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Firms' moral hazard in sickness absences *

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April 29, 2011

Abstract

In case of sickness, workers typically receive sick pay. In several countries, social security insures firms against their workers' sickness absences. Such an insurance may create a moral hazard for firms, leading to inefficient monitoring of absences or to an under-investment in the prevention of absences. We investigate firms' moral hazard in sickness absences by exploiting a legislative change that took place in Austria in 2000. In September 2000, an insurance fund was abolished that refunded firms' for the costs of their blue-collar workers' sickness absences. Firms did not receive a refund for their white-collar sickness absences. Until September 2000, small firms were refunded for all wage costs of blue-collar workers' sickness absences. Large firms, in

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contrast, received only 70% of the wages paid to sick blue-collar workers refunded. Using a difference-in-differences-in-differences approach, we estimate the causal impact of refunding forms for their workers' sickness absences. Our results indicate that the incidence of blue-collar workers' sickness in small firms dropped by almost 10 percent. Sickness durations were about 8 percent shorter due to the removal of the refund. Several robustness checks confirm these results. A regression discontinuity analysis of the incidence and duration of blue-collar workers' sickness in the vicinity of the threshold provides additional evidence that firms reacted to the institutional incentives. Blue-collar workers in firms that received more compensation were more often, and for longer periods, on sickness leave than those who worked in firms that received less compensation.

Keywords: absenteeism, moral hazard, sickness insurance

JEL classification: J22, I38

1 Introduction

Sickness absences lead to significant losses in productivity and, in consequence, to lower income and profits (Barham and Begum, 2005; Brown and Sessions, 1996). Labor laws typically grant workers continued pay if they are unable to work due to being ill. In general, such a guaranteed sick pay may induce workers to "adapt their work-absence behavior" (Johansson and Palme, 2005, p1880), i.e., generate a moral hazard where workers are absent from work without actually being sick. Johansson and Palme (2005) find such a moral hazard for Swedish workers. Similarly, Ziebarth and Karlsson (2010) estimate that a cut of the replacement rate for sick workers in Germany from 100% to 80% led to some 12% fewer days on sick leave.

Countries differ in whether employers, private insurance companies, or social security systems provide the sick pay. If sick pay is borne by firms, they face direct and indirect costs caused by absences. In most OECD countries, firms are, at least to some extent, insured against their workers' sicknesses, either the amount or the period of sick pay is limited, or firmsare 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%, after that period the wages are paid by social security (Markussen, Mykletun and Roed, 2010). In Germany, firms with fewer than 30 employees receive 80% of their costs refunded by an insurance fund (Ziebarth and Karlsson, 2010). Depending on how the insurance system is organized, firms may pass their costs onto the public, e.g., by exerting too little effort in monitoring or preventing absences.

Barmby, Ercolani and Treble (2002) argue that to understand fully the impact of regulations on absence rates, it is necessary to have data that enable the analysis of a regime shift within a single jurisdiction. We use an exogenous change in the way Austrian firms were compensated for their workers' sickness absences to shed light on the firms' moral hazard problem. The Austrian social insurance system provides an excellent setting for the analysis of moral hazard and sickness absences, because Austrian legislation guarantees each worker with a minimum tenure of two weeks continued wage payment for at least six weeks, to be paid by the employer. Until September 2000, small firms received a refund of all 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, in 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, there was no refund after January 2001.[1]

We use the end of the refund in 2000 to investigate whether or not workers' absences were effected by the different policy regimes. The reform abolished the relatively generous reimbursement of absence costs for blue-collar workers and increased the sickness costs for firms. Changes in the incidence or duration of sickness episodes may indicate that not only workers, but also firms reacted to the incentives in the system. The data are from the Austrian Social Security Database (ASSD). The ASSD is the administrative database

[1] A refund was re-introduced in September 2002, however, there is no differential treatment of small and large firms in that regime.

for the calculation of pension benefits for private sector employees in Austria. These data were linked to the administrative database of the public health insurance, covering all employees, to obtain information on workers' sickness absences. Our estimates indicate that the removal of the refund resulted in about 8 percent fewer blue-collar workers' sicknesses and shortened these by about 10 percent. We provide a series of robustness checks, which confirm the reliability of our results.

Sick pay is the rule both within and outside the OECD and according to Scheil-Adlung and Sandner (2010) as many as 145 countries provide for paid sick leave. Our findings are important for the design of sickness insurance systems in many countries, because firms are often insured against their workers' sicknesses. Sick pay regulation is a central component of modern welfare states and provisions aimed at insuring workers against the loss of income due to illness date back to the very origins of the welfare state.[2] The literature on sickness absences, recently summarized in Ziebarth and Karlsson (2010), focuses on workers' behavior. Our research, to the best of our knowledge, is the first to analyze firms' behavior.

2 Institutional settings

Workers in Austria continue to receive their wages if they are unable to work due to sickness for a period of up to twelve weeks, depending on tenure. Since 2001, both blue-collar and white-collar employees with a minimum tenure of

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

two weeks are entitled to sick pay of up to six weeks in case of sickness or accident. The right to sick pay increases to eight weeks for a worker with a tenure of five years, to ten weeks for 15 and up to twelve weeks for a tenure of 25 years.[3]

Until September 2000, firms received a refund for the wages they had had to pay sick blue-collar workers. The amount of the refund depended on the firms' total wage bill. If the sum of all workers' monthly wages in a firm exceeded a threshold, 180 times the maximum daily social security contribution, the firm was a large firm, otherwise it was a small firm.[4] The classification was based on the wage bill in month t for sicknesses of month $t + 2$. 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 social security.

Small firms received all direct costs and large firms received 70% of a blue-collar worker's sickness absence. The regulation intentionally favored smaller firms as they were assumed to have more problems covering sickness absences than larger firms. In particular, small firms may need to hire replacement workers, if some of their workers are sick. Large firms, in contrast, might be

[3]Before 2001, the maximum durations for blue-collar workers were for each tenure category two weeks shorter than for white-collar workers.

[4]Austrian social security is financed by employers' and workers' contributions, and both are based on the gross wage. The contributions are constant above a certain maximum wage, these limits are tabulated in Table **??**. The employer's contributions are 21.65% for blue-collar and 21.85% for white-collar workers. Workers' contributions are 18.15% if they are blue-collar workers and 17.95% if they are white-collar workers. Workers earning below a certain threshold, €366.33 per month in 2010, are exempt (Böheim and Weber, 2011).

able to cover for sick workers by worker reallocation within the firm.[5] There was no refund for wages a firm had to pay white-collar workers when they were on sick leave.

The compensation fund ("Entgeltfortzahlungsfond") was financed by employers' contributions, 2.1% of their workers' wages, and was administered by the Austrian social security administration. Upon abolishment on the 30th of September 2000, only sickness episodes that started before this date were eligible for a refund and there was no refund after the 1st January of 2001.[6]

In case of sickness, a worker needs to see a medical doctor who certifies the sickness and informs the social security administration. The worker is required to inform the 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 necessary, but also because firms are insured against their blue-collar workers' sickness absences. The refund may cause firms to monitor their absent workers less, which will result in higher absence rates.[7] Large firms, as 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 return to work. In-

[5] 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.

[6] 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 ten days. Small firms are now defined as firms where the average employment is less than 51 employees per year. A firm is also considered small if the average number of employees is 53 or less due to the employment of apprentices or disabled workers. Because of the different definition of firm size, we do not analyze firms' behavior in the later regime.

[7] Firms might also encourage prolonged absences, for example, if demand is low.

efficient monitoring of workers will lead to more frequent and longer sickness absences. The abolition of the refund and the change in sickness behavior around this date will provide evidence for the extent of firms' moral hazard.

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. Since utility is increasing in income and leisure, a worker has an incentive to remain absent from work, pretending to be sick ("shirking"). This incentive depends on the probability of detection, the consequences on being found out, and the utility from leisure. A worker's utility from leisure depends also on health, where sickness increases the value of leisure.

Assume that the firm observes in each period a fraction σ of its workers on sick leave, $0 \leq \sigma \leq 1$. A fraction τ of the workers who on sick leave are genuinely unfit for work, $0 \leq \tau \leq 1$ and $1 - \tau$ are shirking. The firm may spend some fixed costs, κ, on a monitoring technique that detects shirking with some probability α, $0 \leq \alpha \leq 1$. If someone is detected shirking, the worker is fired and the firm does not pay sick pay. A firm will monitor its absent workers, if the cost of monitoring, $\kappa\sigma$, is less than 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 towards genuinely sick workers, $(1-\alpha)(1-\tau)\sigma s$ is the sick pay for shirking workers who are not detected, and $\kappa\sigma$ are the expenditures 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 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 percent 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, $\sigma(s-r)$.

We analyze how firms respond to the end of the compensation for sickness absences to obtain an indicator of the magnitude of moral hazard induced by the compensation scheme. 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 workers. Similarly, we sum the sickness spells recorded by white-collar workers and divide it by the number of workers. We also calculate the average number of days on sick leave for firm i in month t, separately for blue-collar and white-collar workers.

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 0, if it was due to a white-collar worker. Our estimation is a "difference-in-differences-in-differences" (DDD) specification, where we regress the sickness indicator, y_{itc}, of worker type c in firm i in month t on a set of explanatory variables:

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

where the β are parameters to be estimated and τ, the coefficient on the triple interaction term, is the parameter of interest. It gives the causal change in sicknesses for blue-collar workers in small firms due to the end of the refund of sickness costs. The vector X contains characteristics, e.g., sector or region, and a linear trend. Standard errors are clustered on firms.

4 Data

We use register data from the Austrian Social Security Database (ASSD).[8] The data provide information on all employees in dependent employment and 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 or the number of sickness absences, or their average durations, in a particular firm. We augmented these data with information from the 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 the days on paid sick leave.[9]

Our initial sample consists of all firms with at least one employee between January 2000 and September 2001. We compare firms' sickness indicators for the period January 2000-September 2000 with those of January 2001-September 2001. We have selected these two periods to minimize variation in sicknesses that is due to the seasonality of sicknesses. We will provide estimates from different periods in our robustness checks below. The estimating

[8]Zweimüller et al. (2009) provide a detailed description of these data. The Austrian Social Security Database (ASSD) are 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.

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

sample consists of 353,686 observations (firm-months).

Table 1 tabulates the mean number of sickness episodes and their durations in small and large firms, by blue-collar and white-collar workers and period. Since it is perhaps easier to remain longer than necessary on sick leave than to obtain sick leave when not actually sick, we expect a stronger reaction of the sickness durations than of the incidences. Before the reform, both incidence and duration of sickness was significantly larger in small than 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 incidence and duration 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. The values for white-collar workers should have remained unchanged since firms did not receive a refund for their sickness absences. These expectations are supported by these mean values. We see that the average duration for blue-collar workers decreased more in small firms than in large firms. Blue-collar workers in small firms were sick less often after the reform than before and there was a very small drop in the sickness incidences in large firms. The sicknesses of white-collar workers did hardly change.[10]

Tables 2 and 3 present summary statistics of our estimating sample, by firm size and period. The summary statistics indicate that the firms' workforce composition did change only slightly over time. While there was some decline in the number of blue-collar workers over time, the decline was similar in

[10]If we exclude firms that operate in sectors that show marked seasonal patters (e.g., tourism or the building sector), the numbers do not change much. Seasonal firms arguably monitor their workers more thoroughly than firms that operate throughout the year. See Del Bono and Weber (2008) for a description of the seasonal sector in Austria.

small and large firms. Figure 2 plots the fraction of blue-collar workers in small and large firms between July 1999 and July 2002. We observe a similar downward trend in both small and large firms over this period.

Figure 3 plots sickness incidences in small and large firms for the years 1998–2002. The Figure shows a lower variance of sickness incidence after the reform, rather than a drop in the level. Figure 4 plots average sickness durations by firm size for the period 1998–2002 and we observe a slight decline in the sickness duration in the vicinity of the reform. We also see that the variation of sickness durations appears to be lower after the reform.

Figures **??** and **??** present the average monthly sickness incidence and duration of blue-collar workers in small and large firms in our estimating sample. Figures **??** and **??** plot the corresponding values for white-collar workers. The graphs show that blue-collar workers in small and large firms had similar sickness patterns before the reform, in particular a slight increase in sickness absences in the early summer months. After the reform, sickness incidences declined in small firms and the typical spike in sicknesses during the winter months is missing. In contrast, sicknesses of blue-collar workers in large firms show the typical increase during the winter months. The difference between small and large firms is more evident when we consider durations. Especially after the reform in September 2000, we see that durations, contrary to the long-term seasonal patterns, were shorter. In contrast, the plots for white-collar workers suggest that, if any changes occurred at all, the changes were minor.

5 Results

Table 4 tabulates the DDD-estimates of average sickness durations. The specifications differ in the included covariates, the first specification has no covariates other than the indicator variables. A set of firm characteristics is included in specification 2 and specification 3, our preferred specification, also includes a linear trend. The causal effect is given by the estimated coefficient on the triple-interaction term described above.

In each of the specifications, the estimated causal effect is statistically significant at conventional error levels and suggests that durations decreased on average by about 9.2 percent due to the removal of the refund.[11] Note that the estimates are almost identical across specifications.

Table 5 tabulates the results for the incidence 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. The effect is also similar in magnitude to the effect found for the durations, the reduction was about 7.6 percent. These are large effects and imply that firms' moral hazard was substantial, which led to inflated sickness absences.

6 Robustness

Our estimates indicate that the removal of the refund resulted in about 8 percent fewer blue-collar workers' sicknesses and shortened these by about

[11]This value is derived by relating the coefficient, 0.139, to the average duration of blue-collar workers in small firms, 1.502.

10 percent. Since these are large responses, we provide a series of robustness checks to gauge the reliability of these results. As a first step, we augment the estimating equation with group-specific linear trends or with higher order polynomials. These refinements yield in essence the same results, which are tabulated in Table 6.

In addition, we refine the estimating sample to remove potentially problematic observations. First, we restrict the sample to firms that do not change their size category, i.e., who are always small or large. This removes 16,912 observations from our sample of 354,602 observations. Second, we restrict the sample to non-seasonal sectors as firms in these sectors may differ in their monitoring from seasonal firms. This reduces the number of observations to 224,072. Again, we essentially obtain the same results.

As a third modification 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 149,976 observations. Alternatively, in order to avoid possible biases by announcement effects, we compare sicknesses in 1999 with sicknesses of 2001 (N= 537,946). While these are short periods, the maximum period after the reform that we are able to investigate consists of the 20 months until September 2002, since the refund was re-instated at that time. If we select the same number of months before September 2000, we compare sicknesses in the period January 1999 to August 2000 to those of the period January 2001 to September 2002 (N=888,974). The estimated treatment effects vary little across the various sample refinements and confirm the robustness of our results.

6.1 Difference in Differences

If white-collar workers are not an appropriate control group for blue-collar workers, e.g., because the reform changed the firms' monitoring for all types of workers, then our DDD-estimates are biased. However, a differences-in-difference comparison of blue-collar workers' absences in small and large firms, before and after the reform, will provide an estimate of the extent of change in sickness behavior for blue-collar workers due to the end of the refund.

We estimate the following specification, where y_{it} is either the incidence or the duration of blue-collar workers' sickness in a firm i at time t:

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

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

Table 7 presents the results from these estimates. The estimates indicate that blue-collar workers in small firms had fewer and shorter sickness spells after the reform, in comparison to blue-collar workers in large firms. While the estimated coefficients are smaller (in absolute value) than the estimated effects we obtain from the DDD estimates, the results provide corroborating evidence for firms' moral hazard.

Since the refund was only available for the sicknesses of blue-collar workers, the end of the refund should have had no effect on how firms treated white-collar workers and we therefore should not be able to estimate causal effects for the sicknesses of white-collar workers. All specifications yield insignificant results for ρ, the coefficient on the interaction term. This implies that we cannot reject the hypothesis that the end of the refund had no effect on the sicknesses of white-collar workers, which is additional evidence for the robustness of our DDD results above.

6.2 Regression Discontinuity

The discontinuity in the refund rates may identify the causal effect of the refund on sicknesses for firms in the vicinity of the threshold. If there is no endogenous sorting of firms around the threshold, any difference in sickness behavior between small and large firms could be attributed to the different refund rates. Such an estimate of differences in sickness outcomes around the threshold provides additional evidence for firms' moral hazard. Because the refunds existed only prior to September 2000, we restrict our sample to non-seasonal firms of the year 1999. We have chosen this period to have a complete year of sicknesses and to avoid possible distortion caused by announcement effects. Note that we can estimate the RD only for the period before the abolition of the refund.

The choice of interval around the cutoff is a non-trivial question and involves finding an optimal balance between the precision of the local linear regressions and the bias resulting from a choice that is too narrow.[12] We

[12] Lee and Lemieux (2010) suggest two procedures, which are generally considered for choosing such an interval. The first procedure consists of characterizing the optimal interval in terms of the unknown joint distribution of all variables (Fan and Gijbels, 1995).

experimented with several intervals around the threshold and obtained fairly stable results. We have settled on the interval (€-500, €500) around the threshold. There are 983 small and 879 large firms in this interval with an average blue-collar workers' sickness incidence of 0.103 (SD 0.109) and 0.106 (SD 0.122). Durations were 0.725 (0.931) for small and 0.725 (0.988) for large firms.

We argue that there is no sorting of firms around the threshold. Endogenous sorting of firms would result in relatively more firms just below the threshold than just above it, because the difference in the refund rate may create an incentive to remain below the threshold. In addition, if firms sorted below the threshold, we would observe firms below the threshold to grow more slowly than firms just above the threshold, in order to remain in the more beneficial region.

We center the firms' wage bills on the threshold by subtracting the threshold from each wage bill. Figure 9 plots the density of the wage bill around the cutoff value.[13] The graph plots the frequencies of the assignment variable in intervals of equal width on each side of the threshold, with local polynomial regressions overlaid for each side of the threshold. The graph shows an almost complete overlap of the two local regressions, indicating that there is no discontinuity in the assignment variable around the threshold. Lalive, Wuellrich and Zweimüller (2009) make similar arguments and find no evi-

Typically, a rule-of-thumb (ROT) bandwidth is estimated over the whole relevant range and, in a second step, the ROT bandwidth is used to estimate the optimal bandwidth around the threshold. Alternatively, cross-validation procedures are used to derive the optimal interval (Imbens and Lemieux, 2008).

[13] We use Kovak's and McCrary's (2008) Stata program.

dence of sorting in their sample.[14] (Note that the continuous density of the random variable is neither necessary nor sufficient for identification except under auxiliary assumptions (McCrary, 2008).)

In addition, formal tests for differences in the growth rates and the ratio of blue-collar to white-collar workers for firms above and below the threshold show no statistically significant differences. See Table 8. While we can reject the equality of means for small and large firms for all firms with high confidence, we cannot reject the equality of means for firms that are in the vicinity of the threshold. The obtained p-value for the equality of the means of their growth rates is 0.22, allowing for unequal variances in the distributions above and below the threshold.[15] Similarly, testing for the equality of the means of the fraction of blue-collar workers in firms below and above the threshold yields a p-value of 0.53. (The standard deviations are 0.3 for both types of firms.)

Figures 10 and 11 plot the sickness indicators of blue-collar workers in this interval. Fourth-order polynomials are overlaid on each side of the threshold. The figures suggest a greater sickness incidence and longer durations for small firms than for large firms in the vicinity of the threshold. The estimates of the

[14]Lalive et al. (2009) consider a threshold of 25 workers that requires firms to hire a disabled worker (or pay a fine). We believe that sorting is even less likely in our case as the wage bill depends not only on the number of workers, but also on promotions, industry-wide wage bargaining and turnover. In addition, the classification is based on the wage bill in month t for sicknesses in month $t+2$.

[15]The standard deviation for firms below the threshold is 1.5 and above it is 0.13; imposing equal variances yields a p-value of 0.25. This relatively high standard deviation of small firms is due to 7 observations (of 768) which lost more than 50 percent of their workers. Dropping these outliers yields a mean growth rate of -0.004 (SD 0.87) for small firms and -0.005 (SD 0.13) for large firms. The p-value after dropping these outliers is 0.96.

discontinuity are tabulated in Table 9 for different bandwidths. All estimates confirm that blue-collar workers in small firms had more and longer sickness absences. Depending on the choice of bandwidth, however, not all of these estimates are statistically significant from zero—the broader the bands, the less precise the estimated effect. The estimate based on a bandwidth of 20 suggests a 12% lower incidence and an almost two days shorter sickness absences of blue-collar workers in large firms. These differences between small and large firms are greater than the results obtained from the DDD specifications. This is perhaps not surprising, because firms that are in the vicinity of the threshold might be more aware of the consequences of a changed refund rate.

7 Conclusion

We analyzed sickness absences in small and large firms who had received different compensations for the wages they had to pay their sick workers. Small firms did receive more compensation than large firms because of presumed difficulties of covering for sick workers. Using administrative data, we find robust evidence for *firms'* moral hazard, using the differential treatments of small and large firms and of blue-collar and white-collar workers as sources of variation. Identification of the causal effect is established by comparing sickness behavior in two different policy regimes, one with and one with no compensation. We estimate that the incidence of blue-collar workers' sickness in small firms dropped by almost 10 percent and sickness durations were about 7.5 percent shorter after the reform.

The findings presented in this paper 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 for paid sick leave. Similar settings to the one analyzed here exist, for example, in Germany, Denmark and the UK. In Germany, for example, firms with less than 30 employees are eligible for a refund of 80% of wage paid to sick workers. These examples could be expanded to all instances where the sick pay system fails to give adequate incentives for firms to monitor their employees' absences. Clearly the moral hazard problem is exacerbated in institutional settings — such as the Austrian until 2000 — where (some) firms have little incentive to monitor absenteeism while at the same time workers benefit from high replacement rates during sickness.

According to our findings, the institutional change which was implemented in Austria in 2000 led to a reduction in the number of absence days by XXX% in small firms, equivalent to YYY% of total sickness absence recorded in 2000. In that year the Austrian social insurance agencies counted 39.2 million absence days of private sector employees. If we approximate the economic costs of an absence day with the median daily gross wage (i.e., €XXX in 2000), the estimated savings to the sick pay system would be about €XXX million (corresponding to XXX% of total costs resulting from continued wage payments and sick pay in that year).[16] These figures represent reference points for situations that are comparable to the case investigated here. They

[16]The Austrian Ministry for Social Affairs estimated these costs at €2.4 billion Euro for 2000 (Ministry for Social Affairs, n.d.).

strengthen the argument for legislation that calls on firms to carry a portion of costs resulting from sick pay, especially with respect to absence periods of short and medium length.

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

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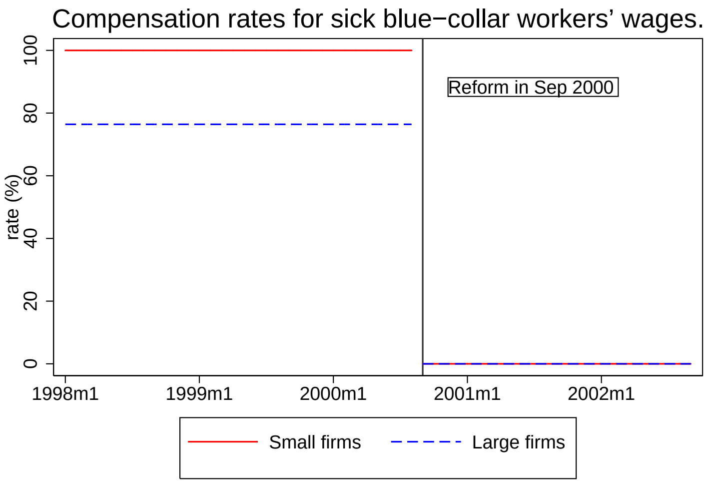
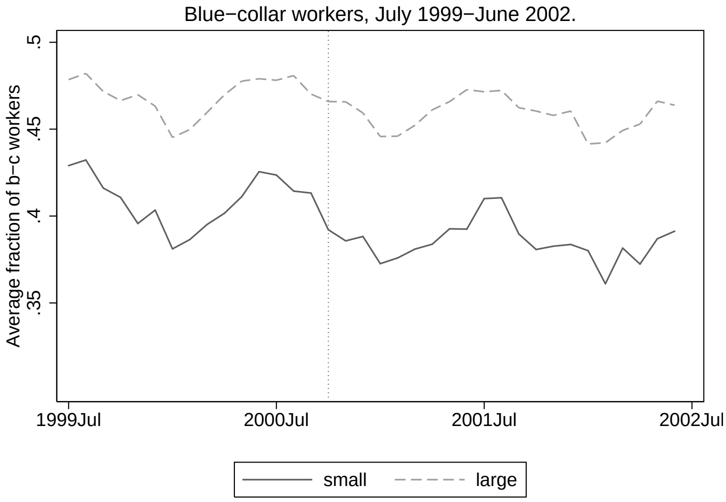
A Figures and Tables

Figure 1: Compensation structure for firms' paid-out wages during workers' sick leaves.



Note: Before September 2000, firms were classified as small if their monthly wage bill was below 180 times the daily maximum amount of the social security contribution. Small firms received full compensation for paid wages to sick blue-collar workers, this included also the employers' contributions to social security, large firms received 70% compensation.

Figure 2: Fraction of blue-collar workers, by firm size.



Note: no outliers, no seasonal sectors.

Figure 3: Average number sickness spells, by firm size, 1998–2002.

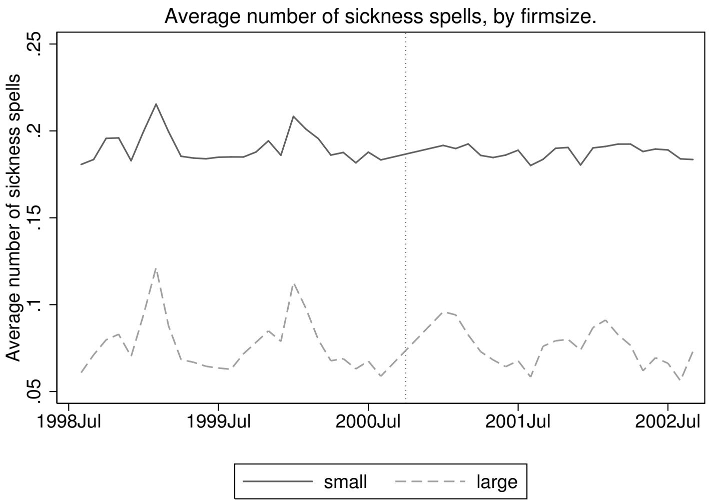


Figure 4: Average duration of sickness spells, by firm size, 1998–2002.

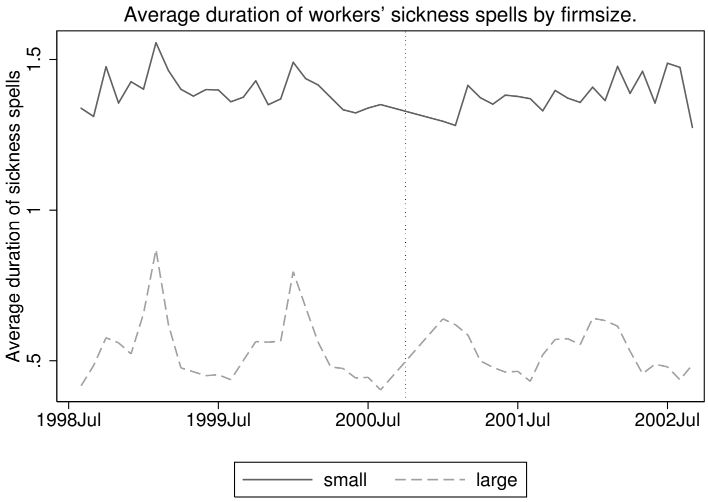


Figure 5: Monthly b-c sickness incidence, averages by firmsize

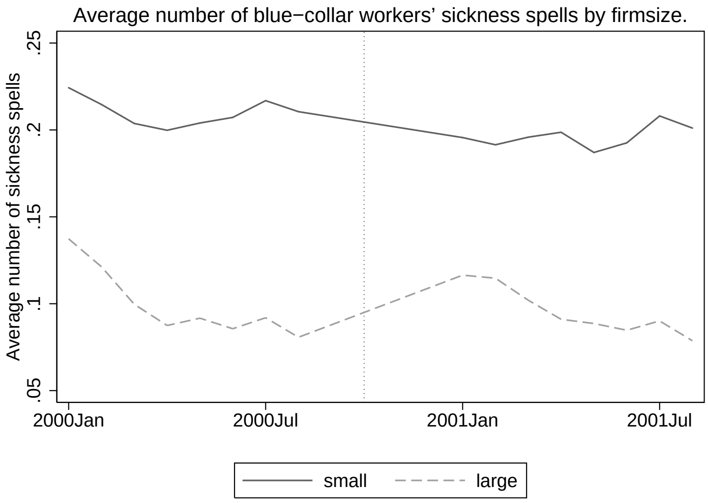


Figure 6: Days b-c workers are sick per month, averages by firmsize

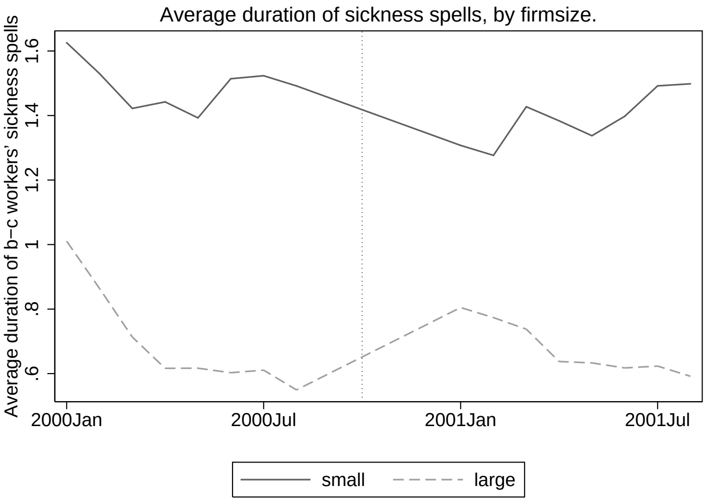


Figure 7: Average w-c sickness incidence, by firmsize

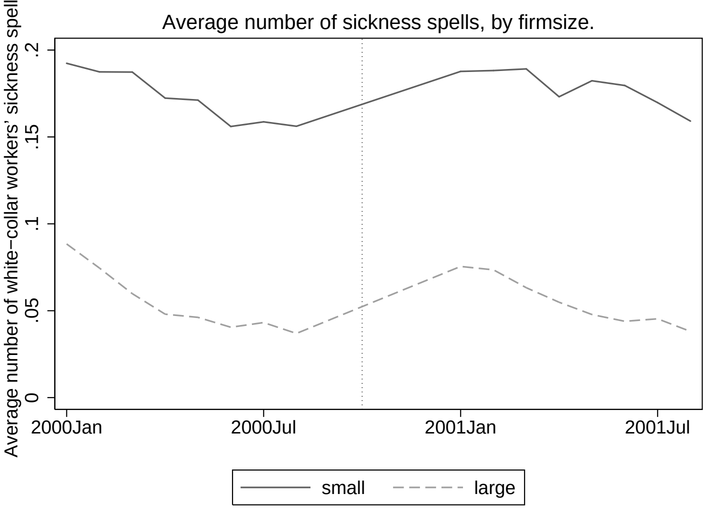


Figure 8: Average w-c sickness durations, by firmsize

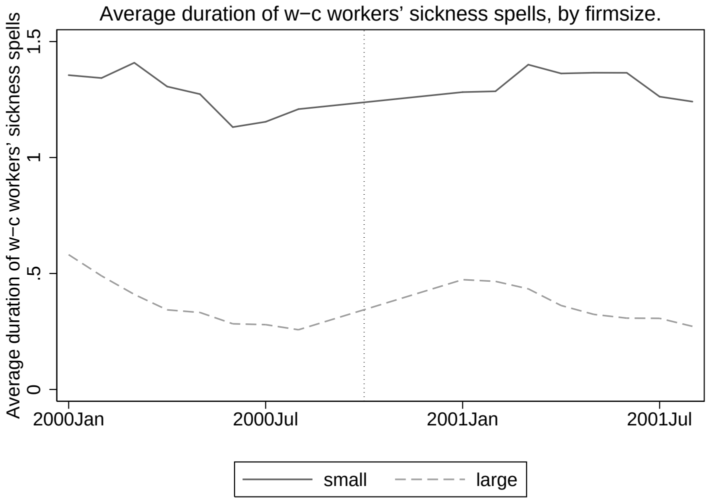
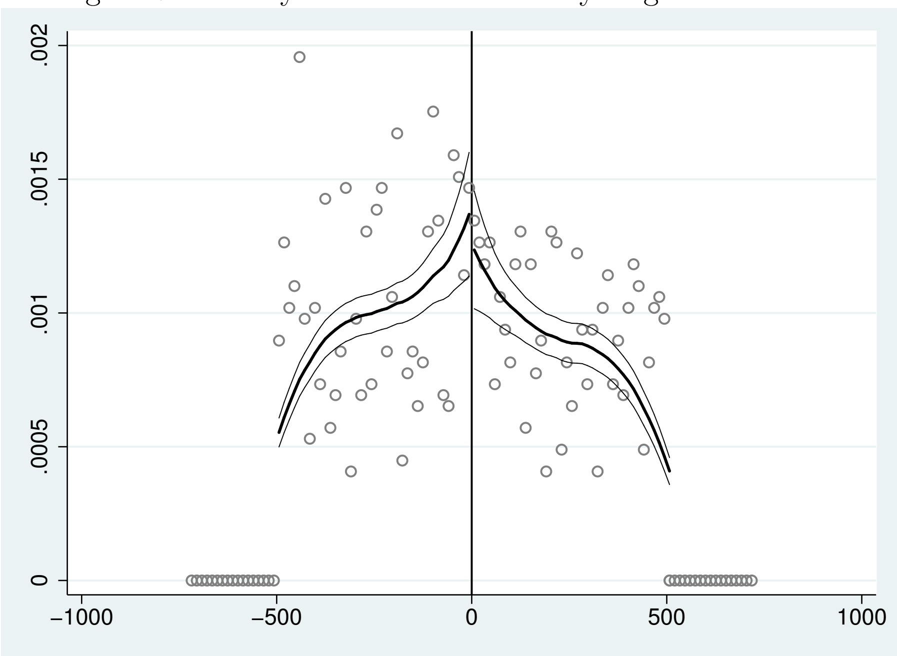
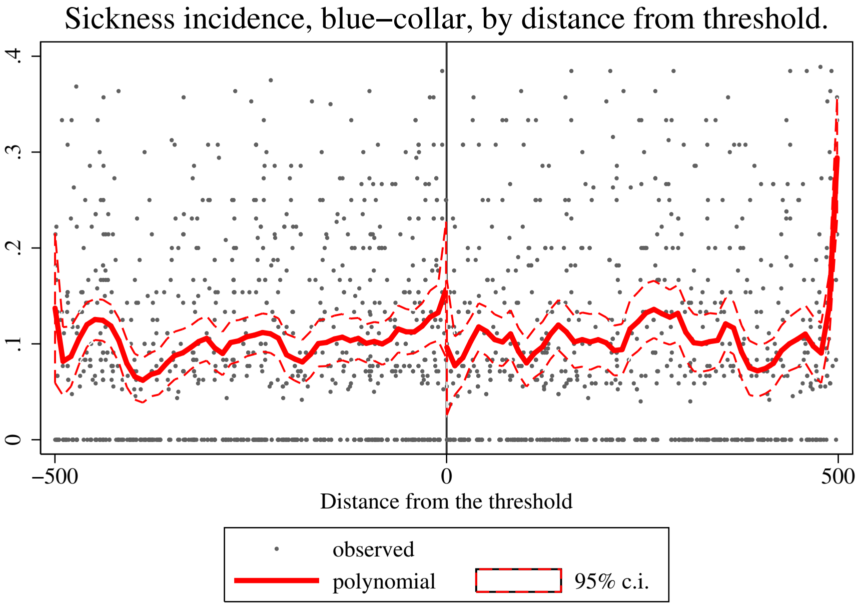


Figure 9: Density of the firms' monthly wage bill around the cutoff.



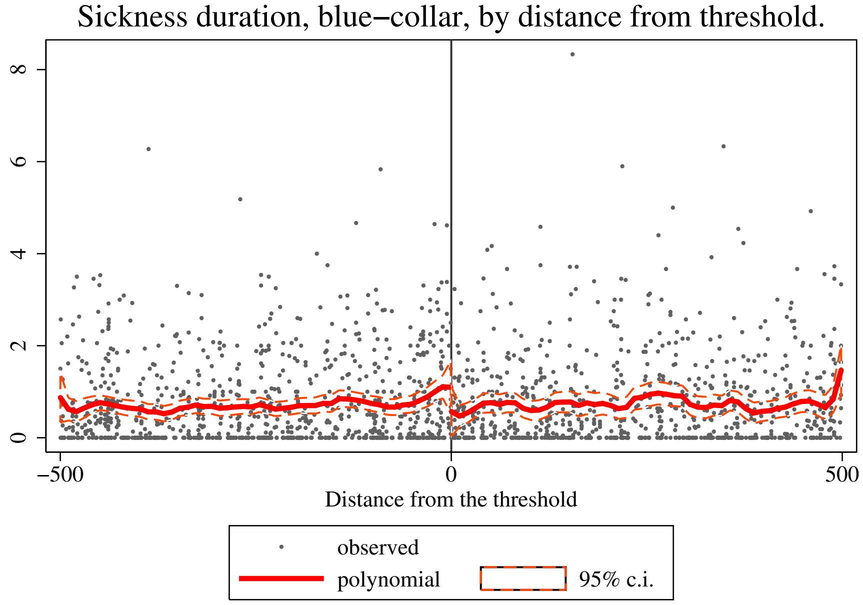
Note: The observations are centered around the cutoff which was €18575.16 in 1999. The density plot was produced using Kovak's and McCrary's (2008) Stata routine.

Figure 10: Extent of sickness incidence near the threshold.



Note: Each dot represents a firm's number of sickness spells in a month in 1999, in the interval (€-500,€500) around the threshold. The solid lines are fourth-order polynomials, estimated separately for each side of the interval, and the dashed lines indicate the 95% confidence bands.

Figure 11: Extent of sickness duration near the threshold.



Note: Each dot represents a firm's number of sickness days in a month in 1999, in the interval (€-500,€500) around the threshold. The solid lines are fourth-order polynomials, estimated separately for each side of the interval, and the dashed lines indicate the 95% confidence bands.

Table 1: Sickness absences in small and large firms.

Size	Blue-collar Before	Blue-collar After	Blue-collar Diff.	White-collar Before	White-collar After	White-collar Diff.
Duration						
Small	1.502	1.381	-0.121	1.284	1.321	0.037
Large	0.703	0.679	-0.024	0.375	0.370	-0.005
Incidence						
Small	0.211	0.196	-0.015	0.175	0.180	0.005
Large	0.100	0.096	-0.004	0.055	0.056	0.001

Before: January--September 2000.
After: January--September 2001.
Incidence: All sickness episodes in a month, divided by firm size.
Duration: All days on sick leave in a month, divided by firm size.
Observations:
Small firms x month: 54,233 before, 52,422 after.
Large firms x month: 69,250 before, 35,563 after.
Unconditional values, i.e., including workers who were not sick.

Table 2: Summary statistics, small firms and period.

Variable	Small firms: Before Mean	Std. Dev.	After Mean	Std. Dev.	Difference	[p-value]
Workforce composition, fraction of workers who are:						
blue-collar	0.482		0.467		0.015	[0.000]
workers 55+	0.034		0.033		0.001	[0.375]
apprentices	0.112		0.112		0.000	[0.833]
foreigners	0.099		0.102		-0.003	[0.000]
women	0.532		0.540		-0.008	[0.000]
firm size	6.013	4.665	5.997	3.910	0.015	[0.408]
average wage	40.996	16.775	42.150	17.354	-1.154	[0.000]
average age	32.888	11.209	32.701	11.333	0.187	[0.000]
N	54,233 before		52,422 after			

Note: Wages are daily and deflated to 2002.
p-values are for two-sided t-tests for the equality of means, allowing for unequal variances, indicating the error level at which the null hypothesis of equality may be rejected.

Table 3: Summary statistics, large firms and period.

Variable	Large firms: Before Mean	Std. Dev.	After Mean	Std. Dev.	Difference	[p-value]
Workforce composition, fraction of workers who are:						
blue-collar	0.497		0.492		0.005	[0.002]
workers 55+	0.037		0.061		0.001	[0.001]
apprentices	0.064		0.122		0.003	[0.000]
foreigners	0.121		0.348		-0.002	[0.003]
women	0.341		0.492		-0.007	[0.000]
firm size	86.273	371.187	85.791	370.053	0.481	[0.808]
average wage	55.834	13.852	57.557	14.480	-1.723	[0.000]
average age	34.629	7.874	34.560	7.903	0.068	[0.105]
N	34,625 before, 35,563 after.					

Note: Wages are daily and deflated to 2002.
p-values are for two-sided t-tests for the equality of means, allowing for unequal variances, indicating the error level at which the null hypothesis of equality may be rejected.

Table 4: Estimated change in sickness duration.

	(1)	(2)	(3)
DDD	-0.139	-0.139	-0.139
	0.041	0.041	0.041
small	0.909	-0.509	-0.509
	0.024	0.031	0.031
after	-0.006	-0.008	0.049
	0.005	0.006	0.054
blue	0.328	0.328	0.328
	0.012	0.012	0.012
small*after	0.043	0.055	0.055
	0.029	0.028	0.028
blue*after	-0.018	-0.018	-0.018
	0.009	0.009	0.009
small*blue	-0.110	-0.110	-0.110
	0.034	0.034	0.034
ln mean wage		0.072	0.072
		0.040	0.040
ln firm size		-0.671	-0.671
		0.015	0.015
fraction women		0.027	0.027
		0.041	0.041
fraction b-c		0.271	0.271
		0.031	0.031
trend			-0.005
			0.004

legend: b (se). N= 353686 observations.

Table 5: Estimated change in sickness incidence.

	(1)	(2)	(3)
DDD	-0.016	-0.016	-0.016
	0.004	0.004	0.004
small	0.120	-0.063	-0.063
	0.003	0.003	0.003
after	0.000	-0.001	0.034
	0.001	0.001	0.005
blue	0.045	0.045	0.045
	0.002	0.002	0.002
small*after	0.005	0.006	0.006
	0.003	0.003	0.003
blue*after	-0.004	-0.004	-0.004
	0.001	0.001	0.001
small*blue	-0.009	-0.009	-0.009
	0.004	0.004	0.004
ln mean wage		0.027	0.027
		0.004	0.004
ln firm size		-0.087	-0.087
		0.002	0.002
fraction women		0.020	0.020
		0.004	0.004
fraction b-c		0.047	0.047
		0.003	0.003
trend			-0.003
			0.000

legend: b (se). N= 353686 observations.

Table 6: Robustness: Estimated change in sickness.

DDD estimates:

	incidence	duration
Baseline	-0.016	-0.139
(N=354,602)	0.004	0.041
group-specific linear trend		
(N=354,602)	-0.016	-0.139
	0.004	0.041
group-specific cubic trend		
(N=354,602)	-0.016	-0-139
	0.004	0.041
No change in size category*		
(N=337,690)	-0.015	-0.130
	0.005	0.042
No seasonal sectors*		
(N=224,072)	-0.015	-0.129
	0.006	0.054
Period: Jan--March 2000 and Jan--March 2001*		
N=149,976	-0.015	-0.101
	0.006	0.054
Period: 1999 and 2001*		
N=537,946	-0.016	-0.091
	0.004	0.036
Period: Jan 1999--August 2000 and Jan 2001--September 2002*		
N=888,974	-0.018	-0.106
	0.003	0.030

legend: b (se).

* As specification (3) in Tables 5 and 6.

Table 7: DD: Estimated sickness changes.

Variable	blue-collar workers incidence	blue-collar workers duration	white-collar workers incidence	white-collar workers duration
small*after	-0.007	-0.066	0.003	0.026
	0.002	0.020	0.002	0.026
small	-0.062	-0.524	-0.071	-0.592
	0.004	0.032	0.004	0.038
after	0.070	0.346	-0.006	-0.254
	0.007	0.076	0.008	0.083
lnwage	-0.017	-0.211	0.072	0.357
	0.004	0.044	0.007	0.072
lnfirm	-0.078	-0.592	-0.097	-0.749
	0.002	0.017	0.002	0.020
femfrac	0.019	0.036	0.021	0.019
	0.005	0.050	0.007	0.071
bcfrac	0.454	3.242	-0.360	-2.703
	0.005	0.043	0.005	0.052
trend	-0.006	-0.028	0.000	0.019
	0.001	0.006	0.001	0.007

legend: b/se. N=176,843.

Table 8: RD sample: Summary statistics of selected variables.

	Small firms Mean (SD)	Large firms Mean (SD)	Difference	[p-value]
Growth rate				
All firms	-0.029 (0.004)	-0.0001 (0.111)	-0.014	[0.000]
N	46,738	48,985		
RD sample	-0.070 (1.486)	-0.004 (0.130)	0.066	[0.223]
N	768	697		
RD, no outlier	-0.004 (0.088)	-0.004 (0.130)	-0.0003	[0.963]
N	761	697		
Fraction blue-collar workers				
All firms	0.485 (0.366)	0.502 (0.305)	0.017	[0.000]
N	83,454	53,057		
RD sample	0.495 (0.304)	0.486 (0.301)	-0.009	[0.526]
N	983	879		
RD sample--other variables:				
frac women	0.383 (0.009)	0.381 (0.010)	-0.001	[0.918]
average wage	49.705 (13.786)	48.350 (13.110)	-1.355	[0.030]
older workers	0.037 (0.059)	0.028 (0.051)	-0.009	[0.001]

Note: RD sample are all firms which are in the interval (-500,500) around the threshold. Outliers the seven small firms with a growth rate in the lowest percentile. p-values are for two-sided t-tests for the equality of means, allowing for unequal variances, indicating the error level at which the null hypothesis of equality may be rejected.

Table 9: RD estimates of being a large firm on sickness outcomes.

	Coef.	(Std. Err.)	[p-value]
Incidence			
bandwidth:			
20	-0.010	(0.041)	[0.801]
40	-0.067	(0.067)	[0.313]
80	-0.031	(0.027)	[0.256]
Duration			
bandwidth:			
20	-0.689	(0.420)	[0.101]
40	-0.883	(0.574)	[0.124]
80	-0.515	(0.264)	[0.051]

Note: RD estimates obtained using Nichol's (2007) program.
Standard errors are bootstrapped with 100 replications.
1024 small and 886 large firms, 1999. Sample restricted to the interval (-500, + 500) around the threshold.