

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.[4] 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. A moral hazard problem arises not only because workers have an incentive to remain absent more often and longer than is necessary, but also because firms are insured against their blue-collar workers' sickness absences. The availability of this refund may cause firms to monitor their absent workers less, which will result in higher absence rates.[5]

Large firms, because they receive only partial compensation for their blue-collar workers' sicknesses, have a relatively stronger incentive to monitor their workers' absences and to encourage earlier returns to work. As stated,

[3]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 result-ing from absences.

[4]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.

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

the inefficient monitoring of workers will lead to more frequent and longer sickness absences. Thus, the abolition of the refund and the change in sickness behavior around this date will provide evidence of the extent of firms' moral hazard problems. Because the reform removed the differential treatment of small and large firms, we would not expect employers to take any specific course of action with respect to firm size or wage bill when learning about the pending reform.[6]

3 Theoretical motivation and empirical research design

We sketch the key features of the institutional setting with a simple conceptual model, based on Barmby, Sessions and Treble's (1994) model. Each firm i is assumed to maximizes its profits, π, subject to wage costs. (The workforce is normalized to one.) Workers receive sick pay s, $0 < s \leq w$, if they are unfit for work. Because utility increases in income and leisure, a worker has an incentive to remain absent from work, namely pretending to be sick ("shirking"). This incentive depends on the probability of detection, the consequences of being found out, and the utility from leisure. A worker's utility from leisure also depends on health, where sickness increases the value of leisure.

Assume that the firm observes in each period a fraction σ of its workers

[6] 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.

on sick leave, $0 \leq \sigma \leq 1$. A fraction τ of the workers that are on sick leave is genuinely unfit for work, $0 \leq \tau \leq 1$, whereas $1 - \tau$ are shirking. The firm may spend some fixed costs per worker, κ, on a monitoring technique that detects shirking with probability α, $0 \leq \alpha \leq 1$. If someone is detected shirking, the worker is fired and the firm no longer pays sick pay. A firm will monitor its absent workers if the cost of monitoring, $\kappa\sigma$, is less than are the gains from detecting shirking workers, $\alpha\sigma(1-\tau)s$. Note that a high level of s will increase the likelihood of monitoring.

Wage costs are given by:

$$\text{wage bill} = (1-\sigma)w + \tau\sigma s + (1-\alpha)(1-\tau)\sigma s + \kappa\sigma, \tag{1}$$

where $(1-\sigma)w$ are the wages paid to workers who appear for work, $\tau\sigma s$ is the sick pay for genuinely sick workers, $(1-\alpha)(1-\tau)\sigma s$ is the sick pay for shirking workers that are not detected, and $\kappa\sigma$ is the expenditure spent on monitoring absent workers.

Now consider the effects of refunds, r, with $r \leq s$, for firms' monitoring. A firm will not monitor its workers if the expected gains from monitoring are less than are the costs:

$$\begin{aligned} \alpha\sigma(1-\tau)(s-r) &< \kappa\sigma \\ r &\geq s - \frac{\kappa}{\alpha(1-\tau)}, \end{aligned} \tag{2}$$

which implies that a sufficiently high refund will cause the firm to stop monitoring its absent workers for shirking. (If $r = s$, equation (2) is trivially true.)

Depending on the costs of monitoring, even refund rates of less than 100% will result in inefficient levels of monitoring. The non-monitoring firm's wage bill is then given by the wages paid to non-absent workers, $(1-\sigma)w$, plus the difference between sick pay paid to sick workers and the refund, $\sigma(s-r)$.[7]

We analyze how firms respond to the end of the compensation 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 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.

[7]This simple model does not consider indirect costs, such as the disruption of an assembly line, that arise from worker absences. 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.

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:

$$\begin{aligned} y_{itc} &= \beta_0 + \tau(\text{blue-collar} \times \text{period} \times \text{small})_{itc} \\ &+ \beta_1 \text{blue-collar}_{itc} + \beta_2 \text{period}_{itc} + \beta_3 \text{small}_{itc} \\ &+ \beta_4 (\text{blue-collar} \times \text{period})_{itc} + \beta_5 (\text{blue-collar}_{itc} \times \text{small}) \\ &+ \beta_6 (\text{period} \times \text{small})_{itc} + X'_{it}\beta + \varepsilon_{itc}, \end{aligned} \tag{3}$$

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.[8]

[8] 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.

4 Data

We use register data from the ASSD.[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 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 data from the health insurance are from a single Austrian state, "Upper Austria," and provide information on sicknesses, in particular on days of paid sick leave.[10]

Our sample consists of all firms with at least one employee in 2000 and 2001. We compare firms' sickness indicators for the period January-August 2000 with those of January-August 2001. We selected these two periods to minimize variation in sicknesses owing to the seasonality of sicknesses. We provide estimates from different periods in our robustness checks in section 6. 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 and we restrict

[9] Zweimüller, Winter-Ebmer, Lalive, Kuhn, Wuellrich, Ruf and Büchi (2009) provide a detailed description of these data. The ASSD provides matched employer-employee data detailing the labor market history of almost 11 million individuals from January 1972 to April 2007 in more than 2.2 million firms.

[10] In 2000, this state accounted for approximately 17.5% of workers and 18% of firms (NACE C-E) in Austria (Austria, 2009).

the data to spells of durations that are longer than three days. We also restrict the sample to those spells that last up to 42 days because this is the minimum duration firms have to pay sick pay. The final sample consists of 256,337 firm-months observations.[11]

Table 1 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. Before the reform, both incidences and durations of sickness were significantly larger in small than they were in large firms. 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 because firms did not receive a refund for their sickness absences.

These expectations are supported by the mean values in Table 1. We see that the average durations for blue-collar workers decreased more in small firms than they did in large firms. Blue-collar workers in small firms were sick less often after the reform than they were before, and there was a very small

[11] In terms of individual workers, 4.1% (3.8%) were blue-collar workers in small firms, 2.8% (2.7%) were white-collar workers in small firms, 45.9% (45.7%) were blue-collar workers in large firms and 47.1% (47.7%) were white-collar workers in large firms between January and August 2000 (2001). These four groups accounted for 7.6% (7.7%) , 4.4% (4.8%) , 55.8% (57.0%) and 32.2% (31.9%) of paid sick days in 2000 (2001).

drop in the sickness incidences in large firms. The sicknesses of white-collar workers hardly changed, however.

Figure 1 shows that blue-collar workers in small and large firms had similar sickness patterns before the reform, in particular an increase in sickness absences during winter months. After the reform, sickness incidences declined in small firms and the typical spike in sicknesses during winter months was not as pronounced in small firms as it was in large firms. The difference between small and large firms is more evident when we turn to sickness durations in Figure 2. Especially after the reform in September 2000, we see that durations, contrary to long-term seasonal patterns, were shorter. By contrast, the plots for white-collar workers in Figures 3 and 4 suggest that neither the incidences nor the durations of sickness absences changed.

The reform could have induced firms to substitute away from blue-collar workers as their relative price increased (slightly) relative to white-collar workers.[12] Figure 5 plots the average monthly proportion of blue-collar workers in small and large firms over time. This plot suggests that there was no visible pattern of substitution after the reform.

Tables 2 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 1. 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

[12] However, this is 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.

indicate that the firms changed only slightly in the two periods, and we do not see different changes for small and for large firms.

5 Results

Table 3 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 approximately 7% 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 4 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 also similar in magnitude to the effect found for sickness durations, the reduction was approximately 6.3%. 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 8.3% in incidences and by 10.8% in durations. The change in large firms (expressed by the coefficient β_4) corresponds to a decrease by 2.0% in incidences and by 3.7% in durations. Together these reductions account for 1.7% of all absences and 2.9% of all absence days recorded by private sector employees (with the restriction that we take into account only absences between 4 and 42 days of duration).

5.1 Robustness

Our estimates indicate that the abolishment of the refund resulted in approximately 6% fewer blue-collar workers' sicknesses and shortened these periods of absence by approximately 7%. We provide a series of robustness checks to gauge the reliability of these results.

Because medical sickness certification is mandatory only after the third day of illness, the coverage of short absences depends on whether firms make use of their right to request a certification for absences of less than four days. The abolition of the refund in 2000 may have influenced the way in which firms handle these short absences. 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 all absences between one and three days. The results in Table 5, column 1, show that the estimated treatment effect differs only marginally.

As previously stated, we restricted the main 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 5. These estimates show indeed that including long spells leads to statistically insignificant estimates of the treatment effect.

We also refine the sample to remove potentially problematic observations. First, we restrict the sample to firms that do not change their size category, namely those firms that are always small or always large. This removes 27,275 observations from our overall sample of 256,337 observations. Again, we essentially obtain the same results (column 3).

We also restrict the sample to non-seasonal sectors because firms in these sectors may differ in their degrees of monitoring compared with seasonal firms. This reduces the number of observations to 164,744. In this subsample, the reform led to a reduction in incidences similar to that observed in the full sample. The reduction in durations was less pronounced (4.3%), however, and it was not statistically different from zero. This suggests that prior to the reform the moral hazard problem was particularly accentuated in the seasonal sectors of the economy, where firms face large variations in capacity utilization—and arguably in incentives for worker monitoring—in the course of the year. (See Del Bono and Weber (2008) for a description of the seasonal sector in Austria).

In the next step, we vary the observational window and compare the first three months of 2000 with the first three months of 2001, which provides us with a sample of 107,868 observations. Limiting the observational period to the first three months of 2000 aims at avoiding the announcement effects from the media coverage that started in April 2000. Announcement effects could lead firms to implement monitoring procedures or to fire sick workers prior to the reform, which would lower the treatment effect.

Although these are short periods, the maximum period after the reform that we are able to investigate is the 20 months until September 2002, because the refund was reinstated at that time. If we select the same number of months before September 2000, we can compare the period of January 1999 to September 2000 with the period of January 2001 to September 2002 (N=646,613). Alternatively, in order to avoid possible biases by announcement effects, we can compare sicknesses in 1999 with sicknesses of 2001 (N=391,831). The estimated treatment effects vary little across the various sample refinements and confirm the robustness of our results (Table 6).

5.2 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, however, an upper 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.

We estimate the following specification, where y_{it} is either the incidence or the duration of blue-collar workers' sicknesses in a firm i at time t, divided by the mean value, $\bar{y}$:

$$\begin{aligned} y_{it} &= \alpha_0 + \rho(\text{small} \times \text{period})_{it} + \\ &\quad +\alpha_1 \text{small}_{it} + \alpha_2 \text{period}_{it} + X'_{it}\alpha + \epsilon_{it}, \end{aligned} \tag{4}$$

where the treatment effect, ρ, and α are the parameters to be estimated; the indicators and the explanatory variables are defined as above.

The estimates presented in Table 7 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 marginally larger (in absolute value) than are 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 only partially fulfilled. The estimated treatment effect for sickness incidences is not statistically different from zero. The estimated effect for sickness durations is, however, statistically different from zero, and this indicates that white-collar workers' sickness absences were shortened by approximately 4.4% following the end of the refund period. This indicates that the end of the refund period did change the sicknesses of white-collar workers slightly and our estimates from the DDD model represent a conservative lower bound for the actual response to the reform.

6 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 8% and that sickness durations were almost 11% shorter after the reform. As expected, the effect of the reform on large firms was less pronounced. Overall, the reform reduced the durations of sickness absences in the private sector by almost 3%.

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, 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 39.2 million absence days for private sector employees. Approximately 25 million days

[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.

were caused by absences of between 4 and 42 days. If we assume that the distribution of workers and firms in our sample is representative of other Austrian provinces, we could attribute 1.9 million absence days to blue-collar workers in small firms, namely 7.6% of all absence days. Our estimated reduction of 10.8% would correspond to approximately 205,000 fewer sickness days for this group alone. The reduction in sickness absences in large firms adds another 515,000 sickness days to this estimate. If we approximate the economic costs of an absence day using the median daily gross wage of €60.4 in 2000, the estimated savings to the sick pay system would be approximately €43.5 million (corresponding to approximately 2.8% of the total costs of continued wage payments for all absences (regardless of duration) in that year).[14] 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, 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

[14]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).

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. Recent 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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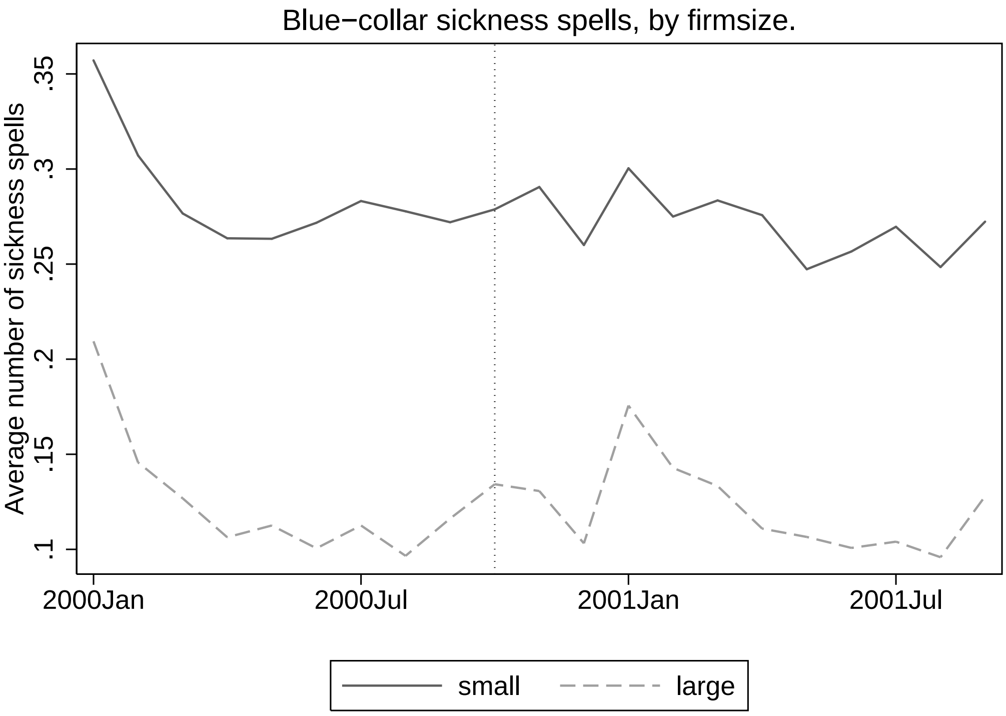
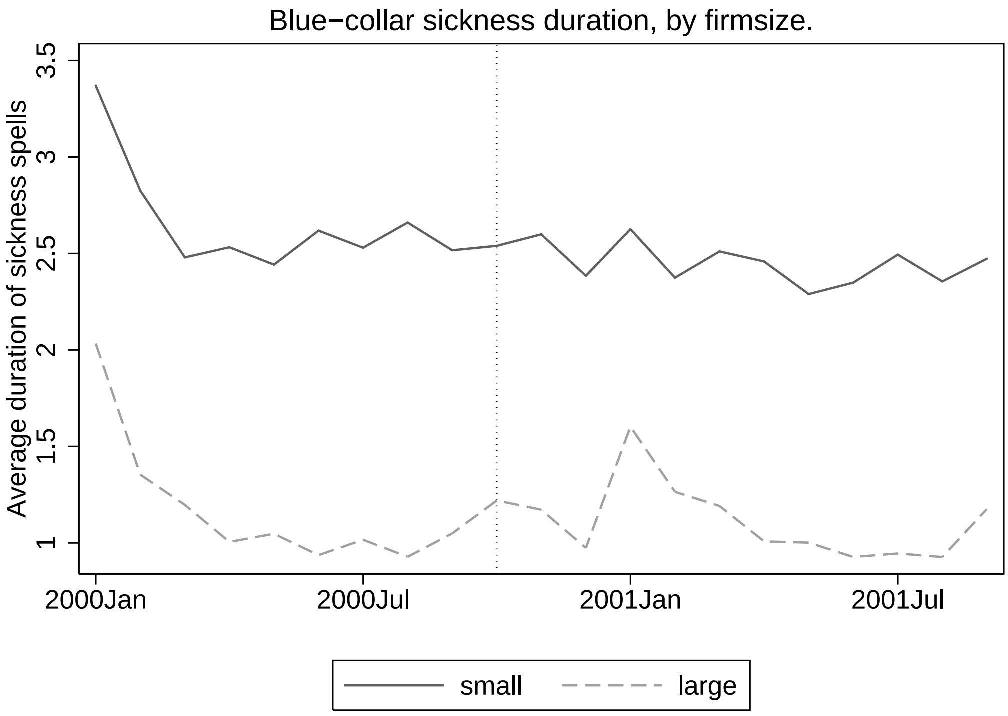
A Figures and Tables

Figure 1: Blue-collar workers were more often sick in small than in large firms.



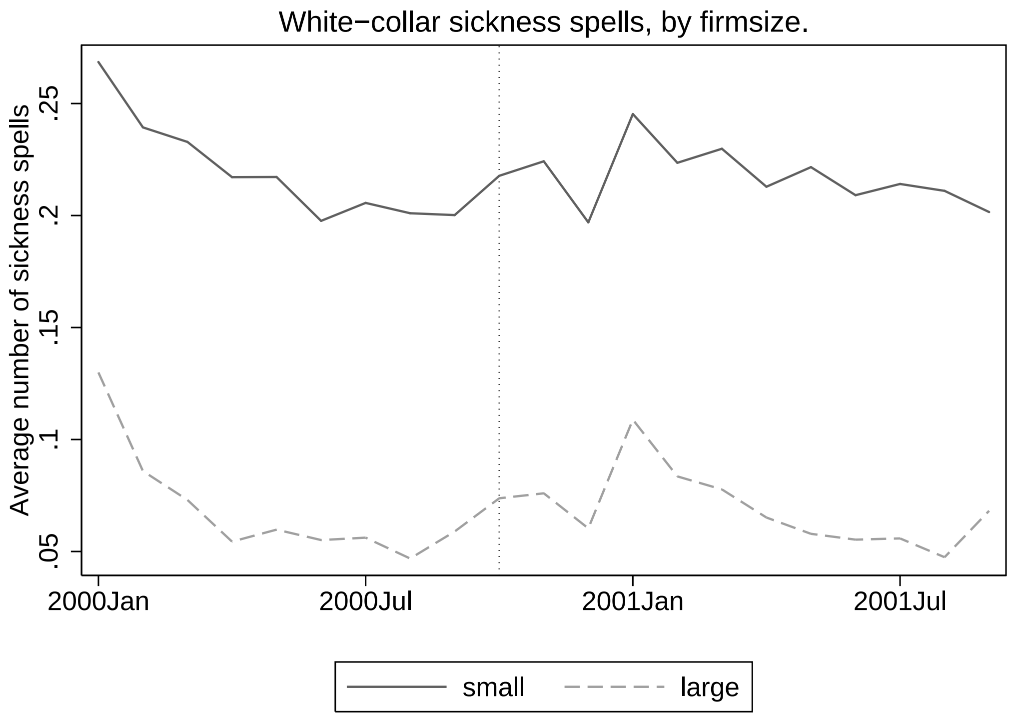
Note: The graph plots the monthly averages of blue-collar workers' sickness spells in a firm, divided by the number of blue-collar workers, counted in the month of the start of the spell. Only spells with durations of 3 to 42 days. Small (large) firms are firms where the monthly wage bill was below (above) a threshold, imposed by the social security. See text for details.

Figure 2: Blue-collar workers were longer sick in small than in large firms.



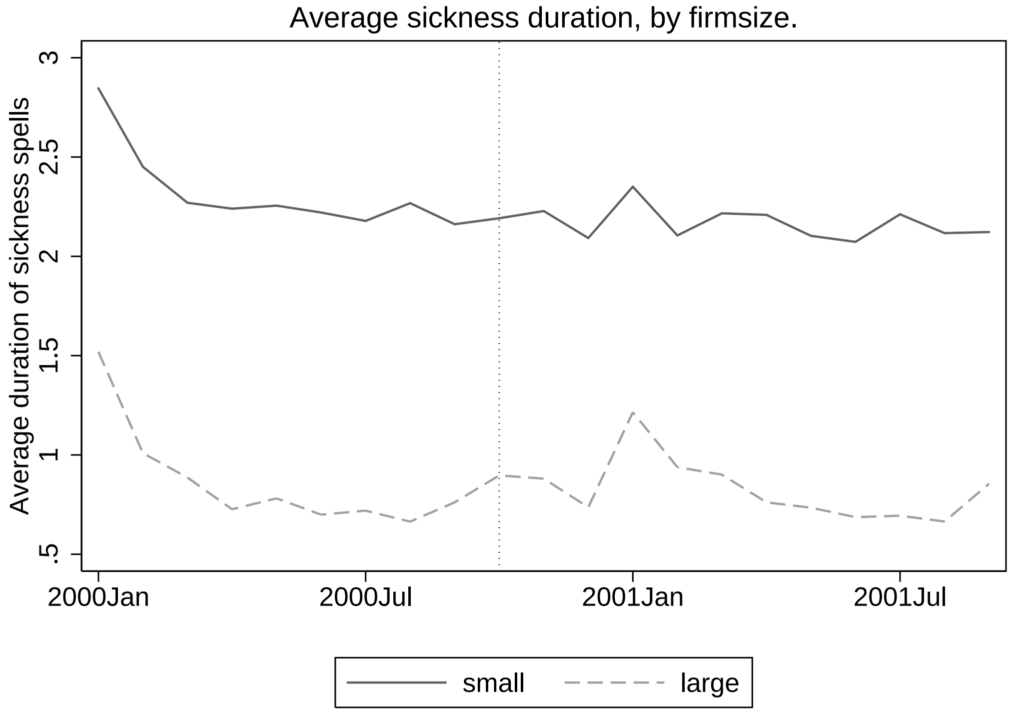
Note: The graph plots the monthly averages of blue-collar workers' days on sick leave in a firm, divided by the number of blue-collar workers, counted in the month of the start of the spell. Only spells with durations of 3 to 42 days. Small (large) firms are firms where the monthly wage bill was below (above) a threshold, imposed by the social security. See text for details.

Figure 3: White-collar workers were more often sick in small than in large firms.



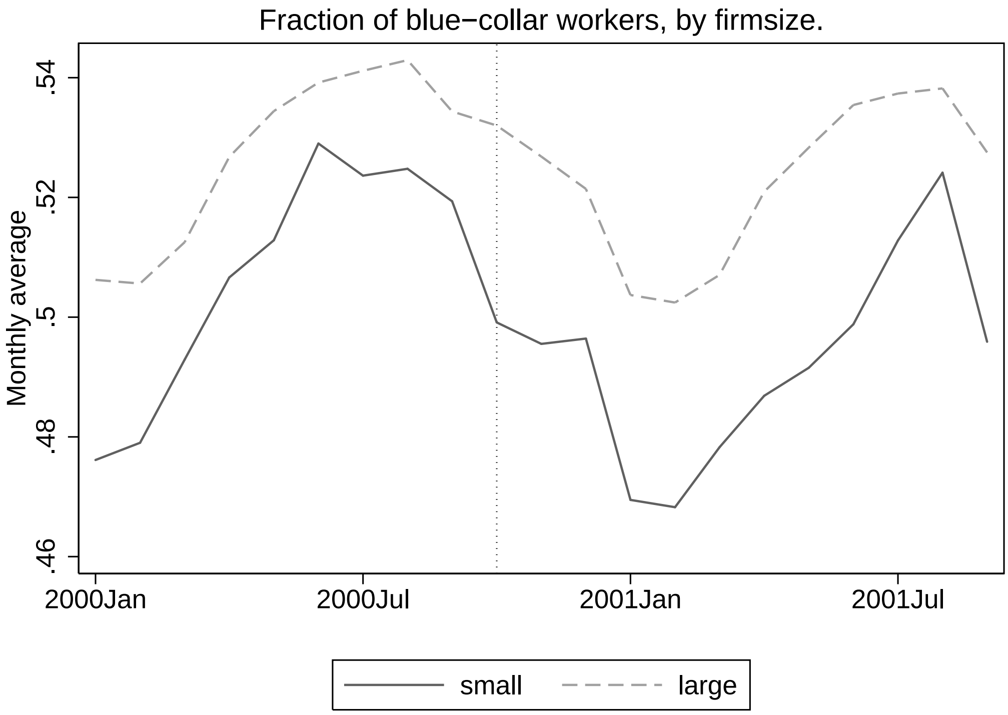
Note: The graph plots the monthly averages of white-collar workers' sickness spells in a firm, divided by the number of white-collar workers, counted in the month of the start of the spell. Only spells with durations of 3 to 42 days. Small (large) firms are firms where the monthly wage bill was below (above) a threshold, imposed by the social security. See text for details.

Figure 4: White-collar workers were longer sick in small than in large firms.



Note: The graph plots the monthly averages of white-collar workers' days on sick leave in a firm, divided by the number of white-collar workers, counted in the month of the start of the spell. Only spells with durations of 3 to 42 days. Small (large) firms are firms where the monthly wage bill was below (above) a threshold, imposed by the social security. See text for details.

Figure 5: Fraction of blue-collar workers in firms, by firm size.



Note: The graph plots the monthly averages of the fraction of blue-collar workers in firms' total number of workers. Small (large) firms are firms where the monthly wage bill was below (above) a threshold, imposed by the social security. See text for details.

Table 1: **Sickness absences in small and large firms.**

Firmsize	Blue-collar workers			White-collar workers		
	Before	After	Diff.	Before	After	Diff.
Small						
Incidence	0.293	0.272	-0.021	0.227	0.223	-0.004
Duration	2.730	2.444	-0.286	2.019	1.922	-0.097
N	33,947	32,194		32,537	32,067	
Large						
Incidence	0.127	0.122	-0.005	0.071	0.070	-0.001
Duration	1.201	1.113	-0.088	0.599	0.574	-0.025
N	29,616	30,042		32,580	33,354	

Note: Before: January–August 2000. *After*: January–August 2001. *Incidence*: All sickness episodes in a month that lasted between 3 and 42 days, divided by the number of blue-collar (white-collar) workers, counted in the month of their start. *Duration*: All days on sick leave for spells that lasted between 3 and 42 days, divided by the number of blue-collar (white-collar) workers, counted in the month of their start.

Table 2: **Summary statistics.**

	Small firms				Large firms			
	Before		After		Before		After	
Incidence	0.158	(0.299)	0.143	(0.289)	0.065	(0.072)	0.061	(0.068)
Duration	2.836	(4.259)	2.638	(4.156)	0.972	(0.828)	0.918	(0.746)
Firm size	6.220	(4.792)	6.203	(3.946)	87.521	(357.192)	87.613	(373.755)
Average age	32.487	(11.168)	32.234	(11.290)	34.199	(8.077)	34.119	(8.148)
Average wage	41.159	(16.406)	42.339	(16.952)	55.738	(13.770)	57.422	(14.376)
Fraction								
blue-collar	0.489	(0.363)	0.473	(0.361)	0.501	(0.304)	0.495	(0.308)
women	0.531	(0.372)	0.538	(0.370)	0.341	(0.275)	0.348	(0.277)
older than 55 years	0.033	(0.113)	0.032	(0.111)	0.037	(0.046)	0.036	(0.044)
N	45,206		43,558		33,190		33,990	

Note: Before: January–August 2000. *After*: January–August 2001. *Incidence*: All sickness episodes in a month that lasted between 3 and 42 days, divided by the number of blue-collar (white-collar) workers, counted in the month of their start. *Duration*: All days on sick leave for spells that lasted between 3 and 42 days, divided by the number of blue-collar (white-collar) workers, counted in the month of their start. *Wage*: Daily wage as used for the calculation of the social security contribution, in €, in 2002 prices.

Table 3: **Estimated impaction on sickness durations, spells between 3 and 42 days length.**

Variable	(1)	(2)	(3)	(4)
Treatment	-0.080	-0.071	-0.071	-0.071
	(0.034)	(0.032)	(0.032)	(0.032)
Small	0.896	-0.269	-0.270	-0.742
	(0.019)	(0.026)	(0.026)	(0.094)
After	-0.016	-0.001*	0.717	0.826
	(0.006)	(0.007)	(0.059)	(0.057)
Blue	0.379	0.416	0.416	0.416
	(0.008)	(0.009)	(0.009)	(0.009)
Small*after	-0.045	-0.041	-0.040	-0.241
	(0.023)	(0.022)	(0.022)	(0.044)
Blue*after	-0.039	-0.037	-0.037	-0.037
	(0.010)	(0.010)	(0.010)	(0.010)
Small*blue	0.069	0.117	0.117	0.116
	(0.026)	(0.024)	(0.024)	(0.024)
Trend			-0.060	-0.069
			(0.005)	(0.005)
Small*trend				0.017
				(0.003)
N	256,337	256,337	256,337	256,337
adjusted R^2	0.052	0.152	0.152	0.152

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 (allowing for heteroskedasticity of unknown form) are in parentheses. 256,337 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. All estimates are statistically significant at 10%, or less, except where indicated by $*$.

Table 4: **Estimated impact on the incidence of sickness absences, spells between 3 and 42 days length.**

Variable	(1)	(2)	(3)	(4)
Treatment	-0.070	-0.062	-0.063	-0.063
	(0.029)	(0.027)	(0.027)	(0.027)
Small	0.883	-0.268	-0.269	-0.606
	(0.017)	(0.024)	(0.024)	(0.078)
After	-0.009	-0.001*	0.732*	0.810
	(0.005)	(0.006)	(0.048)	(0.047)
Blue	0.318	0.361	0.361	0.361
	(0.007)	(0.008)	(0.008)	(0.008)
Small*after	-0.016*	-0.012*	-0.011*	-0.155
	(0.020)	(0.019)	(0.019)	(0.036)
Blue*after	-0.022	-0.020	-0.020	-0.020
	(0.009)	(0.009)	(0.009)	(0.009)
Small*blue	0.052	0.119	0.119	0.118
	(0.022)	(0.021)	(0.021)	(0.021)
Trend			-0.061	-0.068
			(0.004)	(0.004)
Small*trend				0.012
				(0.003)
adjusted R^2	0.068	0.182	0.182	0.182

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 (allowing for heteroskedasticity of unknown form) are in parentheses. 256,337 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. All estimates are statistically significant at 10%, or less, except where indicated by $*$.

Table 5: **Robustness: Sample restrictions.**

Variable	(1)	(2)	(3)	(4)
Incidence				
Treatment	-0.065	-0.056	-0.073	-0.061
	(0.024)	(0.027)	(0.029)	(0.035)
N	256,337	256,337	229,062	164,744
adjusted R^2	0.228	0.195	0.188	0.174
Duration				
Treatment	-0.068	-0.041	-0.074	-0.043
	(0.030)	(0.041)	(0.034)	(0.041)
N	256,337	256,337	229,062	164,744
adjusted R^2	0.162	0.108	0.166	0.143

Note: All specifications as in Tables 1 and 2, column 4. (1) Durations between 1 and 42 days. (2) Durations longer than three days (no upper limit). (3) No change in size category, durations 3–42 days. (4) Removing seasonal sectors, durations 3–42 days.

Table 6: **Robustness: Comparison of different periods.**

Variable	(1)	(2)	(3)
Incidence			
Treatment	-0.065	-0.068	-0.078
	(0.039)	(0.023)	(0.019)
N	107,868	391,831	646,613
adjusted R^2	0.175	0.180	0.179
Duration			
Treatment	-0.069	-0.085	-0.085
	(0.046)	(0.027)	(0.022)
N	107,868	391,831	646,613
adjusted R^2	0.147	0.149	0.149

Note: All specifications as in Tables 1 and 2, column 4. All spells with durations 3–42 days. (1) Pre-period: January–March 2000; post-period: January–March 2001. (2) Pre-period: 1999; post-period: 2001. (3) Pre-period: January 1999–August 2000; post-period: January 2001–August 2002.

Table 7: **Robustness: Difference in difference estimates.**

Variable	Blue-collar	White-collar
Incidence		
Treatment	-0.067	-0.014
	(0.018)	(0.019)
N	125,799	130,538
adjusted R^2	0.178	0.197
Duration		
Treatment	-0.103	-0.044[a]
	(0.022)	(0.022)
N	125,799	130,538
adjusted R^2	0.151	0.156

Note: The table summarizes the estimation results of the effect the abolishment of the refund had on the incidence of sickness absences as in equation 4. 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 (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. Spells with durations between 3 and 42 days. [a] p-value of 0.041.