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 effects of a reduction in sick pay levels on sickness absence behavior, labor costs, and the creation of new jobs. 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 significantly increased the ratio of employees with no days of absence by about 7.5 percent. The mean number of absence days per year significantly decreased by about 5 percent. The effects were more pronounced in East Germany which can be explained by a stricter application of the new law in this region. Effect heterogeneity is of relevance since singles, middle-aged full time employed, and the poor revealed stronger behavioral adaptions than the population average. According to our calculations, 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 all 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 leave benefits and sickness absence despite its enormous relevance for labor supply, labor costs and labor productivity, population health, and the functioning of social insurance systems as well as private insurance markets.

Only 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 4 up to 29 days per year and employee (see Figure 1), suggesting that institutional arrangements as well as cultural influences are of major importance. The numbers illustrate the need for further explanations for these broad disparities. They also reinforce the presumption of a huge potential for efficiency gains in the market for sickness absence insurance.

[Insert Figure 1 about here]

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 legally 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, as Figure 1 demonstrates, Germany lies in the middle field of the countries ranked and some cross country comparisons even rank Germany below the international average in terms of sickness absence rates (Bonato and Lusinyan, 2004). On explanation might lie in the anecdotical evidence claiming Germans to have a strong work ethic. Another reason might be a 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 the 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, labor costs, and employment creation. 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). By relying on two sound control groups, we estimate the actual reform effect rather than the intended effect had the reform been strictly applied in every single firm. 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 sample 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 the 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. We also 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. Finally, this study exemplarily illustrates the pitfalls that policymakers face when planning to implement unpopular reforms. Had the purpose of the reform been better communicated and had the new law been applied one-to-one by all employers, our calculations suggest that twice as much jobs could have been created as actually had been created.

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 provide some broad estimates of the reform induced reduction in labor costs and the creation of new jobs. Secion 6 concludes.

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 of foregone gross wages for those who are insured with the Statutory Health Insurance (SHI).[2]

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

[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 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).

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. Due to political considerations and other laws, public sector employees as well as trainees were exempted from the reform.[5] Likewise unaffected were employees on sick leave due to work accidents. As an alternative to the cut in sick 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.

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

[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.*

[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).

that around 50 percent of these firms did not apply the law. Anecdotical evidence traces this back to a strong mutual trust between employers and employees in small handcraft establishments (Brors and Thelen, 1998). In general, the degree of application was much higher in East Germany suggesting that one would a priori expect stronger effects for this part of 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 stratification and robustness checks on various subsamples to reveal variations in the reform effect patterns. One aim of this study is to provide an example of how intention and actual implementation of unpopular social reforms might diverge.

3 Data And Variable Definitions

The empirical specifications make use of 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 L (1995) up to P (1999) that each contains 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] Respondents who needed medical treatment due to a work accident in the corresponding year are dropped since work accident related absenteeism was exempted from the new regulations. Beside short-term sick pay, long-term sick pay, which is disbursed from the seventh week onwards, was also effectively reduced as of January 1997. Since we inted to isolate the reform

[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 interested 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.

effects on short-term absenteeism, we discard all respondents who ever had a long-term sickness spells of more than six weeks in one of the sampling years.[9] Naturally, 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.

Our main dependent variable measures the total number of absent days and is called *Daysabs*. However, looking at the distribution of this variable, the potential issue of measurement errors, misreporting behavior, and outliers becomes quite obvious. For example, 0.03 percent (i.e. 7 respondents) of the sample indicated a total number of absence days of more than 100 which is, given that these repondents also denied an absence spell of more than six weeks, theoretically possible but very unlikely. While the evaluation of the reform effects should not be seriously hampered as long as the reform did not affect measurement errors, outliers and misreporting potentially inflate standard errors and lead to imprecise estimates. To make the subsamples more comparable and to reduce the influence of outliers and measurement errors, beside our main dependent variable *Daysabs*, we generate an additional variables which includes respondents with up to thirty absence days. We call this variables *Missed30days*. *Missed30days* samples 98.45 percent of the observations that are sampled in *Daysabs*.

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

[9] The identification of these respondents is feasible since a question on whether respondents had such a long-term spell was continuously asked. In section 5.1, we again use the whole sample to estimate the total labor cost savings for Germany.

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 annual regional unemployment rate and include state as well as year dummies.

3.2 Control Groups and Treatment Group

We define one treatment group and two control groups and accordingly generate two treatment dummies. The dummy *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 mainly made up of 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. *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 12,822 observations, Control Group 1 has 6,470 observations, and we count 1,783 observations for the self-employed which make up Control Group 2.

4 Estimation Strategy and Identification

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. We rely on a conventional difference-in-difference specification using pooled data over two pre- and two post-reform years. 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 for our purposes.

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 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.[10] 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:

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

where $f\left(y|\mu,\nu\right)$ is the conditional poisson distribution and $\gamma(\nu|\alpha)$ is assumed to be gamma distributed with ν as an unobserved parameter with variance α. Note that in the special case of $\alpha = 0$ the NegBin collapses to a simple Poisson model. $\Gamma(.)$ denotes the gamma integral and

$$\mu = exp(x'_{it}\beta) = exp(\lambda p97_t + \pi D_i + \theta DiD_{it} + s'_{it}\psi + \epsilon_{it}) \tag{2}$$

where $p97_t$, $t = [1994, 1995, 1997, 1998]$, is a dummy that indicates post-treatment years with a one, the dummy D_i takes on the value one if respondent i belongs to the treated and will later be replaced by *Treatment Group 1* or *Treatment Group 2*. DiD_{it} is also a binary indicator with a zero for the controls and the treated in pre-treatment periods and can be interpreted as an interaction term between D_i and $p97_t$. As usual, ϵ_{it} represents unobserved heterogeneity and the vector s'_{it} incorporates all other personal, educational, and job-related controls as well as 15 county dummies and the annual county unemployment rate.

The marginal effect of the interaction term DiD_{it} is - given the model assumptions are fulfilled - the causal reform effect and is henceforth always displayed when output tables are presented.[11]

[10] The unobserved heterogeneity allowed for in the NegBin-2 is based on functional form and does not capture unobserved heterogeneity which is correlated with explanatory variables.

[11] Puhani (2008) has shown that the advice of Ai and Norton (2004) to compute the discrete double difference is not of relevance in nonlinear models when the interest lies in the estimation of a treatment effect. The average treatment effect on the treated at the time of the treatment is given by $\varphi(y|\alpha, \bar{s}_{it}, p97_t = 1, D_i = 1, DiD_{it} = 1) - \varphi(y|\alpha, \bar{s}_{it}, p97_t = 1, D_i = 1, DiD_{it} = 0)$, where $\bar{s}_{it}$ denotes the average values of the covariates for the treated in the post-treatment period. This is exactly what we calculate and present throughout the paper.

4.1 Identification

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: A rich set of covariates is incorporated in the regression models and accounts 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, sport activities, and the disability status of the respondents. We are able to adjust the sample composition with respect to all factors that the literature has found to be important determinants of absenteeism, namely gender, age, health status, educational level, firm size, as well as the regional annual unemployment rate. We also take time-invariant sick leave differences of the treated and controls into account and adjust for time trends as well as county-specific effects. 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. 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. For example, in our robustness checks, we weight the regressions with the inverse probability that a respondent whom we observed as working in the pre-treatment period, will be observed without missings as working in the post-treatment period.

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 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 the intervention variable and is basically eliminated when focussing on less than five periods. We also use robust standard errors and correct for clustering at the individual level throughout the analysis.[12]

[12] As Imbens and Wooldridge (2007) note, the two-step estimation approach proposed by Donald

One of the biggest issues in evaluation studies are selection effects. Here, the reform was politically determined and the law applied to all private sector firms. 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 upon the continuation of the old sick pay arrangement. There exist no official numbers how many employees did de facto not experience 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 since collective bargaining coverage and union power is much lower in the Eastern part of Germany. Since our main purpose is to evaluate the actual 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 rather be regarded as conservative when putting them in relation to the total, by law implemented, decrease in statutory sick pay. On the other hand, this study exemplarily visualizes the gap between intended and actual reform effects which can be often observed in reality and which are boilt down to concrete and large differentials in the amount of labor costs savings and the number of jobs created (see Section 5.1).

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 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. Note that all factors which the empirical literature has found to be important determinants of sickness behavior can be controlled for. In the year 1997, there was a clear downward trend in the absence rates. However, to be able to causally attribute this trend to the cut in sick pay, we need to differentiate

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 of whether the observed effect goes entirely back to the policy change or not.

between treated and controls.

[Insert Table 1 about here]

In Tables 2 and 3 we find the unconditional DiD estimates on the incidence of zero absence days and the total number of absence days. The former table shows that the ratio of the treated who had not any absence day increased by about 1.7 percentage points as compared to the base period. This incidence rate remained stable for Control Group 1 (- 0.1 percentage points) and even decreased for Control Group 2 (-2.3 percentage points) leading to overall DiD effects of about +1.8 and +4 percentage points, respectively. The latter table shows the evolution of the mean absence days. For the treatment group we observe a decrease from 6.05 to 5.01 mean absence days whilst public sector employees and trainees experienced a decrease from 7.14 to 6.15 days on sick leave. We also observe a decline for the self-employed (-0.19 days) resulting in DiD estimates of around -0.05 and -0.85 absence days, respectively.

[Insert Table 2 and 3 about here]

Figure 2 displays the cumulative distribution function for the pre- and post-reform periods and contrasts those who were affected by the reform with the self-employed (Control Group 2).[13] Interestingly, as for the treated, we find that the whole distribution of absence days shifted to the left. We observe a parallel shift up to 15 total 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. Contrarily, for the self-employed, the cdfs are pretty identical. For up to five total absence days, one can even observe a shift to the right while for more than ten total absence days, a small shift to the left can be identified. The discriptive figures and the observation that every part of the treated's distribution was shifted to the left while we do not find such a pattern for the controls is a first hint that the reform induced changes in the sickness absence behavior.

[Insert Figure 2 about here]

[13] Control Group 1 is omitted due to visualization purposes. As can be already inferred from Table 3, the cdf for Control Group 2 also shifts to the left but the shift is smaller as the treated's shift. Both shifts overlaps making it difficult to identify major differences with the naked eye.

Table 4 shows the regression output when using the equation 1 type of count data models and estimating the reform effect on the probability of having *zero* absence days. Marginal effects are always calculated and displayed. Every column represents one count data model where columns (1) to (3) compare the treated to public sector sector employees and trainees (Control Group 1) and columns (4) to (6) use self-employed as Control Group 2. Consequently the only difference between these two specifications is the use of the dummy *Treatment Group 1* or *Treatment Group 2*, respectively. Models 1 to 3 (4 to 6) only differ by the stepwise inclusion of sets of covariates.

We see that the overall level of absenteeism of the treated is significantly higher as compared to Control Group 1 but significantly lower as compared to Control Group 2. Outcome level differences of treated and controls do not matter as long as they remain stable over the period under consideration. However, here, the outcome level of the treated is embedded in the levels of the two very different control groups which adds to the credibility of the results. Plausibility and robustness of the estimates is thereby automatically checked.

Let us first consider the first three columns which contrast the treated to Control Group 1. The stepwise inclusion of covariates leads to a slight increase of the relevant coefficient (DiDg) and improves the precision of the estimate. In the preferred specification in column (3), the DiD estimate is significant at the ten percent level and takes on the value 0.0271, meaning that the reform led to an increase in the probability of having no absence spell by 2.7 percentage points. In relation the the baseline probability for the treated in the pre-treatment period (49.3 percent, see Table 2), this translates into an increase of zero absence spells by 5.5 percent.

Consider now the last three columns which use self-employed as controls. Again, the coefficients remain very stable when including more controls. All specifications are marginally significant but the coefficient is larger as compared to the first three columns. It is 5.06 percentage points in the preferred specification. Related to the baseline probability, this implies an increased probability of no absence days by 10.3 percent, triggered by the reform.

[Insert Table 4 about here]

Table 5 shows again estimates on the probability of zero absence days but differentiates by East and West Germany. Since the implementation of the reform was more comprehensive in the eastern part of Germany, this differentiation might unfold heterogeneity in the reform effects. To make the sample more homogenous and to reduce the influence of measurement errors, misreporting, and outliers, we additionally present estimates when the sample is restricted to respondents with up to thirty absence days (98.45 percent of all observations, see Section 3.1 for more details).

Let us begin with East Germany (columns (1) to (4)). No matter whether we compare the treated to Control Group 1 or 2 and no matter whether we use the restricted or the full sample, we find for all four specifications positive reform effects which are significant at the ten percent level. As in the pervious table, the coeffients double when using *Treatment Group 2* as compared to *Treatment Group 1* but are invariant to the inclusion of controls and are of reasonable magnitude. We interpret the estimates as upper and lower bounds. Hence, in East Germany, the reform led to an increase in the ratio of employees with no absence spells of between 5.5 and 10 percentage points which equals an increase of between 10.1 and 20.1 percent if related to the baseline probability of 54.72 percent. For West Germany (columns (5) to (8)), the point estimates are substantially smaller (between 0.8 and 3.3 percentage points, i.e. 1.8 and 7 percent, respectively), have positive signs but are imprecisely estimated and not significant at conventional levels.

Given these numbers, our upper and lower bound interpretation would mean that the reform led to an increase in the ratio of employees with no absence days of approximately 15 percent in East Germany, 5 percent in West Germany and 7.5 percent in whole Germany.

[Insert Table 5 about here]

Let us now consider the reform impact on the average number of absence days. We estimate the same regression models as before but calculate and present the marginal effects on the number of absence days which can be looked up by region in Table 6. Again, we present separate estimates comparing the treated to two different control groups and using the full and the 98.45 percent sample.

First, we focus on whole Germany. All four DiD specifications have a negative sign and the coefficients are of very similar magnitude. However, the variant with Control Group 2 gives imprecisely estimated coefficients excecpt for a specifications that contains only respondents with up to ten absence days (not shown). In that case, the coefficent has the value -0.6 and is significant at the five percent level. Anyhow, turning to the variant with Control Group 1, we get an imprecise estimate (p-value 0.17) of -0.3 for the whole sample which is likely to be caused by measurement errors. Using the 98.45 percent sample, our DiD estimate is statistically significant at the 2.4 percent level. For whole Germany, according to our estimates, the reform reduced the average number of absence days by around 0.3 days which equals a decrease of about 5.1 percent given the average number of absence days of the treated in the pre-treatment period (see Table 3).

Second, we investige the effects in East Germany (columns (5) to (8)). The overall pattern is very similar to the one for whole Germany. For all four specifications, the effects have negative signs and are of similar and plausible magnitude. However, when contrasted to Control Group 2, we only find statistically significant effects when we condition on respondents with up to twenty absence days (results not displayed). One reason might be that only 2.38 percent of the self-employed in East Germany had more than twenty absence days in the period under consideration. Like for whole Germany, the variant with Control Group 1 results in an imprecise estimate (p-value 0.16) when the whole sample is used and in an estimate that is signficant at the 5.8 percent level when the 98.45 percent sample is used. The point estimates are higher in East Germany as compared to whole Germany and vary between -0.3 and -0.6 days representing reform induced decreases in the number of annual absence days of 5.2 and 10.5 percent, respectively (baseline probability: 5.8 days).

Third, the effects for West Germany are shown in columns (9) to (12). The same picture as before appears. The coefficients have all negative signs, are substantially lower in magnitude as compared to East Germany, and are more precisely estimated the more we homogenize the sample and reduce the impact of measurement errors which gain in influence the larger the number of total annual absence days is. The upper and lower bounds indicate that the reform reduced the mean number of absence days by between -0.11 and -0.24 translating into decreases of between 1.8 and 3.9 percent given the pre-treatment absence rate of 6.1 days for the treated.

The results allows us to infer that, on average and under consideration of the upper and lower bound estimates, the reform led to a signficant decrease of the annual average number of absence days for those employed in the private sector. For East Germany, the decrease was around 7.5 percent and for West Germany, the decrease was around 4 percent, resulting in an estimated overall decrease of approximately 5 percent.

[Insert Table 6 about here]

To sum up, we would like to emphasize the robustness, stability, and plausibilty of the results although some estimates are admittely imprecise due to outliers and measurement errors. However, the overall picture of this variety of results is the same. Regardless whether we take the specifications that estimate the impact on zero absence days or average positive absence days: in all of the specifications, the coefficients have the correct sign. Moreover, the magnitude of the estimates lies always in a plausible range and does not vary much although we contrast the treated to two different control groups that represent totally different but homogenous employment populations. The reform effect is alway larger in East than in West Germany which is in line with our expectations since the strict application of the new law was more widespread in East Germany. Lastly, the separate estimates for East and West Germany sum in plausible proportions to the estimated effect for whole Germany. Moreover, the two main specifications on zero and positive absence days yield comparable and plausible results.

In addition to the results presented so far, we performed a series of robustness checks that all confirm our main findings. Results for whole Germany on the average number of absence days contrasting the treated to Control Group 1 are displayed in Table 7. Using Control Group 2 yields similar results that are not shown due to space restrictions but are available upon request.

In a first specification, we restricted the sample to full-time employed aged 25 to 55. The decrease is around -0.4, thus very similar to the previous estimates and significant at the eleven percent level. The second specification only uses respondents without a partner since the relevant paramter in a partnership might be the decrease in the household income rather than the individual income. The magnitude of the absolute estimate does not differ much from the generel models and is around -0.4. However, relating both estimates to the baseline probabilities, which are a little bit lower than in the general case, yields reform induced decreases of 7.9 and 7.5 percent which are substantially higher than the estimated 5.1 percent decrease for the whole sample. The higher responsiveness of these subsamples is plausible since the decrease at the household level is dampened by the partner's income and middle-aged full-time employed most likely need to support a family and might be the main earner.

Robustness checks three and four split the sample at the median income. Column (3) shows a highly significant -0.6 average absence day decrease for the poorer half of the sample whereas the estimate for the richer half remains insignificant. Especially when compared to the initial probabilities, the difference in the behavioral effect becomes evident (-13.5 vs. -6.6 percent). Contrariliy to the two prior specifications, it is unplausible to assume that the poorer and richer half of the sample are equally distributed over all jobs and regions. The main reason for the difference in the reform effects remains obscure, since various explanations are imaginable. It might be that a.) the poor are more dependent on their full salary which would imply that the reform induced a higher degree of presenteeism in this subsample, b.) the poor work in less satisfying jobs and, thus, the reform reduced primarily the degree of moral hazard, or c.) 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. The fact that low earners are more likely to live in East Germany where the application of the reform was stricter partly explains the observed effect heterogeneity but not the whole differential.[14]

The last three robustness checks all show that our findings are not driven by selection issues. First, as already stated, the law universally applied to all firms in the private sector. Although it is very unlikely that people selected themselves out of the treatment by changing their jobs, we checked for this possibility by excluding all those who changed their job in the year prior to the interview. The resulting estimate in column (5) is significant at the 13 percent level and the coefficient is of the usual sign and size.

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. Unhealthy employees are more prone to sickness absence and are more likely to voluntarily or involuntarily leave the labor market. We accounted for this possibility by various means. First, as mentioned, we controlled for a bunch of observables, among them the health and disability status. Second, 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 for health reasons. 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 final robustness checks, we took advantage of the panel structure and did the following: we predicted for every individual the probability to be part of the sample in the post-treatment period by means of a probit model under the inclusion of the usual controls plus the total number of absence days as an additioanl explanatory variable. We then used the inverse probability to not drop out of the labor market to weight our regressions. The first estimate in column (6) shows the weighted regression estimate when we use the whole sample while the second estimate in the last column discards the refreshment sample. Both estimates are highly significant at the 2 and 4 percent level, respectively, and are of very similar magnitude to each other and to the baseline regressions in column (2) of Table 6.

Another 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 8 displays placebo regression results on the number of absence days. Columns (1) and (3) use the waves K (1994)

[14] In East Germany, the reform decrease for those who earn less than the median German wage amounted to 17.23 percent, whereas the decrease for low earners in West Germany amounted to 8.8 percent.

to M (1996) to estimate placebo regressions for the year 1994.[15] Columns (2) and (4) use waves K (1994) to N (1997) to sample two pre- and post-treatment periods for the placebo reform year 1995. All estimates furn out to be insignificant.

[Insert Table 8 about here]

5.1 Reduction of Labor Costs and Job Creation

We calculate the overall reduction in labor costs by comparing the total employer-provided sick pay benefit sum in the pre-reform years 1994/1995 to the total benefit sum in the post-reform years 1997/1998. We obtain the first benefit sum by summing over the frequency weighted product of absence days multiplied by the daily gross wage for each individual in the pre-reform years.[16] We do the same for the post-reform years but 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 statutory sick pay for the first six weeks per sickness episode from 100 to 80 percent of foregone gross wages. 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.[17]

[15] Wave J (1993) contains no absence information.

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

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

In the next 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 equaled about 0.44 days for employees with less than thirty total absence 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 €850 million.[18] 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 $(2.165 + 0.850)/2 =$ €1,51 billion per year.[19]

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).[20] 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.

[18] Here again, we focus on the same dataset which we used to obtain the estimated decrease of 0.44 days as we would otherwise overestimate the savings. To be precise, we restrict the sample to employees with less than 30 total absence days and neglect all respondents who had a long-term absence spell in one of the years under consideration. An alternative estimate yields a very similar indirect saving sum of €805 million by using the imprecisely estimated reform decrease of 0.3 days (Table 6, column 1) for all employees (but without considering the long-term sick) and multiplying the product of this decrease and the daily gross wage with the official number of employees suject to social insurance contributions which is available from the Federal Statistical Office (German Federal Statistical Office, 1996). Both approaches to calculate the indirect reform savings neglect spillover effects in the sense that de facto non-treated reduced their sick leave days because of peer-effects, sensitization, or nescience.

[19] 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 also 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 $(2.388 + 0.805)/2 =$ €1,597 billion per year (German Federal Statistical Office, 1998, 1996).

[20] 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.

For whole Germany, one percentage point of 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.[21] 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.[22]

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 moderate short-term strike costs and labor productivity unaffected by the reform. Had the reform been accepted by the employees as fair-minded and had it been implemented strictly by all employers, our calculations suggest that twice as much jobs could have been created, namely 100,000.

6 Conclusion

A natural experiment from Germany enables us to estimate the causal reform effect of a cut in the statutory sick pay level on sickness absence, labor costs, and employment creation. We do this by relying on two different control groups and a conventional difference-in-differences methodology. Typical selection issues common to evaulation studies can be handled with as we employ longitudinal SOEP household data and can identify job changers who are the only ones who could have selected themselves out of the treatment. The law universally applied to every dependent employee in the private sector and was determined at the federal level. We focus on the evaluation of the actual reform implementation rather than to estimate how employees would have reacted, had

[21] 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).

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

every single firm applied the new law, which decreased the replacement level from 100 to 80 percent of foregone gross wages, stringently. Under perfect competition one would have expected a perfect one-to-one implementation as intended by the lawmaker. However, the nonacceptance of the reform in the population which was expressed in mass demonstrations and union pressure, forced some employers to agree voluntarily on the continuation of the old sick pay regime. Insofar, our work also illustrates exemplarily how reform intention and the actual reform implementation may diverge, which in turn gives rise to the conclusion that policymakers should improve their way of communicating reforms.

Our empirical findings suggest that the reform significantly increased the ratio of employees without any absence spell by about 7.5 percent for whole Germany. As for the impact on the average number of absence days, we find that the reform significantly reduced this measure by around 0.3 days, representing a decrease of 5 percent. In both cases, the magnitude of the effects was much stronger in East Germany which is likely to go back to a stricter application of the law in the eastern as compared to the western part of Germany. Effect heterogeneity is also found for various subsamples. Singles, middle aged full-time employed, and the poor have reacted stronger than the population average.

We estimate that the direct labor cost saving effect due to 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 labor cost saving effect 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. Under a perfect implementation of the reform by all firms as intended by the policymaker, the job creation effect could have been twice as large.

To what extend the success of such reforms depends 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: Differences in Annual Absence Days by OECD Country

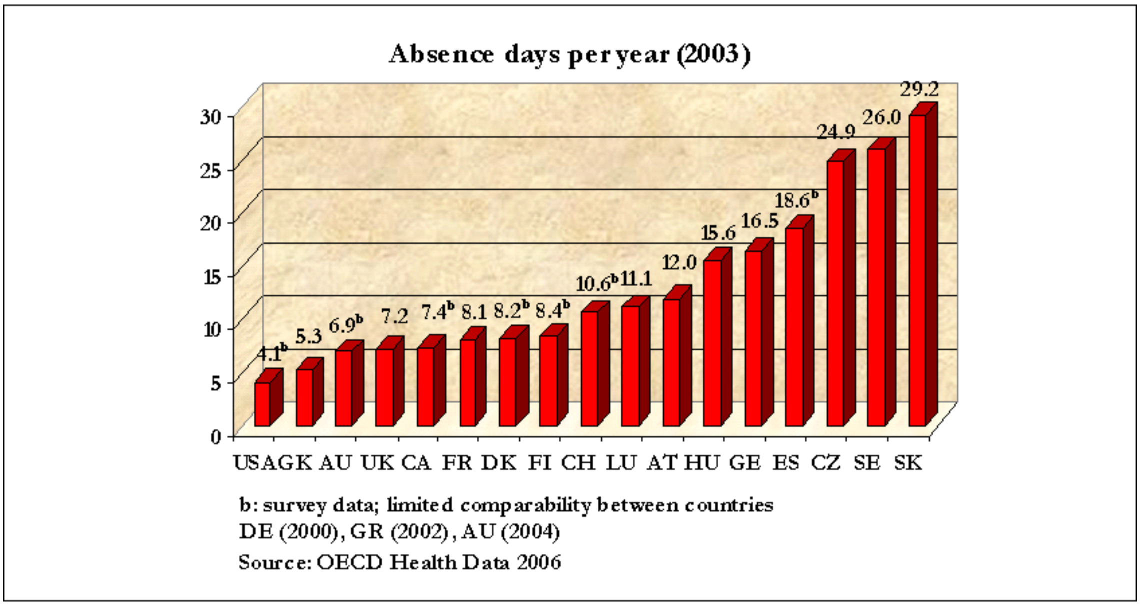

Figure 2: Cdf Pre-and Post-Reform Periods: Treatment Group vs. Control Group 1

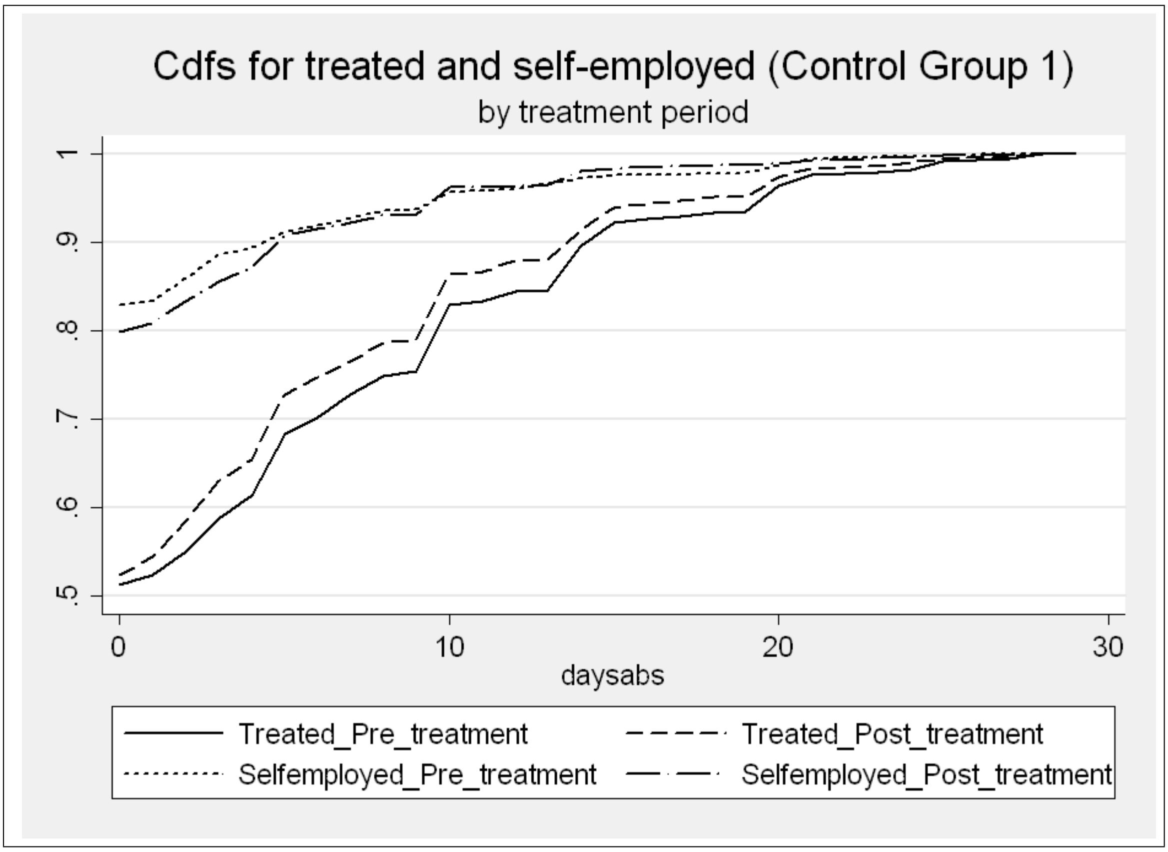

Table 1: Determinants of Short-Term Absenteeism: Zero-Inflated NegBin-2

Variable	Coefficient	Standard Error
Personal characteristics		
Female (d)	1.480***	0.181
Age	-0.272***	0.051
Age square/100	0.003***	0.001
Immigrant (d)	0.368	0.270
East German(d)	1.122***	0.399
Partner (d)	0.212	0.224
Married (d)	0.169	0.221
Children (d)	0.318*	0.166
Disabled (d)	2.086***	0.486
Good health (d)	-1.859***	0.162
Bad health (d)	2.901***	0.329
No sports (d)	-0.177	0.152
Educational characteristics		
Degree after 8 years' schooling (d)	-0.788**	0.380
Degree after 10 years' schooling (d)	-1.002***	0.387
Degree after 12 years' schooling (d)	-1.440***	0.429
Degree after 13 years' schooling (d)	-1.453***	0.371
Other degree (d)	-0.314	0.429
Part time employed (d)	-1.459***	0.208
Work in job trained for (d)	-0.132	0.150
No. years in company t'	0.015	0.011
Job characteristics		
New job (d)	-0.063	0.208
Medium size company (d)	1.539***	0.208
Big company (d)	2.338***	0.233
Huge company (d)	2.870***	0.261
White collar worker (d)	-0.504***	0.164
High job autonomy (d)	-1.263***	0.200
Gross wage per month/1000	-0.024***	0.007
Regional unemployment rate	-0.123***	0.041
Post-reform (d)	-0.112	0.160
Year 1997 (d)	-0.326**	0.139
Log pseudolikelihood	-48282.44	
χ^2	867.061	
N	21075	

(d) for discrete change of dummy variable from 0 to 1
marginal effects, which are calculated at the means of the covariates, 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 Incidence of Zero Absence Days

	1994/1995	1997/1998	Difference	Diff-in-Diff
Treatment Group	0.4931	0.5102	0.0171	
	(0.0063)	(0.0062)	(0.0088)	
Control Group 1	0.4248	0.4235	-0.0013	0.0183
(public sector, trainees)	(0.0089)	(0.0085)	(0.0123)	(0.0149)
Control Group 2	0.8175	0.7947	-0.0228	0.0399
(self-employed)	(0.0134)	(0.0131)	(0.0187)	(0.0204)

Average incidence rate of no absence spells is displayed

Standard errors in parentheses

Table 3: Unconditional DiD Estimates on the Number of Sickness Absence Days

	1994/1995	1997/1998	Difference	Diff-in-Diff
Treatment Group	6.0499	5.0086	-1.0412	
	(0.1177)	(0.1012)	(0.1547)	
Control Group 1	7.1379	6.1494	-0.9885	-0.0527
(public sector, trainees)	(0.2398)	(0.1520)	(0.2799)	(0.3173)
Control Group 2	1.8739	1.6811	-0.1929	-0.8483
(self-employed)	(0.1982)	(0.1541)	(0.2479)	(0.2784)

Average number of absence days is displayed

Standard errors in parentheses

Table 4: Difference-in-Differences Estimation on the Probability of Zero Absence Days

	Treated vs. Controls 1			***Treated vs. Controls 2***		
Variable	**Model 1**	**Model 2**	**Model 3**	**Model 1**	**Model 2**	**Model 3**
DiDg (d)	**0.0199**	**0.0192**	**0.0271***	**0.0550***	**0.0528***	**0.0506**
	(0.0160)	**(0.0159)**	**(0.0163)**	**(0.0313)**	**(0.0313)**	**(0.0321)**
	[0.2124]	**[0.2270]**	**[0.0969]**	**[0.0783]**	**[0.0915]**	**[0.1151]**
Year 1997 (d)	0.0180**	0.0183**	0.0139	0.0165	0.0162	0.0095
	(0.0089)	(0.0089)	(0.0094)	(0.0104)	(0.0104)	(0.0109)
Year 1995 (d)	-0.0253***	-0.0246***	-0.0170*	-0.0154	-0.0155	-0.0083
	(0.0091)	(0.0091)	(0.0097)	(0.0108)	(0.0108)	(0.0114)
Post reform dummy (d)	-0.0287**	-0.0281*	-0.0648***	-0.0582*	-0.0565*	-0.0869***
	(0.0146)	(0.0146)	(0.0159)	(0.0308)	(0.0308)	(0.0315)
Treatment Group (d)	0.0701***	0.0721***	0.0400***	-0.3255***	-0.3236***	-0.2951***
	(0.0126)	(0.0128)	(0.0135)	(0.0160)	(0.0160)	(0.0204)
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
χ^2	144.2766	397.7178	884.2137	141.9363	401.0296	747.4272
N	19292	19292	19292	14605	14605	14605

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

Marginal effects are calculated at the means of the covariates except for *Post reform dummy*(=1), *Treatment Group 1 (2)*(=1), *Year 1995* (=0), *Year 1997* (=1), and *DiDg* (=1)

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

Zero-inflated NegBin-2 models are estimated; every column stands for one regression model

Standard errors in parentheses are adjusted for clustering on person id

P-values in square brackets

Table 5: Difference-in-Differences Estimation on the Probability of Zero Absence Days: East vs. West

	East Germany				***West Germany***			
	Treated vs. Controls 1		**Treated vs. Controls 2**		**Treated vs. Controls 1**		**Treated vs. Controls 2**	
Variable	**All spells *(Daysabs)***	**Up to 30 days *(Missed30)***	**All spells *(Daysabs)***	**Up to 30 days *(Missed30)***	**All spells *(Daysabs)***	**Up to 30 days *(Missed30)***	**All spells *(Daysabs)***	**Up to 30 days *(Missed30)***
DiDg (d)	**0.0548***	**0.0519***	**0.1026***	**0.1099***	**0.0084**	**0.0088**	**0.0332**	**0.0489**
	(0.0303)	**(0.0302)**	**(0.0624)**	**(0.0642)**	**(0.0196)**	**(0.0195)**	**(0.0373)**	**(0.0427)**
	[0.0705]	**[0.0851]**	**[0.1000]**	**[0.0870]**	**[0.6667]**	**[0.6522]**	**[0.3735]**	**[0.2521]**
Post reform dummy (d)	-0.0930***	-0.0898***	-0.1153**	-0.1137*	-0.0406**	-0.0425**	-0.0681*	-0.0874**
	(0.0324)	(0.0321)	(0.0582)	(0.0588)	(0.0191)	(0.0188)	(0.0374)	(0.0376)
Treatment Group (d)	0.0100	0.0115	-0.2566***	-0.2612***	0.0579***	0.0582***	-0.3031***	-0.2436***
	(0.0237)	(0.0237)	(0.0328)	(0.0320)	(0.0164)	(0.0164)	(0.0254)	(0.0232)
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
χ^2	187.0717	173.5587	137.9225	145.3496	719.3092	753.1342	154.0060	219.8394
N	5065	4982	3438	3392	14227	13992	3659	4222

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

Marginal effects are calculated at the means of the covariates except for *Post reform dummy*(=1), *Treatment Group 1 (2)*(=1), *Year 1995* (=0), *Year 1997* (=1), and *DiDg* (=1)

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

Zero-inflated NegBin-2 models are estimated; every column stands for one regression model

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: By Region

	Germany				East Germany				West Germany			
	Controls 1		Controls 2		Controls 1		Controls 2		Controls 1		Controls 2	
Variable	***Daysabs***	***Missed30***	***Daysabs***	***Missed30***	***Daysabs***	***Missed30***	***Daysabs***	***Missed30***	***Daysabs***	***Missed30***	***Daysabs***	***Missed30***
DiDg (d)	**-0.3107**	**-0.4417****	**-0.2473**	**-0.1788**	**-0.6102**	**-0.7022***	**-0.3018**	**-0.9312**	**-0.1095**	**-0.2669**	**-0.2385**	**-0.3604***
	(0.2288)	**(0.1951)**	**(0.5397)**	**(0.4930)**	**(0.4319)**	**(0.3708)**	**(0.8763)**	**(0.9483)**	**(0.2655)**	**(0.2295)**	**(0.6660)**	**(0.3604)**
	[0.1745]	**[0.0236]**	**[0.6467]**	**[0.7168]**	**[0.1577]**	**[0.0583]**	**[0.7305]**	**[0.3261]**	**[0.6801]**	**[0.2449]**	**[0.7202]**	**[0.1060]**
Time dummies	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Treat. Group	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Job	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Education	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Personal	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Unemployment	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
State dummies	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
χ^2	884.2137	930.8861	747.4272	771.751	719.3092	753.1342	187.0717	173.5587	137.9225	145.3496	644.1587	313.7388
N	19292	18974	14605	14402	14227	13992	5065.0000	4982.0000	3438	3392	11167	9333

(d) for discrete change of dummy variable from 0 to 1; marginal effects are displayed
Marginal effects are calculated at the means of the covariates except for *Post reform dummy*(=1), *Treatment Group 1 (2)*(=1), *Year 1995* (=0), *Year 1997* (=1), and *DiDg* (=1)
Columns 2, 4, 6, 8, 10, and 12 use *Treatment Group 1* and thus contrast the treated to Control Group 1 whereas columns 3, 5, 7, 9, 11, and 13 use *Treatment Group 2* and contrast the treated to Control Group 2.
* p<0.1, ** p<0.05, *** p<0.01
Zero-inflated NegBin-2 models are estimated; every column stands for one regression model
Standard errors in parentheses are adjusted for clustering on person id
P-values in square brackets

Table 7: Robustness Checks on the Number of Absence Days: Treated vs. Control Group 1

Model	**full-time; age 25 to 55**	**singles**	**Wage < median**	**Wage > median**	**no job changers**	**weighted**	**no refreshment sample; weighted**
DiDg (d)	**-0.3797**	**-0.3767***	**-0.6668****	**-0.3132**	**-0.3109**	**-0.4621****	**-0.4627****
	(0.2406)	**(0.2245)**	**(0.3073)**	**(0.2605)**	**(0.2051)**	**(0.2007)**	**(0.2260)**
	[0.1145]	**[0.0933]**	**[0.0300]**	**[0.2292]**	**[0.1296]**	**[0.0213]**	**[0.0406]**
Time dummies	yes	yes	yes	yes	yes	yes	yes
Treat. Group	yes	yes	yes	yes	yes	yes	yes
Job	yes	yes	yes	yes	yes	yes	yes
Education	yes	yes	yes	yes	yes	yes	yes
Personal	yes	yes	yes	yes	yes	yes	yes
Unemployment	yes	yes	yes	yes	yes	yes	yes
State dummies	yes	yes	yes	yes	yes	yes	yes
χ^2	708.0456	751.0476	318.3238	634.5962	868.9184	899.6219	747.7892
N	13194	14413	8556	9832	16499	18955	15806

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

Marginal effects are calculated at the means of the covariates except for *Post reform dummy*(=1), *Treatment Group 1 (2)*(=1), *Year 1995* (=0), *Year 1997* (=1), and *DiDg* (=1)

All models use *Treatment Group 1* and thus contrast the treated to Control Group 1

All models use the 98.45 percent sample, i.e. all respondents with a total annual number of absence days up to 30.

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

Zero-inflated NegBin-2 models are estimated; every column stands for one regression model

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: Placebo Estimates

	Treated vs. Controls 1		***Treated vs. Controls 2***	
Model	**1994**	**1995**	**1994**	**1995**
DiDg94 (d)	0.2878		0.9365	
	(0.2379)		(0.6292)	
DiDg95 (d)		-0.2391		0.4621
		(0.2056)		(0.5222)
Post reform dummy	yes	yes	yes	yes
Treatment Group dummy	yes	yes	yes	yes
Job	yes	yes	yes	yes
Education	yes	yes	yes	yes
Personal	yes	yes	yes	yes
Unemployment	yes	yes	yes	yes
State dummies	yes	yes	yes	yes
χ^2	648.9170	834.6185	388.18	623.7938
N	13675	17932	6878	13630

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

Marginal effects are calculated at the means of the covariates except for *Post reform dummy*(=1), *Treatment Group 1 (2)* (=1), and *DiDg94 (95)* (=1)

All models use the 98.45 percent sample, i.e. all respondents with a total annual number of absence days up to 30.

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

Zero-inflated NegBin-2 models are estimated; every column stands for one regression model

Standard errors in parentheses are adjusted for clustering on person id

P-values in square brackets

Appendix A

Table 9: 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	0.502	0.424	0.805	0	1
	(0.500)	(0.494)	(0.396)		
Daysabs	5.517	6.626	1.771	0	365
	(8.773)	(11.258)	(5.224)		
Missed30	4.925	5.764	1.589	0	30
	(7.132)	(7.408)	(4.454)		
Personal characteristics					
Female	0.371	0.525	0.288	0	1
	(0.483)	(0.499)	(0.453)		
Age	39.25	37.64	43.05	18	65
	(10.28)	(11.94)	(9.71)		
Agesq	1,646	1,559	1,948	324	4,225
	(847)	(926)	(862)		
Immigrant	0.211	0.092	0.117	0	1
	(0.408)	(0.289)	(0.322)		
East German	0.232	0.323	0.259	0	1
	(0.422)	(0.468)	(0.438)		
Partner	0.801	0.678	0.825	0	1
	(0.399)	(0.467)	(0.380)		
Married	0.698	0.594	0.750	0	1
	(0.459)	(0.491)	(0.433)		
Children	0.487	0.460	0.496	0	1
	(0.500)	(0.498)	(0.500)		
Disabled	0.034	0.038	0.023	0	1
	(0.182)	(0.191)	(0.150)		
Health good	0.659	0.647	0.629	0	1
	(0.474)	(0.478)	(0.483)		
Health bad	0.069	0.073	0.073	0	1
	(0.254)	(0.261)	(0.260)		
No sports	0.398	0.285	0.421	0	1
	(0.489)	(0.451)	(0.494)		
Educational characteristics					
Drop-out	0.046	0.034	0.024	0	1
	(0.209)	(0.180)	(0.152)		
Degree after 8 years of schooling	0.343	0.227	0.302	0	1
	(0.475)	(0.419)	(0.459)		
Degree after 10 years of schooling	0.327	0.415	0.311	0	1
	(0.469)	(0.493)	(0.463)		
Degree after 12 years of schooling	0.041	0.039	0.057	0	1
	(0.199)	(0.194)	(0.231)		
Degree after 13 years of schooling	0.133	0.243	0.242	0	1
	(0.339)	(0.429)	(0.428)		
Other degree	0.111	0.042	0.065	0	1
	(0.314)	(0.201)	(0.247)		
Work in job trained for	0.557	0.570	0.597	0	1
	(0.497)	(0.495)	(0.491)		

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... Table 9 continued

Variable	Treated: Mean (s.d.)	Controls: Mean (s.d.)	ControlsII: Mean (s.d.)	Min.	Max.
No. of years in company	8.890	9.887	8.678	0	48.7
	(8.818)	(9.467)	(8.521)		
Job characteristics					
Part time employed	0.131	0.146	0.069	0	1
	(0.338)	(0.353)	(0.253)		
Blue collar worker	0.487	0.134	0.003	0	1
	(0.500)	(0.341)	(0.053)		
White collar worker	0.514	0.484	0.002	0	1
	(0.500)	(0.500)	(0.047)		
New job	0.138	0.115	0.090	0	1
	(0.345)	(0.319)	(0.287)		
Small company	0.281	0.147	0.580	0	1
	(0.449)	(0.354)	(0.494)		
Medium company	0.305	0.265	0.031	0	1
	(0.461)	(0.441)	(0.173)		
Big company	0.220	0.264	0.017	0	1
	(0.414)	(0.441)	(0.129)		
Huge company	0.194	0.324	0.019	0	1
	(0.395)	(0.468)	(0.137)		
High job autonomy	0.187	0.258	0.592	0	1
	(0.390)	(0.438)	(0.492)		
Gross income per month	2,060	1,867	2,728	0	51,129
	(1,184)	(1,0131)	(2,658)		
Regional unemployment rate	11.616	12.460	11.918	7.0	21.7
	(3.847)	(4.065)	(3.891)		
N	12,822	6,470	1,783		

Number of observations does not apply for the *Missed30*