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

Nicolas R. Ziebarth
Martin Karlsson

December 2009

york.ac.uk/res/herc/hedgwp

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

Nicolas R. Ziebarth
SOEP at DIW Berlin and TU Berlin[*]

Martin Karlsson
TU Darmstadt[**]

October 13, 2009

[*]Corresponding author: German Institute for Economic Research (DIW) Berlin, Socio-Economic Panel Study (SOEP), Graduate Center of Economic and Social Research, Mohrenstraße 58, D-10117 Berlin, Germany, and University of Technology Berlin (TU Berlin) e-mail: nziebarth@diw.de

[**]Darmstadt University of Technology (TU Darmstadt), Marktplatz 15 - Residenzschloss, e-mail: karlsson@vwl.tu-darmstadt.de

Abstract

This study estimates the reform effects of a reduction in statutory sick pay levels on various outcome dimensions. A federal law reduced the legal obligation of German employers to provide 100 percent continued wages for up to six weeks per sickness episode to 80 percent. This measure increased the ratio of employees having no days of absence by about 7.5 percent. The mean number of absence days per year decreased by about 5 percent. The reform might have reduced total labor costs by about € 1.5 billion per year which might have led to the creation of around 50,000 new jobs.

1 Introduction

The relationship between unemployment benefits and unemployment duration has attracted labor economists' attention for decades and provided material for countless numbers 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, labor productivity, population health, and the functioning of social insurance systems as well as private insurance markets.

While in Europe ownership of sickness absence insurance is more widespread and mostly universal, its significance for the US has often been overlooked or misinterpreted. What Europeans call "sickness absence insurance" or "sick pay" is in the US referred to as "temporary disability insurance" or "cash sickness benefits" and covers absence from work due to temporary *non*-work related sickness or injury. Five US states, among them the most populous state of California, have such insurance programs that are at least similar to the European sickness absence insurance. Their relevance is illustrated by the fact that in California in 2005, the total sum of net benefits for temporary disability insurance amounted to \$ 4.2 billion while the total sum for unemployment insurance amounted to \$ 4.6 billion (Social Security Administration, 2006, 2008). The US workers' compensation insurance (WCI) solely covers wage loss resulting from a work-*related* illness or injury. Another major social insurance, the disability insurance (DI), is well known in the US and in Europe and is, in general, run on the federal level. The DI replaces income losses stemming from a permanent labor market withdrawal due to work disability.

Very few studies have explicitly analyzed the impact of sick pay levels on absence rates. The handful of existing papers exploit legislative changes in the benefit levels in Sweden (Johansson and Palme, 1996, 2002, 2005; Henrekson and Persson, 2004; Pettersson-Lidbom and Skogman Thoursie, 2008). Two older English studies provide some correlation-based evidence using data from the 1970s (Doherty, 1979; Fenn, 1981). In addition, two US papers analyze the impact of changes to the benefit levels in WCI (Curington, 1994; Meyer et al., 1995). Besides the WCI, issues related to the DI have recently attracted a lot of attention. Many studies find that the generosity of the DI affects labor supply decisions on the extensive margin (Bound, 1989; Gruber, 2000; Chen and van der Klaauw, 2008), although there is also evidence that this might not alway be the case (Campolieti, 2004). As mentioned, in contrast to the WCI and the DI, the (European) sickness absence insurance covers a much broader range of illnesses and provides benefits for temporary workplace absences. Thus, although related, empirical findings for the DI and the WCI are very likely to be not fully transferable to the

sickness absence insurance.

All of the aforementioned studies find that employees adapt their sick leave behavior to increases and decreases in benefit levels. This finding is reinforced by various other empirical studies which analyze further determinants of sickness absence behavior. Workplace conditions are relevant,(Dionne and Dostie, 2007) as are probation periods and economic upswings or downturns (Ichino and Riphahn, 2005; Askildsen et al., 2005).

Average sickness absence days differ substantially across countries, ranging from 4 to 29 days per year and employee (see Figure 1). This suggests that institutional arrangements and cultural influences are of major importance. The figures indicate that further explanation for the significant differences is required. They also reinforce the presumption that there is huge potential for efficiency gains in the sickness absence insurance market.

[Insert Figure 1 about here]

Depending on the institutional system of a country, employers, private insurance companies, or social security systems provide sick pay. In the case of employer-provided sick pay, companies have to bear the burden of indirect labor costs in addition to direct productivity losses caused by absences from the workplace.

According to Germany's generous sick pay system, employers are legally obliged to continue to pay employees their full wage for up to six weeks per sickness episode. Unlike in most other countries, no benefit cap is applied. Nevertheless, as Figure 1 demonstrates, Germany is positioned in the middle region of the country ranking and some cross-country comparisons even place Germany below the international average in terms of sickness absence rates (Bonato and Lusinyan, 2004). One explanation might lie in the anecdotal evidence that claims Germans have a strong work ethic. Other explanations may be a well-functioning monitoring system or Germany's relatively high unemployment rate.

In 1996, the Kohl government decided to reduce the statutory sick pay level from 100 to 80 percent of foregone gross wages, effective from October 1, 1996. The intention was twofold: to reduce the degree of moral hazard in 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, representing 1.5 percent of current GDP (German Federal Statistical Office, 1998). Germany was positioned at the top of cross-country rankings comparing total labor costs per hour. There was a consensus among economists that the extraordinarily high labor costs were the main reason for the persistently high unemployment rate in Germany at that time.

While employers initially welcomed the sick pay cut, persistent mass demonstrations and strikes forced some of them to agree in collective wage agreements "voluntarily" to the continuation of the old sick pay scheme. During that time, there was a lot of uncertainty about the scope of the law's application and various lawsuits were filed.

The aim of this study is to estimate the overall causal impact of the cut in statutory sick pay 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 potential effect had the reform been strictly applied by every single company. Those who were totally unaffected by the new law, namely self-employed, public sector employees, and apprentices serve as controls. 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 significantly hamper the analysis . In this context, sorting is unlikely to be an issue as a) the law applied to all dependent private sector employees, b) the law was determined at the federal level, and c) we are able to control for the unlikely case that the privately employed applied for public sector employment or became self-employed as a reaction to the reform.

The paper also contributes to the broader field of literature on the interdepencies between social insurance systems and labor supply. Unlike studies that estimate effects in certain regions or states, we use a representative sample of the largest European economy and the most recent data as compared to the other studies. In addition and in contrast to most previous studies, our identification strategy relies on two sound control groups. Importantly, we avoid a common caveat in evaluation studies by controlling for potential selection issues. In a static view, we also calculate employers' total labor cost savings and roughly estimate the number of jobs which might had been created as a consequence of the reform. Finally, this study illustrates the pitfalls that policy-makers 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 many jobs could have been created as actually occurred.

Section 2 outlines some of the institutional settings in Germany. Section 3 provides more detail on the data. Section 4 discusses the empirical estimation strategy. This is followed by Section 5 in which we provide some broad estimates of the reform-induced reduction in labor costs and the creation of new jobs. Section 6 outlines the study's conclusions.

2 The German Sick Pay Scheme and Policy Reform

2.1 The Sick Pay Scheme and Monitoring System

Germany has one of the most generous sick pay schemes in the world. Before the implementation of the new law, every employer was legally obliged to continue usual wage payments for up to six weeks per sickness episode. In other words, employers had to provide 100 percent sick pay from the first day of a period of sickness with no benefit caps.[1] Henceforth, we use the term short-term sick pay as a synonym for employer-provided sick pay and short-term absenteeism as a synonym for periods of absence of less than six weeks.

In the case of illness, employees are obliged to inform their employer immediately about both the sickness and expected duration. From the fourth day of a sickness episode, a doctor's certificate is required and is usually issued for up to one week, depending on the illness. If the sickness lasts more than six continuous weeks, the doctor 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 under Statutory Health Insurance (SHI).

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. German social legislation codifies that the SHI is obliged to call for the Medical Service and a medical opinion to clarify any doubts about work absences. Such doubts may arise if the insured person is short-term absent with unusual frequency or is regularly sick on Mondays or Fridays. Similarly, if doctorscertify sickness with unusual frequency, the SHI may ask for expert advice. The employer also has the right to call for the assistance of the Medical Service and expert advice. Expert advice is based on available medical documents, information about the workplace, and a statement which is requested from the patient. If necessary, the Medical Service has the right to conduct a physical examination of the patient and to cut benefits.[2] In 2007, about 2,000 full-time equivalent and independent doctors worked for the medical service and examined 1.7 million cases of absenteeism (Medizinischer Dienst der Krankenversicherung (MDK), 2008).

---

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

2.2 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 incited the German government to pass a law which became effective from October 1,1996.[3]

The law reduced the sick pay employees are entitled to claim from 100 to 80 percent of gross wages for the first six weeks per sickness episode. Self-employed were not affected by the new law. Because of political reasons and the existence of other laws, public sector employees and apprentices were exempt from the reform.[4] Similarly unaffected were employees on sick leave due to work accidents. As an alternative to the cut in sick pay, from when the new law was effective, employees had the right to reduce their paid vacation by one day for every five days of sickness absence, thereby avoiding the sick pay cut.

In addition to this law that lowered short-term sick pay and is the focus of this study, another law was passed on November 1, 1996 and became effective from January 1997 onwards.[5] The second law 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 has been analyzed elsewhere (Ziebarth, 2009). However, to not confuse the impact of the cut in long-term sick pay from the impact of the cut in short-term sick pay, it is important to analyze the effects of both reforms seperately. This is also necessary since the subgroups affected differed between the two reforms.

Before and in the aftermath of the cut in statutory short-term sick pay, the German population and the unions put pressure on the employers through mass demonstrations and strikes. There are no official number but according to statements by unionists, around 13 million German employees[6] were de facto not affected by the law since unions successfully negotiated with employer's representatives to provide sick pay voluntarily in many industrial branches. In the end, there existed collective wage agreements for about 100 branches which all entailed different regulations concerning voluntary sick pay provision on top of the statutory regulations. However, these collective wage agreements

---

[3] Passed on September 25,1996 this law is the *Arbeitsrechtliches Gesetz zur Förderung von Wachstum und Beschäftigung (Arbeitsrechtliches Beschäftigungsförderungsgesetz), BGBl. I 1996 p. 1476-1479.*

[4] In the case of apprentices, the so-called *Berufsbildungsgesetz (BBiG)* prevented the application of the law.

[5] This law was called *Gesetz zur Entlastung der Beiträge in der gesetzlichen Krankenversicherung (Beitragsentlastungsgesetz - BeitrEntlG), BGBl. I 1996 p. 1631-1633*

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

were only binding for firms under collective bargaining coverage. Collective bargaining coverage varies substantially accross branches. In 1998, about 68 percent of all employees in West and 50 percent in East Germany underlay a collective wage agreement (Hans Böckler Stiftung, 2009). Since the provision of voluntary sick pay varied accross branches and also accross firms that did not underlie collective wage agreements, it is inherently difficult, if not impossible, to link individual data to the acutal provision of voluntary sick pay. Thus, we apply an intention-to-treat approach and focus on the effects among all those who were affected theoretically by the law, namely all private sector employees. We would like to point out that all studies analyzing the effects of changes in statutory minimum regulations face the same issue.

Polls among craftsmens' businesses suggest that around 50 percent of these firms did not apply the law. Anecdotal evidence traces this back to strong mutual trust between employers and employees in small craftsmens' establishments (Brors and Thelen, 1998). In general, the level of application was much higher in East Germany suggesting that one would, a priori, expect a more significant impact in this part of Germany.

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

In their election campaign, the Social Democrats and the Greens promised to increase statutory sick pay from 80 to 100 percent – should they form the new government. After their successful election in September 1998, the new government immediately passed a law, which became effective from January 1, 1999, increasing statutory short-term sick pay from 80 to 100 percent of foregone gross wages.[7] Since long-term sick pay was cut alongside with short-term sick pay and since both reforms affected different subgroups, in another paper, we analyze the effects of the generosity expansion separately by means of a different identification strategy (Ziebarth and Karlsson, 2009). However, as will be shown below, the effects of the cut in statutory short-term sick pay are very similar to the effects of the increase in statutory short-term sick pay.

All-in-all, in 1997 and 1998, there was great uncertainty and sensitivity among German private sector employees and even employees who were de facto not affected by the law were probably not fully aware of their privileges. We can not clearly identify those employees but compensate for 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 the intention and actual implementation of unpopular social reforms might diverge.

---

[7] Passed on December 19, 1998 this law is the *Gesetz zu Korrekturen in der Sozialversicherung und zur Sicherung der Arbeitnehmerrechte, BGBl.I 1998 Nr. 85 S.3843-3852.*

3 Data And Variable Definitions

The empirical specifications make use of the German Socio-Economic Panel Study (SOEP). The SOEP is the only available representative dataset for Germany that includes information on sickness absence. The SOEP is a longitudinal representative annual household survey that has existed 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 from L (1995) to P (1999) that each contains sickness absence information about the previous year. We discard the reform year 1996 in most of our specifications.[8] We restrict our sample to those of the working labor force who are eligible for sick pay (plus self-employed) and who are between 18 and 65 years of age.[9] Respondents who needed medical treatment due to a work accident in the corresponding year are not included since work accident related absenteeism was exempted from the new regulations. Besides 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 intend to isolate the reform effects on short-term absenteeism, we discard all respondents having had a long-term sickness spell of more than six weeks in one of the sampling years.[10] Obviously, individuals with item non-response can not be used either.

3.1 Endogenous and Exogenous Variables

The SOEP is a rich dataset, particularly 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 such like are sampled. Additionally, there are questions on sick leave behavior.

We generate our dependent variables from the following question: *"How many days off 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

---

[8] What is meant here is that 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 which we are interested in with the current information of the relevant 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 previous year.

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

[10] The identification of these respondents is feasible since a question on whether respondents had such a long-term spell was continuously asked. In Section 6.3, we again use the whole sample to estimate the total labor cost savings for Germany.

certificate or those that are compensated by a certain federal agency as it is the case with most register data. Particularly 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 issues of measurement errors, misreporting behavior, and outliers become 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 theoretically possible but, given that these respondents also denied an absence spell of more than six weeks, very unlikely. While the evaluation of the reform effects should not be seriously distorted as long as the reform did not affect measurement errors, outliers and misreporting do potentially exacerbate standard errors and lead to imprecise estimates. To make the subsamples more easily comparable and to reduce the influence of outliers and measurement errors, alongside our main dependent variable *Daysabs*, we generate an additional variable which includes respondents with up to thirty absence days. We call this variable *Missed30days*. *Missed30days* samples 98.45 percent of the observations that are sampled in *Daysabs*.

The whole set of explanatory variables can be found in the Appendix and is categorized as follows: The 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 higher education degree awarded, number of years in current workplace, and whether the person was trained specifically for their job. The last group contains explanatory variables on job characteristics. Among them are *Blue-collar worker, White-collar worker*, the size of company, or *Monthly gross wage*. We also control for the annual state 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 apprentices. Our first specification contrasts these employees with those who are eligible for sick pay but were exempted from the law for political reasons. *Treatment Group 1* thus has a zero for apprentices and public sector employees (Control Group 1). On the contrary, 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 has a total of up to 12,822 observations, Control Group 1 has 6,470 observations, and there are 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.

5 Zero-Inflated NegBin-2

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. Thus, 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 negative binomial distribution we have employed does not only take excess zeros into account but also allows for overdispersion and unobserved heterogeneity.[11] The NegBin model can be regarded 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:

---

[11] 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.

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

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

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

where $p97_t$, $t = [1994, 1995, 1997, 1998]$, is a dummy that indicates post-treatment years with a one, the dummy D_{it} takes on the value one if respondent i belongs to the treated in period t 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_{it} 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.[12]

5.1 Identification

Our analysis relies on two different control groups which 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 is usually the case in difference-in-differences (DiD) applications, we assume that changes in the absence rates go back entirely to the exposure of the reform. In other words, conditional on the available covariates, we

---

[12] 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 $\varphi(y|\alpha, \bar{s}_{it}^{post}, p97_t = 1, D_{it} = 1, DiD_{it} = 1) - \varphi(y|\alpha, \bar{s}_{it}^{post}, p97_t = 1, D_{it} = 1, DiD_{it} = 0)$, where $\bar{s}_{it}^{post}$ 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.

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), 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, sporting activities, and disability status of the respondents. We are able to adjust the sample composition according to all factors found by literature on the subject to be important determinants of absenteeism, namely gender, age, health status, education, company 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 state-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 in between the levels for the two control groups. Sample composition changes over time and labor market attrition can be addressed because of 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 as working in the post-treatment period.

In recent years, there has been an extensive debate about the drawbacks and limitations of DiD estimation. A particular concern is the underestimation of OLS standard errors due to serial correlation in the case of long time horizons and unobserved (treatment and control) group effects. To deal with the serial correlation issue, we focus on short time horizons. As Bertrand et al. (2004) have shown, the main reason for the understating of standard errors is rooted in serial correlation of the outcome and the intervention variable and is basically eliminated when focusing on less than five periods. While there is consensus about the serial correlation problem, the issue with unobserved common group effects is more of a controversial subject of debate. If one takes the objection of Donald and Lang (2007) seriously, then it would not be possible to draw inferences from DiD analyses in the case of few groups, meaning that no empirical assessment could be performed. We subscribe to the view of Wooldridge (2006) who refers to that as (p. 18):

"DL [Donald and Lang] criticize Card and Krueger (1994) for comparing mean wage changes of fast-food workers across two states because Card and Krueger fail to account for the state effect (New Jersery or Pennsylvania) [...]. But the DL criticism in the $G = 2$ case is no different from a common question

raised for any difference-in-differences analyses: How can we be sure that any observed difference in means is due entirely to the policy change? To characterize the problem as failing to account for an unobserved group effect is not necessarily helpful." [13]

Alongside our focus on short time spans to resolve serial correlation concerns, we use robust standard errors and correct for clustering at the individual level throughout the analysis.

One of the biggest issues in evaluation studies is selection effects. Here, the reform was politically determined and the law applied to all private sector companies. It is very unlikely that people left the labor market due to the 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 equally unlikely. However, information on whether people changed their jobs and information on the labor market status allows us to control for this possibility.

There may also be concerns about the policy change being endogenous in the sense that the reform was a reaction to increasing absence rates (Besley and Case, 2000). We have not found any evidence that this might have been the case. The reform was not a reaction to increasing absence rates but rather a tool for reducing the persistently high labor costs which were rooted in the institutional structure. The reform was insofar random as it was mainly an instrument used by the unpopular Kohl government (which had beenin power since 1982) to demonstrate strength and the capacity to act. Lastly, structural reforms of the employer sick pay system had been debated in Germany since the beginning of the 80s (Lambsdorff, 1982).

As already mentioned in Section 2.2, due to union pressure, some employers agreed to continue the old sick pay arrangement. There are no official figures on how many employees were de facto not affected by the sick pay cuts and we cannot unambiguously identify these employees. We compensate for 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

---

[13] In this very readable extended version of an older published AER paper (Wooldridge, 2003), Wooldridge (2006) discusses several other shortcomings and assumptions of the estimation approach proposed by Donald and Lang (2007). At another juncture, Wooldridge (2007) asks rhetorically whether introducing more than sampling error into DiD analyses was necessary, or desirable. *"Should we conclude nothing can be learned in such settings?"*, he questions (p. 3). Moreover, he uses the well known Meyer et al. (1995) study, which is similar to ours and also obtains marginally significant results, as another example:
"It seems that, in this example, there is plenty of uncertainty in estimation, and one cannot obtain a tight estimate without a fairly large sample size. It is unclear what we gain by concluding that, because we are just identifying the parameters, we cannot perform inference in such cases. In this example, it is hard to argue that the uncertainty associated with choosing low earners within the same state and time period as the control group somehow swamps the sampling error in the sample means." (p. 3 to 4).

to evaluate the actual overall reform effects, this lack of identification is a drawback but does not seriously hamper our analysis and conclusions. Since there was major uncertainty among employees and since employers are always free to provide voluntary lump sum payments, our results should rather be regarded as conservative in relation to the total decrease in statutory sick pay implemented by law. In Sweden, for example,where all previous studies on changes in sickness benefit levels originate, voluntary sick pay on top of the statutory sick pay is very widespread and collective wage agreements concerning these fringe benefits are very fragmented. We have not found any evidence that there were great differences between the agreements at that time in Germany. It seems plausible to assume that up to 50 percent of the employees continued under the old scheme and the rest experienced a real decrease in sick pay to 80 percent of the gross wage. On the other hand, this study exemplarily visualizes what is often observed in reality, namely the disparity between intended and actual reform effects which , in this case, boils down to a concrete and significant difference in the amount of labor cost savings and the number of jobs created (see Section 6.3).

6   Results

Table 1 visualizes the determinants of absence behavior. As expected, the age and health status are important drivers of sickness absence which is also true for schooling level and the level of job autonomy. In line with the literature, males and part-time employees have fewer absence days and company size is positively correlated with absenteeism. High regional unemployment rates serve as a worker discipline device as Shapiro and Stiglitz (1974) would call it. All factors that the empirical literature has found to be important determinants of sickness behavior can be controlled for. In 1997, there was a clear downward trend in absence rates. 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]

6.1   Baseline Specifications

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 that did not have 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).[14] Interestingly, with 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 *total* number of absence days is also illustrated. In contrast, for the self-employed, the cdfs are almost 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 observation that every part of the treated's distribution was shifted to the left and the absence of 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]

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 than Control Group 1 but significantly lower than Control Group 2. Outcome level

---

[14] 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 than the treated's shift. Both shifts overlap making it difficult to identify major differences with the naked eye.

differences of treated and controls do not matter as long as they remain stable over the period under consideration. Here, the outcome level of the treated is embedded in the levels of the two very different control groups which reinforces the credibility of the results, should the results be of similar size and magnitude for both specifications. The plausibility and robustness of the estimates are thereby automatically checked.

Let us first consider the first three columns which contrast the treated with 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, indicating that the reform led to a 2.7 percentage point increase in the probability of having no absence days. In relation to the baseline probability for the treated in the pre-treatment period (49.3 percent, see Table 2), this translates into a 5.5 percent increase of zero absence spells.

Consider now the last three columns which use self-employed as controls. Again, the coefficients remain very stable when we include more controls. All specifications are marginally significant but the coefficient is larger when compared to the first three columns. It is 5.06 percentage points in the preferred specification. Related to the baseline probability, this implies a 10.3 percent increase in the probability for the treated of having zero absence days , triggered by the reform.

[Insert Table 4 about here]

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

Let us begin with East Germany (columns (1) to (4)). Regardless of whether we compare the treated to Control Group 1 or 2 and whether we use the restricted or the full sample, for all four specifications we find positive reform effects which are significant at the ten percent level. As in the previous table, the coefficients 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. Thus, 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.

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

[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 seen by region in Table 6. Again, we present separate estimates comparing the treated to the two different control groups and using the full and the 98.45 percent sample.

Firstly, we focus on the whole of 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 except for a specification that contains only respondents with up to ten absence days (not shown). In this case, the coefficient has the value -0.6 and is significant at the five percent level. 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 the whole of Germany, according to our estimates, the reform reduced the average number of absence days by around 0.3 days which equates to 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).

Secondly, we investigate the effects in East Germany (columns (5) to (8)). The overall pattern is very similar to the one for the whole country. 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. As for the whole of 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 significant 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 the whole country 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).

Thirdly, the effects for West Germany are shown in columns (9) to (12). Here, the same picture is evident.. The coefficients have all negative signs, are substantially lower in magnitude than East Germany, and are more precisely estimated the more we homogenize the sample and reduce the impact of measurement errors which gain in influence as the number of total annual absence days increases. 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 allow us to infer that, on average, and taking into consideration the upper and lower bound estimates, the reform led to a significant decrease in the annual average number of short-term absence days for those employed in the private sector. For East Germany, the decrease was around 7.5 percent and for West Germany, it 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 plausibility of the results in spite of the fact that some estimates are admittedly imprecise due to outliers and measurement errors. However, the overall picture of this range of different of results is the same. Regardless of whether we take the specifications that estimate the impact on zero absence days or average absence days: in all specifications, the coefficients have the correct sign. Moreover, the magnitude of the estimates always lies in a plausible range and does not vary significantly although we contrast the treated with two different control groups that represent totally different but homogenous employment populations. The reform effect is always 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 the whole country. Moreover, the two main specifications on zero absence days and the total number of absence days yield similar and plausible results.

Section 2 discusses the political economy of the reform. Since the reform was rather unpopular among the population, the new government promised in their election campaign to reverse the law and increase statutory short-term sick pay from 80 to 100 percent in case of their election. This is what happened from January 1, 1999 onwards. Using a different identification strategy due to the fact that no other sick pay reform

was implemeted alongside with the generosity expansion in 1999, we evaluate the effects of increasing statutory sick pay separetely (Ziebarth and Karlsson, 2009).We find that the increase in statutory short-term sick pay from 80 to 100 percent led to an increase in the average numer of sick leave days of about 10 percent. This is in line with the finding here and adds to the credibility of our identification strategy.

6.2   Robustness Checks and Heterogeneity of Effects

In addition to the results presented so far, we performed a series of robustness checks that all confirm our main findings. The results for the whole of Germany on the average number of absence days contrasting the treated with 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 the 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 parameter in a partnership might be a decrease in the household income rather than individual income. The magnitude of the absolute estimate does not differ very much from the general models and is around -0.4. However, relating both estimates to the baseline probabilities, which slightly 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 absorbed by the partner's income and the middle-aged full-time employed probably need to support a family and may be the main breadwinners.

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. Particularly when compared to the initial probabilities, the difference in the behavioral effect becomes evident (-13.5 vs. -6.6 percent). In contrast to the two prior specifications, it is implausible to assume that the poorer and the 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 possible. 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 primarily reduced the degree of moral hazard, or c) better paid employees are more likely to work for prosperous companies that underlie collective wage agreements with supplementary sick leave payments exceeding the legal require-

ments. 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.[15]

The last three robustness checks all show that our findings are not driven by selection issues. Firstly, as already stated, the law was universally applied to all private sector companies. 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. Firstly, as mentioned, we controlled for a range of observables, among them health and disability status. Secondly, by excluding 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. Thirdly, in 1998, a refreshment sample was drawn which stabilized the sample size and mitigated such selection issues. Fourthly, we implicitly control for selection out of the labor market as long as it is unrelated to the treatment and employment-group since we have two different control groups. As final robustness checks, we took advantage of the panel structure and carried out the following: we predicted for every individual the probability of being 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 additional explanatory variable. We then used the inverse probability of not dropping 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 the years without 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

---

[15] In East Germany, the reform decrease for those who earned 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.

results on the number of absence days. Columns (1) and (3) use the waves K (1994) to M (1996) to estimate placebo regressions for the year 1994.[16] 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 prove to be insignificant.

[Insert Table 8 about here]

6.3   Reduction of Labor Costs and Job Creation

We calculate the potential overall reduction in labor costs by comparing the total employer-provided sick pay benefit sum in the pre-reform years 1994/1995 with the (fictive) total benefit sum in the post-reform years 1997/1998 had every employer applied the new law strictly. Note that we do not need any of our regression results for this calculation but again use the *full* sample.[17] We obtain the first benefit sum by calculating the product of absence days multiplied by the daily gross wage for each individual in the pre-reform years. This total is then frequency weighted and multiplied with the frequency weighted number of treated employees.[18] 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 between the two total sums yields the potential 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 is rooted in 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

---

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

[17] In contrast to the previous subsection, for this calculation, we use 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 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.

[18] Frequency weights, which are computed according to data from the Federal Statistical Office, are provided by the SOEP group (SOEPGroup, 2001). Absence days and gross wages are included in the SOEP data. The SOEP group makes great effort to collect income data accurately and impute missing data consistently (Frick and Grabka, 2005).

direct savings reduce to € 2.165 billion. Note that this is a conservative estimate as explained in Section 2.2.[19]

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 reduction in absence days equaled about 0.44 days for employees with less than thirty total absence days. Thus we multiply this reduction by the average daily gross wage in the pre-reform years and multiply the product by the frequency-weighted number of employees in both years, resulting in an indirect saving effect of € 850 million.[20] 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.[21]

In 1997, the Research Institute 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 the creation of 120,000 new jobs (Zika, 1997). These statistics were confirmed by other studies (Feil et al., 2008; Meinhardt and Zwiener, 2005).[22] 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. For decades these indirect labor taxes have been of great concern to economists and policymakers as they make labor more expensive and weaken

---

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

[20] 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 by the official number of employees subject 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.

[21] By combining data from the Federal Statistical Office on the total number of employees obliged to pay social insurance contributions in the different years with SOEP data, we checked the plausibility and sensitivity of this estimate. This method also enables us to control for panel attrition. To calculate the different saving elements, we multiply official employment data by 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, 1996, 1998).

[22] 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 counter financing and estimated the job creation effect to be around 100,000.

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 the main objective of various reforms over the last few decades.

For the whole of Germany, in 1997, one percentage point of social security contribution rates equated to about € 5 billion. . If we assume that job creation in the cited simulation models was solely as a result of 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.[23] Based on the assumption that half of the job creation effect resulting from reductions in social contribution rates was the result of an increased labor supply and a higher product demand due to increased net wages, this number falls to 35,000 when related to our labor cost saving effect of € 1.5 billion per year.[24]

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. The notion that the reform did not predominately reduce moral hazard but induced more presenteeism and led to an overall drop in labor productivity should also be taken into consideration.

Based on the combined evidence , it seems reasonable to conclude that approximately 50,000 extra jobs could have been created through the reform in the long run due to lower labor costs - on the assumption of moderate short-term strike costs and a constant labor productivity. Had the reform been accepted by employees and unions as fair-minded and had it been implemented strictly by all employers, twice as many jobs could have been created i.e. 100,000.

7 Conclusion

A natural experiment 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-

---

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

[24] However, the macroeconomic simulation models used to derive the increased employment effects assumed a constant labor supply (Feil et al., 2008). In our rough calculation we neglect to include the fact 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. However, two thirds of German GDP comes from exports, and domestic demand traditionally plays a minor role in Germany; it is, therefore, very insensitive to aggregate wage changes, probably also because of the high savings rate which is more than ten percent. Lastly, we do not account for the possibility that an increased presence at the workplace may lead to a higher productivity and may weaken labor demand.

differences methodology. Typical selection issues common to evaluation studies are dealt with by employing longitudinal SOEP household data and thus identifying job changers who are the only ones who could have selected themselves out of the treatment. The statutory sick pay cut applied universally to every dependent employee in the private sector and was passed at the federal level. We focus on the evaluation of the actual reform implementation rather than on estimating how employees would have reacted had every single firm strictly applied the new law which decreased the replacement level from 100 to 80 percent of foregone gross wages. Under conditions of perfect competition one would have expected a one-to-one implementation as was intended by the lawmaker. However, the non-acceptance of the reform by the population, which was manifested in mass demonstrations and union pressure, forced some employers to agree voluntarily to the continuation of the old sick pay regime. In this context our work also illustrates exemplarily how reform intention and actual reform implementation may diverge which in turn leads us to the conclusion that policymakers should improve their way of communicating reforms.

Our empirical findings suggest that the reform increased the ratio of private sector employees without any absence days by about 7.5 percent. Looking at the impact on the average number of short-term absence days, we find that the reform reduced this figure by around 0.3 days, representing a decrease of 5 percent. In both cases, the magnitude of the effects was much larger in East Germany. This is likely to stem from a stricter application of the law in this part of the country. Effect heterogeneity is also found for various subsamples. Single people, middle-aged full-time employed and the poor have reacted more strongly than the population average.

We estimate that the direct labor cost savings effect due to the decrease in benefit levels was €1.1 billion p.a. for the whole of Germany. Adding the indirect reform savings effect due to the decrease in absenteeism, we end up with a total labor cost savings 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 might have led to the creation of 50,000 new jobs. Had the reform been implemented perfectly by all companies, as was intended by the policymakers, the job creation effect could have been double this size.

To what extent the success of such reforms depends on cultural peculiarities and macroeconomic conditions is of great importance and further studies on this subject would be valuable. Unintended side-effects such as strikes and mass demonstrations may have offset or even overcompensated the pure reform effects but are beyond the scope of this study.

References

Ai, C. and E. C. Norton (2004). Interaction terms in logit and probit models. *Economics Letters 80*, 123–129.

Askildsen, J. E., E. Bratberg, and Ø. A. Nilsen (2005). Unemployment, labor force composition and sickness absence: A panel study. *Health Economics 14*, 1087–1101.

Bertrand, M., E. Duflo, and M. Sendhil (2004). How much should we trust differences-in-differences estimates? *Quarterly Journal of Economics 119*(1), 249–275.

Besley, T. and A. Case (2000). Unnatural experiments? Estimating the incidence of endogenous policies. *Economic Journal 110*(467), 672–694.

Bonato, L. and L. Lusinyan (2004). Work absence in Europe. IMF Working Paper 04/193, IMF. http://imf.org/external/pubs/ft/wp/2004/wp04193.pdf, last accessed at December 19, 2008.

Bound, J. (1989). The health and earnings of rejected disability insurance applicants. *American Economic Review 79*(3), 482–503.

Brors, P. and P. Thelen (1998). Neue Runde im Streit um die Lohnfortzahlung. *Handelsblatt 59: 25.03.1998*, 3.

Cameron, A. C. and P. K. Trivedi (2005). *Microeconometrics: Methods and Applications* (1 ed.). Cambridge University Press.

Campolieti, M. (2004). Disability insurance benefits and labor supply: Some additional evidence. *Journal of Labor Economics 22*(4), 863–890.

Card, D. and A. B. Krueger (1994). Wages and employment: A case study of the fast-food industry in new jersey and pennsylvania. *American Economic Review 84*(4), 772–793.

Chen, S. and W. van der Klaauw (2008). The work disincentive effects of the disability insurance program in the 1990s. *Journal of Econometrics 142*(2), 757–784.

Curington, W. P. (1994). Compensation for permanent impairment and the duration of work absence: Evidence from four natural experiments. *The Journal of Human Resources 29*(3), 888–910.

Dionne, G. and B. Dostie (2007). New evidence on the determinants of absenteeism using linked employer-employee data. *Industrial & Labor Relations Review 61*(1), 108–120.

Doherty, N. (1979). National insurance and absence from work. *The Economic Journal 89*(353), 50–65.

Donald, S. G. and K. Lang (2007). Inference with difference-in-differences and other panel data. *The Review of Economics and Statistics 82*(2), 221–233.

Feil, M., S. Klinger, and G. Zika (2008). Der Beschäftigungseffekt geringerer Sozialabgaben in Deutschland: Wie beeinflusst die Wahl des Simulationsmodells das Ergebnis? *Journal of Applied Social Science (Schmollers Jahrbuch) 128*(3), 431–460.

Fenn, P. (1981). Sickness duration, residual disability, and income replacement: an empirical analysis. *The Economic Journal 91*(361), 158–173.

Frick, J. R. and M. M. Grabka (2005). Item-non-response on income questions in panel surveys: Incidence, imputation and the impact on inequality and mobility. *Allgemeines Statistisches Archiv 89*(1), 49–60.

German Federal Statistical Office (1996). *Statistical Yearbook 1996 for the Federal Republic of Germany.* Metzler-Poeschel.

German Federal Statistical Office (1998). *Statistical Yearbook 1998 for the Federal Republic of Germany.* Metzler-Poeschel.

Gruber, J. (2000). Disability insurance benefits and labor supply. *Journal of Political Economy 108*(6), 1162–1183.

Hans Böckler Stiftung (2009). *WSI Tarifarchiv.* www.boeckler.de, last accessed at September 25, 2008.

Henrekson, M. and M. Persson (2004). The effects on sick leave of changes in the sickness insurance system. *Journal of Labor Economics 22*(1), 87–113.

Ichino, A. and R. T. Riphahn (2005). The effect of employment protection on worker effort. A comparison of absenteeism during and after probation. *Journal of the European Economic Association 3*(1), 120–143.

Jahn, J. (1998). Lohnfortzahlung: Gerichte stehen vor Herkulesaufgabe. *Handelsblatt 124: 02.07.1998*, 4.

Johansson, P. and M. Palme (1996). Do economic incentives affect work absence? Empirical evidence using swedish micro data. *Journal of Public Economics 59*(1), 195–218.

Johansson, P. and M. Palme (2002). Assessing the effect of public policy on worker absenteeism. *Journal of Human Resources 37*(2), 381–409.

Johansson, P. and M. Palme (2005). Moral hazard and sickness insurance. *Journal of Public Economics 89*, 1879–1890.

Lambsdorff, O. G. (1982). Konzept für eine Politik zur Überwindung der Wachstumsschwäche und zur Bekämpfung der Arbeitslosigkeit. Dokumentation, Neue Bonner Depeche 9/82. http://www.archive.org/details/Lambsdorff-Papier, last accessed at March 20, 2009.

Medizinischer Dienst der Krankenversicherung (MDK) (2008). www.mdk.de, last accessed at October 23, 2008.

Meinhardt, V. and R. Zwiener (2005). Gesamtwirtschaftliche Wirkungen einer Steuerfinanzierung versicherungsfremder Leistungen in der Sozialversicherung. Politikberatung kompakt 7, German Institute for Economic Research (DIW) Berlin. http://www.diw.de, last accessed at December 19, 2008.

Meyer, B. D., W. K. Viscusi, and D. L. Durbin (1995). Workers' compensation and injury duration: Evidence from a natural experiment. *American Economic Review 85*(3), 322–340.

Pettersson-Lidbom, P. and P. Skogman Thoursie (2008). Temporary disability insurance and labor supply: evidence from a natural experiment. Working paper, Stockholm University, Department of Economics. http://people.su.se/ pepet/tdi.pdf, last accessed at March 19, 2009.

Puhani, P. A. (2008). The treatment effect, the cross difference, and the interaction term in nonlinear "difference-in-differences" models. IZA Discussion Paper Series 3478, IZA. http://www.iza.org, last accessed at 22.02.2008.

Sachverständigenrat zur Begutachtung der gesamtwirtschaftlichen Entwicklung (1996). *Reformen voranbringen*. Metzler-Poeschel.

Sachverständigenrat zur Begutachtung der gesamtwirtschaftlichen Entwicklung (2002). *Zwanzig Punkte für Beschäftigung und Wachstum*. Metzler-Poeschel.

Shapiro, C. and J. E. Stiglitz (1974). Equilibrium unemployment as a worker discipline device. *American Economic Review 74*(3), 433–444.

Social Security Administration (2006). *Annual Statistical Supplement 2006, Table 9.A2*. http://www.ssa.gov/policy/docs/statcomps/supplement/2006/9a.html, last accessed at March 19, 2009.

Social Security Administration (2008). *Annual Statistical Supplement 2006, Table 9.C1.* http://www.ssa.gov/policy/docs/statcomps/supplement/2008/9c.html, last accessed at March 19, 2009.

SOEPGroup (2001). The German Socio-Economic Panel (GSOEP) after more than 15 years: Overview. *Quarterly Journal of Economic Research (Vierteljahrshefte zur Wirtschaftsforschung) 70*(1), 7–14.

Wagner, G. G., J. R. Frick, and J. Schupp (2007). The German Socio-Economic Panel study (SOEP) - evolution, scope and enhancements. *Journal of Applied Social Science (Schmollers Jahrbuch) 127*(1), 139–169.

Winkelmann, R. (2008). *Econometric Analysis of Count Data* (5 ed.). Springer.

Wooldridge, J. M. (2003). Cluster-sample methods in applied econometrics. *American Economic Review 93*(2), 133–138.

Wooldridge, J. M. (2006). Cluster-sample methods in applied econometrics: an extended analysis. Working paper, Michigan State University, Department of Economics. https://www.msu.edu/ ec/faculty/wooldridge/current research/clus1aea.pdf, last accessed at March 19, 2009.

Wooldridge, J. M. (2007). What's new in econometrics? Imbens/Wooldridge lecture notes; summer institute 2007, lecture 10: Difference-in-differences estimation, NBER. http://www.nber.org/minicourse3.html, last accessed at March 19, 2009.

Ziebarth, N. R. (2009). Long-term absenteeism and moral hazard – Evidence from a natural experiment. DIW Discussion Papers 888, German Institute for Economic Research (DIW). http://www.diw.de/documents/publikationen/73/97949/dp888.pdf, last accessed at May 13, 2008.

Ziebarth, N. R. and M. Karlsson (2009). The effects of expanding the generosity of the statutory sickness insurance system. SOEPpapers 245, German Institute for Economic Research (DIW).

Zika, G. (1997). Die Senkung der Sozialversicherungsbeiträge. IAB Werkstattbericht 7, Research Insitute of the Federal Employment Agency (IAB).

Figure 1: Differences in Annual Absence Days by OECD Country
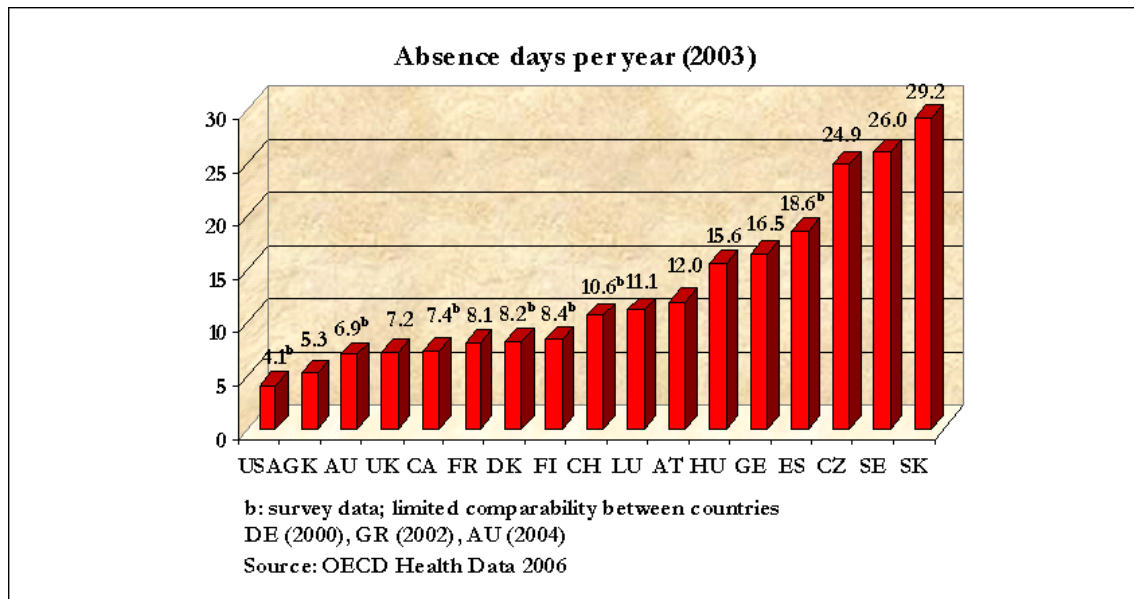
Figure 2: Cdf Pre-and Post-Reform Periods: treated vs. self-employed
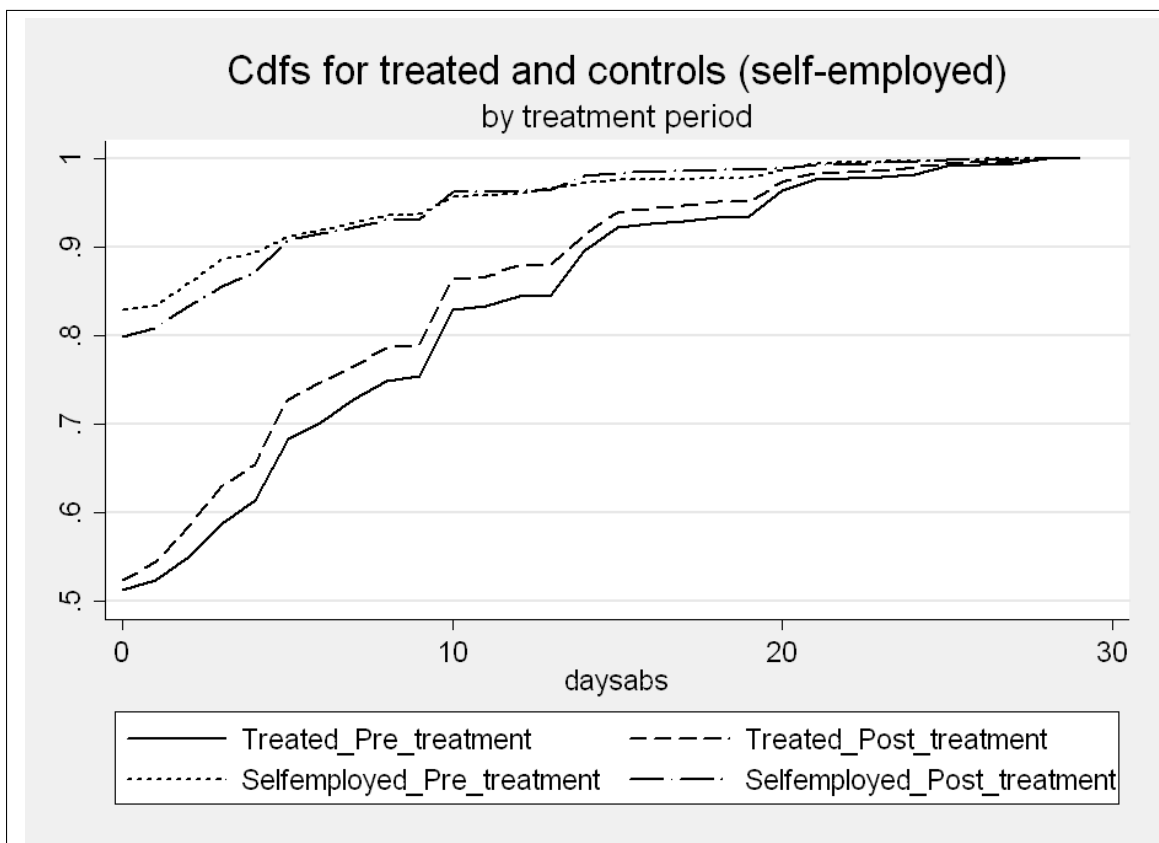
Table 1: Determinants of Short-Term Absenteeism: Zero-Inflated NegBin-2

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	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

Variable	Treated vs. Controls 1			Treated vs. Controls 2		
	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

Variable	East Germany				West Germany			
	Treated vs. Controls 1		Treated vs. Controls 2		Treated vs. Controls 1		Treated vs. Controls 2	
	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

Model	Treated vs. Controls 1		Treated vs. Controls 2	
	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

Table 9: Variable Means by Treatment and Control Groups

Variable	Treated: Mean (s.d.)	Controls 1: Mean (s.d.)	Controls 2: 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)		
No. of years in company	8.890	9.887	8.678	0	48.7

Continued on next page...

Variable	Treated: Mean (s.d.)	Controls: Mean (s.d.)	ControlsII: Mean (s.d.)	Min.	Max.
	(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*