A Natural Experiment on Sick Pay Cuts, Sickness Absence, and Labor Costs[‡]

Preliminary version. Comments welcome.

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Abstract

We estimate the overall reform effect of a reduction in sick pay levels on the incidence and duration of sick leave and labor costs. A federal law reduced the legal obligation of German employers to provide 100 percent continued wage pay up to six weeks per sickness episode. From October 1996 onwards, statutory sick pay was decreased to 80 percent of foregone gross wages. This measure reduced the ratio of employees with absence days by up to 3 percentage points which equals a decrease of 5 percent. The mean number of absence days per year also significantly decreased by approximately 5 percent. The effects were more pronounced in East Germany and in bigger firms which can be traced back to a stricter application of the new law. We calculate that the reform reduced total labor costs by about €1.5 billion per year which led to the creation of around 50,000 new jobs. We derive these numbers by means of difference-in-differences, longitudinal survey data from the SOEP, and two control groups.

Keywords: sickness absence, sick pay levels, natural experiment, SOEP

JEL classification: C93; H51; I18; J22

1 Introduction

The relationship between unemployment benefits and unemployment duration has attracted labor economists' attention for decades and provided material for a countless number of publications. In light of this, it seems odd that comparably little research has been conducted on the relationship between sick pay benefits and sickness absence despite its enormous relevance for labor supply, population health, social insurance systems, and the functioning of insurance markets.

A handful of studies explicitly and convincingly analyze the effect of sick pay levels on absence rates by exploiting legislative changes in the benefit levels in Sweden (Johansson and Palme, 2005; Henrekson and Persson, 2004; Johansson and Palme, 2002, 1996). Two old studies from England provide some correlation- based evidence with data from the 70s (Doherty, 1979; Fenn, 1981). In addition, some papers from the US analyze the impact of benefit levels in the workers' compensation insurance (Meyer et al., 1995; Curington, 1994). However, the workers' compensation insurance differs from the European sickness absence insurance as only work-related injuries or illnesses are covered. All mentioned studies find that employees adapt their absence behaviour to increases and decreases in benefit levels. This finding is reinforced by various other empirical studies analyzing further determinants of sickness absence behaviour. Workplace conditions matter (Dionne and Dostie, 2007) as well as probation periods and economic upswings or downturns (Ichino and Riphahn, 2005; Askildsen et al., 2005).

Substantial cross-country differences in sickness absence range from 5 up to 27 days per year and employee, suggesting that institutional arrangements as well as cultural influences play a major role. These numbers illustrate the need for further explanations for these broad variations. They also underline the presumption of huge potential for efficiency gains in the market for sickness absence insurance.

Depending on the legal institutions, employers, private insurance companies, or social security systems provide sick pay. In case of employer provided sick pay, firms bear a burden in form of indirect labor costs in addition to the direct productivity losses due to workplace absences.

In Germany's generous sick pay system, employers are obligated to continue to pay employees their full wages up to six weeks per sickness episode. No benefit cap is imposed as in most other countries. Nevertheless, cross country comparisons rank Germany far below the international average in terms of sickness absence rates (Bonato and Lusinyan, 2004). On explanation might be the anecdotical evidence claiming Germans to have a strong work ethic. Another reason might be the well-functioning monitoring system or the relatively high unemployment rate in Germany.

In 1996, the Kohl government decided to reduce the legally required replacement level from 100 to 80 percent of foregone gross wages, to be effective from 1 October 1996 onwards. The intention was twofold: to reduce the degree of moral hazard in the sickness absence insurance and to reduce labor costs in order to foster employment creation. At that time, employers were confronted with sick leave payments that amounted to €28.2 billion per year (German Federal Statistical Office, 1998).

While employers welcomed this initiative at the beginning, ongoing mass demonstrations and strikes forced some of them to "voluntarily" agree on the continuation of the old sick pay scheme. During that time, there was a big uncertainty about the scope of the law and several lawsuits were filed.

The aim of this study is to estimate the overall causal impact of the law on sickness absence and labor costs. We exploit the exogenous variation in the absence costs by using a difference-in-differences methodology and longitudinal survey data from the German Socio-Economic Panel Study (SOEP). Although we cannot perfectly identify those employees who were de facto not affected as their employer agreed voluntarily to provide 100 percent sick pay, we are able to estimate the overall reform effect by relying on two sound control groups. As controls serve those who were totally unaffected by the new law, namely self-employed, public sector employees, and trainees. Thanks to the panel structure of the data, we are able to take the worker composition into account. Most of the evaluation literature struggles with selection issues which often hamper the analysis crucially. In our setting, sorting is unlikely to be an issue, since the law applied to all dependent private sector employees, since it was determined at the federal level, and since we are able to control for the unlikely case that privately employed applied for public sector employment or became self-employed as a reaction to the reform.

We contribute to the literature in several ways. This is the first causal estimate of cuts in sick pay levels on sickness absence with non-Swedish and uncensored data. Moreover, we use a representative sample of the third largest economy in the world and most recent data. Unlike most of the previous studies, our identification strategy relies on two sound control groups which are observed over time. Additionally, we avoid a common caveat in evaluation studies by controlling for potential selection issues. Finally, we derive an estimate of the total amount of labor costs that were saved and estimate the number of jobs which were created as a reaction to the reform.

Section 2 specifies some of the institutional settings in Germany. Section 3 gives more details about the data, following Section 4 which discusses the empirical estimation strategy. Finally, we give some broad estimates of the reform induced reduction in labor costs and conclude in Secion 6.

2 The German Sick Pay Scheme and the Policy Reform

2.1 The Sick Pay Scheme and Monitoring System

Germany has one of the most generous sick pay schemes worldwide. Before the implementation of the new law, every employer was legally obligated to continue the usual wage payments up to six weeks per sickness episode. Putting it differently, employers had to provide a 100 percent sick pay from the first day of a sickness spell without any benefit caps.[1]

When falling sick, employees are obligated to immediately inform their employer about their sickness as well as the expected duration. From the third day of a sickness episode, a physician's certificate is required and is usually issued for up to one week, depending on the illness. If the sickness lasts more than six ongoing weeks, the physician needs to issue a different certificate. From the seventh week onwards, sick pay is disbursed by the sickness fund and lowered to 80 percent on foregone gross wages for those who are insured with the Statutory Health Insurance (SHI).[2]

[1] The entitlement is codified in the so called *Gesetz über die Zahlung des Arbeitsentgelts an Feiertagen und im Krankheitsfall (Entgeltfortzahlungsgesetz)*, article 3, 4. Sick pay is only provided for regular earnings and not for overtime payments.

[2] In addition to the law which lowered short-term sick pay and which stands in the focus

The monitoring system mainly consists of an institution called *Medical Service of the SHI*. One of the original objectives of the medical service is to monitor sickness absence. The German Social legislation codifies that the SHI is obliged to call for the medical service and a medical opinion to clear out doubts about work absences. Such doubts may arise if the insured is unusually often short-term absent or sick on Mondays or Fridays. Likewise, if physicians uncommonly often certify sickness, the SHI may ask for an expertise. The employer also has the right to call for the medical service and an expertise. Expertises are based on available medical documents, information about the work place, and the statement of the patient which is asked for. If necessary, the medical service has the right to examine the patient physically and to cut benefits.[3] In 1997, about 2,000 full-time equivalent and independent physicians worked for the medical service and examined 1,719,386 cases of absenteeism (Medizinischer Dienst der Krankenversicherung (MDK), 2008).

2.2 The Policy Reform

In 1996, the total sum of employer provided sick pay amounted to DM 55.3 billion (€28.2 billion) (German Federal Statistical Office, 1998) and was claimed to contribute to persistently high unemployment rates by functioning like a tax on labor. Together with speculations about a high degree of moral hazard in the generous German sick pay scheme, these considerations induced the German government to pass a law which became effective from October 1, 1996. [4]

The law reduced the employees' sick leave claims from 100 to 80 percent of gross wages for the first six weeks per sickness episode. Self-employed were - by construction - not affected by the new law. Likewise unaffected by the cut in sick pay were employees on sick leave due to a work accident. As an alternative to the cut in sick

of this study, another law was passed on November 1, 1996 and became effective from January 1, 1997 onwards. This law was called *Gesetz zur Entlastung der Beiträge in der gesetzlichen Krankenversicherung (Beitragsentlastungsgesetz - BeitrEntlG), BGBl. I 1996 p. 1631-1633* and reduced sick pay from the seventh week onwards from 80 to 70 percent of forgone gross wages. The impact of this law on long-term absenteeism was analyzed elsewhere (Ziebarth, 2008).

[3] The wordings of the laws can be found in the Social Code Book V, article 275, para. 1, 1a; article 276, para. 5.

[4] The correct German name of this law that was passed on September 25, 1996 is *Arbeitsrechtliches Gesetz zur Förderung von Wachstum und Beschäftigung (Arbeitsrechtliches Beschäftigungsförderungsgesetz), BGBl. I 1996 p. 1476-1479.*

pay, employees had from then on the right to reduce their paid vacation by one day for every five days of sickness absence, thereby avoding the sick pay cut. Due to political considerations and other laws, public sector employees as well as trainees were exempted from the reform.[5]

Before and in the aftermath of the law's implementation, the German population and unions put pressure on the employers through mass demonstrations and strikes. According to statements by unionists around 13 million German employees [6] were de facto not affected by the law since unions successfully forced - mostly industrial - firms to agree upon voluntary payments. However, since there are no offical numbers, this estimate could be part of a unionist propaganda campaign and should hence be regarded as an upper threshold. On the other hand, polls among handcraft establishments suggest that around 50 percent of these firms did not apply the law. There was a very low application rate among handcraft establishments with less than 5 employees. Anecdotical evidence traces this back to a strong mutual trust between employers and employees in such small firms (Brors and Thelen, 1998). In general, the degree of application was much higher in East Germany, in branches with a low collective bargaining coverage, and in very small firms suggesting that the strongest effects can be expected for bigger firms and in East Germany.

Another point which is worthwile to mention is that around 2,000 lawsuits were filed in labor courts to clarify the scope of application of the law. The first judgements were pronounced mid-1998 (Jahn, 1998).

All in all, there was a big uncertainty and sensitization among German employees at that time and even employees who were de facto not affected by the law were probably not fully aware of their privileges. We can not perfectly identify those employees but compensate this deficit by regional and job stratification and by focusing on the overall reform effects rather than the precise calculation of elasticities.

[5] In case of trainees, the so called *Berufsbildungsgesetz (BBiG)* prevented the application of the law.

[6] In relation to 27.7 million employees reliable for social insurance (German Federal Statistical Office, 1998).

3 Data And Variable Definitions

The empirical specifications use the German Socio-Economic Panel Study (SOEP). The SOEP is a longitudinal representative annual household survey that exists since 1984. Wagner et al. (2007) provide further insights.

We extract two pre- and two post-reform years from the survey, i.e. the waves 1995 up to 1999 that each contain sickness absence information about the previous year. We discard the reform year 1996 in most of our specifications.[7] We restrict our sample to those of the working labor force who are eligible for sick pay (plus self-employed) and whose age lie between 18 and 65.[8] Individuals with item non-response can not be used either.

3.1 Endogenous and Exogenous Variables

The SOEP is a rich dataset, especially with respect to job characteristics. Detailed questions about the type of job, the number of years with the employer, the gross and net wage, and the like are sampled. Additionally, there are questions on sick leave behavior.

We generate our dependent variables from the following question: *"How many days off from work did you have in 19XX because of illness? Please enter all days, not just those for which you had a doctor's certificate."* The great advantage of the SOEP and this question is that the *total* number of absent days is documented, not only those with a certificate as with most register data. Especially when the focus is on short-term absenteeism, it is a big advantage to have such a total measure. However, this comes at the cost of not having detailed spell data. We discard respondents with long-term sickness spells of more than six weeks and likewise do not consider

[7] By this means, we collect data from the years 1994/1995 and 1997/1998. Since current as well as retrospective information is sampled in every wave, we match the retrospective information in which we are interesed in with the current information of the according year as long as the respondent was interviewed in both years. If this was not the case, we use both types of information from the same interview and assume that the current statements have not changed since the last year.

[8] Although marginally employed (employees who earn less than €400 per month) are eligible for sick pay and are on a par with full-time employed since June 1, 1994, we drop them since it is likely that marginally employed were not fully aware of their rights at that time and since anecdotical evidence suggests that a significant fraction of employers refused to provide this benefit.

respondents with the total number of absent days exceeding thirty, since we focus on short-term absenteeism.[9]

Two sets of dependent variables are generated. The first measures the incidence of sick leave and consists of three dummy variables. *Noabs* takes a one if the respondent claimed to not having been absent at all in the previous year and a zero otherwise. *Incmissed10days* and *Incmissed20days* have a zero for people without any absence days and a one for respondents with up to 10 (20) absence days.

The second set consists of three variables that measure the duration of sickness absence. The variable *Daysabs* takes on values from zero to thirty depending on the number of total absence days. *Missed10days* has values from zero to ten depending on the number of missed days at work and discards all respondents with more than ten absence days. The variable *Missed20days* is analogously constructed.

The whole set of explanatory variables can be found in Appendix A and is categorized as follows. A first group incorporates variables on personal characteristics, like the dummies *Female, Immigrant, East German, Partner, Married, Children, Disabled, Good health, Bad health, No sports*, and *Age* (*Age*^{2}). The second group consists of educational controls such as the degree obtained, the number of years with the company, and whether the person was trained for the job. The last group contains explanatory variables on job characteristics. Among them are *Blue collar worker, White collar worker*, the size of the company, or *Monthly gross wage*. We also control for the regional unemployment rate and include state and year dummies.

3.2 Control Groups and Treatment Group

We define one treatment group and two control groups and accordingly generate two treatment dummies. *Treatment Group 1* has a one for the treated, i.e. those who were eligible for sick pay and affected by the new law. This group is made up of all employees who work in the private sector and who are not in vocational training. Our first specification contrasts these employees with those who are eligible for sick pay but were exempted from the law due to political considerations. The dummy

[9] As explained in Section 2, long-term sick pay was also cut at that time which might distort our results if we do not discard these observations. In section 5.1 we use again the full sample for estimating the total labor cost savings for Germany.

Treatment Group 1 has hence a zero for people in vocational training and for public sector employees (Control Group 1) . Contrarily, the dummy *Treatment Group 2* compares the same eligible respondents as *Treatment Group 1* with those who are not eligible for sick pay, namely self-employed (Control Group 2). The treated sum up to 15,648 observations, the Control Group 1 has 7,734 observations, and we count 2,009 observations for the self-employed which make up Control Group 2.

4 Estimation Strategy and Identification

4.1 Probit Specification

To estimate the effect of the cut in sick pay on the incidence of absenteeism, we specify the following conventional difference-in-differences (DiD) specification by pooling the data over the pre- and post-treatment years:

$$P(y_{it} = 1|\,\boldsymbol{X}) \quad = \quad \Phi(\alpha_g + \eta_p + \gamma\theta_{it} + \boldsymbol{\xi_t\vartheta} + \boldsymbol{\varsigma_s\nu} + \boldsymbol{x_{it}\beta}) \tag{1}$$

where α_g is the treatment group effect and has a one for respondents belonging to the treatment group, η_p is an overall post-reform dummy, and θ_{it} has a one for individuals in the treatmentgroup for post-reform years and can be interpreted as the interaction term between α_g and η_p. The model also includes a vector of additional time dummies $\boldsymbol{\xi_t}$ with t=1,...,T being the time dimension, a vector of state dummies $\boldsymbol{\varsigma_s}$ with s=1,..., S being the state, and a set of various other controls, $\boldsymbol{x_{it}}$, which be already described in Section 3. $\Phi(.)$ is the cumulative distribution function for the standard normal distribution.

The parameter of interest is γ which gives us the causal effect of the reform on absenteeism under the assumption that, conditional on our set of controls, treatment and control group would have underlain a common time trend in the absence of the reform.

4.2 Count Data Specifications

The second empirical specification intends to estimate how the policy reform affected the duration of absenteeism. We do not have spell data at hand but a measure of the total number of absence days. Since the number of absent days is a count with exzess zero observations (about 50 percent of the sample) and overdispersion, i.e. the conditional variance exceeding the conditional mean, we fit count data models. Based on the Akaike (AIC) and Bayesian (BIC) information criteria and various Vuong tests, we found the so called *Zero-Inflated Negative Binominal Model (NegBin)* to be appropriate.

The underlying statistical process differentiates between absent employees and non-absent employees and assigns different probabilities, which are parameterized as functions of the covariates, to each group. The binary process is specified in form of a logit or a probit model and the count process is modeled as an untruncated NegBin-2 model for the binary process to take on value one. Hence, zero counts may be generated in two ways: as realizations of the binary process and as realizations of the count process when the binary process is one (Winkelmann, 2008). In contrast to the more restrictive Poisson distribution, the employed negative binomial distribution does not only take excess zeros into account but also allows for overdispersion and unobserved heterogeneity. The NegBin model can be seen as a special case of a continuous mixture model. In the notation of Cameron and Trivedi (2005), the NegBin distribution can be described as a density mixture of the following form:

$$\begin{aligned}
\varphi(y|\mu,\alpha) &= \int f(y|\mu,\nu) \times \gamma(\nu|\alpha)\, d\nu \\
&= \int_0^\infty \left(\frac{e^{-exp(\boldsymbol{X\beta})\nu}\{exp(\boldsymbol{X\beta})\nu\}^y}{y!}\right)\left(\frac{\nu^{\delta-1}e^{-\nu\delta}\delta^\delta}{\Gamma(\delta)}\right) d\nu \\
&= \frac{\Gamma(\alpha^{-1}+y)}{\Gamma(\alpha^{-1})\Gamma(y+1)}\left(\frac{\alpha^{-1}}{\alpha^{-1}+\mu}\right)^{\alpha^{-1}}\left(\frac{\mu}{\mu+\alpha^{-1}}\right)^y \qquad (2)
\end{aligned}$$

where $f(y|\mu,\nu)$ is the conditional poisson distribution and $\gamma(\nu|\alpha)$ is assumed to be gamma distributed with ν as an unobserved parameter with variance α. $\Gamma(.)$ denotes the gamma integral and $\mu = exp(\boldsymbol{X\beta})$. The NegBin can be derived in different ways, has different variants, and different interpretations. Note that in the special case of $\alpha = 0$ the NegBin collapses to a simple Poisson model.

4.3 Identification

In constrast to most empirical studies of this spirit, our analysis relies on two different control groups that were not affected by the cut in sick pay. We compare them over time to those who were affected by the law to identify the causal reform effects. However, as usual in difference-in-differences (DiD) applications, we assume that changes in the absence rates go entirely back to the exposure of the reform. With other words, conditional on the available covariates, we assume the absence of unobservables with a differential impact on the work absence *dynamic* for treatment and control groups.

Although treatment and control groups differ with respect to most of their observable characteristics (see Appendix A), we argue that the common time trend assumption is likely to hold for various reasons: Various different covariates are incorporated in our models and account for differences in the sample composition with respect to personal, educational, and job characteristics. It should be emphasized that we observe the (self-reported) health status of the respondents. We also take time-invariant sick leave differences of the treated and controls into account and adjust for year-effects as well as state-effects and the annual state unemployment rate. Since we contrast the treated with two different control samples, we automatically crosscheck for the plausibility and robustness of the results. Note that the sickness absence level of the treated lies between the levels for the two control groups. The focus on short time spans and the experimentation with different years add to the credibility of our analysis. Sample composition changes over time and labor market attrition can be addressed due to the panel structure of the data and a refreshment sample which was drawn in 1998.

In recent years, there has been an extensive debate about drawbacks and limitations of DiD estimation. A particular concern is the underestimation of OLS standard errors due to serial correlation in case of long time horizons and unobserved (treatment and control) group effects when the number of groups is small. We focus on short (and varying) time horizons. As Bertrand et al. (2004) have shown, the main source for understating the standard errors stem from serial correlation of the outcome and intervention variable and is basically eliminated when focussing on less than 5 periods. We also use robust standard errors and correct for clustering at the individual level.[10]

One of the biggest issues in evaluation studies are selection effects. Here, the reform was politically determined and applied to all private sector employees. It is very unlikely that people left the labor market due to a cut in sick pay. Selection out of the treatment in the sense that a substantial amount of Germans became self-employed (with no sick pay at all) or public sector employees is likewise unlikely. However, information on whether people changed their jobs as well as information on the labor market status allows us to control for this possibility.

As already mentioned in Section 2.2, due to union pressure, some employers agreed on the continuation of the old sick pay arrangement. There exist no official numbers how many employees did in reality not suffer the sick pay cuts and we cannot unambiguously identify these employees. We compensate this drawback by differentiating in our analysis between East and West Germany (collective bargaining coverage and union power is much lower in the Eastern part of Germany) and the firm size. Since our main purpose is to evaluate the overall reform effects, this lack of identification is a clear drawback but does not seriously hamper our analysis and conclusions. As there was a big uncertainty among employees and as employers are always free to provide voluntary lump sum payments, our results should not be severly downward biased in comparison to other sick pay studies.

5 Results

Table 1 visualizes the determinants of absence behavior. As expected, the age and the health status are important drivers of sickness absence which is also true for the schooling level and the level of job autonomy. In line with the literature, males and

[10] As Imbens and Wooldridge (2007) note, the two-step estimation approach proposed by Donald and Lang (2007) has several shortcomings and can not be applied in the case of only one treatment and one control group. Imbens and Wooldridge (2007) show that for the two group case, Donald and Lang's criticism is equivalent to the fundamental question in any DiD analysis on whether the observed effect goes entirely back to the policy change or not.

newly hired employees have fewer absence days and firm size is positively correlated with absenteeism. High regional unemployment rates serve as a worker discipline device as Shapiro and Stiglitz (1974) would call it. In the years 1997 and 1998, there was a clear downward trend in the absence rates as compared to 1994/1995. However, to be able to causally attribute this trend to the cut in sick pay, we need to differentiate between treated and controls.

[Insert Table 1 about here]

In Table 2 and 3 we find the unconditional DiD estimates on the duration and the incidence of sick leave behavior. The former table shows that the ratio of treated who were on sick leave for at least one day decreased by about 1.4 percentage points as compared to the base period. This incidence rate also slighty decreased for Control Group 1 (-0.1 percentage points) and even increased for Control Group 2 (+ 1.3 percentage points) leading to an overall DiD effect of about -1.3 and -2.7 percentage points, respectively. The latter table shows the evolution of the mean absence days. For the treatment group we observe a decrease from 5.9 to 5.1 mean absence days whilst public sector employees and trainees experienced a decrease from 6.6 to 6.1 days on sick leave. We also observe a decline for the self-employed (-0.4 days) resulting in DiD estimates of around -0.3 and -0.4 absence days, respectively.

[Insert Table 2 and 3 about here]

Figure 1 displays the cumulative distribution function for the pre- and post-reform period and those who were affected by the reform. Analogously to considering various measures of sickness absence in our regression specifications, this diagram visualizes the change in the *distribution* of absence days. Interestingly, we find that the whole distribution of absence days shifted to the left. We should emphasize that it is a parallel shift up to 15 absence days. For more than 15 days, the magnitude of the shift shrinks and is barely visible for more than 25 absence days. This supports the presumption that cuts in sick pay levels predominately affect short-term absenteeism rather than long-term absenteeism. The merit of having data on the the *total* number of absence days is likewise illustrated. The fact that every part of the distribution was shifted is a first hint that a causal effect might have played a role.

[Insert Figure 1 about here]

Table 4 shows the regression output which we obtain when estimating equation 1. Every column represents such a model where columns (1) to (3) compare the treated to the public sector sector employees and trainees (Control Group 1) and columns (4) to (6) use self-employed as Control Group 2. We see that the overall level of absenteeism is significantly higher for Control Group 1 as compard to the treated but significantly lower for Control Group 2. Outcome level differences of treated and controls do not matter as long as the common time trend assumption holds but the fact that the outcome level of the treated is embedded in the levels of the two control groups adds to the credibility of the results. The plausibility and robustness is automatically checked by the relying on two control groups.

In the first three models we get negative DiD point estimates of around 1.4 percentage points and in the last three models of around 3.0 percentage points. These point estimates are close to the unconditional DiD estimates in Table 2 and decrease only very slightly when further controls are stepwise included. [11] However, the effects are very imprecisely estimated such that no coefficient is significant at conventional levels.

[Insert Table 4 about here]

Table 5 reports further estimates of the effects on absence incidence. We now differentiate by East and West Germany and restrict the analysis to respondents with less than 10 (20) total absence days. This might be a useful tool to obtain more precise estimates since different parts of the distribution are likely to be affected differently as we have seen in Figure 1. Additionally, a priori considerations suggest that the effects are more pronounced in East Germany (see Section 2.2). Indeed,

[11] The effects are also very similar to OLS point estimates, e.g. for Model 3 in column 3 we get exactly the same DiD estimate along with a slighly smaller standard error of 0.0133.

we infer from Table 5 that the incidence rate of sickness absence decreased in East Germany signficantly by around 5 percentage points if we consider Control Group 1 and by around 8 percentage points if we consider Control Group 2 (p-values: 6.8 and 10.7 percent). We thereby focus on up to 20 total absence days. For West Germany and up to 10 absence days, we find a 5 percentage point decrease that is significant at the 10.7 percent level.

[Insert Table 5 about here]

Let us now consider the change in the number of absence days instead of the ratio of absent respondents. Models equivalent to equation 2 are estimated. Table 6 differentiates by region and by part of the absence distribution and contrasts the treated to Control Group 1. For whole Germany and up to 30 sickness days, the number of absence days fell by around 0.3 or about 5 percent as the initial level was 6 absence days (see Table 3). For West Germany, the parameters have negative signs but are imprecisely estimated. As for East Germany, we obtain a highly significant 0.7 absence day reform decrease once we include people with up to 20 sickness days.

[Insert Table 6 about here]

We repeat the same exercise with Control Group 2 (Table 7). The results confirm our previous findings. For whole Germany and up to 10 absence days, we find that the number of absence days decreased by 0.4 days (p-value: 6.5), a point estimate that we also get for West Germany with lower precision (p-value: 0.11). As in Table 6, in East Germany, the decrease was with up to 1 absence day much stronger.

[Insert Table 7 about here]

Polls and anecdotical evidence suggest that the effects differ by firm size and should be smallest for small firms due to a strong personal relationship and trust among employers and employees. Hence, we present regression output by firm size in Table 8. Every cell stands for one regression and only the relevant DiD-coefficient is displayed. As expected, the effect was smallest for small firms and is only for one specification at the border to significance (column 4, Inc10: -0.3 days, p-value: 0.11). For medium-size firms, the decrease amounts to about 0.35 days, no matter whether we use Control Group 1 or 2. One specification is significant for Control Group 2 (Column 5, Inc10: p-value: 0.06) and a second is almost significant using Control Group 1 (Column 2, Inc20: p-value: 0.11). The largest effects are obtained for big firms with more than 2000 employees. Here, we find a significant reform induced decrease of about 0.5 absence days when using Control Group 1 and a significant decrease of about 0.4 absence days when using Control Group 2.

[Insert Table 8 about here]

Critics might claim that - although we already accounted for the sample composition by controlling for various observable characteristics - selection out of the labor market might drive our results. Since unhealthy employees who are more prone to sickness absence are more likely to voluntarily or involuntarily leave the labor market, absence rates naturally decrease over time.

We accounted for this cirumstance by various means. First, as already mentioned, we controlled for a bunch of observables. Second, we restricted the sample to various measures of short-term absenteeism to avoid capturing selection effects. By dropping those with more than 30 total absence days, this concern is substantially alleviated since those employees are most likely to leave the labor market. Third, in 1998, a refreshment sample was drawn which stabilized the sample size and mitigated such selection issues. Fourth, we implicitly control for selection out of the labor market as long as it is treatment and employment-group unrelated since we have two different control groups. As a final robustness check, we use now the panel structure, identify and drop job changers, and balance the sample. Results are displayed in Table 9. The first row shows results when we balanced over four years. The estimates confirm our previous findings but the coefficients increase in size. For Germany and West Germany, we find highly significant effects of about 0.7 fewer absence days. Probably due to the fragile labor market in East Germany and the low job stability which leads to a substantial loss of observations in case of balancing and dropping job changers, the estimate for East Germany is very imprecise but of reasonable size and sign.

For medium-sized and huge firms we find a significant decrease of around half a day. Since balancing over four years leads to a substantial loss of observations, we alternatively balance over the two years 1996 and 1997 and present the results in row two. Now the estimate for East Germany turns out to be significant and is -1. The estimate for firms with more than 2,000 employees is also significant and indicates a decrease of half a day.

[Insert Table 9 about here]

In addition we performed various other robustness checks that all confirm what we have presented so far. No matter whether we use specifications with Control Group 1 or 2, for East Germany and huge firms, we almost always find significant negative effects. For the rest of the specifications, the results are mixed and we get precisely as well as imprecisely estimated negative effects. [12]

First, we restricted the sample to full-time employed aged 25 to 55. For whole Germany, the decrease is around 0.3, composed of a 0.6 day decrease in East and a 0.2 day decrease in West Germany. For medium-sized firms, the effect is about -0.4 and for large firms it is approximately -0.6. In general, the size of the estimates is a little big smaller for this subsample.

For singles we find the same pattern but inversely to middle-aged full time employed, the coefficients' sizes increase moderately. As work accident related sick leave was excluded from the sick pay cut, we performed the same analyses excluding those who had a work accident in the relevant year. The results remained stable and did not change substantially.

We also split the sample at the mean wage. While the effects for the poorer half of the population follow the usual discussed pattern, both in significance and magnitude, most of the estimates for the high earners are imprecise and the point estimates are smaller. Various explanations for this finding are imaginable. Low income earners might be more dependent on their salary and react stronger to cuts in benefits. An alternative argument could be that low paid jobs are less satisfying and that thus the degree of moral hazard and its reduction are higher. It could also

[12] The results are not displayed but available from the authors upon request.

simply be that better paid employees are more likely to work in properous firms that underlie collective wage agreements with supplementary sick leave payments which exceed the legal requirements (see Section 2).

A method for checking the plausibility of the common time trend assumption is to perform placebo regressions and to estimate reform effects for years without a reform. For the assumption of common time trends of controls and treated to hold, none of the placebo reform effects should be significant. Table 11 displays placebo regression results on the duration of long-term absenteeism for the years 1993 to 1996. All placebo estimates turn out to be insignificant.

[Insert Table 11 about here]

Summing up, we would like to emphasize that the point estimates are remarkably robust to a variety of specifications, irrespective of whether we contrast the treated to Control Group 1 or Control Group 2. This speaks for the credibility of our identification strategy and estimates as both control groups differ significantly with respect to most of their characteristics and as the absence rate level for the treated lies in between the absence rate levels of the two control groups. Although we obtain imprecise estimates for some specifications, we find significant effects for any of the main subspecifications.

5.1 Reduction of Labor Costs and Job Creation

In a first step, we calculate the overall reduction in labor costs by comparing total employer-provided sick pay in the pre-reform years 1994/1995 to the total benefit sum in the post-reform years 1997/1998. We do this by summing over the frequency weighted product of absence days multiplied by the daily gross wage for each individual in the pre-reform years.[13] We do the same for the post-reform years but

[13] In contrast to the previous subsection, we use for this calculation all employees between 18 and 65 who work in the private sector and who were affected by the law. For employees who claimed that they had a long-term absence spell of more than six weeks, we set the value for absence days to 42 as only the first six weeks of sick leave are paid by the employer. Frequency weights, which are computed according to data from the federal statistical office, are provided by the SOEP group (SOEPGroup, SOEPGroup). Absence days and gross wages are included in the SOEP data. The

multiply each absence day with only 80 percent of the daily gross wage. The difference of these total sums yields the total labor cost savings if we assume that all employers provided sick pay according to the legal requirements. We obtain a total saving estimate of €6.126 billion for the two post-reform years.

This total amount of labor cost savings can be decomposed into three components. The first component goes back to the lowering of the legally defined sick pay for the first six weeks per sickness episode from 100 to 80 percent of foregone gross wages. In a second step, this amount is approximated by comparing the total sick leave payments in the pre-reform period to hypothetical sick leave payments for the same period and individuals assuming that the sick pay was already lowered at that time. We thus disentangle the direct savings effect from the savings effect that is induced by decreasing absence rates as a consequence of the reform. Our estimates yield a total direct saving effect of €4.329 billion for both years. If we assume that only half of the firms applied the new law stringently, these direct savings reduce to €2.165 billion. Note that this is a conservative estimate as explained in Section 2.2.[14]

In a third step, we calculate the indirect labor cost savings which were triggered by the reform induced decrease in absenteeism and which represent the second component of total reform savings. From Table 6, we infer that the overall reform-induced reducation in absence days equals about 0.3 days. Hence we multiply this reduction by the average daily gross wage in the pre-reform years and multiply the product with the frequency weighted number of employees in both years, resulting in an indirect saving effect of €736 million.[15] The third component is the residual saving amount which is caused by a decreasing time trend and changes in the wage structure.

The total reform induced decrease in labor costs is thus $(2.165+0.736)/2 =$ €1,45

SOEP group takes great effort in accurately collecting income data and imputing missing data consistently (Frick and Grabka, 2005).

[14] Of course, we thereby implicitly assume that employees who worked in firms which applied the new law did not differ systematically in terms of absence days and wages from those who worked in firms which provided the old sick pay voluntarily.

[15] Here again, we focus on the same dataset which we used to obtain the estimated decrease of 0.3 days as we would otherwise overestimate the savings. To be precise, we restrict the sample to employees with less than 30 total absence days. This approach to calculate the indirect reform savings neglects spillover effects in the sense that de facto non-treated reduced their sick leave days because of peer-effects, sensitization, or nescience.

billion per year.[16]

In 1997, the Research Insitute of the Federal Employment Agency (IAB) calculated by means of a general macroeconomic simulation model for Germany that a reduction of the social security contribution rate by one percentage point would lead to 120,000 new jobs (Zika, 1997). These numbers were confirmed by other studies (Feil et al., 2008; Meinhardt and Zwiener, 2005).[17] In Germany, social contribution rates finance five pillars of the German pay-as-you-go Social Security system, are mandatorily charged on the salary, equally paid by employer and employee, and amount to around 40 percent of the gross wage. Since decades these indirect labor taxes have been of great concern for economists and policy makers as they make labor more expensive and weaken incentives to take up work. Therefore, a reduction or stabilization of these contribution rates is one of the most important objectives for every government and was a main intention for many reforms.

For whole Germany, one percentage point social security contribution rates equaled about €5 billion in 1997. If we assume that job creation was solely incuded by decreasing labor costs and increasing labor demand, our back-of-the envelope calculation yields that the reform led to the creation of approximately 70,000 new jobs.[18] Under the assumption that half of the job creation effect induced by reductions in social contribution rates goes back to an increased labor supply and a higher product demand due to increased net wages, this number reduces to 35,000.[19]

[16] By combining data from the federal statistical office on the total number of employees subject to social insurance contributions in the different years with SOEP data, we checked the plausibility and sensitivity of this estimate. By this means we control for panel attrition. To calculate the different saving elements, we multiply official employment data with SOEP absence rates and income data and get a very similar estimate of $(2.388 + 0.786)/2 =$ €1,587 billion per year (German Federal Statistical Office, 1998, 1996).

[17] Feil et al. (2008) employed three different simulation models and found employment effects up to 194,000 although it was assumed that the cut in contribution rates was financed by a flat-rate premium or an increase in VAT. Meinhardt and Zwiener (2005) also assumed counterfinancing and estimated the job creation effect to lie around 100,000.

[18] At that time, it was common consensus among economists that the comparatively high labor costs were one of the main barriers for job creation in Germany (Sachverständigenrat zur Begutachtung der gesamtwirtschaftlichen Entwicklung, 1996; Sachverständigenrat zur Begutachtung der gesamtwirtschaftlichen Entwicklung, 2002).

[19] However, the macroeconomic simulation models with which the increased employment effects were derived, assumed a constant labor supply, either (Feil et al., 2008). In our rough calculation, we neglect that the reduction in sick pay led to *lower* net wages and that a potential associated reduction in demand might have offset parts of the job creation effect. We also abstain from the

As the reforms led to mass demonstrations and strikes, the reduction in sick leave payments should be contrasted with the costs that arose from this by-product of the reform. That the reform did not predominately reduce moral hazard but induced more presenteeism and led to an overall decreasing labor productivitly can not be excluded as well.

Taking all evidence together it seems reasonable to conclude that approximately 50,000 extra jobs could have been created in the long run due to lower labor costs under the assumption of a constant labor productivity and moderate short-term strike costs.

6 Conclusion

A natural experiment from Germany enables us to estimate the causal effect of a cut in the replacement rate from 100 to 80 percent of foregone gross wages on sickness absence. We do this by relying on two different control groups which makes the conventional common time trend assumption in our difference-in-differences setting reliable. Typical selection issues common to evaulation studies can be handled with as we employ longitudinal SOEP household data and can identify job changers. The law universally applied to every dependent employee in the private sector and was determined at the federal level.

Focussing on employees with up to 30 absence days in total, our findings suggest that the reform significantly reduced the ratio of employees with at least one absence day per year by up to 5 percent. We also show that the reform reduced the number of absence days on average by around 0.3 days or 5 percent. The effect was more pronounced in East Germany and increased with firm size. This can be traced back to the fact that more employers in West Germany and small (handcraft) establishments voluntarily agreed to provide sick pay according to the old regulations. Under the conservative assumption that only half of the employers applied the new law strictly, the overall labor supply elasticity with respect to the benefit level and for employees in the private sector would lie around 0.5 which is in line with previous findings

effect that an increased presence at the workplace may lead to a higher productivity and may weaken labor demand.

(Curington, 1994).

Keeping this assumption, we estimate that the direct labor cost saving effect due the decrease in benefit levels was €1.1 billion p.a. for whole Germany. Adding the indirect reform saving effect which results from the decrease in absenteeism, we end up with a total amount of approximately €1.5 billion p.a. Using the findings of various other studies which are derived from macroeconomic simulation models for Germany, a rough calculation suggests that the reform led to the creation of 50,000 new jobs.

To what extend the success of such reforms depend on cultural peculiarities and macroeconomic conditions is of great importance and should be the focus of further studies. Unintended side-effects like strikes and mass demonstrations may have offset or even overcompensated the pure reform effects but are beyond the scope of this study.

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Figure 1: Cdf Pre-and Post-Reform Periods: Treatment Group

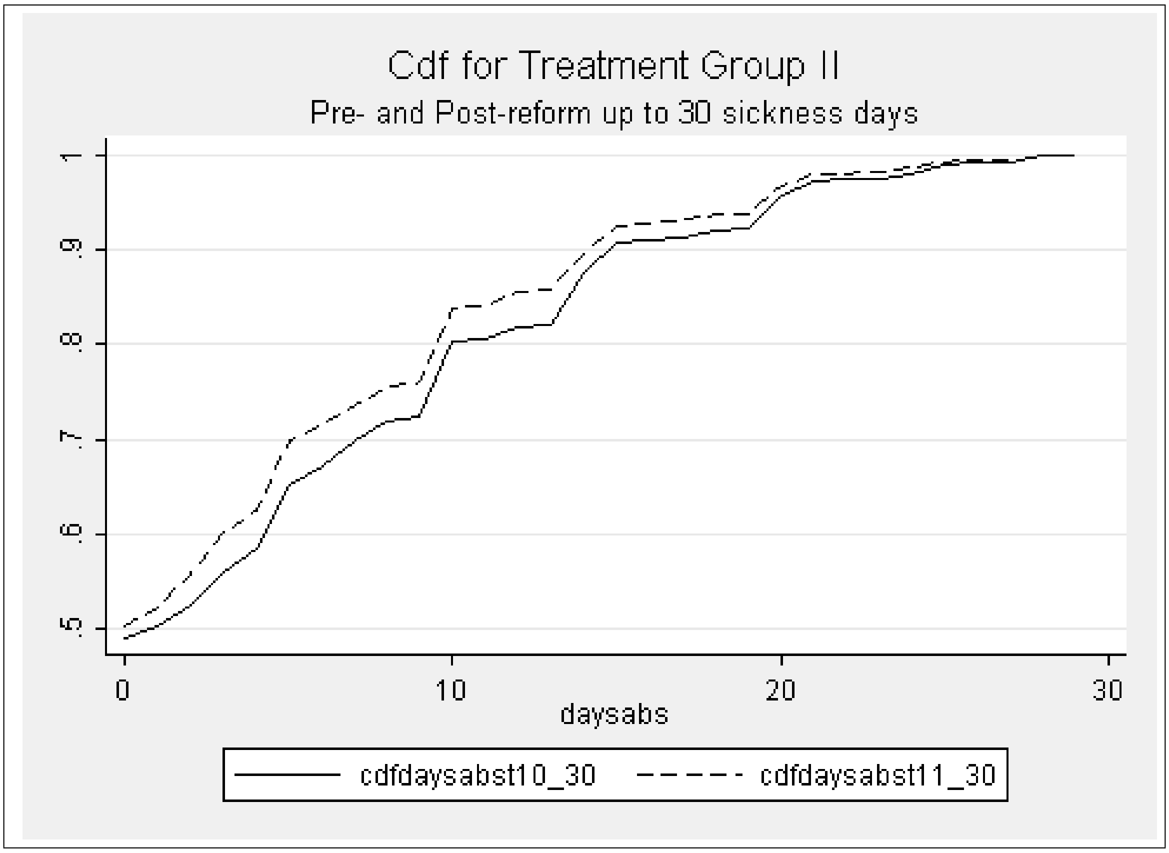

Table 1: Determinants of Long-Term Absenteeism: Zero-Inflated NegBin-2

Variable	**Coefficient**	**Standard Error**
Personal characteristics		
Female (d)	1.054***	0.176
Age	-0.344***	0.054
Age square/100	0.354***	0.001
Immigrant (d)	0.641**	0.295
East German(d)	1.326***	0.383
Partner (d)	0.322	0.224
Married (d)	-0.024	0.226
Children (d)	0.125	0.178
Disabled (d)	2.351***	0.464
Good health (d)	-2.306***	0.168
Bad health (d)	3.450***	0.367
No sports (d)	-0.087	0.160
Educational characteristics		
Degree after 8 years' schooling (d)	-0.531	0.402
Degree after 10 years' schooling (d)	-0.883**	0.408
Degree after 12 years' schooling (d)	-1.641***	0.451
Degree after 13 years' schooling (d)	-1.738***	0.387
Other degree (d)	-0.055	0.454
Work in job trained for (d)	-0.180	0.156
No. years in company last year	-0.002	0.012
Job characteristics		
New job (d)	-0.476***	0.177
Medium size company (d)	1.725***	0.217
Big company (d)	2.581***	0.244
Huge company (d)	2.993***	0.267
White collar worker (d)	-1.031***	0.168
High job autonomy (d)	-1.671***	0.209
Gross wage per month/1000	-0.130	0.000
Regional unemployment rate	-0.108***	0.040
Post-reform (d)	-0.666***	0.144
Year 1997 (d)	-0.1909	0.1320
Log pseudolikelihood	-63783.73	
χ^2	992.298	
N	26066	

(d) for discrete change of dummy variable from 0 to 1; marginal effects are displayed
* p<0.10, ** p<0.05, *** p<0.01
Dependent variable: number of sick leave days
Zero-inflated NegBin-2 model is estimated
Robust standard errors in parentheses are adjusted for clustering on person id
Regression includes state dummies
Left out reference categories are dropout, blue collar worker, and small company

Table 2: Unconditional DiD Estimates on the Sickness Absence Incidence

	1994/1995	1997/1998	Difference	Diff-in-Diff
Treatment Group	0.5202	0.5063	-0.0138	
	(0.0057)	(0.0056)	(0.0079)	
Control Group 1	0.5888	0.5876	-0.0012	-0.0127
(public sector, trainees)	(0.0080)	(0.0078)	(0.0112)	(0.0136)
Control Group 2	0.2000	0.2135	0.0135	-0.0273
(self-employed)	(0.0129)	(0.0126)	(0.0181)	(0.0198)

Average incidence rate is displayed

Standard errors in parentheses

Table 3: Unconditional DiD Estimates on the Sickness Absence Duration

	1994/1995	1997/1998	Difference	Diff-in-Diff
Treatment Group	5.9446	5.1317	-0.8129	
	(0.0900)	(0.0841)	(0.1233)	
Control Group 1	6.5917	6.0691	-0.5226	-0.2565
(public sector, trainees)	(0.1307)	(0.1213)	(0.1781)	(0.2124)
Control Group 2	2.1539	1.7941	-0.3598	-0.4193
(self-employed)	(0.1819)	(0.1425)	(0.2389)	(0.2643)

Average number of absence days is displayed

Standard errors in parentheses

Table 4: Difference-in-Differences Estimation on the Incidence of Sickness Absence

	Treated vs. Controls I			***Treated vs. Controls II***		
Variable	**Model 1**	**Model 2**	**Model 3**	**Model 1**	**Model 2**	**Model 3**
DiDg (d)	**-0.0157**	**-0.0146**	**-0.0137**	**-0.0341**	**-0.0342**	**-0.0309**
	(0.0140)	**(0.0140)**	**(0.0141)**	**(0.0270)**	**(0.0270)**	**(0.0268)**
	[0.2608]	**[0.2974]**	**[0.3292]**	**[0.2064]**	**[0.2059]**	**[0.2494]**
Post reform dummy (d)	0.0202	0.0197	0.0183	0.0391	0.0401	0.0299
	(0.0132)	(0.0133)	(0.0133)	(0.0264)	(0.0264)	(0.0262)
Treatment Group (d)	-0.0656***	-0.0617***	-0.0505***	0.3033***	0.3025***	0.3174***
	(0.0113)	(0.0114)	(0.0116)	(0.0166)	(0.0167)	(0.0160)
Job characteristics	no	no	yes	no	no	yes
Educational characteristics	no	yes	yes	no	yes	yes
Personal characteristics	yes	yes	yes	yes	yes	yes
Regional unemployment rate	yes	yes	yes	yes	yes	yes
State dummies	yes	yes	yes	yes	yes	yes
R-squared	0.0342	0.0348	0.0407	0.0584	0.0590	0.0384
χ^2	776.9837	788.2713	906.1472	925.0549	932.2012	561.8180
N	23382	23382	23382	17657	17657	17657

(d) for discrete change of dummy variable from 0 to 1; marginal effects are displayed

* p<0.1, ** p<0.05, *** p<0.01

Dependent variable: dummy that is 1 if respondent was on sick leave

Probit models are estimated

Standard errors in parentheses are adjusted for clustering on person id

P-values in square brackets

Table 5: Difference-in-Differences Estimation on the Incidence of Sickness Absence: East vs. West

	Treated vs. Controls I				***Treated vs. Controls II***			
	West		**East**		**West**		**East**	
Variable	**Inc10**	**Inc20**	**Inc10**	**Inc20**	**Inc10**	**Inc20**	**Inc10**	**Inc20**
DiDg (d)	**0.0086**	**0.0067**	**-0.0300**	**-0.0485***	**-0.0539**	**-0.0339**	**-0.0620**	**-0.0811**
	(0.0190)	**(0.0174)**	**(0.0280)**	**(0.0266)**	**(0.0335)**	**(0.0321)**	**(0.0490)**	**(0.0503)**
	[0.6524]	**[0.6996]**	**[0.2846]**	**[0.0682]**	**[0.1075]**	**[0.2912]**	**[0.2051]**	**[0.1074]**
Post reform dummy (d)	0.0075	0.0026	0.1057***	0.1222***	0.0753**	0.0467	0.0987*	0.1013*
	(0.0172)	(0.0157)	(0.0373)	(0.0354)	(0.0325)	(0.0311)	(0.0564)	(0.0572)
Treatment Group (d)	-0.0690***	-0.0731***	-0.0182	-0.0218	0.2422***	0.2808***	0.1982***	0.2654***
	(0.0161)	(0.0147)	(0.0218)	(0.0212)	(0.0219)	(0.0234)	(0.0294)	(0.0315)
Job characteristics	yes	yes	yes	yes	yes	yes	yes	yes
Educational characteristics	yes	yes	yes	yes	yes	yes	yes	yes
Personal characteristics	yes	yes	yes	yes	yes	yes	yes	yes
Regional unemployment rate	yes	yes	yes	yes	yes	yes	yes	yes
State dummies	yes	yes	yes	yes	yes	yes	yes	yes
R-squared	0.0389	0.0378	0.0373	0.0413	0.0641	0.0669	0.0436	0.0519
χ^2	496.8129	573.6356	203.2591	268.2459	603.3558	735.7517	154.0060	219.8394
N	13406	15892	5130	6099	10789	12479	3659	4222

(d) for discrete change of dummy variable from 0 to 1; marginal effects are displayed

* p<0.1, ** p<0.05, *** p<0.01

Dependent variable: dummy that is 1 if respondent was on sick leave in case that total numer of absence days did not exceed 10 (20)

Probit models are estimated

Standard errors in parentheses are adjusted for clustering on person id

P-values in square brackets

Table 6: Difference-in-Differences Estimation on the Number of Absence Days: Treated vs. Controls I

	Germany			**West Germany**			**East Germany**		
Variable	**Inc10**	**Inc20**	**Inc30**	**Inc10**	**Inc20**	**Inc30**	**Inc10**	**Inc20**	**Inc30**
DiDg (d)	**-0.0692**	**-0.2441**	**-0.3446***	**0.0257**	**-0.0046**	**-0.1684**	**-0.2038**	**-0.7052****	**-0.5724**
	(0.1070)	**(0.1621)**	**(0.2001)**	**(0.1270)**	**(0.1938)**	**(0.2372)**	**(0.1970)**	**(0.2970)**	**(0.3789)**
	[0.5178]	**[0.1320]**	**[0.0851]**	**[0.8398]**	**[0.9810]**	**[0.4778]**	**[0.3009]**	**[0.0176]**	**[0.1309]**
Post reform dummy(d)	0.1323	0.1778	0.1134	0.0076	-0.0682	-0.0615	0.3815*	0.8503**	0.4033
	(0.0945)	(0.1467)	(0.1832)	(0.1095)	(0.1684)	(0.2080)	(0.2244)	(0.3527)	(0.4572)
Treatment Goup (d)	-0.2312***	-0.5054***	-0.4846***	-0.3002***	-0.6708***	-0.5729***	-0.1882	-0.2351	-0.5449*
	(0.0894)	(0.1377)	(0.1676)	(0.1091)	(0.1680)	(0.2009)	(0.1576)	(0.2465)	(0.3133)
Job characteristics	yes	yes	yes	yes	yes	yes	yes	yes	yes
Educational characteristics	yes	yes	yes	yes	yes	yes	yes	yes	yes
Personal characteristics	yes	yes	yes	yes	yes	yes	yes	yes	yes
Regional unemployment rate	yes	yes	yes	yes	yes	yes	yes	yes	yes
State dummies	yes	yes	yes	yes	yes	yes	yes	yes	yes
Log pseudo likelihood	-30998.98	-47957.14	-56013.88	-23263.85	-35452.4	-41233.96	-7683.519	-12442.3	-14709.67
χ^2	447.2195	916.3293	1164.9220	361.8355	754.6602	978.8278	78.3795	157.3640	173.9755
N	18536	21991	23382	13406	15892	16892	5130	6099	6490

(d) for discrete change of dummy variable from 0 to 1; marginal effects are displayed

* p<0.1, ** p<0.05, *** p<0.01

Dependent variable: total number of absence days; sample restricted to respondents with less than 31 (21, 11) total absence days

Zero-inflated NegBin-2 models are estimated

Standard errors in parentheses are adjusted for clustering on person id

P-values in square brackets

Table 7: Difference-in-Differences Estimation on the Number of Absence Days: Treated vs. Controls II

	Germany			**West Germany**			**East Germany**		
Variable	**Inc10**	**Inc20**	**Inc30**	**Inc10**	**Inc20**	**Inc30**	**Inc10**	**Inc20**	**Inc30**
DiD (d)	**-0.4055***	**-0.1156**	**0.3080**	**-0.4229**	**0.1470**	**0.2646**	**-0.4642**	**-0.9846**	**0.2739**
	(0.2198)	**(0.3785)**	**(0.4842)**	**(0.2653)**	**(0.4551)**	**(0.5793)**	**(0.3740)**	**(0.6435)**	**(0.8533)**
	[0.0651]	**[0.7600]**	**[0.5247]**	**[0.1109]**	**[0.7467]**	**[0.6479]**	**[0.2146]**	**[0.1260]**	**[0.7482]**
Post reform dummy (d)	0.5070**	0.1009	-0.4779	0.4972*	-0.1839	-0.4410	0.6262	0.8805	-0.7896
	(0.2162)	(0.3714)	(0.4757)	(0.2595)	(0.4414)	(0.5653)	(0.3993)	(0.6978)	(0.8989)
Treatment Group (d)	1.4764***	2.5030***	2.9824***	1.5154***	2.4061***	2.8665***	1.3356***	2.6967***	3.1775***
	(0.1073)	(0.1918)	(0.2518)	(0.1315)	(0.2395)	(0.3133)	(0.1809)	(0.3011)	(0.4115)
Job characteristics	yes	yes	yes	yes	yes	yes	yes	yes	yes
Educational characteristics	yes	yes	yes	yes	yes	yes	yes	yes	yes
Personal characteristics	yes	yes	yes	yes	yes	yes	yes	yes	yes
Regional unemployment rate	yes	yes	yes	yes	yes	yes	yes	yes	yes
State dummies	yes	yes	yes	yes	yes	yes	yes	yes	yes
Log pseudo likelihood	-21640.86	-32910.07	-38544.56	-16736.75	-25140.89	-29432.18	-4855.142	-7710.862	-9049.574
χ^2	390.0577	786.2499	941.3771	333.9078	692.5018	860.4560	72.1082	116.7472	116.5507
N	14448	16701	17657	10789	12479	13212	3659	4222	4445

(d) for discrete change of dummy variable from 0 to 1; marginal effects are displayed

* p<0.1, ** p<0.05, *** p<0.01

Dependent variable: total number of absence days; sample restricted to respondents with less than 31 (21, 11) total absence days

Zero-inflated NegBin-2 models are estimated

Standard errors in parentheses are adjusted for clustering on person id

P-values in square brackets

Table 8: Difference-in-Differences Estimation on the Number of Absence Days: By Firm Size

	Treated vs. Controls I			***Treated vs. Controls II***		
Model	**small**	**medium**	**huge**	**small**	**medium**	**huge**
Inc10: DiDg (d)	-0.0028	-0.0799	-0.1870	-0.2916	-0.3657*	-0.3994**
	(0.1468)	(0.1466)	(0.1675)	(0.1840)	(0.1915)	(0.1852)
	[0.9847]	[0.5858]	[0.2641]	[0.1131]	[0.0563]	[0.0311]
Inc20: DiDg (d)	-0.1103	-0.3475	-0.5030**	-0.0613	-0.2663	-0.3738
	(0.2290)	(0.2191)	(0.2482)	(0.3367)	(0.3461)	(0.3330)
	[0.6300]	[0.1127]	[0.0427]	[0.8554]	[0.4417]	[0.2616]
Inc30: DiDg (d)	-0.3409	-0.2558	-0.6190**	0.1415	0.2531	-0.0716
	(0.2746)	(0.2770)	(0.3009)	(0.4271)	(0.4555)	(0.4400)
	[0.2144]	[0.3559]	[0.0396]	[0.7405]	[0.5785]	[0.8707]

(d) for discrete change of dummy variable from 0 to 1; marginal effects are displayed

* p<0.1, ** p<0.05, *** p<0.01

Every cell represents one regression model. All available controls are included.

Dependent variable: total number of absence days; sample restricted to respondents with less than 31 (21, 11) total absence days

Zero-inflated NegBin-2 models are estimated

Standard errors in parentheses are adjusted for clustering on person id

P-values in square brackets

Table 9: Difference-in-Differences Estimation on the Number of Absence Days: Balanced Sample

	Region			***Firm Size***		
Variable	**All**	**West**	**East**	**Small**	**Medium**	**Huge**
1994/1995 vs. 1997/1998: DiD (d)	-0.6878**	-0.8074**	-0.5049	-0.3885	-0.4759*	-0.5069**
	(0.3285)	(0.3852)	(0.6013)	(0.2550)	(0.2606)	(0.2511)
	[0.0363]	[0.0361]	[0.4011]	[0.1277]	[0.0679]	[0.0436]
1996 vs. 1997: DiD (d)	-0.4145	-0.2119	-0.9958*	-0.3177	-0.3578	-0.4944*
	(0.3694)	(0.4566)	(0.5337)	(0.2836)	(0.2977)	(0.2786)
	[0.2618]	[0.6427]	[0.0621]	[0.2627]	[0.2295]	[0.0760]

(d) for discrete change of dummy variable from 0 to 1; marginal effects are displayed

* p<0.1, ** p<0.05, *** p<0.01

Every cell represents one regression model. All available controls are included.

Dependent variable: total number of absence days; sample restricted to respondents with less than 31 (21, 11) total absence days

Zero-inflated NegBin-2 models are estimated

All regressions include Treatment Group II, i.e. contrast the treated to the self-employed.

Standard errors in parentheses are adjusted for clustering on person id

P-values in square brackets

Table 10: Robustness Checks on the Number of Absence Days (not shown)

	Treated vs. Controls I					***Treated vs. Controls II***				
Model	**full-time; age 25 to 55**	**singles**	**Wage > median**	**Wage < median**	**no work accident**	**full-time; age 25 to 55**	**singles**	**Wage > median**	**Wage < median**	**no work accident**
Inc10: DiDg (d)	-0.0625	-0.0836	-0.0019	-0.2078	-0.0404	-0.4189*	-0.2343	-0.2154	-0.5537*	-0.4460*
	(0.1350)	(0.2135)	(0.1487)	(0.1563)	(0.1080)	(0.2527)	(0.5386)	(0.3057)	(0.3219)	(0.2501)
	[0.6437]	[0.6953]	[0.9897]	[0.1835]	[0.7084]	[0.0974]	[0.6636]	[0.4810]	[0.0854]	[0.0745]
Inc20: DiDg (d)	-0.1784	-0.2105	-0.0886	-0.4542*	-0.2670*	-0.2821	-1.5552**	0.0616	-0.3192	0.0479
	(0.2034)	(0.3246)	(0.2234)	(0.2412)	(0.1619)	(0.4201)	(0.7416)	(0.4830)	(0.5528)	(0.4250)
	[0.3803]	[0.5166]	[0.6918]	[0.0597]	[0.0991]	[0.5018]	[0.0360]	[0.8986]	[0.5636]	[0.9103]
Inc30: DiDg (d)	-0.3453	-0.0844	-0.2753	-0.5038*	-0.4082**	0.1138	-1.3919	0.4221	0.0685	0.5018
	(0.2490)	(0.3944)	(0.2686)	(0.3078)	(0.1976)	(0.5260)	(1.0416)	(0.6059)	(0.7973)	(0.5398)
	[0.1656]	[0.8305]	[0.3055]	[0.1017]	[0.0389]	[0.8287]	[0.1815]	[0.4860]	[0.9315]	[0.3526]

(d) for discrete change of dummy variable from 0 to 1; marginal effects are displayed
* p<0.1, ** p<0.05, *** p<0.01
Every cell represents one regression model. All available controls are included.
Dependent variable: total number of absence days; sample restricted to respondents with less than 31 (21, 11) total absence days
Zero-inflated NegBin-2 models are estimated
Standard errors in parentheses are adjusted for clustering on person id
P-values in square brackets

Table 11: Difference-in-Differences Estimation on the Number of Absence Days: Placebo Estimates

	Treated vs. Controls I			*Treated vs. Controls II*		
Model	**All**	**West Germany**	**East Germany**	**All**	**West Germany**	**East Germany**
DiD96 (d)	-0.1797	-0.1512	-0.2683	-0.3229	-0.4120	-0.0686
	(0.1418)	(0.1697)	(0.2526)	(0.3172)	(0.3746)	(0.5861)
DiD95 (d)	-0.0392	-0.0578	0.0504	0.4930	0.4942	0.4868
	(0.1482)	(0.1748)	(0.2814)	(0.3446)	(0.4132)	(0.6078)
DiD94 (d)	0.1875	0.1460	0.3091	0.0827	0.1906	-0.2434
	(0.1444)	(0.1728)	(0.2674)	(0.3315)	(0.3857)	(0.6573)

(d) for discrete change of dummy variable from 0 to 1; marginal effects are displayed

* $p<0.1$, ** $p<0.05$, *** $p<0.01$

Every column represents one regression model. All available controls are included.

Dependent variable: total number of absence days; sample restricted to respondents with less than 11 total absence days

Zero-inflated NegBin-2 models are estimated

Standard errors in parentheses are adjusted for clustering on person id

Appendix A

Table 12: Variable Means by Treatment and Control Groups

Variable	Treated: Mean (s.d.)	Controls: Mean (s.d.)	ControlsII: Mean (s.d.)	Min.	Max.
Dependent variables					
Noabs	0.513	0.588	0.207	0	1
	(0.500)	(0.492)	(0.405)		
Incmissed10days	0.394	0.466	0.152	0	1
	(0.489)	(0.499)	(0.359)		
Incmissed20days	0.483	0.561	0.190	0	1
	(0.500)	(0.496)	(0.393)		
Daysabs	5.552	6.325	1.965	0	30
	(7.643)	(7.833)	(5.125)		
Missed10days	2.316	2.689	0.790	0	10
	(3.413)	(3.530)	(2.183)		
Missed20days	4.276	5.012	1.469	0	20
	(5.773)	(6.054)	(3.842)		
Personal characteristics					
Female	0.370	0.528	0.291	0	1
	(0.483)	(0.499)	(0.454)		
Age	39.394	37.704	42.982	18	65
	(10.474)	(12.203)	(9.772)		
Agesq	1,662	1,570	1,943	324	4,225
	(865)	(950)	(868)		
Immigrant	0.210	0.095	0.113	0	1
	(0.408)	(0.293)	(0.317)		
East German	0.250	0.334	0.267	0	1
	(0.433)	(0.472)	(0.443)		
Partner	0.801	0.671	0.820	0	1
	(0.399)	(0.470)	(0.384)		
Married	0.699	0.586	0.746	0	1
	(0.459)	(0.493)	(0.436)		
Children	0.478	0.450	0.498	0	1
	(0.500)	(0.497)	(0.500)		
Disabled	0.041	0.042	0.027	0	1
	(0.199)	(0.201)	(0.162)		
Health good	0.633	0.637	0.617	0	1
	(0.482)	(0.481)	(0.486)		
Health bad	0.081	0.081	0.078	0	1
	(0.272)	(0.273)	(0.268)		
No sports	0.411	0.295	0.421	0	1
	(0.492)	(0.456)	(0.494)		
Educational characteristics					
Drop-out	0.046	0.036	0.022	0	1

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... Table 12 continued

Variable	Treated: Mean (s.d.)	Controls: Mean (s.d.)	ControlsII: Mean (s.d.)	Min.	Max.
	(0.209)	(0.186)	(0.146)		
Degree after 8 years of schooling	0.354	0.239	0.313	0	1
	(0.478)	(0.427)	(0.464)		
Degree after 10 years of schooling	0.330	0.412	0.314	0	1
	(0.470)	(0.492)	(0.464)		
Degree after 12 years of schooling	0.038	0.037	0.054	0	1
	(0.191)	(0.189)	(0.226)		
Degree after 13 years of schooling	0.122	0.234	0.235	0	1
	(0.328)	(0.423)	(0.424)		
Other degree	0.109	0.042	0.062	0	1
	(0.312)	(0.201)	(0.242)		
Work in job trained for	0.545	0.553	0.592	0	1
	(0.498)	(0.497)	(0.492)		
No. of years in company	8.700	9.694	8.467	0	49.7
	(8.962)	(9.665)	(8.874)		
Job characteristics					
Blue collar worker	0.512	0.145	0.003	0	1
	(0.500)	(0.352)	(0.059)		
White collar worker	0.488	0.477	0.005	0	1
	(0.500)	(0.500)	(0.070)		
New job	0.194	0.179	0.159	0	1
	(0.395)	(0.384)	(0.366)		
Small company	0.287	0.154	0.578	0	1
	(0.452)	(0.361)	(0.494)		
Medium company	0.307	0.268	0.031	0	1
	(0.461)	(0.443)	(0.173)		
Big company	0.215	0.258	0.018	0	1
	(0.411)	(0.437)	(0.133)		
Huge company	0.190	0.320	0.019	0	1
	(0.392)	(0.466)	(0.136)		
High job autonomy	0.172	0.246	0.586	0	1
	(0.378)	(0.431)	(0.493)		
Gross income per month	2,009	1,817	2,677	0	51,128
	(1,134)	(1,013)	(2,583)		
Regional unemployment rate	11.459	12.234	11.651	7.0	21.7
	(3.852)	(4.026)	(3.856)		
N	15,648	7,734	2,009		

Number of observations only applies for the controls, *Noabs*, and *dayabs*