

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 compensation levels are a common instrument to tackle moral hazard and to enhance efficiency in the health insurance sector. In Germany's statutory health insurance system, the replacement level for sickness spells up to six weeks was reduced from 100 per cent to 80 per cent of the employees' earnings at the end of 1996. At the same time, the replacement level from the seventh week onwards was reduced from 80 to 70 per cent. In a first step, we theoretically show how these reforms affected long-term absenteeism. More precisely, we disentangle the net effect of both reforms into a direct and an indirect effect by means of a dynamic model of absence behavior. In a second step, we empirically estimate the causal effects of this natural experiment on long-term absenteeism by difference-in-differences. The two-track health care system in Germany provides a natural control group and two treatment groups that were differently affected by both reforms. In contrast to earlier studies, we find that the reforms have not affected long-term absenteeism significantly, neither directly nor indirectly. This is in accordance with our theoretical predictions if we assume that long-term absent employees are really severely sick. Thus, moral hazard and presenteeism are less of an issue in this part of the sickness spell distribution. Finally, our estimates suggest that from 1997 until 2006, between 4 and 5.8 billion euros have been redistributed from the long-term sick to the statutory health 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 results in 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.

There is a large amount of literature on absenteeism but only a few studies explicitly analyze the role of sick leave regulations or 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 a common view 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 discussed the issue of presenteeism instead of moral hazard as a possible source of behavioral changes that are induced by cuts in sick pay levels. Only one study – out of a handful that empirically analyzes the relationship between absence behavior and compensation levels – did not employ data from Sweden but from the U.S. Curington (1994) used 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. 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 in 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 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. Thus these behavioral reactions are induced by moral hazard, i.e. employees absent 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 absen-

teeism in Germany. At the end of 1996, sick leave compensation for the first six weeks was reduced from 100 per cent to 80 per cent of foregone gross wages. The second reform came into force at the beginning of 1997 and reduced the compensation level from 80 to 70 per cent 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 positively on long-term absenteeism. However, under the assumption that the long-term sick 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 long-term absenteeism. As the legislator also decreased the upper limit of long-term sick pay from 100 per cent to 90 per cent of monthly net wages, the treatment intensity is also exogenously varied proportionally to the income. This makes it possible not only to define treatment and control groups but also to analyze the reform effects by treatment intensity in relation to the gross wage.

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. Second, the legal regulations do not allow selection in or out of the treatment. Moreover, we focus on a short time span and are able to control for many socioeconomic characteristics and the health status. Third, due to the panel data format, the

¹Henceforth, 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.

composition of the labor force can be considered. Consequently, it is very unlikely that the central identifying assumption - conditional on the covariates, the long-term absence behavior of control and treatment groups underlies a common time trend - is violated. Finally, the character of the reforms with respect to the individual was unambiguously exogenous.

We find that the cut in replacement levels had 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 sick employees are seriously sick. In contrast to the previous literature, these findings suggest that work absence behavior of more than thirty days is not affected by economic incentives which implies that moral hazard or presenteeism 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. Our estimation strategy is presented in section 4, and section 5 describes which data we use and how our variables were generated. Section 6 presents our estimation results which are discussed and summarized in section 7.

2 The German Health Care System And The Policy Reform in 1997

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 popula-

tion. 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 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.

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.” They are primarily PHI insured as the state reimburses around 50 per cent of their health expenditures (*Beihilfe*) and almost all 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.”

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 lose 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 may 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 equalled 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 per cent to 80 per cent of foregone earnings.³ This reform 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 per cent to 70 per cent of gross wages for those insured with the SHI.⁴ Figure 7 illustrates the reduction in the

³ The correct German name of this law that was passed on September 15, 1996 is *Arbeitsrechtliches Gesetz zur Förderung von Wachstum und Beschäftigung (Arbeitsrechtliches Beschäftigungsförderungsgesetz)*, *BGBI. 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 issue 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 (Beitragsentlastungs-*

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 upper thresholds. 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) per cent 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 per cent of gross wages if the amount did not exceed 100 per cent of net wages. After 1997, the replacement level decreased to 70 per cent of gross wages up to 90 per cent of net wages. These upper limits allow 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 per cent of gross wages.

We 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 sickness compensation but not by the cut in short-term compensation due to political decisions. The same holds for SHI insured trainees (subsample 3). The last two subsamples, PHI insured public sector

gesetz - BeitrEntlG), BGBl. I 1996 p. 1631-1633.

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:

$$u_t = (1 - \sigma_t)c_t + (\sigma_t)l_t, \quad t = t, t + 1; \sigma_t \in [0, 1] \quad (1)$$

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:

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

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 probability distribution $f(\sigma_{t+1})$ is known.

To adequately 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 net 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 implicitly 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 .⁵

The individual decides whether to be absent from work by maximizing

⁵We assume a rigid employment contract without the possibility of working overtime or less than the contracted hours h .

utility over both periods. If $\sigma_t > \sigma_t^*$, i.e. the actual sickness level exceeds the reservation sickness level, the individual absents 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.

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

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

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_{t+1}^{a*} , the expected value of σ_{t+1} , which is $\frac{\sigma_{t+1}^{a*}}{2}$ 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_{t+1}^{a*} and

$1, \frac{1+\sigma_{t+1}^{a*}}{2}$, is substituted into the utility function for an absent employee.

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

$$E(U_{t+1}^{work}) = \sigma_{t+1}^{w*} \left[\left(1 - \left(\frac{\sigma_{t+1}^{w*}}{2} \right) \right) w + \left(\frac{\sigma_{t+1}^{w*}}{2} \right) (T - h) \right] + (1 - \sigma_{t+1}^{w*}) \left[\left(1 - \left(\frac{1 + \sigma_{t+1}^{w*}}{2} \right) \right) r_h + \left(\frac{1 + \sigma_{t+1}^{w*}}{2} \right) T \right] \quad (4)$$

Finally, we derive σ_{t+1}^{a*} and σ_{t+1}^{w*} as:

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

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

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 very good 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:

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

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

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 (multiple) 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 theoretically 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 reduction in r_l .

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

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 reduction 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 the future absence costs, and thus the effect on long-term absenteeism is negative and strengthens the direct effect.

Now we consider a reduction in r_h . Notice 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.

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

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 5 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.

The results of the model can be summarized as follows. If we assume that the sick pay level for short-term absence spells exceeds those for longer sickness spells, we find a direct effect of a cut in the replacement level for long-term absence spells that leads to a decrease in long-term absenteeism. However, an indirect effect arises from the gap in the compensation levels for short and long-term absences. If both replacement levels for short and long-term sick pay, r_h and r_l , are reduced and $\Delta r_h > \Delta r_l$, the difference between both replacement levels shrinks, which is exactly what the two reforms in Germany provoked. Before the German reforms, the difference in the replacement levels amounted to 20 per cent of the gross wage (100 vs.

80 per cent) which decreased to 10 per cent after the reforms (80 vs. 70 per cent).

The direct effect leads to a decrease in long-term absenteeism as the absence costs unambiguously increase, i.e. the direct costs of being long-term absent increased from 20 to 30 per cent of gross wages in Germany. At the same time, the indirect effect has a positive impact on long-term absenteeism as the costs of being long-term absent decrease relative to the costs for being short term absent from 20 to 10 per cent of the gross wage. Theoretically, the indirect effect may compensate or even overcompensate the direct effect which needs to be taken into account by policy makers. These predictions hold if we assume that the sickness level is independent of the former period and that every sickness level is equally probable in every period.

If we relax this assumption and assume that employees who are long-term sick are seriously sick then the sickness level exceeds the sickness reservation level in every period if the replacement levels do not take on extreme values. Consequently, the incentive structure of our model breaks down for severely sick employees; those employees remain on sick leave in every period regardless of the change in sick pay levels.

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 Estimation Strategy

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:

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

and the dependent variable y_{it} as:

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

With $t = 0$ as the pre-treatment and $t = 1$ as the post-treatment period, the following formula gives us the average treatment effect on the treated ($ATOT$):

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

This intuitively appealing 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 group would have underlied a common time trend if there had been no treatment.

The DiD estimator can be equivalently obtained in a regression framework that allows us to test the statistical significance (Wooldridge, 2005). As our dependent variable y_i is binary we fit a probit model:

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

where $y97$ is a dummy that takes on the value 1 for the year 1997 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. $\Phi(\cdot)$ is the cumulative distribution function for the standard normal distribution.

As the interaction effect is our variable of interest, we should consider a pitfall in the calculation of interaction effects in nonlinear models. The marginal effect of the interaction term between the two dummy variables, $y97$ and t , is for the probit model:

$$\begin{aligned}
\frac{\partial \Phi(\cdot)}{\partial (y97 * D)} &= Pr[y_i | DiD = 1] - Pr[y_i | DiD = 0] \\
&= \Phi(\beta_0 + \beta_1 y97 + \beta_2 D + \delta DiD + \beta_3 X) \\
&\quad - \Phi(\beta_0 + \beta_1 y97 + \beta_2 D + \beta_3 X)
\end{aligned} \tag{13}$$

As Ai and Norton (2004) have shown, this is not equal to the magnitude of the interaction effect $\frac{\partial^2 \Phi(\cdot)}{\partial y97 \partial D}$ which is the discrete double difference:

$$\begin{aligned}
\frac{\partial^2 \Phi(\cdot)}{\partial y97 \partial D} &= Pr[y_{i1} - y_{i0} | D = 1] - Pr[y_{i1} - y_{i0} | D = 0] \\
&= \Phi(\beta_0 + \beta_1 y97 + \beta_2 D + \delta DiD + \beta_3 X) \\
&\quad - \Phi(\beta_0 + \beta_2 D + \beta_3 X) \\
&\quad - \Phi(\beta_0 + \beta_1 y97 + \beta_3 X) \\
&\quad + \Phi(\beta_3 X)
\end{aligned} \tag{14}$$

Note that the direction of the interaction effect may change according to different values of the covariates. In the following, we report the correct interaction effect when presenting results.⁶

It has been mentioned above that the estimation strategy relies on the assumption of common time trends. In practice, it is very rarely the case that control and treatment groups are absolutely identical over a broad range of 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 which makes the assumption of common time trends stronger (Cameron and Trivedi, 2005).

⁶ $\frac{\partial^2 \Phi(\cdot)}{\partial (y97 * D)}$ is usually reported by standard statistical software packages and commands, such as `dprobit` in Stata. We use the user written command `inteff` to calculate and display the correct interaction effect $\frac{\partial^2 \Phi(\cdot)}{\partial y97 \partial D}$ (Norton et al., 2004).

$$\begin{aligned}
p_i = Pr[y_i = 1 | X] = & \Phi(\beta_0 + \beta_1 y97 + \beta_2 D + \delta DiD \\
& + \beta_3 \mathbf{PERS}' + \beta_4 \mathbf{EDU}' + \beta_5 \mathbf{JOB}' + \beta_6 \mathbf{UNEMP}' + \beta_7 \mathbf{STATE}')
\end{aligned}
\tag{15}$$

where **PERS'** contains 12 variables on personal characteristics like gender, age, and health status. **EDU'** incorporates 8 educational variables and **JOB'** 8 job related variables (see Table 3). **UNEMP'** and **STATE'** include 15 state dummies as well as the state unemployment rates.

It is important to include these covariates since control group and treatment groups differ significantly with respect to most of these 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 in a better health condition. 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, it is very unlikely that the assumption of a common time trend 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 should be 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 within a time span of only 2 years 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, it is obvious that the subsamples differ in their probability to be affected by such health shocks but the crucial assumption to hold is that there were on average no unobserved time *trends* that affected long absence spells of the subsamples differently. We believe this to be very plausible after having controlled for this multitude of personal, educational, job related and regional covariates. Moreover, we are able to control for the labor force composition and need not to worry about selection in or out of the treatment.⁷ Our basic empirical strategy is thus to pool the available data for the years 1996 and 1997 and estimate equation 15.

People are not uniformly affected by the reforms but the treatment intensity varies between zero and ten per cent of gross wages. As the variation in the treatment intensity is distinct exogenous, we exploit this additional source of variation to identify causal effects. Again equation 15 is estimated but the treatment dummy D is replaced by a continuous variable.

As a second identification strategy, we estimate the reform effects not only on the incidence but also on the duration of long-term absenteeism, measured by the number of long-term sick benefit days per case. The logarithm of this second quasi-continuous dependent variable is used to estimate a log-linear

⁷ As mentioned above, the only group that has the right to opt out of the SHI are optional 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 of 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 anyhow. We consider the possibility that selection out of the treatment played a role in section 6.

model instead of the probit model of equation 15. The rest of the specification is kept.

5 Data And Variable Definitions

The dataset we employ in this study 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 who 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.

5.1 Endogenous and Exogenous Variables

The SOEP contains various questions about the usage of health services and the health insurance. We generate our main 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 employ 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 days sick pay is provided by the employer,

namely 30.⁸ ⁹ Adding one and taking the logarithm yields a measure of the duration of long-term sick pay. Clearly, this 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 probability to have additional sickness spells, this duration measure is sufficient to evaluate the reform effects. While the former assumption clearly holds, 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 (with 100 per cent sickness compensation) in which case the total number of absent days would not change or even increase. If the long term sick adapted their behavior in such a manner, it is very likely that we would find a decrease in the incidence of long-term absenteeism as well. Comparing those who were affected by *both* reforms (80 vs. 70 per cent replacement level) with those being totally unaffected (100 vs. 80 per cent) eliminates the incentives of respondents to start new short-term sickness episodes. But then we are no longer able to identify whether a reduction in the number of absent days was due to the first or the second reform. We are aware of this drawback in comparison to administrative data but are confident that we are able to draw reliable conclusions when taking all regression results jointly into account.

⁸ As already remarked, public servants enjoy special privileges. The period in which their employer provides a 100 per cent sickness compensation varies from 6 weeks to 26 weeks depending on the 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. They coincide then with the period of the lower sick pay which is disbursed by the SHI. Hence, for public servants, we subtract the benefit days that are provided by the employer and varies between 6 and 26 weeks.

⁹ 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 their values as follows: the period in which long-term sick pay was drawn was replaced by the mean value of the respondents with valid information in the corresponding year. We are thereby likely to overestimate the true value which poses not problem as long as the treatment had no impact on the reporting of invalid information.

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*.

5.2 Definition of Treatment and Control Groups

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

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 a treatment index that

displays the decrease in replacement levels continuously from 0 to 10 per cent of individual gross wages.

Table 1 shows how different subsamples were affected by both reforms and Table 2 defines the control and treatment groups. For the estimation of the reform effects, we 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. We interact these treatment dummies with the reform year dummy to estimate the causal effects of the reforms on long-term absenteeism.

Beside the universal rule that long-term sick pay is 80 (70) per cent 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 per cent of the net wage before the reform and 90 per cent of the net wage after the reform. The individual wages are clearly exogenous to the treatment. Depending on the individual gross and net wages for those being treated, we can calculate the individual decrease in long-term sick pay in per cent of the gross wage. Hence, the treatment intensity varies from 0 per cent of the gross wage for those being unaffected by the reform to a maximum decrease of 10 per cent of the gross wage. We generate a continuous variable called *Treatment Index 1* that has the value zero for those in the control group and values from 0.57 (per cent) up to 10.00 (per cent) 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 the values 6 (per cent) and 10 (per cent). About 80 per cent of the treated faced a cut in long-term sick pay that lay between 4 and 8 per cent of their gross wage and about 12 per cent experienced a cut of 10 per cent of their gross wage.

6 Results

Table 5 provides the simple 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 6.6 per cent in 1996 to 5.7 per cent in 1997. 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 as well but only from 4.1 to 3.5 per cent, resulting in an overall difference-in-differences estimate of -0.2 per cent. Table 6 shows the same estimates for the duration of long-term sickness absence. The average number of benefit days per insured person fell sharply from 4.09 to 3.07 days for Treatment group 1 and rose slightly from 2.17 to 2.31 days for the control group leading to an unconditional DiD estimate of -1.158 days.

[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 incorporated into a regression framework according to equation 15. 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 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. If we repeat the exercise but perform an OLS regression on the logarithm of the duration of

long-term sick pay, the coefficient turns out to be insignificant as well.¹⁰

[Insert Table 7 about here]

In the next step, we first evaluate the impact on the incidence by disentangling the net effect of the reform through estimating the direct effect. As has been theoretically shown in section 4 this is crucial since it may be that the indirect reform effect, which works in the opposite direction than the direct effect, compensates 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 15 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 only affected by the cut in long-term sick pay with those being affected by both reforms.

[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 per cent of the individual's 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]

¹⁰ For the sake of saving space, the results are not reported but are available upon request.

Table 10 gives us the DiD estimates when we use the logarithm of the number of days with long-term sick pay as dependent variable. As has extensively been discussed in section 6, we have to make some assumptions when estimating the impact on the duration of long-term absenteeism. However, if we consider the results on the incidence of long-term absenteeism together with these specifications, this measure allows us to evaluate to reform effects on the duration of long-term absenteeism.

[Insert Table 10 about here]

In column 2 and 3, those who were solely affected by the cut in long-term sick pay are compared to the control group. One might argue that the non-significance of the coefficients goes back to a substitution of long-term spells with short-term spells. Though we can not exclude this possibility, we argue that this is unlikely as otherwise we would have observed a significant decline in the incidence of long-term absenteeism. Moreover, since employers are obligated to pay for the first 6 weeks of a spell they clearly disapprove multiple short-term spells as compared to one long-term spell. Comparing people that were affected by both reforms with our control group, column 4 and 5 provide additional evidence that there has not been a significant decrease in the length of long-term sick pay.¹¹ If we observed a significant decrease in the *number of absent days over 30*, we would not be able to conclude that this was caused by a reduction in the duration of long-term absenteeism as it might have been caused by a reduction in short-term absence spells. However, we should observe a significant decrease in the *number of absent days over 30* if the reforms led to a decrease in the duration of long-term sick pay. Irrespective of whether the first reform reduced short-term absenteeism or not.¹² As we do not observe such a decrease it seems feasible to infer

¹¹ As robustness checks, we also experimented with the inclusion of *Treatment Group 3*, restricted the sample to full-time employees aged 25 to 55, respondents without a partner, used a balanced sample, and experimented with different reference years but the results, which are available upon request, do not change.

¹²Notice that the impact of the first reform on sickness spells of less than 30 days is unambiguously negative.

that the reforms had no significant impact, neither on the incidence nor on the duration of long-term absenteeism.

One piece of “eyeball evidence” supporting this conclusion are 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) (German Federal Statistical Office, 2008).

Robustness Checks

Up to now our estimation strategy was to pool the data over two years which means that the sample composition changed over both 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. However, it is even more remarkable that despite this selection issue, we did not find a significant reform effect. As a decreasing absence rate is obviously natural in such a setting, especially in the absence of a control group, one could easily confuse the selection out of the labor market due to illness with a causal reform effect.

As a first test, we balance our sample and consider only employees who were eligible for long-term sick pay in both years and answered the SOEP questionnaire (Table 11, column 1). Second, in column 2, we centered the data four years around the reform to see whether an increased sample size would change the results. Additionally, we thereby examine whether illness related selection matters when a longer period is considered. Another check is to take 1995 as reference year as it might be that people adapted their behaviour in 1996 when the reform plans were made public and pull-forward effects played a role (column 3) which is implausible as many catalysts of long-term absences happen unexpected. As people who started their long-term absence spell in 1996 took advantage of a transitory arrangement and were not exposed to the reduced sick pay even if they continued to be on sick leave in 1997, we contrasted 1996 and 1998 in column 4. Another robustness

check is to restrict the sample to full-time employed aged 25 to 55 (column 4) and to singles (column 5) as the income of other household members may have an impact on the exposure to the treatment. On the household level, the relevant parameter seems to be the decrease in the household income rather than individual wages. Since optionally SHI insured could have switched to the PHI system as a reaction of the reforms, we exclude all optionally insured in column 6.

For the sake of saving space, we only report results for the direct effects on the incidence of long-term absenteeism using Treatment Index 2.¹³ As can be seen in Table 11, none of the difference-in-differences estimates is significant which proves the robustness of our previous results.

[Insert Table 11 about here]

Another standard method to check the robustness of DiD estimates is to perform placebo regressions and to estimate the reform effect for years without a reform. For the assumption of parallel time trends of control and treatment group to hold, none of the placebo reform effects should be significant. Table 12 displays placebo regression results on the incidence of long-term absenteeism for the years 1994 to 1998.¹⁴ All placebo estimates turn out to be insignificant.

[Insert Table 12 about here]

Calculation of SHI Reform Savings

In this section we will estimate the total amount that the SHI has saved from 1997 to 2006 through the cut in long-term sick pay. The sum reflects

¹³ We performed the same checks with *Treatment Group 1, 2, 3*, and *Treatment Index 1*. Moreover, we regressed all specifications on the logarithm of benefit days but the results remained insignificant. All estimates are available from the author upon request.

¹⁴ We therefore pool data over the years 1994 to 1999. Apart from that, we keep our basic model specification.

the redistributive 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.

Therefore, 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 per cent of the monthly gross wage before the reform and to 70 per cent after the reform up to the contribution ceiling. The upper limit decreased from 100 per cent of the monthly net wage before the reform to 90 per cent 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.¹⁵ The Court criticized that SHI insured were forced to pay contribution rates on lump sum payments like Christmas 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.¹⁶ 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 per cent 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

¹⁵ The judgement was pronounced at January 11, 1995 and is categorized under BVerfGE 92, 53.

¹⁶ The judgement was pronounced at March 25, 2003 and is categorized under B 1 KR 36/01 R.

cut in long-term sick pay sensitized the population and caused the lawsuits. 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 been made, the change in the basis of calculation would have been effective from 2000 on. Specification II equals Specification I but it is assumed that lump sum payments were 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 effective not 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.¹⁷

In a second step, we used administrative data from the German Ministry of Health about the total number of SHI long-term sick pay cases and the average length of days benefits were drawn. Every statutory health insurance (2005: 391) 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 survey with the administrative data reveals that the SOEP underestimates the number of cases. The same is true when the average length of sick pay drawn is considered. This is not surprising as especially long-term sick with very long sickness spells have a higher probability to not

¹⁷ 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.

participate in the survey.

Now consider Table 13. All values are expressed in Euros and inflation-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 by the average length of days the benefits were received 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. The average sum of long-term sick pay per case was around €2,900 between 1997 and 2006.¹⁸

[Insert Table 13 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 reduced its expenditures 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. Row four, by contrast, shows the estimates when we consider all SHI insured who are eligible for long-term sick pay. All in all, we estimate the amount of health expenditures that has been saved by the SHI from 1997 to 2006 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.

¹⁸Under the assumption of no reimbursement of the miscalculated sick pay between 1997 and 2000 and in nominal values.

7 Discussion and Conclusion

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

This paper analyzes the causal effects of two health reforms on long-term absenteeism. Both reforms came into force in Germany at the end of 1996. The first reduced the compensation level for the first six weeks of a sickness spell from 100 per cent to 80 per cent of foregone earnings. The second reduced the compensation level from the seventh week onwards from 80 to 70 per cent.

We show that within a dynamic model of absence behavior, the net effect of the two reforms 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; as to the two reforms in Germany, it has a positive impact on long-term absenteeism as the costs of being long-term absent decrease relative to the costs of being short-term absent. These reform effects are derived under the assumption that the individual's sickness level is 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 severely sick, we find that the sick pay incentive structure breaks down and that long-term sick employees would not change their behavior as a reaction to moderate cuts in replacement levels.

The empirical identification and estimation of the direct as well as the net effect is feasible by means of difference-in-differences. The 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 per cent of net wages to 90 per cent 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 per cent of 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 it is practically impossible to switch between the SHI and the PHI due to rigid legal restrictions.

The main results of the empirical part can be summarized as follows. The two health reforms have not significantly affected the incidence and duration of long-term absenteeism in any form, neither directly nor indirectly. These results are robust to a battery of tests. By balancing the sample we exclude the possibility that the composition of the labor force played a role. We also centered the data four years around the reform and experimented with different pre-reform and post-reform years. Additionally, we restricted the sample under consideration to full-time employees, singles, and compulsorily insured and performed placebo regressions to prove the robustness of our findings.

After a thorough empirical investigation, we come to the conclusion that the long-term sick have not adapted their behavior to the economic incentives of the reforms. Our empirical finding is in line with our theoretical predictions if we assume that people who are long-term sick are severely sick. This is plausible as the most common causes for sickness spells of more than 6 weeks in Germany are chronic diseases of the spine and arthropathy, accidents, cancer, and mental diseases. Moreover, 43 per cent of the concerned persons have strong or very strong fears to get dismissed and to become unemployed

(Müller et al., 1998). In view of that, we are confident to conclude that moral hazard is of minor or no importance when sickness spells of more than 30 days are the focus. Interestingly, this is in contrast to earlier studies which found that absence behavior is affected by economic incentives, even in case of absence spells of more than 90 days. These differences 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 per cent 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 has been redistributed from the long-term sick to the insurance pool for the benefit of lower contribution rates.

We conclude that we do not find evidence for morally hazardous behavior or presenteeism when sickness spells of more than six weeks are considered. Consequently, health reforms in the spirit of the German induce no behavioral changes and do not lead to more efficient insurance markets but are merely an instrument to cut health expenditures. On the other hand, if applied with moderate cuts in replacement levels, it seems to be a cost containment instrument that does not lead to adverse health outcomes as a consequence of presenteeism. Policy makers should be aware of the reform effects as well as the distributional consequences. It is simply a normative question whether this instrument to cut health expenditures should be applied.

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

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

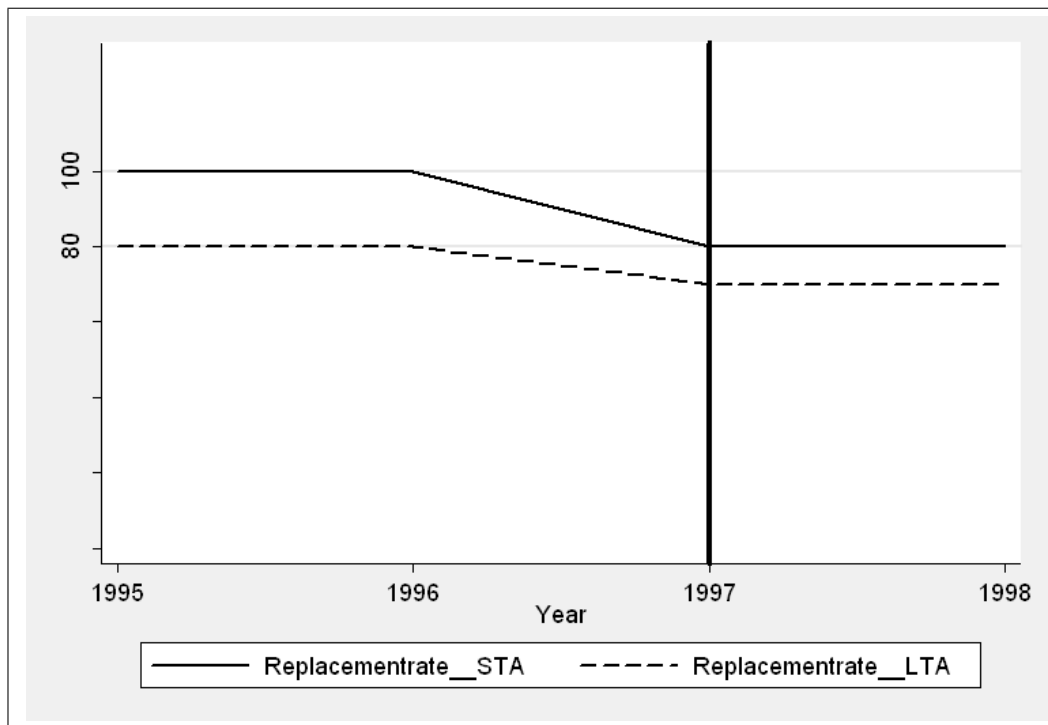


Table 1: Definition of Subsamples

	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

Estimated Effect	Treatment group	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 last year	0.038	0.061	0.033
Personal characteristics			
Female	0.420	0.366	0.589
Age	40.755	39.866	37.426
Age square	1,770.172	1,700.798	1,554.963
Immigrant	0.089	0.215	0.112
Eastgerman	0.156	0.258	0.378
Partner	0.770	0.803	0.653
Married	0.687	0.693	0.568
Children	0.486	0.473	0.432
Disabled	0.036	0.052	0.051
Good health	0.645	0.603	0.599
Bad health	0.072	0.098	0.106
No sports	0.325	0.445	0.376
Educational characteristics			
Dropout	0.021	0.049	0.047
Degree after 8 years of schooling	0.222	0.358	0.267
Degree after 10 years of schooling	0.291	0.329	0.436
Degree after 12 years of schooling	0.051	0.036	0.037
Degree after 13 years of schooling	0.373	0.117	0.166
Other degree	0.043	0.111	0.047
Work in job trained for	0.614	0.545	0.515
New job	0.183	0.172	0.171
No. of years in company	10.645	9.040	8.802

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... Table 3 continued

Variable	Control Group	Treatment Group 1	Treatment Group 2
Job characteristics			
No tenure	0.091	0.050	0.273
Small company	0.323	0.274	0.166
Medium company	0.175	0.312	0.284
Big company	0.128	0.220	0.290
Huge company	0.279	0.194	0.260
One man firm	0.104	0.001	0.000
Self employed	0.296	0.000	0.000
Blue collar worker	0.109	0.524	0.185
White collar worker	0.134	0.476	0.587
Civil servant	0.435	0.000	0.028
Public servant	0.525	0.000	0.832
High job autonomy	0.522	0.160	0.154
Gross income per month	2,375.435	2,014.731	1,682.643
Regional unemployment rate	11.781	12.426	13.476
N	1435	7988	3251

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

Table 4: Determinants of Long-Term Absenteeism

Variable	Coefficient	Standard Error
Personal characteristics		
Year 1997 (d)	-0.006	0.006
Female (d)	-0.000	0.004
Age	-0.001	0.001
Age square	0.000	0.000
Immigrant (d)	0.007	0.007
Partner (d)	-0.000	0.006
Married last year (d)	-0.003	0.006
Children (d)	-0.007*	0.004
Disabled (d)	0.035***	0.010
Good health (d)	-0.026***	0.004
Bad health (d)	0.075***	0.010
No sports (d)	0.009**	0.004
Educational characteristics		
Degree after 8 years' schooling (d)	-0.001	0.008
Degree after 10 years' schooling (d)	-0.003	0.008
Degree after 12 years' schooling (d)	-0.019**	0.008
Degree after 13 years' schooling (d)	-0.009	0.008
Other degree (d)	-0.007	0.008
Work in job trained for (d)	-0.003	0.004
New job (d)	0.011**	0.005
No. years in company last year	-0.000	0.000
Job characteristics		
No tenure last year (d)	-0.010*	0.005
Medium size company (d)	0.011**	0.005
Big company (d)	0.016***	0.006
Huge company (d)	0.011*	0.006
White collar worker (d)	-0.018***	0.004
High job autonomy last year (d)	-0.004	0.006
Own generated gross wage per month	-0.000	0.000
Regional unemployment rate	0.001	0.004
Year 1997 (d)	-0.006	0.006
R-squared	0.116	
χ^2	552.626	
N	12895	

(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 size firm

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

	1996	1997	Difference	Diff-in-Diff
Treatment Group 1	0.066	0.057	-0.009	
Control Group	0.041	0.035	-0.006	-0.002

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

	1996	1997	Difference	Diff-in-Diff
Treatment Group 1	4.094	3.077	-1.017	
Control Group	2.171	2.312	0.141	-1.158

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
y1997 (d)	-0.0088 (0.0131)	-0.0145 (0.0148)	-0.0118 (0.0144)	-0.0134 (0.0143)	-0.0101 (0.0133)	-0.0091 (0.0128)
Treatment Group 1 (d)	0.0231*** (0.0081)	0.0230*** (0.0080)	0.0134 (0.0088)	0.0226*** (0.0082)	0.0125 (0.0077)	0.0124 (0.0081)
DiD1 (d)	-0.0022 (0.0106)	-0.0053 (0.0112)	-0.0020 (0.0123)	-0.0036 (0.0111)	-0.0023 (0.0122)	-0.0018 (0.0120)
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	0.0040	0.0088	0.0279	0.0269	0.1074	0.1205
χ^2	14.8174	30.9727	96.0399	91.2304	412.6251	455.0880
N	9423	9423	9423	9423	9423	9423

(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)	-0.002 (0.012)		
Treatment Group 1 (d)	0.012 (0.008)		
DiD2 (d)		-0.003 (0.013)	
Treatment Group 2 (d)		-0.012 (0.008)	
DiD3 (d)			-0.002 (0.008)
Treatment Group 3(d)			-0.023*** (0.005)
y1997 (d)	-0.009 (0.013)	0.007 (0.008)	-0.006 (0.007)
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	0.120	0.129	0.129
χ^2	455.088	180.147	597.535
N	9423	4686	11239

(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
DiD (d)	0.0003 (0.0017)	-0.0007 (0.0015)
Year 1997 (d)	-0.011 (0.011)	0.009 (0.009)
Educational characteristics	yes	yes
Job characteristics	yes	yes
Personal characteristics	yes	yes
Regional unemployment rate	yes	yes
State dummies	yes	yes
R-squared	0.121	0.126
χ^2	456.368	192.414
N	9423	4686

(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

Variable	Direct effect	Direct effect: Varying Intensity	Net effect	Net effect: Varying Intensity
DiD (d)	-0.018 (0.041)	0.001 (0.006)	-0.009 (0.045)	-0.004 (0.007)
Year 1997 (d)	-0.024 (0.044)	-0.044 (0.043)	-0.038 (0.047)	-0.046 (0.048)
Treatmentgroup 1	0.066* (0.036)			
Treatment Index 1		0.010** (0.004)		
Treatmentgroup 2			-0.048 (0.042)	
Treatment Index 2				0.002 (0.006)
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
R-squared	0.073	0.073	0.048	0.048
χ^2	5.47	5.51	2.18	2.16
N	9416	9416	4681	4681

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

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

Dependent variable: logarithm of long-term benefit days

Standard errors in parentheses are adjusted for clustering on person id

Table 11: Robustness Checks: Direct Effect on Incidence with Varying Treatment Intensity

Variable	Balanced sample	1995-1998	1995/1997	1996/1998	Full-time; age 25 - 55	Singles	No optionally insured
DiD97	0.001 (0.001)	0.001 (0.001)	-0.000 (0.001)	0.001 (0.001)	0.001 (0.001)	0.000 (0.000)	-0.000 (0.001)
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	0.202	0.127	0.129	0.181	0.179	0.252	0.1350
χ^2	197.679	297.044	216.475	293.962	201.647	84.557	145.83
N	3317	9126	4489	2586	4626	1427	4279

(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: Differences-in-Differences Placebo Estimates on Incidence

Variable	Net effect (Index)	Direct effect (Index)
DiD98 (d)	0.001 (0.001)	0.000 (0.001)
DiD97 (d)	-0.001 (0.001)	-0.001 (0.001)
DiD96 (d)	-0.002 (0.001)	-0.001 (0.001)
DiD95 (d)	-0.001 (0.001)	-0.002 (0.001)
DiD94 (d)	-0.000 (0.001)	-0.000 (0.000)
Educational characteristics	yes	yes
Job characteristics	yes	yes
Personal characteristics	yes	yes
State dummies	yes	yes
R-squared	0.131	0.122
χ^2	1106.284	386.200
N	28298	13772

(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 13: 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	4,094,026,130	4,030,306,809	4,555,888,018
Total amount redistributed: All eligible SHI insured	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 14: Descriptive Statistics

Variable	Mean	Std. Dev.	Min.	Max.	N
Long-term absence last year	0.05	0.219	0	1	12895
Personal characteristics					
Female	0.425	0.494	0	1	12895
Age	39.371	11.085	18	65	12895
Age square	1672.95	901.99	324	4225	12895
Immigrant	0.172	0.378	0	1	12895
East Germany	0.274	0.446	0	1	12895
Partner	0.761	0.426	0	1	12895
Married	0.66	0.474	0	1	12895
Children	0.464	0.499	0	1	12895
Disabled	0.049	0.217	0	1	12895
Good health	0.609	0.488	0	1	12895
Bad health	0.096	0.295	0	1	12895
No sports	0.411	0.492	0	1	12895
Educational characteristics					
Drop out	0.045	0.207	0	1	12895
Degree after 8 years' schooling	0.318	0.466	0	1	12895
Degree after 10 years' schooling	0.35	0.477	0	1	12895
Degree after 12 years' schooling	0.039	0.195	0	1	12895
Degree after 13 years' schooling	0.162	0.369	0	1	12895
Other degree	0.086	0.28	0	1	12895
Work in job trained for	0.547	0.498	0	1	12895
New job	0.173	0.379	0	1	12895
No. years in company	9.163	9.196	0	47.8	12895

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... Table 14 continued

Variable	Mean	Std. Dev.	Min.	Max.	N
Job characteristics					
No tenure last year	0.111	0.314	0	1	12895
Small size company	0.251	0.434	0	1	12895
Medium size company	0.288	0.453	0	1	12895
Big company	0.228	0.42	0	1	12895
Huge company	0.222	0.415	0	1	12895
Blue collar worker	0.386	0.487	0	1	12895
White collar worker	0.472	0.499	0	1	12895
High job autonomy	0.207	0.405	0	1	12895
Gross wage per month	2007.384	1155.451	59.821	20451.68	12895
Regional unemployment rate	12.593	3.913	7.9	21.7	12895