Long-Term Absenteeism And Moral Hazard– Evidence From A Natural Experiment[‡]

Preliminary version. Comments welcome.

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Abstract

Sick leave payments represent a significant portion of health expenditures and labor costs. Reductions in replacement levels are a common instrument to tackle moral hazard and to enhance efficiency in the sickness insurance sector. In Germany's Statutory Health Insurance (SHI), the replacement level for sickness spells up to six weeks was reduced from 100 percent to 80 percent of an employee's gross wage at the end of 1996. At the same time, the replacement level for the long-term absent, i.e. from the seventh week onwards, was reduced from 80 to 70 percent. We show theoretically that the net reform effects on long-term absenteeism can be disentangled into a direct and an indirect effect. Using SOEP data, a natural control group, and two different treatment groups, we estimate the net and the direct effect on the incidence and duration of long-term absenteeism by difference-in-differences. Our findings suggest that, on population average, the reforms have not affected long-term absenteeism significantly which is in accordance with our theoretical predictions if we assume that the long-term absent are seriously sick. However, we find some heterogeneity in the effects and a small but significant decrease in the duration of long-term absenteeism for singles and middle-aged full-time employed. All in all, moral hazard and presenteeism seems to be less of an issue in the right tail of the sickness spell distribution. Finally, our calculations suggest that from 1997 until 2006, around 5.5 billion euros were redistributed from the long-term sick to the SHI insurance pool.

Keywords: long-term absenteeism, sick pay, moral hazard, natural experiment
JEL classification: C93; I18; J22

1 Introduction

The average number of sick days per year and employee varies between 5 and 27 among OCED countries (OECD, 2006). Sick leave payments determine health expenditures and labor costs to a large degree. Depending on the legal regulations that differ largely by country, either the employer or the health insurance compensates foregone earnings.

A common problem in insurance markets is moral hazard which drives up insurance costs and leads to an inefficient allocation of resources. As for sick leave, moral hazard prevails if insured employees call in sick although being able to work. Therefore, full compensation of foregone earnings is seldom provided both in private and public health insurance systems.

This study exploits a natural experiment that occurred in Germany at the end of 1996. At that time, compensation payments for long-term absent employees with sickness spells above six weeks amounted to 9.3 billion euros and made up 7.3 per cent of all expenditures in the German Statutory Health Insurance (SHI). Employers, who are legally obligated to pay employees for the first six weeks of sick leave, had to burden 28.2 billion euros (German Federal Statistical Office, 1998). As a reaction, two health reforms, which both cut the level of paid sick leave, were implemented. The main aim of this paper is to analyze how these reforms affected work absence spells of *more* than six weeks and to what extend moral hazard or presenteeism is of relevance in that part of the sickness spell distribution. Additionally, we calculate the SHI reform savings and redistributional effects.

There is a large amount of literature on absenteeism but only a few studies explicitly analyze the role of sick leave regulations and the design of insurance contracts. Some studies theoretically and empirically modeled determinants of sick leave behavior (Jensen and McIntosh, 1999; Johansson and Palme, 1996) and others showed how workplace conditions affect sickness absence (Dionne and Dostie, 2007; Ose, 2005). It is well documented that unemployment rates and absenteeism are negatively correlated. One reason refers to changes in the composition of the labor force but behavioral factors seem to play a major role (Askildsen et al., 2005). It has also been found that workers are more often on sick leave after the end of probation once employment protection is granted (Lindbeck et al., 2006; Ichino and Riphahn, 2005; Engellandt and Riphahn, 2005; Riphahn, 2004). The theoretical paper of Chatterji and Tilley (2002) is one of the few that explicitely discusses the role of presenteeism as a possible source of behavioral changes due to cuts in sick pay.

Only a handful of studies have empirically analyzed the relationship between absence behavior and compensation levels where only data from Sweden or the U.S. is employed. Curington (1994) used U.S. data on claim records of "minor permanent partial impairments" and estimated the effects of several legislative changes in the benefit levels on the length of work absences from 1964 through 1983. The results are mixed; some amendments induced changes in the work absence behavior, others did not. Another study from the U.S. showed that increases in workers' compensation for "temporary total disabilities" due to work related injuries led to an increase in injury duration in several states in the U.S. in the 80s (Meyer et al., 1995). Johansson and Palme (2002) modeled the impact of a tax reform and a reduction in replacement levels in the Swedish sickness insurance in 1991 on the hazard of work absence. They found that the increase in the costs of being absent reduced the incidence and length of sickness spells. Henrekson and Persson (2004) used long time series data for Sweden and took advantage of several legislative changes of the compensation levels to show that economic incentives strongly affect absence behavior. The study that comes closest to the one at hand has been conducted by Johansson and Palme (2005) who took the health reform in Sweden in 1991 as an exogenous source of variation. They found that even for absence spells of more than 90(!) days, employees adapt their absence behavior to changes in replacement levels. To our knowledge, it is at the same time the only study that (indirectly) analyzes how long-term absenteeism is affected by reductions in replacement levels.

All in all, the existing literature suggests that sick people react to economic incentives as classical economic theory would predict. These behavioral reactions could be induced by moral hazard, i.e. employees absent themselves from work although being healthy, or presenteeism, i.e. employees go to work although being sick.

We analyze the causal effects of two health reforms on long-term absenteeism in Germany. At the end of 1996, sick leave compensation for the first six weeks was reduced from 100 percent to 80 percent of foregone gross wages. The second reform came into force at the beginning of 1997 and reduced the compensation level from 80 to 70 percent from the seventh week onwards.[1] Both reforms generate exogenous sources of variation and yield testable implications.

To theoretically predict the effects of both reforms on long-term absenteeism we employ a dynamic model of absence behavior. First, if moral hazard plays a role and long-term sick employees react to economic incentives, long-term absenteeism should decrease as the direct costs of being long-term absent unambiguously increase. Second, the costs of long-term absences decrease relative to the costs of short term absences. This indirect effect theoretically impacts long-term absenteeism in a positive way. However, under the assumption that the long-term absent are severely sick, the incentive structure of the sick pay scheme breaks down and individuals do not adapt their labor supply to moderate cuts in sick pay.

Since Germany has two independent health care systems existing side by side, we are able to define subsamples that were affected by none, one, or both of the reforms. Thus, using data from the German Socio-Economic Panel Study (SOEP) and difference-in-differences methods, we can directly estimate the net effect and the direct effect of the two reforms on the incidence and duration of long-term absence spells. As the legislator also decreased the upper limit of long-term sick pay from 100 percent to 90 percent of monthly net wages, the treatment intensity is likewise exogenously varied. Hence, we are not only able to define treatment and control groups but also to analyze the reform effects by treatment intensity in relation to the gross wage.

[1] Henceforth, sickness spells that last less than six weeks are defined as short-term absence and sickness spells that last longer than six weeks are defined as long-term absence.

We are confident that we have not just captured a diverging time trend but causal effects for several reasons. First, the control and treatment groups are legally clearly defined by political decisions the character of the reforms with respect to the individual was unambiguously exogenous. Second, the legal regulations do not allow selection in or out of the treatment. Moreover, we control for many socioeconomic characteristics and the health status which is by far the most important determinant of long-term absenteeism. Third, due to the panel data format, the composition of the labor force can be considered. Finally, the reform effects on the incidence and the length of long-term absence spells are taken into account and differentiation by treatment intensity is possible.

We find that the cut in replacement levels had on average no significant effect on the incidence and duration of long-term sickness spells, neither directly nor indirectly. This result is in line with our model predictions if we assume that long-term absent employees are seriously sick. However, we find evidence for heterogeneity in the effects. For singles and middle-aged full-time employed, the duration of long-term absenteeism decreased significantly, although this decrease was of small magnitude. In contrast to the previous literature, these findings suggest that work absence behavior of more than thirty days does not react, or only react very weakly, to economic incentives which implies that moral hazard is of little importance in this context. We calculate that the SHI saved from 1997 until 2006 between 4 and 5.8 billion euros due to the cut in long-term sick pay. This amount was redistributed from the long-term sick to the insurance pool for the benefit of lower contribution rates.

The remainder of this paper is organized as follows. Section 2 explains the institutional features of the German health care sector, outlines the two health reforms, and describes which subsamples were affected by the health reforms. In Section 3 we derive theoretically how both reforms affected long-term absenteeism by means of a dynamic model on absence behavior. Section 4 describes which data we use and how our variables were generated, whereas our estimation and identification strategy is detailed in Section 5. Section

6 presents our estimation results which are discussed and summarized in Section 7.

2 The German Health Care System And The Policy Reforms

The German health care system actually consists of two independent health care systems existing side by side. The major of the two is the Statutory Health Insurance (SHI) that covers about 90 percent of the German population. Employees whose income from salary is below a politically defined income threshold (2007: €3,975 per month) are compulsorily insured with the SHI. High-income earners who exceed that threshold as well as self-employed have the right to choose between the SHI, a private health insurance (PHI), or to remain uninsured. Non-working spouses and dependent children are automatically insured by the SHI family insurance at no charge. Special groups such as students or unemployed are subject to special arrangements but mostly SHI insured. In principle, insurance coverage is the same for all SHI insured (German Ministry of Health, 2008).

The SHI is primarily financed by mandatory payroll deductions which are not risk-related. These contributions are equally paid by employer and employee up to a contribution ceiling (2007: €3,562.50 per month). Despite several health care reforms that tried to tackle the problem of rising health care expenditures, the contribution rates rose from 12.6 per cent in 1990 to 13.9 per cent in 2007 mainly due to demographic changes, medical progress, and system inefficiencies. The SHI is embedded in the German social legislation and is subject to the Social Code Book V (German Federal Statistical Office, 2008).

The second track of the German health care system is the Private Health Insurance (PHI). It basically covers private sector employees over the income threshold, public sector employees, and self-employed.[2] Privately insured

[2] We need to distinguish two types of employees in the German public sector. First, there are civil servants with tenure (called *Beamte*), henceforth called "civil servants."

people pay risk-related insurance premiums based on a health checkup at the beginning of the insurance period. The premiums exceed the expected expenditures in younger ages as the health insurer makes provisions for rising expenditures in older ages. Coverage is provided according to different health plans and insurance contracts are subject to private law. Consequently, in Germany, public health care reforms affect the SHI rather than the PHI.

It is important to note that, once an optionally insured opts out of the SHI system, a switch back is practically not possible. Employees above the income threshold are legally not allowed to switch back and employees who fall below the income threshold in subsequent years may switch back but loose their provisions as they are not transferable (neither between PHI and SHI, nor between the different private health insurances). In reality, a change to a private health insurance can be regarded as a lifetime decision and switching between the SHI and the PHI system as well as between private health insurances is very rare.

If an employee falls sick, a certificate from a physician is required from the third day in a sickness period. The employer is legally obligated to pay sickness compensation up to six weeks per sickness spell regardless of the employee's health insurance. From the seventh week on, the physician needs to issue a different certificate and sick leave is paid by the SHI or the PHI. The replacement level for SHI insured is codified in the social legislation and is the same for all SHI insured. In 1996, SHI payments for long-term absenteeism made up 7.3 per cent of all SHI expenditures, which equaled 9.3 billion euros (German Federal Statistical Office, 1998).

Two health reforms were implemented at the end of 1996. From October 1996 on, the replacement level during the first six weeks of sickness was reduced from 100 percent to 80 percent of foregone gross wages.[3] This reform

They are primarily PHI insured as the state reimburses around 50 per cent of their health expenditures (*Beihilfe*) and almost all of them insure the non-reimbursable expenditures privately. Second, we need to consider employees in the public sector without tenure (called *Angestellte im öffentlichen Dienst*). They have some privileges, too, but are mostly insured with the SHI (under the same conditions like everybody else). We call them "public servants."

[3] The correct German name of this law that was passed on September 15, 1996 is *Arbeit-*

had, at least theoretically, an indirect influence on sickness spells of more than six weeks and should therefore be considered. A second health reform act became effective on January 1, 1997. The replacement level from the seventh week on was cut from 80 percent to 70 percent of foregone earnings for those insured with the SHI.[4] Figure 1 illustrates the reduction in the replacement rates for short and long-term absence spells.

[**Insert Figure 1 about here**]

Sick leave payments for long-term absence spells are additionally limited by two benefit caps. First, if the wage of an SHI insured employee exceeds the legally defined contribution ceiling, then long-term sick pay is limited to 70 (80) percent of this contribution ceiling (2007: €0.7*3,562.50 per month) as contributions are capped over this ceiling as well. Second, before 1997, the replacement level was 80 percent of the gross wage if the total amount did not exceed 100 percent of net wage. After 1997, the replacement level decreased to 70 percent of the gross wage and the benefit cap to 90 percent of net wage. These upper limits introduce additional exogenous variation and allows us to generate an index that mirrors the cut in long-term sick pay on a continuous scale from zero per cent of gross wages to 10 percent of gross wages.

To deter people from substituting one long-term absence spell with several short-term absence spells to be compensated with the higher sick pay, the according law on employer-provided sick pay contains a specific passage.[5]

srechtliches Gesetz zur Förderung von Wachstum und Beschäftigung (Arbeitsrechtliches Beschäftigungsförderungsgesetz), BGBl. I 1996 p. 1476-1479. The law became effective at October 1, 1996. It should be noted that we are not able to perfectly identify those employees who were affected by this law as employers and unions voluntarily agreed in some collective wage agreements upon the continuity of the old sick pay arrangement. However, as this reform is not the focus of this paper, this is of minor importance.

[4] The correct German name of this law that was passed on November 1, 1996 is *Gesetz zur Entlastung der Beiträge in der gesetzlichen Krankenversicherung (Beitragsentlastungsgesetz - BeitrEntlG), BGBl. I 1996 p. 1631-1633.*

[5] The correct German name of this law is *Gesetz über die Zahlung des Arbeitsentgelts an Feiertagen und im Krankheitsfall (Entgeltfortzahlungsgesetz - EntgFG), BGBl. I 1994 p. 1014,1065.* Para. 3 contains the passage.

The passage particularizes that if an employee repeatedly has absence periods due to the same illness, the claim for employer-provided 100 percent sick pay is only sustained in case that a.) the employee was not on sick leave due to that particular illness for at least six month or b.) at least 12 month have elapsed since the employees fell sick for the first time. Consequently, the substitution of multiple short-term spells for one long-term spell is practically not possible and poses hence no problem.

We now define subsamples that have been affected differently by the two health reforms, thereby serving as treatment and control groups in the evaluation of this natural experiment. As the sickness compensation for long-term absence is paid by the health insurance and not by the employer, the second reform did not affect privately insured people as their replacement levels are subject to individual insurance contracts.

[**Insert Table 1 about here**]

We can easily see from Table 1 that private sector employees who were insured with the SHI (Subsample 1) were affected by both reforms. In contrast, SHI insured public sector employees (Subsample 2) were affected by the reduction in long-term sick pay but not by the cut in short-term sick pay due to political decisions. The same holds for SHI insured trainees (Subsample 3). The last two subsamples, PHI insured public sector employees and self-employed, were affected by none of the reforms. Like Table 2 visualized, we accordingly defined two treatment groups and one control group.

[**Insert Table 2 about here**]

3 A Dynamic Model of absence behavior

In the following, we analyze the absence behavior of an individual i within a two-period model. We modify a model by Brown (1994) so as to be able to study the theoretical effects of the German health reforms on long-term absence behavior. The individual's utility function can be specified as:

$$u_t = (1-\sigma_t)c_t + (\sigma_t)l_t, \qquad t = t, t+1;\ \sigma_t \in [0,1] \tag{1}$$

where t is the time period, c_t represents consumption in period t, and l_t leisure in period t. The sickness level in t is specified by σ_t, where larger values of σ_t represent a higher degree of sickness. If the sickness index tends towards unity, i.e. a high level of sickness prevails, the individual draws utility only from leisure or recuperation time rather than consumption. On the other hand, if the sickness level is relatively low, the individual attaches more weight to consumption as opposed to leisure. To simplify the analysis, we assume that $f(\sigma_t)$ follows a uniform distribution:

$$f(\sigma_t) = \begin{cases} 1 \quad if \quad 0 \leq \sigma_t \leq 1 \\ 0 \qquad\quad otherwise \end{cases}$$

This means that each sickness level is equally probable. At time t, individuals are aware of their sickness level σ_t but concerning the subsequent period, only the probality distribution $f(\sigma_{t+1})$ is known.

To adequatly model the German sick pay scheme, we define the replacement level during long-term sickness spells as r_l with $0 < r_l < 1$, and the replacement level during short term sickness spells as r_h with $0 < r_h < 1$. Moreover, $r_l < r_h < w$, where w represents the gross wage and is normalized to one. Sick pay is always provided when the individual is absent from work. Long-term sickness prevails if an individual is on sick leave for at least two continuous periods. Hence, in the first absence period after a working period, the sick pay is r_h which is reduced to r_l in the second period. If a working period follows a long-term sickness period, the replacement level for the next sickness period is again r_h.

A key feature of this simple dynamic model is the concept of the reservation sickness level, σ_t^*, as introduced by Barmby et al. (1994). The reservation sickness level is defined as the value of σ_t such that an individual is indifferent between attending work and staying home. To be more precise, at σ_t^* the utility from working in period 1 plus the expected utility in period 2 equals the utility from being absent in period 1 plus the expected utility in period 2. As we are primarily interested in the reform effects on long-term absenteeism, we assume that our individual was on sick leave in $t-1$ and is eligible for sick pay in t with r_l as the replacement level. In t, the reservation level is hence implicitely defined by:

$$(1-\sigma_t^*)r_l + \sigma_t^* T + \frac{1}{1+\rho}E(U_{t+1}^{absent}) = (1-\sigma_t^*)w + \sigma_t^*(T-h) + \frac{1}{1+\rho}E(U_{t+1}^{work}) \quad (2)$$

The left hand side of this equation represents the utility in period t if the individual continues to be on sick leave with sick leave compensation r_l and leisure T, where T is the total time available. The expected utility from period $t+1$ is added and discounted with the individual's time preference rate ρ. Analogously, the right hand side adds up the discounted utility in $t+1$ with the utility from working h hours and enjoying $T-h$ hours leisure in t.[6]

The individual decides whether to be absent from work by maximizing utility over both periods. If $\sigma_t > \sigma_t^*$, i.e. the actual sickness level exceeds the reservation sickness level, the individual stays away from work as more weight is placed on leisure rather than consumption. With other words, if employees are seriously sick, they value recuperation time far more than materialistic needs and go on sick leave. On the other hand, if $\sigma_t < \sigma_t^*$, individuals maximize their utility by working h hours.

On has to bear in mind that the decision to be absent from work or not has implications for the sick pay level in the next period. If individuals are absent from work in t, they get r_l in t as well as in $t+1$ if their sickness continues to be so severe that $\sigma_{t+1} > \sigma_{t+1}^{a*}$, where σ_{t+1}^{a*} is the reservation sickness level in $t+1$ conditional on having been absent in t. If they work in

[6] We assume a rigid employment contract without the possibility of working overtime or less than the contracted hours h.

t and fall sick in $t+1$, with $\sigma_{t+1} > \sigma^{w*}_{t+1}$, their sick pay is r_h. Hence we can define $E(U^{absent}_{t+1})$ which is the expected utility in $t+1$ conditional on having been absent at time t:

$$\begin{aligned}
&E(U^{absent}_{t+1}) \\
&= (1-\sigma^{a*}_{t+1})\left[\left(1-E(\sigma_{t+1}\left|\sigma^{a*}_{t+1} < \sigma_{t+1} < 1\right.)\right) r_l + E(\sigma_{t+1}\left|\sigma^{a*}_{t+1} < \sigma_{t+1} < 1\right.)\, T\right] + \\
&\quad \sigma^{a*}_{t+1}\left[\left(1-E(\sigma_{t+1}\left|0 < \sigma_{t+1} < \sigma^{a*}_{t+1}\right.)\right) w + E(\sigma_{t+1}\left|0 < \sigma_{t+1} < \sigma^{a*}_{t+1}\right.)\,(T-h)\right] \\
&= (1-\sigma^{a*}_{t+1})\left[\left(1-\left(\frac{1+\sigma^{a*}_{t+1}}{2}\right)\right) r_l + \left(\frac{1+\sigma^{a*}_{t+1}}{2}\right) T\right] + \\
&\quad \sigma^{a*}_{t+1}\left[\left(1-\left(\frac{\sigma^{a*}_{t+1}}{2}\right)\right) w + \left(\frac{\sigma^{a*}_{t+1}}{2}\right)(T-h)\right] \qquad (3)
\end{aligned}$$

As can be seen from (3), the expected utility in $t+1$ is expressed as the weighted average of the expected utility from attending work and being absent from work. The weights represent the probability that σ_{t+1} is less than the reservation sickness level and exceed the reservation sickness level, respectively. The expected values of consumption and leisure are evaluated by using the conditional probability distribution. Conditional on σ_{t+1} being between 0 and σ^{a*}_{t+1}, the expected value of σ_{t+1}, which is $\frac{\sigma^{a*}_{t+1}}{2}$ for the uniform distribution, is taken to evaluate the utility of a working employee. Analogously, the expected value of σ_{t+1}, conditional on being between σ^{a*}_{t+1} and 1, $\frac{1+\sigma^{a*}_{t+1}}{2}$, is substituted into the utility function for an absent employee.

Equivalently defined is $E(U^{work}_{t+1})$ which is the expected utility in $t+1$ conditional on having worked in t:

$$\begin{aligned}
E(U^{work}_{t+1}) &= \sigma^{w*}_{t+1}\left[\left(1-\left(\frac{\sigma^{w*}_{t+1}}{2}\right)\right) w + \left(\frac{\sigma^{w*}_{t+1}}{2}\right)(T-h)\right] + \\
&\quad (1-\sigma^{w*}_{t+1})\left[\left(1-\left(\frac{1+\sigma^{w*}_{t+1}}{2}\right)\right) r_h + \left(\frac{1+\sigma^{w*}_{t+1}}{2}\right) T\right] \qquad (4)
\end{aligned}$$

Finally, we derive $\sigma^{a*}_{t+1}$and σ^{w*}_{t+1} as:

$$\sigma_{t+1}^{a*} = \frac{w - r_l}{w - r_l + h} \tag{5}$$

$$\sigma_{t+1}^{w*} = \frac{w - r_h}{w - r_h + h} \tag{6}$$

We find that $\frac{\partial \sigma_{t+1}^{a*}}{\partial r_l} < 0$ and $\frac{\partial \sigma_{t+1}^{w*}}{\partial r_h} < 0$ which means that a decrease in sick pay levels has a positive impact on the reservation sickness levels resulting, ceteris paribus, in a lower probability to be absent from work. This is what we intuitively would expect when the costs of sickness rise. Moreover, static labor supply models also predict a decrease in absenteeism with decreasing sick pay rates (Brown and Sessions, 1996). Henceforth, we call this the direct effect of a reduction in sick pay.

As $r_l < r_h < w$, we get $\sigma_{t+1}^{a*} > \sigma_{t+1}^{w*}$ meaning that the probability to work in $t+1$ is higher for an employee who stayed home in t as opposed to an employee who worked in t. The reason is that the gap between wages and sick pay, i.e. the costs of absence, is bigger for an employee who experiences a continuous sickness spell as opposed to a one-period sickness spell. This is a reasonable approximation of the statutory sick leave regulations in Germany.

Plugging equations (3) to (6) into (2) and solving for the reservation sickness level σ_t^* yields:

$$\sigma_t^* = \sigma_{t+1}^{a*} + \frac{\varpi}{(1+\rho)(w - r_l + h)} \tag{7}$$

$$\varpi = \frac{(r_h - r_l)h^2}{2(w - r_l + h)(w - r_h + h)} > 0 \tag{8}$$

We see that σ_t^* equals σ_{t+1}^{a*} plus a discounted positive term which we interpret as the impact of future absence costs on the today's decision to be absent from work or not. It illustrates how the German sick pay scheme, which penalizes long absence spells in comparison to short absence spells, impacts the probability to stay home in the current period. In case of a flat sick pay level, which would not depend on the length of absence, the second term would vanish and the probability to be absent from work today would equal the probability to be absent from work tomorrow. Remember that this holds under the assumption that every health status is equally probable and outside the individual's influence. Utility maximizing individuals need to take the impact of today's absence behavior on future sick pay entitlements into account.

We now predict how long-term absenteeism is affected if the sick pay levels for short and long absence spells decrease and the employee is entitled for r_l in case of being absent. Consider first the effects of a reducation in r_l.

$$\frac{\partial \sigma_t^*}{\partial r_l} = \underbrace{\frac{\partial \sigma_{t+1}^{a*}}{\partial r_l}}_{<0} + \underbrace{\frac{\frac{\partial \varpi}{\partial r_l}(w - r_l + h) + \varpi}{(1+\rho)(w - r_l + h)}}_{<0} \tag{9}$$

We see from equation (9) that the total effect of a decrease in r_l is the sum of the direct effect $\frac{\partial \sigma_{t+1}^{a*}}{\partial r_l}$ and an additional factor. Hence, it is crucial to consider the impact of the discounted future term when evaluating the impact of a reducation in r_l. The second term represent the indirect effect that arises from the gap in the replacement levels between long and short-term absence spells, $r_h - r_l$. In case of a flat compensation scheme the gap closes and the indirect effect disappears. Ceteris paribus, a reduction in r_l widens the compensation gap, increases future absence costs, and thus effects long-term absenteeism negatively thereby strengthening the direct effect.

Now we consider a reducation in r_h. Note that there is no direct effect of a decrease in r_h for people who are in an ongoing long-term sickness spell. These people continue to get r_l if they remain absent and get their full wage if they go back to work. However, a reduction in r_h would, ceteris paribus, diminish the compensation gap between short and long-term absences and thus exert a positive effect on long-term absenteeism.

$$\frac{\partial \sigma_t^*}{\partial r_h} = \underbrace{\frac{\partial \sigma_{t+1}^{a*}}{\partial r_h}}_{=0} + \underbrace{\frac{\frac{\partial \varpi}{\partial r_h}}{(1+\rho)(w - r_l + h)}}_{>0} \tag{10}$$

We now want to relax the rather restrictive assumption that the sickness level σ_t is independent of the sickness level in the previous periods and that every sickness level is equally probable in every period. Suppose that the sickness levels are serially correlated and that r_h is paid for sickness spells up to 6 periods. If the employee continues to be on sick leave in the seventh period, r_l is paid. For a sickness spell to last more than 6 periods, the illness is supposed to be so severe that $\sigma_t > \sigma_t^*$ in every period. If that is the case, the incentive structure of our sick leave scheme breaks down and the employee is absent from work in every period.

In section 6, we empirically estimate the net effect as well as the direct effect of the German health reforms on long-term absenteeism.

4 Data And Variable Definitions

The dataset that we use is the German Socio-Economic Panel Study (SOEP). The SOEP is an annual representative household survey that started in 1984 and sampled more than 20,000 persons in 2006. Further details can be found elsewhere (Wagner et al., 2007).

Depending on our empirical estimation strategy, we use data of the years 1994 to 1999. As our goal is to evaluate a reduction in wage compensation levels, we drop non-working respondents and those you are not eligible for long-term sickness compensation (i.e. people who earn less than 400 euros per month, working students). Furthermore, we drop observations with missings and restrict our sample to respondents aged 18 to 65.

4.1 Endogenous and Exogenous Variables

The SOEP contains various questions about the usage of health services and the health insurance. We generate our first dependent dummy variable, which measures the incidence of long-term absenteeism, from the following question that was continuously asked from 1994 on: *"Were you sick from work for more than six weeks at one time last year?"* Since the sick pay is lowered after six weeks, since it is no longer disbursed by the employer but by the health insurance, and since a different certificate needs to be issued by the physician, measurement errors should play a minor role.

To measure how many days long-term sick pay was drawn, we use the following SOEP question: *"How many days were you not able to work in 199X because of illness?"* We generate our second dependent variable by subtracting, for those who had a long-term absence spell, from the total number of absent days the number of employer provided sick pay days, namely 30.[7] [8] Clearly, this duration variable is subject to measurement errors as we assume that the respondents had no other absence spells. Moreover, comparing the average duration of long-term sick pay with official data, it becomes clear that we face a systematic underreporting in the survey data as persons with long-term sickness spells are less likely to participate in the survey. However, if the cut in long-term sick pay did not affect the probability to participate in the survey and did not affect the sickness spell distribution, this duration measure is sufficient to evaluate the reform effects. While the former assumption clearly holds, one might argue that the latter is more problematic. Those who were only affected by the cut in long-term sick pay have an incentive to interrupt their long-term sickness spell and to start a new sickness spell. However, we do not need to fear such behavioral effects as, according to German law, the claim for employer-provided sick pay expires in case of such sickness spell substitutions (see Section 2 for more details). Once more the importance of having various treatment groups is emphasized here. By

[7] As already remarked, public servants enjoy special privileges. The period in which their employer provides a 100 percent sickness compensation varies from 6 weeks to 26 weeks depending on their seniority. As we have detailed information about the seniority levels, we are able to identify privileged public servants and redefine for them long-term absence spells. Eventually, they coincide with the period of the lower SHI sick pay. Hence, for public servants, we subtract the benefit days that are provided by the employer and that vary between 6 and 26 weeks.

[8] For those respondents who indicated to have been absent for more than 6 weeks but reported a total number of sick days of less than 30, we replaced the values as follows: the number of benefit days was replaced by the mean number of benefit days for respondents with valid information in the corresponding year. We are thereby likely to overestimate the true value which poses no problem as it is hardly imaginable that the treatment had an impact on the misreporting behavior.

comparing Treatment Group 1 with our controls, we cannot identify potential reform effects as a negative effect on the duration measure might as well be caused by the first reform. Contrasting Treatment Group 2, which was *only* affected by the cut in long-term sick pay, with the controls and bearing in mind that sickness spell substitutions are not of relevance here, we can reliably estimate the reform impact on the length of long-term sickness spells.

As both questions on absenteeism refer to the last year, we take the information of time variant covariates from the previous year if the respondent was interviewed the year before. For respondents who were not interviewed in the previous year, we take the current information and assume that it did not change meanwhile.

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 on *gender, immigrant, East Germany, partner, married, children, disabled, good health, bad health, no sports*, and *age* *(age^2)*. The second group consists of educational controls such as the degree obtained, the number of years with the company, and whether the person was trained for the job. The last group contains explanatory variables on job characteristics. Among them are *blue collar worker, white collar worker, the size of the company*, or the *monthly gross wage*.

4.2 Control Group, Treatment Groups, and Treatment Intensity Indices

As described in Section 2 and visualized in Table 2 and Table 2, we generate one control group and two treatment groups. For each of the treatment groups we compute a treatment index that represents the treatment intensity. By these means, we estimate the net effect and the direct effect.

The SOEP is very detailed about the insurance status and the workplace of the respondents which allows us to precisely assign them to the control and treatment groups. However, self-employed SHI insured have the option to exclude long-term sick pay for the benefit of lower contribution rates. As we are unable to identify respondents with such contracts, we drop them.

Another advantage of the SOEP is the extensive data about gross wages, net wages, and variable income components such as christmas or vacation bonuses. The SOEP group deals precisely with the problem of missing income data and imputes values thoroughly (Frick and Grabka, 2005). Thanks to this information and the legally defined upper limits for long-term sick pay (see Section 2), we are able to accurately generate treatment indices that display the decrease in replacement levels continuously from 0 to 10 percent of individual gross wages.

We firstly specify three treatment dummy variables. *Treatment Group 1* is a dummy variable that equals 1 if the respondent belongs to Treatment Group 1 and 0 if the respondent is in the Control Group. *Treatment Group 2* is a dummy variable that takes on the value 1 for respondents in Treatment Group 2 and 0 for respondents in the Control Group. Finally, *Treatment Group 3* has a 1 for people belonging to Treatment Group 2 and a 0 for people belonging to Treatment Group 1. In our basic specification, Treatment Group 1 contains 16,020 observations, Treatment Group 2 has 6,519 observations, and the Control Group contains 2,737 observations (see Appendix A).

Beside the universal rule that long-term sick pay is 70 (80) percent of the gross wage up to the contribution ceiling, legally defined upper limits induce an additional, continuous, and more precise source of exogenous variation. The maximum amount of long-term sick pay was restricted to 100 percent of the net wage before the reform and to 90 percent of the net wage after the reform. Depending on the individual gross and net wages for those being treated, we can calculate the individual decrease in long-term sick pay in percent of the gross wage. Hence, the treatment intensity varies from 0 percent of the gross wage for those being unaffected by the reform to a maximum decrease of 10 percent of the gross wage. We generate a continuous variable called *Treatment Index 1* that has the value 0 for those in the Control Group and values from 0.57 (percent) up to 10.00 (percent) for those in the *Treatment Group 1*. Equivalently built is *Treatment Index 2* who includes people in the Control Group and *Treatment Group 2*. The density of both variables *Treatment Index 1* and *Treatment Index 2* peaks around 6 (percent) and 10 (percent). About 80 percent of the treated faced a cut in long-term sick pay that lay between 4 and 8 percent and about 12 percent experienced a cut of 10 percent.

5 Estimation Strategy and Identification

We would like to measure the effect of a decrease in sick pay on absenteeism. Thinking of the policy intervention as a treatment, we define:

$$D_i = \begin{cases} 1 & \text{if individual } i \text{ belongs to the treatment group} \\ 0 & \text{otherwise} \end{cases}$$

and the first dependent variable $y_{it}^{(1)}$, which measures the incidence of long-term absenteeism, as:

$$y_{it}^{(1)} = \begin{cases} 1 & \text{if individual } i \text{ was long long-term absent in } t \\ 0 & \text{otherwise} \end{cases}$$

with $t = 0$ as the pre-treatment and $t = 1$ as the post-treatment period. The second outcome variable measures the number of days with long-term sick pay benefits, represents as such a non-negative integer count, and can be expressed as:

$$y_{it}^{(2)} = \begin{cases} 1, ..., 335 & \text{if individual } i \text{ was long long-term absent in } t \\ 0 & \text{otherwise} \end{cases}$$

The following formula gives us the average treatment effect on the treated ($ATOT$):

$$\widehat{ATOT} = E(y_{i1} - y_{i0} \,|D = 1) - E(y_{i1} - y_{i0} \,|D = 0) \tag{11}$$

This difference-in-differences (DiD) estimator differences out the overall time trends common to both groups. We can easily compute it by calculating the mean absence rates of the treatment group and the control group in both periods. Taking differences according to equation 11 yields the causal effect of the treatment on the outcome variable. However, the identifying assumption claims that the difference in the changes of the absence rates over time goes entirely back to the exposure of the treatment. With other words, it is assumed that the average outcome for both groups would have underlied a common time trend if there had been no treatment.

5.1 Probit Specification

The DiD estimator can be equivalently obtained in a regression framework that allows us to make statistical inferences. As our first dependent variable $y_{it}^{(1)}$ is binary we fit a probit model:

$$p_i = Pr[y_i^{(1)} = 1 \mid \text{p97, D, y97*D}] = \Phi(\beta_0 + \beta_1 \text{y97} + \beta_2 \text{D} + \delta \underbrace{(\text{p97*D})}_{\text{DiD}}) \quad (12)$$

where *p97* is a dummy that takes on the value 1 for post-treatment years and D is the treatment dummy. The interaction term between both dummies gives us the same DiD estimator as equation 11 with δ being the causal effect of the policy intervention. To evaluate how the reform affected our outcome variable, henceforth, we always compute and display the marginal effect of the interaction term $\frac{\Delta\Phi(.)}{\Delta(\text{p97*D})}$.[9] $\Phi(.)$ is the cumulative distribution function for the standard normal distribution. When estimating models that differentiate by treatment intensity, we replace the dummy Treatment Group 1 (or 2) by the continuous variable Treatment Index 1 (or 2), respectively,

[9] Puhani (2008) has shown that the advice of Ai and Norton (2004) to compute the discrete double difference $\frac{\Delta^2\Phi(.)}{\Delta \text{p97} \Delta \text{D}}$ is not of relevance in nonlinear models when the interest lies in the estimation of a treatment effect.

but keep the rest of the specification.

In practice, it is very rarely the case that control and treatment groups are absolutely identical over all observable characteristics. If the samples differ with respect to those characteristics that are related to the dynamic of the outcome, the DiD estimator is no longer consistent. Hence, it is standard practice to control for observed individual characteristics making the assumption of common time trends more reliable (Cameron and Trivedi, 2005). In our case we can formulate:

$$\begin{aligned} p_i = Pr[y_i = 1 \mid \boldsymbol{X}] = \Phi(&\beta_0 + \beta_1 \text{p97} + \beta_2 \text{D} + \delta \text{DiD} \\ &+ \boldsymbol{\gamma'}\beta_3 + \boldsymbol{\xi'}\beta_4 + \boldsymbol{\psi'}\beta_5 + \boldsymbol{\zeta'}\beta_6 + \boldsymbol{\omega'}\beta_7) \end{aligned} \tag{13}$$

where $\boldsymbol{\gamma'}$ contains 12 variables on personal characteristics like gender, age, and health status. $\boldsymbol{\xi'}$ incorporates 8 educational variables and $\boldsymbol{\psi'}$ 8 job related variables (see Appendix A). $\boldsymbol{\zeta'}$ and $\boldsymbol{\omega'}$ include 15 state dummies as well as the state unemployment rates.

5.2 Count Data Specifications

The second empirical specification intends to estimate how the policy reform affected the length of long-term absence spells in posttreatment periods. As the number of benefits days is a count with exzess zero observations and overdispersion, e.g. the conditional variance exceeding the conditional mean, we fit count data models. Based on the Akaike (AIC) and Bayesian (BIC) information criteria as well as different tests, we found two model specifications to be appropiate.

The first is a *Hurdle-at-Zero Negative Binomial Model*, also simply referred to as two-part model, which models two distinct statistical processes for the incidence and the duration of long-term absenteeism. The first part represents the probability of crossing the hurdle, e.g. of being long-term absent, and can be estimated by a logit or probit model equivalent to that in equation 13. The second part models the duration of long-term absenteeism by fitting a truncated at zero Negative Binomial-2 (NegBin-2) model (Deb and Trivedi, 1997).

The second count data model to be employed is the so called *Zero-Inflated Negative Binominal-2 Model* that equally allows diverging statistical processes for the incidence and duration of long-term absenteeism. The underlying statistical mechanism differentiates between the long-term ill and the non-long-term ill and assigns different probabilities to each group which are parameterized as functions of the covariates. The binary process is again specified in form of a logit or a probit model and the count process is now modeled as an untruncated NegBin-2 model for the binary process to take 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).

Both count data models incorporate the negative binomial distribution. The reason is that, in contrast to the more restrictive Poisson distribution, it does not only take excess zeros into account but also allows for overdispersion and unobserved heterogeneity. The NegBin model can be seen as a special case of a continuous mixture model. In the following, we use the notation of Cameron and Trivedi (2005). Assume that y_i is poisson distributed conditional on λ_i, where λ_i is random. More specificly, let $\lambda_i = \mu_i \nu_i$ with

$$\mu_i = exp(\boldsymbol{X\beta}) = e^{\beta_0+\beta_1 \mathrm{p97}+\beta_2 \mathrm{D}+\delta \mathrm{DiD}+\boldsymbol{\gamma}'\beta_3+\boldsymbol{\xi}'\beta_4+\boldsymbol{\psi}'\beta_5+\boldsymbol{\zeta}'\beta_6+\boldsymbol{\omega}'\beta_7} \tag{14}$$

Moreover, assume that $\nu_i > 0$ and iid with density $\gamma(\nu_i|\alpha_i)$ and the unknown parameter α_i. Unobserved heterogeneity is thus modeled by λ_i which takes on different values for each individual and incorporates the unobserved random component ν_i. We obtain the conditional poisson distribution for y as:

$$f\left(y|\lambda\right) = f\left(y|\mu, \nu\right) = \frac{e^{-\lambda}\lambda^y}{y!} = \frac{e^{-exp(\boldsymbol{X\beta})\nu}\{exp(\boldsymbol{X\beta})\nu\}^y}{y!} \tag{15}$$

To express the marginal density of y_i conditional on the two deterministic parameters μ_i and α_i but *unconditional* on the random parameter ν_i, we have to integrate ν_i out which gives us the NegBin distribution:

$$\varphi(y|\mu,\alpha) = \int f\left(y|\mu,\nu\right) \times \gamma(\nu|\alpha)\, d\nu \tag{16}$$

Here $f\left(y|\lambda\right)$ is poisson distributed as shown above and $\gamma(\nu|\alpha)$ is assumed to be gamma distributed with $E[\nu] = 1$ and $V[\nu] = \frac{1}{\delta} = \alpha$. Hence, with $\nu, \delta > 0$,

$$\gamma(\nu|\alpha) = \frac{\nu^{\delta-1}e^{-\nu\delta}\delta^{\delta}}{\Gamma(\delta)} \tag{17}$$

Plugging $f\left(y|\mu,\nu\right)$ and $\gamma(\nu|\alpha)$ into equation 16, we get the NegBin distribution as a density mixture:

$$\begin{aligned}
\varphi(y|\mu,\alpha) &= \int f\left(y|\mu,\nu\right) \times \gamma(\nu|\alpha)\, d\nu \\
&= \int_0^{\infty} \left(\frac{e^{-exp(\boldsymbol{X\beta})\nu}\{exp(\boldsymbol{X\beta})\nu\}^y}{y!}\right)\left(\frac{\nu^{\delta-1}e^{-\nu\delta}\delta^{\delta}}{\Gamma(\delta)}\right) d\nu \\
&= \frac{\Gamma(\alpha^{-1}+y)}{\Gamma(\alpha^{-1})\Gamma(y+1)}\left(\frac{\alpha^{-1}}{\alpha^{-1}+\mu}\right)^{\alpha^{-1}}\left(\frac{\mu}{\mu+\alpha^{-1}}\right)^{y} \qquad (18)
\end{aligned}$$

$\Gamma(.)$ denotes the gamma integral. The Negbin-2 model leaves α as a parameter to be estimated. The conditional variance function is quadratic in the mean and the first two conditional moments of the Negbin-2 model are $E[y|\mu,\alpha] = \mu$ and $V[y|\mu,\alpha] = \mu(1+\alpha\mu)$. The NegBin can be derived in different ways, has different variants, and different interpretations. Note that in the special case of $\alpha = 0$ the NegBin collapses to a simple Poisson model.

5.3 Identification

Our identification strategy is based on various pillars making us confident to reliably identify the causal effects of the two German health reforms on the incidence and duration of long-term absence spells.

First, we should point out that we use a dinstinct control group which was not affected by the reforms and which we observe over time. Additionally, the identification of two different treatment groups that were affected by a single and both reforms, respectively, makes it possible to distinguish between direct and net reform effects. As the insurance status of the respondents as well as their job characteristics and earnings are accurately collected, we can assign people very precisly to the control and treatment groups.

Second, we exploit an additional source of exogenous variation which allows us to distinguish effects by treatment intensity (see Section 2 for more details). By using income information that differentiates between gross wages, net wages, and fringe benefits we are able to generate treatment intensity variables remarkably exact. The implementation of the reform and the variation in the treatment intensity are distinct exogenous to the individuals and were politically determined.

Third, as in almost every study that builds upon natural experiments, the control group and the two treatment groups differ significantly with respect to most of the observed characteristics (see Table 3). For example, in comparison to the Control Group, Treatment Group 1 includes less females but more immigrants and the employees are less educated. Treatment Group 2 is younger than the other subsamples, less often married, and includes more white collar workers without tenure. As by definition, self-employed are only included in the Control Group and Treatment Group 1 excludes public sector employees, we do not incorporate these covariates in our regression framework. The heterogeneity in most of the observable characteristics is due to the federal regulations of the German health insurance and hence unavoidable. However, we argue that it is very unlikely that the common time trend assumption is violated as a.) the differences in characteristics are not the result of treatment related self-selection but politically determined, b.) we have a very rich dataset and are able include a variety of controls, c.) the key determinant of long-term absenteeism is the health status which we are able to control for. Recall that it poses no problem if the subsamples have different probabilities to be affected by long-term sickness; the identifying assumption would only be violated if unobservables existed that would impact the *change* of these probabilities differently.

[Insert Table 3 about here]

We can see from Table 4 that relatively few covariates affect long-term absenteeism significantly. More educated employees are less often long-term absent and firm size is positively correlated with long absence spells. As expected, the most important driver of long-term absenteeism is the health status. This is not surprising since the main reasons for long-term absences are a persistently low health stock and health shocks like unexpected illnesses and accidents (Müller et al., 1998).

[Insert Table 4 about here]

Once again, differences in the levels of the observables pose no problem per se as the crucial assumption to hold is that no unobserved time *trends* affected the long-term absence spells of the subsamples differently. In case of long-term absenteeism it is difficult to think of unobservables that have a diverging effect on the dynamic of the outcome, all the more, after having controlled for a rich set of health related, personal, educational, job related and regional covariates.

Fourth, we do not only estimate the reform effects on the incidence of long-term absenteeism but also the effects on the length of long-term absence spells. Although we work with survey data, which makes it possible to control for health, personal, and job characteristics at the cost of having no detailed spell data, we have good arguments why the available sick absence information is sufficient (see Section 4).

Fifth, to prove the consistency of our results, we perform various robustness checks. Thanks to the panel structure, we are able to control for the labor force composition by using balanced panels. Moreover, we experiment with different pre- and post-reform years and pool the data over only two years. Additionally, we restrict the sample size to singles and full-time employed aged 25 to 55.

Finally, an important feature of this study is that there is no selection in or out of the treatment which is a central issue in other settings, e.g. when labor market programs are evaluated. Switching between the two diverse health care systems, which were differently affected by the reforms, is not allowed for the great majority. We are able to identify the only subsample that has this right and exclude it in our robustness checks.[10]

Our basic empirical strategy is thus to pool the data for the years 1995 to 1998 and to estimate probit models as well as count data models where we employ the variables Treatment Group 1 to 3 as well as Treatment Index 1 and 2, respectively.

6 Results

Table 5 provides the unconditional DiD estimate of the reform's net effect on the incidence of long-term absenteeism which has been calculated according to equation 11. The unconditional long-term absence rate fell for Treatment Group 1 from 6.26 percent in the pre-treatment years 1995 and 1996 to 5.99 percent in 1997 and 1998. Without the availability of a control group and by means of before-after estimators one could erroneously attribute the total decrease to the reform. However, the absence rate for the Control Group fell even stronger from 3.51 to 3.06 percent, resulting in an overall difference-in-differences estimate of + 0.18 percent. Table 6 shows the same estimates for the duration of long-term absence spells. The average number of benefit days per insured person fell from 2.96 to 2.62 days for Treatment Group 2 and rose slightly from 2.05 to 2.15 days for the Control Group leading to an unconditional DiD estimate of -0.45 days.

[10] The only group that has the right to opt out of the SHI are optionally insured employees (self-employed and high-income earners above the income threshold). However, it is very unlikely that employees opted out of the SHI as a reaction to the cut in long-term sick pay. Opting out is a lifetime decision which is practically not feasible for the old due to extremely high premiums and makes no sense for the young as they are very likely to be unaffected by long-term absenteeism anyway. We consider the possibility that selection out of the treatment played a role in Section 6.

[Insert Table 5 and 6 about here]

To judge whether the decreases in the incidence and duration are statistically significant and to include further control variables, the DiD estimator is now incorporated into a regression framework. Table 7 reports the results from six model specifications that differ with respect to the inclusion of additional controls and measure the impact on the incidence of long-term absenteeism. Each specification represents a probit model equivalent to equation 13 with a dependent variable that is 1 if the respondent had a long-term sickness spell in the previous year and zero otherwise. The variable of interest is displayed as DiD1 and consists of an interaction between the dummy *Treatment Group 1* and the year dummy *y1997*. In every specification, marginal effects are calculated and displayed. In none of the model specifications, the DiD estimate is statistically different from zero. Notice that there was no time trend in 1997 that significantly affected the absence rates.

[Insert Table 7 about here]

In the next step, we disentangle the net effect of the reform into a direct effect and an indirect effect and separately estimate their impact on the incidence of long-term absenteeism. As has been theoretically shown in Section 3 this is crucial since it may be that the indirect reform effect compensated the direct effect rendering the net reform effect insignificant. This highlights the importance of a separate analysis which is displayed in Table 8. Column 1 shows once again the net effect; the regression model equals Model 6 in Table 7. Column 2 displays the direct effect of the reduction in long-term sick pay on the absence rate. Again, we used equation 13 but in contrast to column 1, *Treatment Group 2*, i.e. those *only* affected by the cut in long-term sick pay, has been interacted with the reform year dummy to get the DiD2 estimate. It is easy to see that the DiD2 coefficient is statistically not different from zero which is also the case for DiD3 in column 3 where we used *Treatment Group 3* which contrasts those solely affected by the cut in long-term sick pay with those being affected by both reforms. Note that all DiD point estimates are close to zero in magnitude.

[**Insert Table 8 about here**]

Treatment Index 1 and *2* represent the treatment intensity of the reform, namely the cut in long-term sick pay in percent of the individuals' gross wage. As before, we use these variables to estimate the net effect as well as the direct effect of the reforms on the incidence of long-term absenteeism. And as before, we are unable to reject the hypothesis that the difference-in-differences estimate is statistically different from zero (see Table 9).

[**Insert Table 9 about here**]

Table 10 gives us the DiD estimates when we use the long-term absent benefit days as dependent variable and estimate count data models. We always contrast Treatment Group 2 to our controls. As we do not find that those who were only affected by the cut in long-term sick pay significantly reduced their long-term absence durations, we infer that the reforms had no significant impact on absenteeism, neither on the incidence nor on the length of long-term absence spells.

[**Insert Table 10 about here**]

One piece of "eyeball evidence" supporting this conclusion is descriptive statistics from the German Federal Statistical Office. These statistics show a slight decrease from 5.84 benefit days per SHI member in 1996 to 5.07 benefit days in 1997 which lies within the usual fluctuation range (e.g. 1993: 4.88) and is in line with our results (German Federal Statistical Office, 2008).

6.1 Robustness Checks and Heterogeneity in Effects

Until now our estimation strategy was to pool the data over four years, which means that we allowed the sample composition to change over the years. As people with long-term absence spells have a higher probability to leave the labor force as a result of their (possibly severe) illness, we should check whether this selection out of the labor market distorted our results. From those who had a long-term absence spell in 1996, 7.1 percent did not answer the questionnaire one year later for unknown reasons (one respondent died and one moved abroad). We do not find evidence that long-term illness led to a higher probability to drop out of the sample in the subsequent year as 7.7 percent of the respondents without long-term absence spells did not participate in the following year. One the other hand, 74.6 percent of those who were long-term absent in 1996 were full-time employed at that time, whereas one year later, this number decreased to 62.3 percent for those who remained in the sample.[11] Especially if we had found a significant reform effect, one could have argued that the estimate was biased and caused by selection out of the labor market. There are several reasons why this selection effect is only of minor importance in our setting. First, in light of the selection, it is even more remarkable that we do not find significant reform effects. Second, in 1998 (with information about 1997) the SOEP group draw a random refreshment sample that covered all existing subsamples and included in total 1067 observations (Wagner et al., 2007). Thanks to this refreshment sample, the employment status distribution over those who had long-term sickness spells in 1996 and 1997 remained very stable. Under the consideration of the new observations, in total 73.1 percent of those who suffered long-term absence spells in 1997 were full-time employed (as compared to 62.3 percent without considering the refreshment sample).[12] Third, through the availability of a control group that we observe over time, we are able to control for treatment

[11] The ratio of full time employed who were not long-term absent was 71.9 percent in 1996 and 72.6 percent in 1997

[12] For the other employment status groups the deviation was less than 1.6 per cent.

independent selection.[13] In the absence of a control group one could easily confuse the illness related selection out of the labor market with a causal reform effect since it is natural that sickness absence rates decrease over time as the sample ages. Once again, this illustrates how crucial the availability of a real control group is. To our knowledge, this study is the only one that analyzes the effect of sick pay cuts and relies on a sound control group. Finally, as we use panel data, we can take account of the labor force composition by using a balanced sample.

In the following, we perform additional tests to prove the robustness of our results and to check whether heterogeneity in the reform effects is of importance. Table 11 reports results for the direct effect specification on the incidence of long-term absenteeism using Treatment Group 2. As a first test, we centered the data two years around the reform (column 1). Afterwards, we restrict our sample to the years 1996 and 1997, balance it, and consider only employees who were eligible for long-term sick pay in both years and answered the SOEP questionnaire in both years (column 2). An alternative robustness check is to take 1995 as reference year and contrast it to 1997 and 1998. It might be that pull-forward effects played a role and people adapted their behaviour in 1996 when the reform plans were made public (column 3). However, this is not very probable as many catalysts of long-term absences happen unexpected. Since people who started their long-term absence spell in 1996 and carried it over to 1997 took advantage of a transitory arrangement and were not exposed to the reduced sick pay, we contrasted the years 1995/1996 and 1998 in column 4. Another check is to restrict the sample to full-time employed aged 25 to 55 (column 5) and to singles (column 6) as the income of other household members may have an impact on the exposure to the treatment. On the household level, the relevant parameter might be the decrease in total household income rather than individual wages. Since optionally SHI insured could have switched to

[13] We can not, however, entirely exclude the possibility that the reform had an effect on the decision to leave the labor market voluntarily. We are unable to observe how large the share of voluntary labor market quitters was. However, as the cut in long-term sick pay was moderate and financial penalties are substantially higher for unemployed or retirees, we believe that reform induced selection out of the labor market plays a negligible role.

the PHI system as a reaction to the reform, we exclude all optionally insured in column 7. As can be easily seen in Table 11, none of the difference-in-differences estimates is statistically significant.

We employ the same specifications with the number of benefit days as dependent variable and estimate count data models using Treatment Index 2. As can be seen in Table 12, we find significant and negative reform effects on the length of long-term absence spells for singles as well as for middle-aged full-time employed. This suggests that heterogeneity in the reform effects are likely to play a role. However, the effects are small in magnitude. According to the estimates, a 1 unit increase in Treatment Index 1, i.e. a decrease of the long-term sick pay of 1 percent, led to a decrease in the average number of benefit days of around 0.06 and 0.09, respectively. Nevertheless, a linear relationship between decrease in sick pay and reduction of the number of benefit days is doubtful.

[**Insert Table 11 about here**]

Another standard method for checking the robustness of DiD estimates is to perform placebo regressions and to estimate the reform effects for years without a reform. For the assumption of common time trends of control and treatment group to hold, none of the placebo reform effects should be significant. Table 13 displays placebo regression results on the incidence and duration of long-term absenteeism for the years 1993 to 1997. All placebo estimates turn out to be insignificant.

[**Insert Table 13 about here**]

6.2 Calculation of SHI Reform Savings

In this subsection we calculate the total amount that the SHI has saved from 1997 to 2006 through the cut in long-term sick pay. The sum reflects the redistributional effect of the reform; reducing the replacement level for the long-term sick benefits the rest of the statutory health insurance pool through lower contribution rates.

For every eligible individual and the years 1997 to 2006, we calculate the sick pay according to the old and the new regulations, take the difference, and sum over the frequency weighted number of long-term absences for the whole period. The long-term sick pay amounted to 80 percent of the monthly gross wage before the reform and to 70 percent after the reform up to the contribution ceiling. The benefit cap decreased from 100 percent of the monthly net wage before the reform to 90 percent after the reform.

Already in 1995, the German Federal Constitutional Court (*Bundesverfassungsgericht*) pronounced the common practice to calculate long-term sick pay to be unconstitutional.[14] The Court criticized that SHI insured were forced to pay contribution rates on lump sum payments like cristmas or vacation bonuses (up to the contribution ceiling) but that these lump sum payments were not considered in the calculation of the sick pay. However, the legislator ignored these objections when passing the reform bill at the end of 1996. From 1997 to 2000, sick pay was calculated without considering lump sum payments but several Federal Social Court (*Bundessozialgericht*) actions were filed. In 2003, the Federal Social Court found judgement for plaintiff.[15] The claimants whose sick pay was miscalculated between January 1, 1997 and June 22, 2000 were set a time limit of one month to make an application for reimbursement of their miscalculated sick pay. From June 22, 2000 on, lump sum payments were considered (up to the contribution ceiling) in the calculation of long-term sick pay.

As it is unknown how many percent of the claimants filed an application within this rather restrictive time frame, our calculation specifications assume both full and zero reimbursement. Another question is whether the cut in long-term sick pay sensitized the population and caused the lawsuits.

[14] The judgement was pronounced at January 11, 1995 and is categorized under BVerfGE 92, 53.

[15] The judgement was pronounced at March 25, 2003 and is categorized under B 1 KR 36/01 R.

To deal with these unknowns, we formulate three scenarios. Specification I assumes that full reimbursement of the miscalculated sick pay was provided. It further assumes that if no reform had taken place, the change in the basis of calculation would have been effective from 2000 on. Specification II equals Specification I but it assumes that lump sum payments would have been considered from 1997 onwards in case of no reform. Specification III assumes that there had not been a change in the basis of calculation without the reform and that in reality, the change became not effective until 2000.

We take advantage of the rich SOEP dataset that does not only provide generated gross and net income measures but it also provides the sum of yearly bonuses per employee. In a first step, we calculate the amount of long-term sick pay that every eligible individual would receive per day according to the pre- and the post-reform regulations and our three specifications. Observations with nonsense income data were dropped.[16]

In a second step, we use administrative data from the German Ministry of Health on the total number of SHI long-term sick pay cases and the average number of benefit days benefits provided by the SHI. Every statutory health insurance (2006: 253) is legally obligated to file information about the insured and the benefits provided. The data are collected, aggregated, and published by the German Ministry of Health. Unfortunately, only the total number of eligible SHI insured, the ratio of long-term sickness cases, and the average length of sick pay received is available. No personal data and no income information is collected. Hence, we combine administrative data with the SOEP dataset that contains very detailed income information.

Comparing the frequency weighted number of SHI long-term sickness cases in the SOEP with the administrative data reveals that the SOEP underestimates the number of cases and well as the average benefit days per case. This is not surprising as especially long-term sick with very long sickness spells have a higher probability to not participate in the survey.

Now consider Table 14. All values are expressed in euros and inflation-

[16] We dropped respondents who claimed to be full-time employed and to earn less than €400 per month. Additionally, we dropped part-time employees who claimed to earn less than €200 per month.

adjusted with 2005 as the reference year. Columns 1, 2, and 3 show the estimates according to our three model specifications. The first row displays the difference between the average sick pay per case when the pre- and the post-reform regulations are compared. The sick pay per day and individual affected is calculated with SOEP data and is then multiplied with the average number of benefit days for those who had a long-term absence spell according to the Ministry of Health (2006: 76.07 days per case). Through the reform, the long-term sick pay has been cut on average by approximately €300 per case and year. Between 1997 and 2006 and according to our calculations, the average sum of long-term sick pay per case lay around €2,900.[17]

[Insert Table 14 about here]

The second row presents the estimates when we consider the frequency weighted long-term absence cases of the SOEP. All eligible SHI insured are included but as we slightly underestimate the total number of cases, we take these estimates as the lower bound. According to these estimates, the SHI expenditures decreased between 4 and 4.4 billion euros as a result of the reform. The third row displays the total amount saved when we only consider compulsorily SHI insured and use administrative data on the number of cases instead of SOEP data. Row four, by contrast, shows the estimates when we consider all SHI insured who are eligible for long-term sick pay according to official statistics. All in all, we estimate the total reform induced SHI health expenditure savings from 1997 to 2006 to lie between 4 and 5.8 billion euros depending on the assumptions. When considering all eligible SHI insured and under the assumption that the change in the calculation basis was independent of the reform, our estimate yields a total saving of 5,583,292,817 euros.

[17] Under the assumption of no reimbursement of the miscalculated sick pay between 1997 an 2000 and in nominal values.

7 Discussion and Conclusion

Economists often assume that moral hazard is responsible for a significant fraction of workplace absences, thereby contributing to rising health expenditures and labor costs. If this assumption holds true, it justifies reductions in sick pay replacement levels which would eventually lower the absence rate and duration, increase efficiency in the insurance market, and decrease health expenditures and labor costs. Several countries with public health insurance systems have indeed reduced the replacement levels for sick pay in recent years. Concurrently, some studies have found that people adapt their short-term absence behavior to economic incentives providing evidence for the existence of a considerable degree of moral hazard in the decision to go on sick leave.

The aim of this study is to analyze the causal effects of cuts in sick pay on long-term absenteeism. In Germany, two health reforms came into force at the end of 1996. The first reduced the compensation level for the first six weeks of a sickness spell from 100 percent to 80 percent of foregone gross wages. The second reduced the compensation level from the seventh week onwards from 80 to 70 percent.

We show that within a dynamic model of absence behavior, the net effect of the two reforms on long-term absenteeism is a priori unclear, as it is composed of two diverging effects. The direct effect increases the costs of being long-term absent and leads to a decrease of long-term absenteeism. The indirect effect arises as the replacement level for long-term absences is lower than the one for short-term absences. It has a positive impact on long-term absenteeism since through the two reforms, the costs of being long-term absent decreased relative to the costs of being short-term absent. The reform effects are derived under the assumption that the individuals' sickness levels are independent from previous periods and that every sickness level is equally probable. If we relax this assumption and assume that employees who are long-term sick are seriously ill, the sick pay incentive structure breaks down and long-term sick employees do not change their absence behavior as a reaction to moderate cuts in replacement levels.

The identification and estimation of the direct as well as the net effect is feasible by difference-in-differences. SOEP data and the two-track health insurance system in Germany allow us to identify subsamples that were affected by both reforms, only by the reduction in long-term sick pay, and by neither reform. Moreover, the legislator defined an upper limit for long-term sick pay that decreased from 100 percent of net wages to 90 percent of net wages as a consequence of the reform. Hence, an additional source of exogenous variation is provided that does not only allow us to assign employees to treatment and control groups but makes it possible to differentiate by treatment intensity in percent of the gross wages. Every part of the reform was distinct exogenous to the individual and politically determined. Moreover, selection into or out of the treatment is not an issue here as switching between the SHI and the PHI is not allowed due to rigid legal restrictions.

Our empirical findings suggest that the health reforms have, for the population average, not led to a significant change in the incidence and length of long-term absence spells. These results are robust to a battery of specifications. Beside a thorough assignment to treatment and control groups, we differentiate by treatment intensity. Moreover, the panel structure of the data allows us to take the labor force composition into account. We also experimented with different pre-reform and post-reform years and excluded optionally insured. Additionally, we performed placebo regressions to prove the common time trend assumption. Although we do not find general reform effects, we find evidence for heterogeneity in the effects. According to our estimates, the reform induced a small but significant decrease in the length of long-term sickness spells as far as singles and full-time employed middle-aged people are concerned.

After a thorough empirical investigation, we come to the conclusion that the long-term sick have not adapted their behavior to the monetary reform incentives in a significant manner. The empirical findings are in line with our theoretical model predictions if long-term sick are assumed to be seriously ill. This is plausible as, in Germany, the most common causes for sickness spells of more than 6 weeks are chronic diseases of the spine and arthropathy, accidents, cancer, and mental diseases. Moreover, 43 percent of the concerned persons have strong or very strong fears to be laid-off and to become unemployed (Müller et al., 1998). Interestingly, our results are in contrast to a study from Sweden that found absence behavior to be considerably affected by economic incentives even when absence spells of more than 90 days are assessed. The differences in the findings might be due to a.) cultural peculiarities, e.g. Germans are said to have a strong work ethic, b.) different reform settings, e.g. in this study the majority of the treated faced a 4 to 8 percent gross wage cut in long-term sick pay which is smaller than the cut in other studies, c.) the application of different econometric techniques, e.g. to our knowledge, this is the only study which does not rely on before-after estimators but employs a sound control group. By combining SOEP income data with administrative data we estimate the total SHI reform savings from 1997 to 2006 to lie between 4 and 5.8 billion euros in real terms as of 2005. The most realistic scenario yields a sum of 5.5 billion euros that was redistributed from the long-term sick to the insurance pool for the benefit of lower contribution rates.

Various pieces of evidence throughout this study let us infer that moral hazard is of minor importance when sickness spells or more than 30 days are considered. Consequently, health reforms in the spirit of the German do not lead to more efficient sickness insurance markets by decreasing moral hazard but are merely an instrument to cut health expenditures. On the other hand, if applied with moderate cuts in replacement levels, this cost containment instrument seems to be economically efficient in the sense that it induces no major behavioral changes which might lead to undesirable equilibria. Policy makers should be aware of the reform effects and the distributional consequences. It is simply a normative question whether this instrument to cut health expenditures should be applied.

Further research on how sickness absence, moral hazard, and presenteeism are related to the design of insurance contracts is essential as it has short and long-term consequences for health expenditures, health outcomes, labor costs, and productivity.

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Figure 1: Replacement Rates for Short and Long-Term Absence Spells in Percent of Foregone Gross Wages

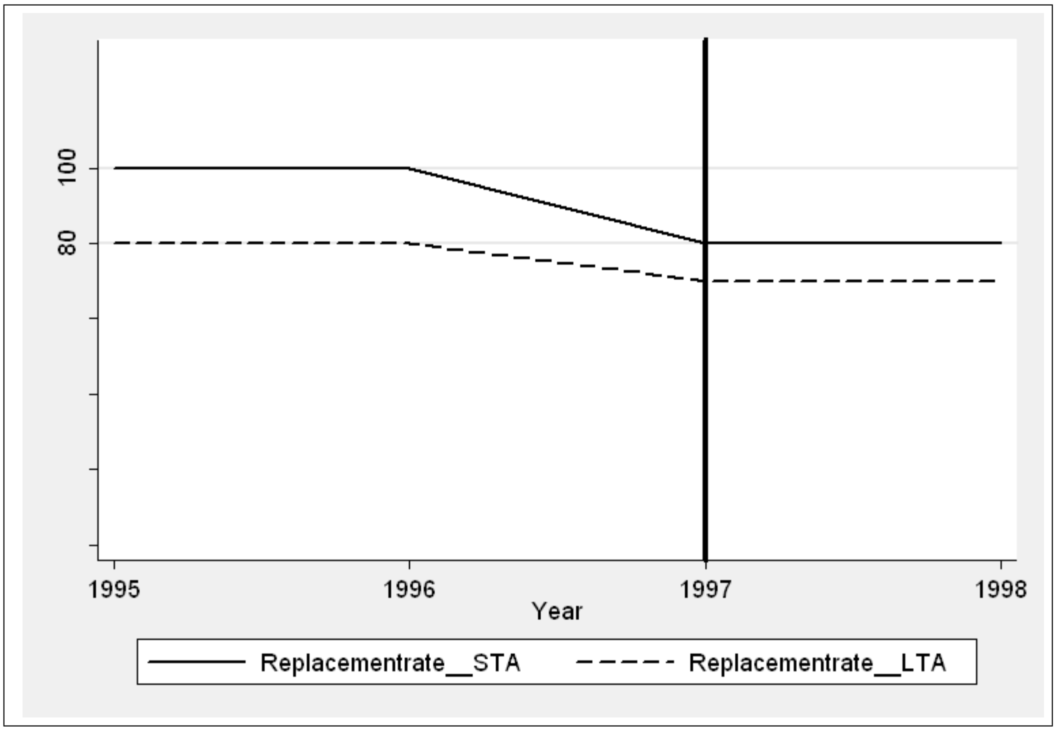

Table 1: Definition of Subsamples

	Reduction Sickness Compensation $<$ 30 days (paid by employer)	**Reduction Sickness Compensation $>$ 30 days (paid by SHI)**
Private sector Employees with SHI (1)	yes	yes
Public sector employees with SHI (2)	no	yes
Trainees with SHI (3)	no	yes
Public sector employees with PHI (4)	no	no
Self-employed with PHI (5)	no	no

Table 2: Overview Treatment and Control Groups

Effect to be estimated	Treatment groups	Control group
Net effect	subsample (1) **Treatment Group 1**	subsamples (4) + (5)
Direct effect	subsample (2) + (3) **Treatment Group 2**	subsamples (4) + (5)

Table 3: Variable Means by Treatment and Control Groups

Variable	Control Group	Treatment Group 1	Treatment Group 2
Long-term absent	0.033	0.061	0.028
Long-term absent benefit days	2.099	3.744	2.786
Personal characteristics			
Female	0.417	0.366	0.586
Age	40.6	39.9	37.5
Age square/100	17.6	17.0	15.6
Immigrant	0.096	0.215	0.112
East Germany	0.166	0.258	0.379
Partner	0.762	0.802	0.650
Married	0.676	0.696	0.568
Children	0.486	0.470	0.435
Disabled	0.033	0.052	0.053
Good health	0.646	0.607	0.604
Bad health	0.081	0.099	0.105
No sports	0.293	0.409	0.332
Educational characteristics			
Dropout	0.022	0.050	0.044
Degree after 8 years of schooling	0.236	0.358	0.271
Degree after 10 years of schooling	0.289	0.330	0.438
Degree after 12 years of schooling	0.051	0.035	0.035
Degree after 13 years of schooling	0.357	0.115	0.162
Other degree	0.046	0.112	0.051
Work in job trained for	0.602	0.545	0.511
New job	0.205	0.179	0.179
No. of years in company	10.2	9.0	8.8

Continued on next page...

... Table 3 continued

Variable	Control Group	Treatment Group 1	Treatment Group 2
Job characteristics			
No tenure	0.107	0.051	0.274
One man firm	0.101	0.000	0.000
Small company	0.332	0.274	0.169
Medium company	0.177	0.312	0.281
Big company	0.125	0.221	0.290
Huge company	0.265	0.193	0.260
Self employed	0.308	0.000	0.000
Blue collar worker	0.115	0.528	0.189
White collar worker	0.152	0.472	0.578
Civil servant	0.388	0.000	0.031
Public servant	0.488	0.000	0.829
High job autonomy	0.501	0.160	0.152
Gross income per month	2,347	2,013	1,672
Regional unemployment rate	11.5	12.0	13.1
N	2,737	16,020	6,519

Table 4: Determinants of Long-Term Absenteeism

Variable	Coefficient	Standard Error
Personal characteristics		
Female (d)	-0.001	0.003
Age	0.000	0.001
Age squared/100	0.001	0.001
Immigrant (d)	0.002	0.005
East Germany (d)	-0.015	0.010
Partner (d)	0.004	0.004
Married(d)	-0.006	0.004
Children (d)	-0.006*	0.003
Disabled (d)	0.032***	0.007
Good health (d)	-0.024***	0.003
Bad health (d)	0.072***	0.007
No sports (d)	0.007***	0.003
Educational characteristics		
Degree after 8 years' of schooling (d)	-0.006	0.006
Degree after 10 years' of schooling (d)	-0.009	0.006
Degree after 12 years' of schooling (d)	-0.017**	0.007
Degree after 13 years' of schooling (d)	-0.014**	0.006
Other degree (d)	-0.004	0.006
Work in job trained for (d)	-0.002	0.003
New job (d)	0.009**	0.004
No. of years in company	-0.000	0.000
Job characteristics		
No tenure last year (d)	-0.010***	0.004
Medium size company (d)	0.010***	0.004
Big company (d)	0.011***	0.004
Huge company (d)	0.010**	0.004
White collar worker (d)	-0.014***	0.003
High job autonomy (d)	-0.007*	0.004
Gross wage per month	-0.004**	0.002
Regional unemployment rate	0.003*	0.002
Year 1996 (d)	0.004	0.004
Year 1997 (d)	-0.004	0.005
Year 1998 (d)	-0.000	0.005
R-squared	0.108	
χ^2	904	
N	25276	

(d) for discrete change of dummy variable from 0 to 1

* p<0.10, ** p<0.05, *** p<0.01

Dependent variable: dummy that is 1 if respondent had long-term absence spell

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 5: Simple Difference-in-Differences Estimate on the Incidence of Long-Term Absenteeism

	1995/1996	1997/1998	Difference	Diff-in-Diff
Treatment Group 1	0.06256	0.05986	-0.0027	
Control Group	0.03508	0.03059	-0.00449	0.00179

Average incidence rate of long-term absenteeism is displayed

Table 6: Simple Difference-in-Differences Estimate on the Duration of Long-Term Absenteeism

	1995/1996	1997/1998	Difference	Diff-in-Diff
Treatment Group 2	2.9649	2.6232	-0.3417	
Control Group	2.0476	2.1535	0.1059	-0.4476

Average number of long-term benefit days is displayed

Table 7: Precise Difference-in-Differences Estimation on the Incidence of Long-Term Absenteeism

Variable	**Model 1**	**Model 2**	**Model 3**	**Model 4**	**Model 5**	**Model 6**
DiD1 (d)	0.0041	0.0032	0.0064	0.0042	0.0070	0.0073
	(0.0111)	(0.0111)	(0.0108)	(0.0108)	(0.0101)	(0.0098)
Post reform dummy (d)	-0.0015	-0.0115	-0.0124	-0.0097	-0.0106	-0.0094
	(0.0109)	(0.0122)	(0.0118)	(0.0119)	(0.0109)	(0.0105)
Year 1996 (d)	0.0068	0.0005	0.0009	0.0010	-0.0001	0.0007
	(0.0045)	(0.0053)	(0.0052)	(0.0052)	(0.0047)	(0.0047)
Year 1997 (d)	-0.0035	-0.0055	-0.0046	-0.0060	-0.0053	-0.0050
	(0.0042)	(0.0043)	(0.0042)	(0.0041)	(0.0038)	(0.0038)
Treatment Group 1 (d)	0.0269***	0.0265***	0.0161**	0.0240***	0.0160***	0.0139**
	(0.0058)	(0.0058)	(0.0064)	(0.0061)	(0.0055)	(0.0060)
Educational characteristics	no	no	yes	no	no	yes
Job characteristics	no	no	no	yes	no	yes
Personal characteristics	no	no	no	no	yes	yes
Regional unemployment rate	no	yes	yes	yes	yes	yes
State dummies	no	yes	yes	yes	yes	yes
R-squared	0.0053	0.0094	0.0311	0.0259	0.1052	0.1157
χ^2	33.49	54.25	189.90	154.99	709.47	785.51
N	18757	18757	18757	18757	18757	18757

(d) for discrete change of dummy variable from 0 to 1

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

Dependent variable: dummy that is 1 if respondent had long-term absence spell

Standard errors in parentheses are adjusted for clustering on person id

Table 8: DiD Estimation on Incidence: Direct vs. Indirect Effect

Variable	**Net effect**	**Direct effect**	**Direct vs. indirect effect**
DiD1 (d)	0.007		
	(0.010)		
Treatment Group 1 (d)	0.014**		
	(0.006)		
DiD2 (d)		0.003	
		(0.006)	
Treatment Group 2 (d)		-0.011**	
		(0.005)	
DiD3 (d)			-0.003
			(0.006)
Treatment Group 3 (d)			-0.027***
			(0.003)
Post reform dummy(d)	-0.009	0.004	-0.000
	(0.011)	(0.007)	(0.005)
Year 1996 (d)	0.001	0.013**	0.003
	(0.005)	(0.006)	(0.004)
Year 1997 (d)	-0.005	0.003	-0.005*
	(0.004)	(0.004)	(0.003)
Educational characteristics	yes	yes	yes
Job characteristics	yes	yes	yes
Personal characteristics	yes	yes	yes
Regional unemployment rate	yes	yes	yes
State dummies	yes	yes	yes
R-squared	0.116	0.110	0.124
χ^2	785.51	271.14	1136.85
N	18757	9256	22539

(d) for discrete change of dummy variable from 0 to 1

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

Dependent variable: dummy that is 1 if respondent had long-term absence spell

Standard errors in parentheses are adjusted for clustering on person id

Table 9: DiD Estimation on Incidence with Varying Treatment Intensity

Variable	**Net effect**	**Direct effect**
DiD1	0.000	
	(0.001)	
Treatment Index 1	0.003***	
	(0.001)	
DiD2		0.000
		(0.001)
Treatment Index 2		-0.000
		(0.001)
Post reform dummy(d)	-0.004	0.005
	(0.009)	(0.007)
Year 1996 (d)	0.001	0.013**
	(0.005)	(0.006)
Year 1997 (d)	-0.005	0.003
	(0.004)	(0.004)
Educational characteristics	yes	yes
Job characteristics	yes	yes
Personal characteristics	yes	yes
Regional unemployment rate	yes	yes
State dummies	yes	yes
R-squared	0.117	0.107
χ^2	790.07	275.91
N	18757	9256

(d) for discrete change of dummy variable from 0 to 1

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

Dependent variable: dummy that is 1 if respondent had long-term absence spell

Standard errors in parentheses are adjusted for clustering on person id

Table 10: DiD Estimation on the Duration of Long-Term Absenteeism

	Zero-Inflated Model		*Hurdle-at-Zero Model*	
Variable	**Direct effect**	**Direct effect: Varying Intensity**	**Direct effect**	**Direct effect: Varying Intensity**
DiD2	0.099	-0.024	2.566	-0.736
	(0.383)	(0.050)	(17.96)	(2.27)
Treatment Group 2	0.470***		0.468	
	(0.156)		(0.180)	
Treatment Index 2		0.051***		0.051***
		(0.017)		(0.014)
Post reform dummy(d)	-0.053	0.112	-0.053	0.110
	(0.287)	(0.276)	(0.300)	(0.289)
Year 1996 (d)	0.043	0.081	0.042	0.079
	(0.157)	(0.155)	(0.180)	(0.181)
Year 1997 (d)	0.121	0.067	0.121	0.067
	(0.143)	(0.144)	(0.157)	(0.156)
Educational characteristics	yes	yes	yes	yes
Job characteristics	yes	yes	yes	yes
Personal characteristics	yes	yes	yes	yes
Regional unemployment rate	yes	yes	yes	yes
State dummies	yes	yes	yes	yes
χ^2	7130.91	7689.79	111.29	108.45
N	9256	9256	272	272

(d) for discrete change of dummy variable from 0 to 1

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

Dependent variable: Number of long-term benefit days

Standard errors in parentheses are adjusted for clustering on person id

Table 11: Robustness and Heterogeneity of Effects: Direct Effect on Incidence Using Treatment Group 2

Variable	**1996-1997**	**Balanced sample: 1996-1997**	**1995 vs. 1997/1998**	**1995/1996 vs. 1998**	**Full-time: age 25 - 55**	**Singles**	**No optionally insured**
DiD2	0.0004	0.0009	0.0007	0.0013	0.0009	0.0016	0.0007
	(0.0009)	(0.0007)	(0.0007)	(0.0007)	(0.0009)	(0.0009)	(0.0007)
Educational characteristics	yes	yes	yes	yes	yes	yes	yes
Job characteristics	yes	yes	yes	yes	yes	yes	yes
Personal characteristics	yes	yes	yes	yes	yes	yes	yes
Regional unemployment rate	yes	yes	yes	yes	yes	yes	yes
State dummies	yes	yes	yes	yes	yes	yes	yes
R-squared	0.136	0.205	0.123	0.164	0.15	0.158	0.135
χ^2	197.40	210.69	246.71	288.77	229.92	100.92	252.20
N	4575	3314	6800	6851	5215	2800	8448

(d) for discrete change of dummy variable from 0 to 1

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

Dependent variable: dummy that is 1 if respondent had long-term absence spell

Standard errors in parentheses are adjusted for clustering on person id

Table 12: Robustness and Heterogeneity of Effects: Direct Effect on Duration Using Treatment Index 2

Variable	**1996-1997**	**Balanced sample: 1996-1997**	**1995 vs. 1997/1998**	**1995/1996 vs. 1998**	**Full-time: age 25 - 55**	**Singles**	**No optionally insured**
DiD2	0.0341	0.0051	-0.0527	-0.0277	-0.0970**	-0.0614**	-0.0457
	(0.070)	(0.0418)	(0.0361)	(0.0449)	(0.0449)	(0.0317)	(0.0461)
Educational characteristics	yes	yes	yes	yes	yes	yes	yes
Job characteristics	yes	yes	yes	yes	yes	yes	yes
Personal characteristics	yes	yes	yes	yes	yes	yes	yes
Regional unemployment rate	yes	yes	yes	yes	yes	yes	yes
State dummies	yes	yes	yes	yes	yes	yes	yes
χ^2	185.11	84475.54	316.35	238.28	180.87	57379.92	202.64
N	4575	3314	6800	6851	5215	2800	8448

(d) for discrete change of dummy variable from 0 to 1

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

Dependent variable: number of long-term benefit days

Zero-Inflated NegBin-2 Model is estimated

Standard errors in parentheses are adjusted for clustering on person id

Table 13: Differences-in-Differences Placebo Estimates Using Treatment Group 2

Variable	**Direct effect (Incidence)**	**Direct effect (Duration)**
DiD97 (d)	-0.001	0.932
	(0.007)	(0.893)
DiD96 (d)	-0.008	0.521
	(0.005)	(0.651)
DiD95 (d)	-0.008	0.271
	(0.005)	(0.568)
DiD94 (d)	-0.003	0.574
	(0.007)	(0.729)
DiD93 (d)	0.008	1.800
	(0.011)	(1.396)
Educational characteristics	yes	yes
Job characteristics	yes	yes
Personal characteristics	yes	yes
Regional unemployment rate	yes	yes
State dummies	yes	yes
χ^2	383.63	1256.82
N	13763	13763

(d) for discrete change of dummy variable from 0 to 1

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

Dependent variable: dummy that is 1 if respondent had long-term absence spell

Standard errors in parentheses are adjusted for clustering on person id

Table 14: Total Amount Saved by SHI Due to Reform: 1997-2006

Average: 1997-2006	**Specification I (1)**	**Specification II (2)**	**Specification III (3)**
SHI reform savings per case	287	305	314
Total amount redistributed: Frequency weighted SOEP cases	3,996,043,598	3,882,017,377	4,426,565,983
Total amount redistributed: Compulsorily insured (Federal Statistical Office)	4,094,026,130	4,030,306,809	4,555,888,018
Total amount redistributed: All eligible SHI insured (Federal Statistical Office)	5,217,821,195	5,583,292,817	5,843,037,704

Source: SOEP, German Ministry of Health, own calculations

All values are in Euro, inflation-adjusted (2005=100), and weighted

Specification I assumes that there wouldn't have been a change in the basis of calculation until 2000 if the reform had not been implemented; furthermore, full reimbursement of the miscalculated sick pay is assumed (1997-2000).

Specification II assumes that there would have been a change in the basis of calculation from 1997 on, if the reform had not been implemented; full reimbursement of the miscalculated sick pay is assumed (1997-2000).

Specification III assumes that there wouldn't have been a change in the basis of calculation at all, if the reform had not been implemented; zero reimbursement of the miscalculated sick pay is assumed (1997-2000).

Appendix A

Table 15: Descriptive Statistics

Variable	Mean	Std. Dev.	Min.	Max.	N
Long-term absence	0.05	0.217	0	1	25276
Long-term absent benefit days	3.319	20.945	0	335	25276
Treatment Group 1	0.854	0.353	0	1	18757
Treatment Group 2	0.704	0.456	0	1	9256
Treatment Group 3	0.289	0.453	0	1	22539
Treatment Index 1	5.686	2.765	0	10	18757
Treatment Index 2	4.638	3.331	0	10	9256
Personal characteristics					
Female	0.428	0.495	0	1	25276
Age	39.325	11.161	18	65	25276
Age squared/100	16.7	9.1	3.2	42.3	25276
Immigrant	0.176	0.381	0	1	25276
East Germany	0.279	0.449	0	1	25276
Partner	0.759	0.428	0	1	25276
Married	0.661	0.473	0	1	25276
Children	0.463	0.499	0	1	25276
Disabled	0.05	0.218	0	1	25276
Good health	0.61	0.488	0	1	25276
Bad health	0.099	0.298	0	1	25276
No sports	0.376	0.485	0	1	25276
Educational characteristics					
Drop out	0.045	0.208	0	1	25276
Degree after 8 years' of schooling	0.322	0.467	0	1	25276
Degree after 10 years' of schooling	0.354	0.478	0	1	25276
Degree after 12 years' of schooling	0.037	0.188	0	1	25276

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... Table 15 continued

Variable	Mean	Std. Dev.	Min.	Max.	N
Degree after 13 years' of schooling	0.153	0.36	0	1	25276
Other degree	0.089	0.285	0	1	25276
Work in job trained for	0.542	0.498	0	1	25276
New job	0.182	0.386	0	1	25276
No. years in company	9.1	9.2	0	47.9	25276
Job characteristics					
No tenure	0.115	0.318	0	1	25276
One man company	0.011	0.105	0	1	25276
Small size company	0.253	0.435	0	1	25276
Medium size company	0.289	0.453	0	1	25276
Big company	0.228	0.42	0	1	25276
Huge company	0.218	0.413	0	1	25276
Blue collar worker	0.396	0.489	0	1	25276
White collar worker	0.465	0.499	0	1	25276
Civil servant	0.05	0.218	0	1	25276
Self-employed	0.033	0.18	0	1	25276
High job autonomy	0.195	0.396	0	1	25276
Gross wage per month	1961	1108	0	40903	25276
Regional unemployment rate	12.24	3.97	7	21.7	25276