Switch-On and Switch-Off Effects of Sick Pay Reform on Absence from Work

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Abstract: We evaluate the switch-on and switch-off effects of a natural experiment that reduced sick pay in Germany from 100 to 80% of the wage rate but that effectively only applied to workers without a collective bargaining agreement. Two years following implementation of the reform, a newly elected federal government repealed it. We therefore estimate the reform's impact on annual days of absence by applying a difference-in-differences strategy to person-level data from the German Socio-Economic Panel. We find a 2-day reduction in the number of days of absence, almost a quarter of the pre-reform mean. The reform also reduced the average number of days spent in hospital by almost half a day, but at the same time did not have an affect on subjective health outcomes.

Quantile regression reveals higher point estimates (both in absolute and relative terms) at higher quantiles, meaning that the reform predominantly reduced long durations of absence. A further analysis of the switch-on and switch-off effects shows higher point estimates for switch-off than switch-on effects, a result confirmed by the fixed-effect regressions. This finding is consistent with experimental evidence on the effect of incentive contracts on voluntary cooperation.

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1 Introduction

Absence from work carries a high cost in terms of workdays lost, with rates ranging from 2.0% in the United States to 4.2 or 7.2% in continental European countries like Germany or France, respectively (Osterkamp, 2002). Theoretically, apart from genuine health reasons, absence is seen either as a form of labor supply adjustment in the presence of hours constraints in work contracts or as a form of shirking (Barmby, Sessions and Treble, 1994; Drago and Wooden, 1992; Johansson and Palme, 1996; Winkelmann, 1999). In a continental European context with high employment protection, absence is hard to sanction, especially in countries like Germany and Sweden, where a worker can remain absent for 2 and 7 days, respectively, without a physician's certificate (Johansson and Palme, 2005; Riphahn and Thalmaier, 2001). In addition, sick pay in Germany is 100% of the wage, so that presence at the workplace is – at least in the short run – a form of voluntary cooperation by the worker.

In this study, to extend the limited but recent literature using natural experiments to estimate the effects of incentives on absence (Henrekson and Persson, 2004; Ichino and Riphan, 2005; Johansson and Palme, 2002, 2005; Riphahn, 2004; Riphahn and Thalmaier, 2001), we estimate the effects of the introduction and then repeal of a late 1996 reform that reduced sick pay in Germany from 100 to 80% of the wage. Most particularly, because a newly elected federal government repealed the reform in 1999, a little over two years after its introduction, we can distinguish between both the switch-on and switch-off effects of the reform. In addition, because this reform affected only workers not covered by collective bargaining contracts, we can apply a difference-in-differences identification strategy to German-Socio Economic Panel data so as to distinguish the effects of the reform from time- or group-specific effects. Fixed effects regressions provide an additional control for unobserved individual heterogeneity.

The relationship between financial incentives and absence is amply shown in earlier papers using regression analysis on observational data (i.e., without natural experiments). For example, Allen's (1981a, 1981b) finding that absence, in line with both the labor supply and shirking interpretation, is negatively related to the wage rate has subsequently been reconfirmed by several studies, including Barmby, Orme and Treble (1995), Barmby and Stephan (2000), Brown (1999), Brown, Fakhfakh and Sessions (1999), Drago and Wooden (1992), Heywood, Jirjahn and Wei (2008) and Winkelmann (1999). Consistent with shirking models, absence has also been shown to be negatively correlated with local unemployment rates (Arai and Thoursie, 2005; Askildsen, Bratberg and Nilsen, 2002; Brown, Fakhfakh, and Sessions, 1999; Henrekson and Persson, 2004; Johansson and Palme, 1996; 2005; Leigh, 1985; Markham and McKee, 1991; and Riphahn and Thalmaier, 2001).

Fewer studies, however, explicitly relate the *cost of absence* to its incidence or duration. Among these, Ichino and Riphahn (2005) and Riphahn and Thalmaier (2001) – using data for Italy and Germany, respectively – exploit probation periods as a natural experiment in which the end of the probationary period leads to extended employment protection. Both studies reveal a significant increase in absence following the end of the probationary period, one that Riphahn (2004) also identifies when German public sector workers reach a virtually "undismissable" status after 15 years of tenure. For Sweden, Henrekson and Persson (2004) use time series data for 1955–1999 to show that reforms that make sick pay more generous increase absence from work and vice versa. Likewise, Johansson and Palme (2002, 2005) use person-level data to evaluate the Swedish sick pay reform of 1991, which resembled that investigated here for Germany but applied only to blue-collar workers. Specifically, this reform reduced the rate of sick pay in Sweden, which had generally been 90% of the wage rate, to 65% during the first 3 days of sickness and 80% up to the 89th day of sickness. Thereafter, sick pay remained at 90%. The authors identify a worker reaction to the incentives created by the reform: both the incidence and the duration of absence decrease when the cost of absence increases.

This present paper investigates the case of the late 1996 German reform that reduced sick pay from 100 to 80% during the first 6 weeks of sickness for workers without collective bargaining contracts (after 6 weeks, sick pay is generally 70% and paid by the health insurance company rather than the employer). However, unlike previous studies using natural experiments to evaluate the incentives linked to sick pay, we can also evaluate the effects of the early 1999 repeal of the reform, which re-set sick pay to 100% of the wage rate from day 1.

The paper is structured as follows. Section 2 describes the sick pay regulations in Germany, their reform in late 1996 under Chancellor Kohl's government, and their subsequent repeal in 1999 under the newly elected government led by Chancellor Schröder. Section 3 outlines how treatment and control groups are defined based on the German Socio-Economic Panel (GSOEP) and provides descriptive statistics for the estimation sample. Section 4 then explains the estimation strategy and presents the empirical results for absence from work. Specifically, we find that for workers aged 20 to 55 years who remained with their firm during the estimation period, the average number of days absent from work fell by 2.1 days per year (according to a fixed-effects estimate). In Section 5 we show that part of this decrease (0.4 days) coincides with a reduction in the average number of days spent in hospital, although we cannot find any effects of the reform on subjective health indicators.

The results also indicate that the switch-on effects of the reform are smaller than the switch-off effects on absence from work (at 2.0 and 2.5 days, respectively); however, this difference is not statistically significant. The difference-in-differences quantile regressions suggest that the reform had significant effects only for longer durations of absence. Altogether, it seems that the reform reduced the – in international comparison – long and frequent contacts of Germans with their health care system. These contacts are costly both for employers and the health care system, but their reduction due to the sick pay reform seemingly had no effects on subjective health indicators. Section 6 concludes the paper.

2 Sick Pay in Germany

Germany has one of the most generous sick pay regulations among industrialized countries. German federal law dictates that employees reporting sick are entitled to 100% of their pay for the first 6 weeks of sickness, to be paid by the employer (Bundesministerium der Justiz, 2003a/b; Schmitt, 2005). Only after this period does the percentage reduce to the 70% covered by mandatory health insurance (Bundesministerium der Justiz, 2007).[1] Moreover, in contrast to regulations in the United States, the United Kingdom or Switzerland, German federal law regulates sick pay for the first few days of illness (Osterkamp, 2002). That is, in Germany, an employee can simply report in sick and need not hand in a medical certificate until the absence exceeds 2 days. This practice is similar to that in Sweden, where a certificate is only required after the 7th day of absence (Johansson and Palme, 2002, 2005).

This generosity of sick pay has been an issue of political debate in Germany for decades. Before 1957, the regulations for blue-collar and white-collar workers differed enormously: unlike white-collar workers who continued to receive full pay from day 1 of their sickness (i.e., 100% of their salary), blue-collar workers received only 50% after 3 unpaid days. Between 1957 and 1961, blue-collar workers received 90% of their salary after 2 unpaid days. Subsequently, until December 1969, both white-collar and blue-collar workers received sick pay of 100% of the wage, but blue-collar workers still had one unpaid day. Not surprisingly, the trade union movement long resented this unequal treatment, and in 1970 the law was changed so that both blue- and white-collar workers received 100% of their wage from day 1 in case of sickness. Unfortunately, no good microdata are available on which to

[1] Some employees are subject to more generous sick pay rules arrived at through collective bargaining agreements. For example, public sector employees already in place before July 1, 1994, receive sick pay of 100% of their wage for more than 6 weeks depending on their tenure (9, 12 15, 18 and 26 weeks for 2, 3, 5, 8 and 10 years of tenure, respectively). For public sector employees hired after this date, the 6-week rule applies (Clemens et al., 2006). However, after the first 6 weeks, public sector employers must pay an additional allowance into the 70% sick pay covered by the mandatory health insurance. Such allowances in addition to health insurance sick pay after the sixth week of sickness also exist in other sectors of the economy and depend on the specific collective bargaining contract.

evaluate this reform (nor the previous reforms), but a study of aggregate time series suggests that it led to an increase in lost workdays for blue-collar workers (van Lith, 1975).

In the last two decades of the twentieth century (according to Federal Ministry of Health data), about 4 or 5% of working days were lost in Germany due to reported sickness, which would rank Germany somewhere in the middle of an international comparison for the years 1998-2000 (Germany, 4.2%; France,7.2%; UK, 3.7%; Switzerland, 3.0%; and the U.S., only 2.0%; Osterkamp, 2002). If labor contributes two-thirds to the GDP, a reduction in working days lost from the German to the U.S. level would raise the GDP by about 2.2 x (2/3) = 1.5%. Admittedly, this number may be somewhat lower if genuinely sick employees going to work are not only less productive but may also decrease the productivity of others through infection, yet 1.5% of the GDP is a large enough number to illustrate the potential importance of policies affecting workers' absence.

As of October 1, 1996, the former Kohl government reformed the federal law regulating sick pay in Germany so that all employees (whether blue- or white-collar) were entitled to only 80% (rather than 100%) of their previous wage from day 1 of sickness through the first 6 weeks of absence (Schmitt, 2005).[2] This law, however, was heavily resisted by the trade unions, which prior to 1970 had fought for years to gain 100% sickness pay for all workers. Hence, the implementation of the new law was followed in 1996 and 1997 by a plenitude of lawsuits (each referring to a particular collective bargaining contract) in which the unions argued that collective bargaining contracts based on the old version of the law were still valid and implied sick pay corresponding to 100% of the wage. According to Bispinck and WSI-Tarifarchiv (1997), these lawsuits were generally won. Hence, as of December 1997, over 15 million employees were covered by collective bargaining contracts that guaranteed them sick pay of 100% of their wage, which implies full coverage of about

[2] Besides reducing the sick pay covered by the employer for the first 6 weeks, the January 1, 1997, changes to the law on mandatory health insurance reduced sick pay from the 7th week onwards (covered by mandatory health insurance) from 80 to 70% (Bundesministerium der Justiz, 2003a/b). This type of sick pay is paid for

55% of all employees (not counting civil servants, who were not affected by the reform). Indeed, according to a 1998 publication by the German Parliament (Deutscher Bundestag), 80% of employees were receiving sick pay corresponding to 100% of their wage, and the remaining 20% were largely those not covered by collective bargaining contracts (p. 17).[3]

This group of workers without collective bargaining coverage comprise our treatment group, which we compare to the control group of workers covered by collective bargaining contracts using a difference-in-differences estimation strategy. Nevertheless, some measurement error can be expected in the treatment status for two major reasons. First, some workers in our control group did in fact receive "treatment" because their collective contract did not provide for sick pay covering 100% of their wage. Second, more workers received treatment immediately after the reform became effective (October 1, 1996) than by the middle of 1997 or later because it took time for lawsuits to establish that the old rules applied for most workers covered by collected bargaining. Both these sources of measurement error lead to an attenuation bias; that is, because estimates of the treatment effect are biased toward zero, the true effects might be larger than those estimates. However, in Section 4, we partly address the second measurement problem by assessing whether estimates using only 1998 as the treatment period are larger than when 1997 (a time of ongoing lawsuits) is included, which generally turns out not to be the case.

Two years after the late 1996 reduction in sick pay, the 16-year old coalition government between the Christian Democrats and the Liberals (led by Chancellor Helmut Kohl) ended after a regular election installed the first federal coalition government between the Social Democrats and the Green Party. As a result, on January 1, 1999, only two months after Gerhard Schröder was elected as the new chancellor, the 1996 sick pay reform was repealed. This introduction and then repeal of the reform within such a short period allows us

up to 78 weeks within 3 years for a single type of sickness (Bundesministerium der Justiz, 2007). It should also be noted that this reform (the reduction from 80 to 70%) had not been reversed at the time of writing.

to estimate the effects of reduced sick pay through both the switch-on and switch-off effects of the policy change.

Nevertheless, any evaluation of a policy reform raises the question of policy endogeneity; that is, whether the policy reform was a reaction to developments in the outcome variable of interest (here, the number of days of absence per employee). For example, if the sick pay reform was a reaction to a *transitory* increase in absence and this transitory shock happened to be over just at the time of the reform, a simple comparison of before and after absence rates might wrongly indicate an effect of the reform. In this case, rather than the reduction in absence being causally driven, the disappearance of the transitory shock at about the time that the reform went into effect would be simple coincidence. However, as long as the transitory component in absence affected both the treated and control groups (workers without and with collective bargaining coverage, respectively) in the same way, we can avoid this problem by using a treatment-control group design in a difference-in-differences setting.

In addition, we estimate the "switch-on" and "switch-off" effects of the reform separately. Whereas the introduction of the reform ("switch on") was politically motivated by the low number of hours worked per worker in Germany (due to a combination of long vacations, a low number of hours worked per week in collective bargaining contracts and significant absence rates), it was not a reaction to a transitory shock, but to gradual realization that the regulations built up over decades had reduced German labor market competitiveness. The repeal by the political left, on the other hand, was a decision motivated by the principle of sick pay covering 100% of wages, for which the trade union movement had lobbied for decades. Hence, it was definitely not a decision influenced by transitory movements in absence rates.

[3] We could not find other statistics on the share of employees who still obtained 100% of their wage as sick pay. We did contact all major trade unions, but most information they provided referred to regulations in specific contracts rather than statistics on the number of employees covered by different sick pay regimes.

Another potential bias may arise because of anticipation effects; for example, if the treatment group reacts to the *announcement* of the policy by increasing absence before the new regulations go into effect. However, because we do not consider any data for the year 1996, in which the reform became effective on October 1, announcement effects would have to have taken place in 1995, at least 9 months before the reform. To check this possibility, we ran an electronic search of a major German newspaper, the *Frankfurter Allgemeine Zeitung*, for articles on sick pay. Whereas a debate on sick pay did occur in summer 1995, mainly driven by the smaller government coalition partner (the Liberals), it only heated up after April 6, 1996, when then Minister of Labor Norbert Blüm (a member of the larger coalition partner, the Christian Democrats) demanded reform of sick pay. The debate continued and Parliament passed the reform on September 13, 1996, only slightly more than 2 weeks before it became effective. Hence, given that our pre-treatment period ends in 1995, we do not believe that the scope for anticipation effects is large.

3 Data and Descriptive Statistics

To the best of our knowledge, the German Socio-Economic Panel (GSOEP), in existence since 1984, is the only person-level dataset providing information on both workers' absence from work and worker coverage by collective bargaining contracts. Whereas information on absence, asked as the number of days the worker was absent from work in the previous year, is collected annually, information on a worker's coverage by a collective bargaining contract is only available for the 1995 survey. However, because average tenure in Germany is longer than in the UK or the U.S. (in 1998, 10.4 years versus 8.2 and 6.6 years, respectively; Auer and Cazes, 2000), one option for the empirical strategy is to use the 1995 information on coverage by a collective bargaining contract and impute this value for each individual in all other waves. Nevertheless, because an employee may alter the treatment status by changing employer, this procedure may blur the partition of the sample into treatment and control groups to produce a third source of potential attenuation bias in our estimates (see Section 2). We therefore restrict the sample to workers who did *not* change employer during the years under consideration (hereafter, "firm stayers").[4] Specifically, this means that when defining treatment and control groups, we include only workers who responded to the 1995 question on collective bargaining coverage and did not change employer during the 1996/1997/1998 period when reduced sick pay was in place (the treatment period). Appendix Table A1 and Table A2 detail our selection of the estimation sample for this study.

For the years prior to the reform, we use GSOEP data for 1994 and 1995 (surveyed in 1995 and 1996, respectively) but exclude 1996 data because they could be partly affected by the October 1 implementation of the reform. In addition, 1996 saw the beginning of the lawsuits clarifying that previous collective bargaining contracts made the reform ineffective for most workers these contracts covered (see Section 2). These exclusions leave 1997 and 1998 as the viable years for examining effects when the reduced sick pay reform was in place (because of the 1997 lawsuits, we also check the sensitivity of our results when only 1998 is considered as the treatment year). Because the reform was repealed on January 1, 1999, GSOEP data referring to the years 1999 and 2000 provide the sample for the post-reform period.

Table 1 displays the sample means by reform period (pre-reform, reform, post-repeal) and by coverage by collective bargaining. The sample consists of workers aged between 20 and 64 years who are not self-employed nor students or apprentices. Although the sample size changes across the years due to panel attrition and panel refreshment samples, it is lowest during the reform years because we exclude workers who changed employer during these years. Nevertheless, not only should the rich set of control variables contained in the

[4] It should be noted that in Germany, in contrast to some other countries, employers agreeing to a collective bargaining contract must apply its terms to *all* workers in the company, not simply to workers that belong to the trade union negotiating the contract. Employers can avoid collective bargaining contracts, however, by

GSOEP account for attrition based on observables, we also present fixed-effect estimates (see Section 4 below) that account for attrition based on unobserved variables as long as their effects are constant over time.

It should be noted, however, that as the outcome variable, we only observe the total number of absence/sickness days in a calendar year, not the number and length of specific sickness spells. Moreover, although the original GSOEP question asks about workdays lost due to illness, the fact that we observe some people reporting sickness durations exceeding the number of working days indicates that the measurement of absence might be a mix of lost workdays and the total number of sick days (including weekends and public holidays).

As illustrated in the upper part of Figure 1, the average number of days absent differs between treatment (not covered by a collective agreement) and control (covered workers) groups, indicating that the former generally report fewer days of absence. This finding holds true before, during and after the reform, except for workers under 40 following repeal (see the lower part of Fig. 1). Nevertheless, the raw means also suggest that the reform did have an effect on absence. That is, whereas the absence gap between treated and control observations prior to the reform was -3.4 days (8.8 days for workers without coverage and 12.2 days for covered workers), this gap widened to -4.7 days during the reform years only to shrink again to -2.0 days after its repeal. The rise and fall of this gap between treated and control observations is even more pronounced when the analytical focus shifts to younger workers. As the lower part of the figure illustrates, younger workers (below 55 or 40 years of age) seem to have reacted more strongly to the reform. For treated workers younger than 55 years of age, the average number of days absent decreased from 8.1 to 6.4 days during the reform period but rose to 9.3 days following repeal. The change in the gap between treated and control observations is even more pronounced, moving from -3.3 pre-treatment to -5.6

leaving the employers' federation. However, if employers had so changed their status, it would be yet another source of attenuation bias.

during treatment and down to -1.4 after treatment (repeal). For workers younger than 40, these averages are -2.5, -4.3 and +0.9, respectively.

Although these numbers represent raw gaps that do not take observed or unobserved heterogeneity into account, they nevertheless suggest that the reform did have an effect on workers, especially those younger than 55 years of age. Older workers, in contrast, are likely to be less credit constrained and may thus be less sensitive to reduced sick pay. Their absence may also be more strongly driven by genuine health concerns and hence less influenced by financial incentives. We therefore conduct an analysis of the treatment effects for all workers (aged 20 to 64) and then examine restricted age groups.

As Table 1 shows, treatment and control groups not only differ systematically in their average number of days absent but also in other characteristics. For instance, the treatment group earns lower hourly wages than the control group (by between 4 and 9%, depending on the period considered).[5] Moreover, although both groups have roughly the same average age, gender, civil status and health indicators, the treatment group is somewhat more educated and somewhat less likely to be blue collar or work part time. However, the most striking differences between the treatment and control groups are in terms of tenure, firm size, industry and civil service status. That is, workers without collective bargaining coverage (the treatment group) have lower tenure; work in smaller firms; are much more likely to work in services like trade, real estate and business activities; and are much less likely to be civil servants.[6] These differences between the two groups persist across the observation period: there are no *major* compositional changes between the covered and uncovered groups across

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[5] Based on the assumption that reduced sick pay might lead to lower (efficiency) wages and thus might have both a direct effect on absence and an indirect effect through the wage rate, we estimated the effects of the sick pay reform on regular wages using standard difference-in-differences models with control variables and fixed-effect estimates. However, contrary to what efficiency wage theory might predict, all estimates of wage effects are insignificant, with most point estimates positive. This observation is consistent with experimental evidence in Dürsch, Oechsller and Vadovic (2008) who find barely any effort response by workers to sick pay. In our study, both these results support the interpretation that changes in the raw wage gap between treatment and control groups can be explained by compositional effects.

[6] Treated observations indicating that the individual is a civil servant most probably represent coding error in the civil service variable in the original GSOEP data.

time. The regression analysis reported below controls for any compositional changes related to observed or time-constant unobserved characteristics.

Table 2 displays the distribution of the outcome variable, the annual number of days a worker was absent from work. In almost all periods, the 4th decile of the absence days distribution is 0 or 1, meaning that almost half the workers are not absent from work for a single day. Moreover, even though the median number of days absent is 2 in the treatment group and 4 or 5 among the controls, the distribution is highly skewed to the left with the 7th decile between 6 and 12 days, the 9th decile between 20 and 30 days and the 99th percentile at 98 or more days. Thus, our estimation strategy must take into account the heavy censoring of the outcome distribution at zero.

4 Effects of the Sick Pay Reform on Absence from Work

We begin by estimating linear difference-in-differences models with the following specification:

$$absence_{it} = \alpha + \beta_1 X_{it} + \beta_2 reform_t + \beta_3 nocoverage_i + \tau(reform_t \times nocoverage_i) + \varepsilon_{it} \tag{1}$$

where *absence* denotes the number of days of absence and *reform* is a dummy variable indicating the time period during which the reduced sick pay reform was in place (1997 and 1998) and valued at zero pre-reform and post-repeal. Likewise, *nocoverage* is a dummy variable indicating that a worker was *not* covered by a collective bargaining contract (the treatment group). This *nocoverage* indicator controls for differences in absence rates between the treatment and control groups, which in the absence of any reform are assumed to be constant across time (the identifying assumption of the difference-in-differences estimator).

Time-specific variations in absence affecting both groups similarly are controlled for by the *reform* dummy as well as further time effects. The difference-in-differences estimator is given by τ, which indicates the change in the absence differential between treatment and control groups after sick pay was reduced from 100 to 80%. Specification (1) includes no control variables. However, specifications (2)-(4) stepwise add control variables to allow for compositional changes in the two groups across time and improve the efficiency of the difference-in-differences estimator as long as they can be regarded as exogenous. The first group of variables, included in specification (2), are the regional unemployment rate, the log hourly wage, age, civil status (married, children), gender and some interactions between them. We employ these standard controls from the absence literature because of their likely impact on the incidence of sickness through their effect on the benefits and costs of shirking through absence. Specification (3) then integrates a further set of controls by including education, citizenship, and job and firm characteristics, as well as a dummy for West Germany (see Table 1 for details). The full specification (4) adds a last set of controls that refer to "health at present" and "satisfaction with health" as asked in the GSOEP. If respondents answer these health-related questions truthfully irrespective of their potential shirking behavior and if the reform had no impact on health (see Section 5), these variables are valid controls; otherwise, they are endogenous.

Table 3 shows the results for the OLS difference-in-differences estimator for firm stayers when both the pre-reform and post-repeal period are simultaneously included as the reference period. Hence, this phase of the analysis does not distinguish between the reform's switch-on and switch-off effects but rather compares the difference between the treatment and control groups during the reform with that before or after its repeal. This approach increases the sample size and hence the precision of the estimates.

To check the sensitivity of the estimates with respect to the control variables just described (the coefficients of the control variables are reported in Table A3 in the Appendix), in Table 3, we report the estimated treatment effects for specification (1) through (4). In this table, we also report the marginal effects at the mean of the difference-in-differences estimates of a count-data model (the negative binomial, NEGBIN; see also Winkelmann, 1999), which is expected to fit the data better because of the dependent variable's count nature. The parameters reported here are the incremental effects of the treatment indicator (the interaction term) at the data mean.[7]

However, because the restriction of the sample to firm stayers may cause systematic attrition not only based on observed characteristics (which we control for in the OLS and NEGBIN estimates) but also based on unobserved characteristics, we control for unobserved heterogeneity by reporting (linear) fixed-effects estimates. More specifically, we identify the policy reform effect using only the "within individual variation", because the fixed-effects estimator effectively assesses the response to the reform by considering only treated and control individuals observed both *during* the reform *and* in a period without reform.

In terms of the estimate's sensitivity to the inclusion of control variables, controlling for compositional changes definitely matters: the comparison between specification (1) and (2) demonstrates that raw effects (with no observables controlled for) are smaller and generally insignificant. The situation changes, however, once state unemployment, log hourly wage, civil status indicators, gender and some interactions are included. This sensitivity of the point estimates to the inclusion of major control variables from the absenteeism literature is not unexpected given that attrition occurs in our data and we must control for compositional effects. Moreover, as illustrated by the results for specification (3) and (4), adding further sets of control variables like education, citizenship, job and firm characteristics and health characteristics produces no major changes in the point estimates compared to specification (2). Therefore, below we report estimates from the full specification (4).

[7] Ai and Norton (2003) derive a correct presentation of the cross derivative and cross difference in nonlinear models with interaction terms. However, this cross difference is not equal to the treatment effect shown in Puhani (2008).

As regards the different modeling strategies, the differences in the point estimates between OLS and NEGBIN are minor, meaning that despite its theoretical deficiencies, the OLS model seemingly provides a very good approximation of the treatment effect at the mean. However, not surprisingly, most standard errors are somewhat smaller in the NEGBIN model, which is tailored to fit the count data. In addition, even though both the OLS and NEGBIN suggest that the decrease in sick pay reduced the number of absence days per year by 2, this effect is only statistically significant in the NEGBIN model. Once we control for unobserved heterogeneity in a (linear) fixed effects model, the point estimate becomes somewhat smaller (and marginally insignificant) with an estimated reduction of 1.6 days in specification (4). Given that the mean number of days absent was 8.8 before the reform, this figure amounts to a reduction in absence of almost a fifth, which is sizable.

As hinted at in the previous section, based on the raw data, we might expect the effect of the reform to be higher among younger workers. Therefore, in Table 4, we provide the difference-in-differences estimates for the different age groups (here and subsequently, for full specification 4 with all control variables). We find that in all models, the point estimates become larger when older workers (aged 56–64) are excluded. More specifically, for the age group 20–55, the fixed-effects estimate shows a significant 2.1 reduction in days absent per year. For the further restricted age group 20–40, at 2.2 days, this reduction is even a little higher, although because of the GSOEP's comparatively small sample size, the changes in point estimates due to sample restriction by age are smaller than the standard deviation of the estimate (which is around 1). Nevertheless, we still find the larger point estimates interesting, most particularly because in the OLS and NEGBIN models, which do not control for unobserved heterogeneity, the point estimates for the younger age groups are even higher – between 2.0 and 2.9 in absolute value (and all statistically significant).

4.1 Effects at different points of the distribution

The skewed nature of the distribution of absence days, with a high probability mass at zero, raises the question of whether the reform's effect works mainly through the incidence of absence or through its duration. As a corollary, does the reform have a larger effect on longer or on shorter durations of absence? To answer these questions, Table 5 reports not only the previous OLS estimates (see column 1) but also OLS estimates for different definitions of the outcome variable: we censor the dependent variable successively at 1, 30 and 60 days. Censoring at day 1 effectively estimates a linear probability model for the incidence of absence, while censoring at day 30 and 60 cuts the length of (very) long spells of absence. In the last column, we restrict the sample to absences greater than zero; that is, we estimate the reform's effect on absence conditional on absence being positive. A clear picture emerges: the reform had virtually no effect on the incidence of absence[8] but apparently affected longer absence durations (censoring reduces the point estimates). However, when only positive absences are considered, the effects become large, suggesting a 3.73 day (insignificant) reduction in absence days for the total sample to between (significant) 4.91 and 5.82 days in age groups 20–55 and 20–40, respectively. These figures, which represent over twice the estimate obtained for the full sample, are not surprising given that almost half the observations report zero absence days.

A more systematic description of the reform's effect at different parts of the distribution can be provided by difference-in-differences quantile regressions (Athey and Imbens, 2006; Song and Manchester, 2007), which imply stronger identifying assumptions than does OLS because we must assume that the differences in the distributions (not simply the differences in the means) between treatment and control groups would have remained constant in the absence of reform. Hence, in Table 6, we show difference-in-differences quantile estimates by decile, again for the three age groups sampled. Theoretically, the OLS

[8] This finding also holds true in a probit specification (not shown).

estimate is the mean of the coefficients at all quantiles; however, as the results show, up to the 4th decile, the effect is (virtually) zero. This finding is not surprising given that around 40% of all workers in the sample report not having been absent for a single day. The point estimates of the reform's effect on days absent then grow ever more negative with increasing deciles. For all workers (i.e., aged 20–64), by the 9th decile, the point estimate is a statistically significant 4.8 days reduction in absence.[9] It should also be noted that the 9th decile of the number of days absent for the treatment group is 23, which corresponds to a sizable reduction in absence of more than 20%. In other words, the quantile regressions reveal that it is mainly absence durations of several weeks (cumulated over the year) that are reduced by the reform.

Once the sample is restricted to workers aged 20–55 or 20–40, we obtain statistical significance from the 7th decile onwards, with 7th decile estimates of -0.8 in both cases. Given that the 7th decile of absence days in the treatment group before the reform was 8 days, this figure constitutes a reduction of almost one tenth. The reduction becomes larger at higher deciles (both absolutely and relatively) for the group aged 20–55. In the 20–40 age group, the point estimates at the very high quantiles (95th and 98th) are the largest of all quantiles but are statistically insignificant because of the large standard errors associated with the sensitivity to outliers of quantile regressions for extreme quantiles of the distribution.

4.2 Attenuation bias

As explained in Section 2, we may have measurement error in the treatment indicator because lawsuits on the control group's sick pay (100% of the wage) continued into 1997 and could affect the estimates through attenuation bias. Therefore, we check for this bias by omitting the data not only for 1996 but also for 1997 (see Table 7). In general, the OLS

[9] The displayed estimates, obtained using the econometric software package Stata, take sampling weights into account. Standard errors reported for the quantile regressions do not allow for clustering; however, we find that block-bootstrapped standard errors that do take clustering into account differ little from the asymptotic standard errors ignoring clustering in an unweighted regression.

estimates do not become larger. In fact, not only are most estimates even smaller, but the fixed-effects estimates are all insignificant, which can be explained by the inflated standard errors (up to 50% or even more) due to loss of observations when the 1997 data are excluded. For example, in Table 7, using the standard error given in Table 4, the fixed-effects estimate for the 20–55 age group would still be significant at the 10% level. In sum, we find no evidence of attenuation bias from the 1997 lawsuits. This finding is consistent with control group members (workers covered by collective bargaining) expecting their unions to win the lawsuits, which was generally the case.

4.3 Switch-on and switch-off effects

Because our dataset includes information on absence before and during the reform and after its repeal, we can estimate the effect of the reduction in sick pay (the switch on) separately from the subsequent repeal and increase in sick pay (the switch off). Doing so has two advantages. First, the difference-in-differences approach used here relies on the identifying assumption that in the counterfactual absence of the reform, the gap in absence days between treatment and control groups would have remained constant. One reason for violating this assumption would be another incident or reform of which the researcher is unaware that might have had a differential impact on absence days for both groups. To dissipate such doubts, research designs that introduce and subsequently take back a reform are very helpful. If both effects have similar values and both indicate that absence is lower with lower sick pay, we can have more confidence that the effects estimated are genuinely caused by the sick pay reform.

In fact, the above-mentioned estimates do not distinguish between the pre-reform and post-repeal periods, which implies that the introduction of the reform has the same impact on absence (in absolute terms) as its repeal. To check this assumption, we estimate the effects of the reform implementation (switch on) and repeal (switch off) separately. Table 8 presents the switch-on and switch-off estimates separately by age group based on data for the years 1994, 1995, 1997 and 1998 for the switch-on effects and for the years 1997, 1998, 1999 and 2000 for the switch-off effects. Table 9 then reports the corresponding quantile regression estimates. The models are specified so that a negative estimate always implies that, as expected, absence is lower during the period of reduced sick pay.

In Table 8, all switch-on and switch-off point estimates are negative in absolute value. However, in the NEGBIN model, all coefficients are statistically significantly different from zero, an outcome that also holds in the fixed-effects models once workers older than 55 years are excluded. Hence, we argue that the reform had a genuine effect on days absent. Interestingly, however, when we compare the absolute size of the switch-on and switch-off effects in Table 8, we find that, without exception, the switch-off point estimates are larger than the switch-on effects (and at virtually all quantiles, as shown in Table 9). However, this difference is only statistically different from zero in a few cases (the OLS model for the 20–55 age group and the OLS and NEGBIN models for the 20–40 age group). For instance, for the 20–55 age group, the difference in the two estimates both in the NEGBIN and in the fixed-effects models is half a day, around a quarter of the corresponding switch-on effects. Because these differences are not statistically significant, one choice would be to ignore them; however, the differences become large for the 20–40 age group (more than one and a half days), which in the NEGBIN model is significant.

One possible basis for interpreting these larger switch-off point estimates is the experimental and psychological literature on extrinsic versus intrinsic motivation. That is, the fact that the switch-off effects are larger than the switch-on effects is congruent with experimental evidence from Gächter, Kessler and Königstein (2007) that incentive contracts negatively impact voluntary cooperation among individuals, and that these negative effects persist even after the incentive contract is repealed. It also relates to an ongoing debate in the psychological literature on whether extrinsic motivation may crowd out intrinsic motivation

(Pinder, 2008, p. 86ff.). That is, because sick pay before the reform was 100%, showing up for work in Germany had an aspect of voluntary cooperation, at least for workers not seeking promotion, and such voluntary cooperation can be linked to intrinsic motivation. Reduced sick pay then added an element of immediate extrinsic motivation that was abolished after the reform was repealed. Hence, in light of Gächter, Kessler and Königstein's (2007) findings, the experience of extrinsic motivation may have crowded out some intrinsic motivation even after repeal at a point estimate of half a day of absence per annum on average for workers aged 20–55 (see the NEGBIN and fixed-effects estimates in Table 8). Nevertheless, the extrinsic motivation of reduced sick pay was much more effective in reducing absence (by between 1.5 and 2 days according to the corresponding estimates in Table 8) than reliance on intrinsic motivation alone, which supports the economists' paradigm that people react to incentives.

4.4 Placebo estimates and estimates by calendar year

Because we have two years of observations for each "regime" (pre-reform, reform, after repeal), we can in theory test the identifying assumption of the difference-in-differences estimator by, for example, testing whether a " placebo treatment effect" estimate for the year 1995 (pre-reform) with 1994 as the base year (also pre-reform) is equal to zero. In Table 10, we therefore define 1994 as the base year and estimate "treatment effects" for all further years used in the previous estimates: 1995 (pre-reform), 1997, 1998 (both reform) and 1999, 2000 (both post-repeal). If the difference-in-differences identifying assumption is correct, only the grey-shaded coefficients (reform period) should be different from zero.

Table 10 shows that standard errors become very large when estimating treatment effects by calendar year (most standard errors are between 1 and 2 days, some are even larger), so that hardly any coefficient is statistically significant. Still, larger negative coefficients are (with few exceptions) observed mainly during the reform period. After the repeal of the reform, some point estimates turn quite large and positive, especially for the year 2000 (two of them even significant), but the standard errors are large as well. We cannot determine whether this finding is due to crowding out of intrinsic motivation as mentioned in the previous subsection or due to a violation of the identifying assumption. In general, we observe a clear decrease in absence with the onset of the reform period and a subsequent increase after the repeal of the reform. This holds both across the defined age group samples and across estimation methods (OLS, NEGBIN, and linear fixed-effects).

5 Effects of the Sick Pay Reform on Other Health-Related Outcomes

Although reduced sick pay decreased absence from work, it remains unclear whether this means that the reform was beneficial from a welfare perspective. In this section, we show that the reform reduced the average number of days spent in hospital, but at the same time did not reduce subjective health outcomes.

According to the OECD, in 1995, just before the sick pay reform, health expenditure in Germany as a percentage of the GDP was 10.1%, higher than in the United Kingdom (6.9%) but lower than in the United States (13.3%). Life expectancy at birth, however, was rather similar in these three countries (between 75.7 and 76.7 years). The number of doctor visits per year was highest in Germany (6.4), followed by the United Kingdom (6.1) and the United States (3.3); by 2003, these gaps had become even larger, at 7.6, 5.2 and 3.9, respectively. The average number of hospital stays per person was 0.18, 0.21 and 0.12 and the average length of stay for acute care was 11.4, 7.1 and 6.5 days for Germany, the United Kingdom and the United States, with Germany having the longest average duration of acute care stays. Hence, contacts with the medical system are seemingly more frequent and longer in Germany. Because these OECD figures (for the whole population) correspond roughly to the sample means in the GSOEP (for a sample of workers aged 20–64), we consider three further outcome variables: the number of doctor visits in the last 3 months (asked in the

GSOEP), number of days in hospital (including zeros) and number of hospital stays (see Table 11 for the sample distributions).

In Table 12, we report difference-in-differences estimates for these three outcomes by age group. Not only are all point estimates negative, but those for number of days in hospital and number of hospital stays are all statistically significant. Moreover, the fixed-effect estimates for these two variables are similar to the OLS results, implying that the OLS findings are not driven by unobserved heterogeneity. Once the NEGBIN model is fitted to the count data outcomes, the point estimates become somewhat smaller in absolute value; however, they still remain economically and statistically significant. For the 20–64 age group, compared to a pre-reform treatment group average of 1.35, the reform reduced the average number of days in hospital by 0.42 days (almost one third, 31%) on average. Given that it also reduced the pre-reform treatment group mean for number of stays (0.11) by an estimated 0.046 (42%) the reduction in number of days hospitalized may be explainable by the actual elimination of some hospital stays. Although these estimates may seem large, they can be made plausible by doctors' incentives given the low occupancy rates of hospital beds: these ranged between only 76 and 82 percent in Germany in the period 1996 through 2006 (data from the German Hospital Society, *Deutsche Krankenhaus Gesellschaft*).

Given the reform's effects on absence from work and hospital stays, we ask whether the estimated reductions had a detrimental effect on health. Hence we use the two subjective health indicators asked in the GSOEP (*Health At Present* and *Satisfaction With Health*) as outcome variables. The reform's effect on these outcomes are presented in Table 13 for the whole population and in Table 14 for the sample of persons who reported at least one doctor visit during the year. The outcomes variables (asked on Likert scales) have been normed to range between 0 and 1. Control variables are the same as in specification (3) of Table 3. We report OLS and fixed-effects estimates, for the whole sampling period and separately for switch-on and switch-off effects.

In the two tables, almost none of the estimates are statistically significant. Notable exceptions are the OLS estimates for the age group 20-64 for *Satisfaction With Health* as the outcome. However, the point estimates are tiny, -0.022 in Table 13 and -0.028 in Table 14, and they become even smaller and insignificant in the fixed-effects estimates. Hence, we conclude that there is no convincing evidence that the sick pay reform reduced health outcomes, despite of the fact that it reduced absence from work and stays in hospital.

6 Conclusions

The economic costs of absence from work can be influenced by economic policy. For example, in contrast to the cases of Switzerland, the United Kingdom or the United States, German federal law (as well as statutes in other continental European countries) dictates that employees receive 100% of their wages as sick pay from day 1 of their absence spell. However, whereas the literature to date does suggest that such absence is influenced by economic incentives like wages, local unemployment, probation periods or sick pay, few studies estimate the effects of sick pay on absence by way of natural experiments. Moreover, to the best of our knowledge, ours is the first to analyze both the switch-on and switch-off effects of sick pay reform; that is, the effects of the reform's implementation and its subsequent repeal by a changed federal government.

The basis of our empirical strategy is a difference-in-differences approach that controls for time and group effects. In some specifications, we also control for unobserved individual heterogeneity by explicitly using the panel nature of the data in a fixed-effects estimation. Overall, we estimate the effect of a reduction in sick pay from 100% of the wage to 80% for the first 6 weeks of absence to decrease absence days by about 2 days per annum on average, which is equal to about one percent of annual working days in Germany (about half the difference between U.S. and German absence rates). As our quantile regressions demonstrate, this reduction is primarily driven by a shortening of very long spells. These results are confirmed by separate estimates for switch-on and switch-off effects. Our finding is significant in that if labor contributes two-thirds of the GDP, then the ceteris paribus effect of the reform amounts to an increase in the GDP of about two thirds of a percent through the reduction of absence from work alone.

Besides reducing absence from work, decreased sick pay also reduces reliance on the health care system, which in Germany has almost zero marginal cost to most individuals (the system is mostly public). We find that the reduction in absence due to the reform (by about 2 days) also reduces the average number of days spent in hospital (by not quite half a day, a reduction of 31%). Data from the German Hospital Society (*Deutsche Krankenhaus Gesellschaft*) report hospital costs as a percent of GDP at a fairly steady 2.4%. This would imply that the sick pay reform had saved 0.74% of GDP through hospital costs alone, and in addition to the two thirds of GDP saved for employers. In sum, the reform might have saved up to 1.4 percent of GDP. Although costs saved might be lower if the reduction in absence and hospital stays referred to less than average productivity days at work and less than average costs per day in hospital, even half a percent of GDP saved would be sizable. The policy relevance of these results is reinforced by the fact that we did not find any remarkable effects of the reform on subjective health indicators.

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Table 1
Sample means by year and treatment status (firm stayers)

	Treated			Control		
	1994/95	1997/98	1999/00	1994/95	1997/98	1999/00
Days absent	8.8	8.0	9.7	12.2	12.7	11.7
Hourly wage	2.47	2.65	2.65	2.56	2.69	2.70
Regional unemployment rate	10.6	12.6	11.3	10.4	12.4	11.3
Civil status indicators						
Age	38.8	42.9	41.6	40.5	43.5	42.7
Married	0.60	0.64	0.57	0.63	0.68	0.65
Female	0.44	0.45	0.48	0.42	0.42	0.41
Children younger than 16	0.38	0.35	0.35	0.37	0.35	0.34
Female × children younger than 16	0.15	0.16	0.16	0.14	0.12	0.11
Female × married	0.25	0.28	0.26	0.25	0.27	0.26
Educational attainment						
High school degree	0.23	0.22	0.29	0.21	0.20	0.23
Academic tertiary degree	0.20	0.21	0.25	0.18	0.19	0.21
Job and firm characteristics						
Temporary work contract	0.04	0.02	0.06	0.06	0.03	0.05
Working fulltime	0.80	0.81	0.79	0.85	0.86	0.85
Blue-collar worker	0.32	0.26	0.28	0.38	0.37	0.35
White-collar worker	0.67	0.72	0.70	0.50	0.51	0.54
Civil servant	0.02	0.01	0.02	0.12	0.12	0.11
Citizenship/region						
German	0.92	0.93	0.94	0.92	0.91	0.91
West Germany	0.77	0.79	0.80	0.82	0.82	0.82
Firm size						
Firm size (1–19)	0.41	0.40	0.34	0.14	0.12	0.14
Firm size (20–199)	0.32	0.35	0.31	0.28	0.29	0.28
Firm size (200–1,999)	0.15	0.14	0.19	0.26	0.27	0.27
Firm size (>2,000)	0.12	0.11	0.15	0.32	0.32	0.30
Tenure						
Tenure (<1 year)	0.06	0.00	0.17	0.03	0.00	0.08
Tenure (1–3 years)	0.31	0.06	0.20	0.16	0.04	0.09
Tenure (3–5 years)	0.18	0.20	0.11	0.14	0.11	0.08
Tenure (5–10 years)	0.19	0.37	0.25	0.21	0.31	0.25
Tenure (10–15 years)	0.07	0.13	0.11	0.12	0.12	0.14
Tenure (15–20 years)	0.06	0.06	0.06	0.11	0.13	0.12
Tenure (>20 years)	0.12	0.18	0.11	0.23	0.29	0.24
Industry						
Agriculture, hunting and forestry	0.02	0.02	0.02	0.01	0.00	0.01
Mining and quarrying	0.00	0.00	0.00	0.01	0.00	0.00
Manufacturing	0.29	0.32	0.28	0.29	0.31	0.30
Electricity, gas and water supply	0.01	0.00	0.01	0.02	0.02	0.02
Construction	0.10	0.08	0.06	0.08	0.06	0.06
Wholesale and retail trade	0.24	0.21	0.23	0.11	0.09	0.10
Transport and communication	0.04	0.03	0.03	0.07	0.06	0.05
Financial intermediation	0.03	0.04	0.05	0.04	0.05	0.05
Real estate and business activities	0.14	0.18	0.19	0.03	0.02	0.04
Public administration and defense	0.02	0.01	0.02	0.14	0.15	0.15
Education	0.01	0.01	0.02	0.07	0.08	0.06
Health and social work	0.06	0.05	0.07	0.11	0.11	0.12
Other social and personal service	0.04	0.04	0.03	0.03	0.04	0.04

Table 1 (continued)
Sample means by year and treatment status (firm stayers)

	Treated			Control		
	1994/95	1997/98	1999/00	1994/95	1997/98	1999/00
Satisfaction with health						
Very poor	0.01	0.01	0.00	0.01	0.01	0.01
Poor	0.06	0.09	0.09	0.07	0.06	0.07
Satisfactory	0.27	0.31	0.26	0.28	0.30	0.30
Good	0.45	0.45	0.48	0.44	0.47	0.45
Very good	0.21	0.14	0.17	0.20	0.16	0.17
Health at present						
Very poor	0.01	0.01	0.01	0.01	0.01	0.01
Poor	0.10	0.13	0.12	0.11	0.10	0.11
Satisfactory	0.32	0.34	0.32	0.31	0.34	0.36
Good	0.49	0.44	0.45	0.47	0.47	0.45
Very good	0.08	0.08	0.10	0.10	0.08	0.07
n	2,227	1,056	1,620	8,024	5,044	5,731

Note: Only persons remaining with their 1995 employer are included in these samples because 1995 is the year for which we observe the treatment status (coverage by collective bargaining).
Source: German Socio-Economic Panel (GSOEP), own calculations.

Table 2
Percentiles of absence days by period and treatment status (firm stayers)

	1994 / 1995 (Pre-reform)		1997 / 1998 (Treatment Period)		1999 / 2000 (Repeal)	
Percentile	Treated (no coll. agreement)	Control (coll. agreement)	Treated (no coll. agreement)	Control (coll. agreement)	Treated (no coll. agreement)	Control (coll. agreement)
30	0	0	0	0	0	0
40	0	1	0	0	0	1
50	2	5	2	4	2	4
60	5	8	4	7	5	6
70	8	12	6	10	8	10
80	14	16	10	15	12	15
90	23	30	20	30	21	28
95	40	49	30	50	36	44
96	42	60	30	60	42	53
97	51	65	40	75	52	64
98	65	90	50	110	80	90
99	105	125	98	165	117	124
100	210	365	365	365	365	365
Mean	8.8	12.2	8.0	12.7	9.7	11.7
n	2,227	8,024	1,056	5,044	1,620	5,731

Source: German Socio-Economic Panel (GSOEP), own calculations.

Table 3
Difference-in-differences estimates (firm stayers aged 20–64)

	OLS	NEGBIN	FE
Specification (1)	-0.54	-0.56	-1.08
(s.e.)	(1.44)	(1.60)	(0.95)
Specification (2)	-1.90	-2.11*	-1.55
(s.e.)	(1.40)	(1.25)	(1.06)
Specification (3)	-1.72	-1.91*	-1.54
(s.e.)	(1.38)	(1.10)	(1.03)
Specification (4)	-2.03	-2.09**	-1.64
(s.e.)	(1.34)	(0.91)	(1.02)
n	23,702	23,702	23,702

Note: *, ** and *** denote significance at the 10%, 5% and 1% level, respectively. The specifications are distinguished by the set of control variables: specification (1) includes no controls; specification (2) adds state unemployment, log hourly wage, civil status indicators, gender and some interaction terms to account for compositional changes: specification (3) adds education, citizenship, job and firm characteristics, and a dummy for West Germany; specification 4 extends the set of control variables by adding reported health status and satisfaction with health.
Source: German Socio-Economic Panel (GSOEP), own calculations.

Table 4
Difference-in-differences estimates for restricted age groups (firm stayers)

	OLS	NEGBIN	FE
Aged 20–64	-2.03	-2.09**	-1.64
n = 23,702	(1.34)	(0.91)	(1.02)
Aged 20–55	-2.94**	-2.35***	-2.07**
n = 21,451	(1.25)	(0.83)	(0.98)
Aged 20–40	-2.61**	-1.97***	-2.15**
n = 12,097	(1.14)	(0.76)	(1.09)

Note: *, ** and *** denote significance at the 10%, 5% and 1% level, respectively.
Source: German Socio-Economic Panel (GSOEP), own calculations.

Table 5
Restricted OLS for different age groups (firm stayers)

	OLS	OLS Cens1	OLS Cens30	OLS Cens60	OLS CondPos
Aged 20–64	-2.03	0.01	-1.05**	-1.51**	-3.73
	(1.34)	(0.03)	(0.46)	(0.68)	(2.30)
n	23,702	23,702	23,702	23,702	13,854
Aged 20–55	-2.94**	0.00	-1.18**	-1.85***	-4.91**
	(1.25)	(0.03)	(0.48)	(0.66)	(2.07)
n	21,451	21,451	21,451	21,451	12,589
Aged 20–40	-2.61**	0.06*	-1.05	-1.71**	-5.82***
	(1.14)	(0.04)	(0.65)	(0.79)	(1.73)
n	12,097	12,097	12,097	12,097	7,418

Note: *, ** and *** denote significance at the 10%, 5% and 1% level, respectively.
Source: German Socio-Economic Panel (GSOEP), own calculations.

Table 6
Difference-in-differences quantile regression estimates

Percentile	Aged 20–64	Raw Decile	Aged 20–55	Raw Decile	Aged 20–40	Raw Decile
40	0.02	0	-0.07	0	-0.04	0
	(0.21)		(0.14)		(0.15)	
50	-0.20	2	-0.39	2	0.01	3
	(0.32)		(0.29)		(0.59)	
60	-0.28	5	-0.28	5	-0.71	5
	(0.45)		(0.53)		(0.72)	
70	-0.79	8	-0.77*	8	-0.76**	8
	(0.56)		(0.45)		(0.36)	
80	-1.33	14	-1.32**	14	-1.18***	14
	(0.99)		(0.58)		(0.36)	
90	-4.83**	23	-4.85***	21	-3.94**	20
	(2.05)		(1.69)		(1.53)	
95	-9.30***	40	-7.66***	35	-4.01	30
	(2.86)		(2.06)		(2.52)	
98	-10.49*	65	-15.84**	60	-7.31	50
	(6.04)		(6.30)		(6.49)	
OLS	-2.03	-	-2.94**	-	-2.61**	-
	(1.34)		(1.25)		(1.14)	
n	23,702		21,451		12,097	

Note: *, ** and *** denote significance at the 10%, 5% and 1% level, respectively. Percentiles of 30 and lower are zero because more than 30% of the sample did not report a single day of absence.
Source: German Socio-Economic Panel (GSOEP), own calculations.

Table 7
Difference-in-differences estimates excluding 1997 (the year of the lawsuits)

	OLS	NEGBIN	FE
Aged 20–64	-2.24	-1.59	-1.43
n = 20,426	(2.01)	(1.44)	(1.43)
Aged 20–55	-3.45**	-2.18*	-1.79
n = 18,553	(1.64)	(1.25)	(1.38)
Aged 20–40	-2.22*	-1.70	-1.58
n = 10,546	(1.28)	(1.08)	(1.55)

Note: *, ** and *** denote significance at the 10%, 5% and 1% level, respectively.
Source: German Socio-Economic Panel (GSOEP), own calculations.

Table 8
Switch-on versus switch-off difference-in-differences estimates

	Aged 20–64		Aged 20–55		Aged 20–40	
	Switch-on	Switch-off	Switch-on	Switch-off	Switch-on	Switch-off
OLS	-1.43	-3.11**	-2.05	-4.62***	-1.42	-5.52***
	(1.44)	(1.54)	(1.30)	(1.53)	(1.11)	(1.85)
Difference (off-on)	1.68		2.57*		4.10**	
(p-value)	(0.22)		(0.06)		(0.02)	
NEGBIN	-2.06**	-2.14**	-2.23**	-2.74***	-1.43*	-3.10***
	(1.04)	(0.90)	(0.94)	(0.78)	(0.86)	(0.83)
Difference (off-on)	0.08		0.51		1.67*	
(p-value)	(0.89)		(0.50)		(0.06)	
FE	-1.55	-1.82	-1.95**	-2.47**	-1.71*	-3.12**
	(1.05)	(1.14)	(0.99)	(1.06)	(0.94)	(1.22)
Difference (off-on)	0.27		0.52		1.41	
(p-value)	(0.86)		(0.72)		(0.36)	
n	16,351	13,451	14,865	11,959	8,552	6,338

Note: *, ** and *** denote significance at the 10%, 5% and 1% level, respectively.
Source: German Socio-Economic Panel (GSOEP), own calculations.

Table 9
Switch-on versus switch-off quantile regression difference-in-differences estimates

	Aged 20–64		Aged 20–55		Aged 20–40	
Percentile	Switch-on	Switch-off	Switch-on	Switch-off	Switch-on	Switch-off
40	0.31	-0.05	-0.00	-0.28	0.27	-0.67***
	(0.17)	(0.22)	(0.21)	(0.22)	(0.23)	(0.01)
50	0.10	-0.65**	-0.20	-0.88***	0.41**	-1.49***
	(0.28)	(0.29)	(0.55)	(0.28)	(0.20)	(0.37)
60	0.25	-0.93**	0.08	-1.10**	-0.32	-2.42***
	(0.29)	(0.39)	(0.56)	(0.51)	(0.61)	(0.21)
70	-0.49	-1.57***	-0.51	-1.68***	-0.18	-2.30***
	(0.45)	(0.38)	(0.45)	(0.46)	(0.37)	(0.49)
80	-0.94	-2.81***	-1.19	-2.19***	0.10	-3.44***
	(0.84)	(0.72)	(0.85)	(0.38)	(0.63)	(0.31)
90	-3.86**	-4.33**	-3.94**	-5.31***	-2.05*	-7.71***
	(1.95)	(1.75)	(1.90)	(1.29)	(1.08)	(1.94)
95	-7.74***	-10.16***	-7.28**	-10.75***	-4.37	-8.72***
	(2.65)	(2.56)	(3.05)	(1.84)	(3.34)	(2.85)
98	-10.68	-8.20	-9.62*	-13.05*	-2.80	-12.63***
	(8.66)	(6.34)	(10.91)	(6.88)	(10.44)	(4.77)
OLS	-1.43	-3.11**	-2.05	-4.62***	-1.42	-5.52***
	(1.44)	(1.54)	(1.30)	(1.53)	(1.11)	(1.85)

Note: *, ** and *** denote significance at the 10%, 5% and 1% level, respectively. Percentiles of 30 and lower are zero because more than 30% of the sample did not report a single day of absence.
Source: German Socio-Economic Panel (GSOEP), own calculations.

Table 10
"Treatment Effects" by Calendar Year (Base Year 1994) – Including Placebo Estimates

	OLS	NEGBIN	FE
Age 20-64			
1995 * no coll. agr.	-1.24	-0.36	1.70
	(1.49)	(1.19)	(1.60)
1997 * no coll. agr.	-1.98	-2.12	-1.73
	(1.81)	(1.31)	(1.13)
1998 * no coll. agr.	-2.50	-1.58	-0.74
	(2.29)	(1.78)	(1.74)
1999 * no coll. agr.	0.90	-0.25	0.73
	(1.68)	(1.49)	(1.4)
2000 * no coll. agr.	0.49	-0.57	1.69
	(2.63)	(2.1)	(1.74)
Age 20-55			
1995 * no coll. agr.	-0.37	-0.30	2.35
	(1.29)	(1.23)	(1.58)
1997 * no coll. agr.	-2.06	-2.31*	-1.31
	(1.61)	(1.28)	(1.08)
1998 * no coll. agr.	-2.86	-2.10	-1.09
	(1.81)	(1.59)	(1.49)
1999 * no coll. agr.	1.52	-0.51	1.22
	(1.48)	(1.54)	(1.37)
2000 * no coll. agr.	2.91	0.45	3.47*
	(2.70)	(2.22)	(1.92)
Age 20-40			
1995 * no coll. agr.	-0.72	-1.12	1.44
	(1.41)	(1.35)	(1.12)
1997 * no coll. agr.	-2.06	-2.48*	-1.00
	(1.54)	(1.42)	(1.11)
1998 * no coll. agr.	-1.63	-1.91	-0.68
	(1.58)	(1.47)	(1.33)
1999 * no coll. agr.	1.23	-0.78	1.75
	(1.57)	(2.1)	(1.65)
2000 * no coll. agr.	6.8	4.23	5.28**
	(4.57)	(3.45)	(2.65)

Note: *, ** and *** denote significance at the 10%, 5% and 1% level, respectively.
Source: German Socio-Economic Panel (GSOEP), own calculations.

Table 11
Percentiles of other health-related outcomes

	Doctor visits (last 3 months)	Days in hospital	Number of hospital stays
30	0	0	0
40	1	0	0
50	1	0	0
60	2	0	0
70	2	0	0
80	3	0	0
90	6	0	0
95	10	7	1
96	10	10	1
97	10	12	1
98	12	15	1
99	17	24	2
100	90	220	20
Mean	2.41	1.16	0.12
n	23,701	23,680	23,612

Source: German Socio-Economic Panel (GSOEP), own calculations.

Table 12
Effects on other health-related outcomes

	OLS	NEGBIN	FE	Pre-reform mean
Aged 20–64 (*n* = 23,702)				
Doctor visits (last 3 months)	-0.26	-0.21	-0.01	2.20
	(0.18)	(0.15)	(0.14)	
Days in hospital	-0.66**	-0.42***	-0.67**	1.35
	(0.30)	(0.11)	(0.28)	
Number of hospital stays	-0.062***	-0.046***	-0.065***	0.111
	(0.023)	(0.013)	(0.024)	
Aged 20–55 (*n* = 21,451)				
Doctor visits (last 3 months)	-0.26	-0.23	-0.02	2.21
	(0.19)	(0.16)	(0.15)	
Days in hospital	-0.67**	-0.38***	-0.66**	1.12
	(0.28)	(0.10)	(0.29)	
Number of hospital stays	-0.069***	-0.047***	-0.062**	0.108
	(0.024)	(0.012)	(0.025)	
Aged 20–40 (*n* = 12,097)				
Doctor visits (last 3 months)	-0.42*	-0.20*	-0.35*	1.91
	(0.22)	(0.11)	(0.19)	
Days in hospital	-0.53**	-0.27*	-0.53**	0.65
	(0.23)	(0.14)	(0.26)	
Number of hospital stays	-0.079***	-0.050***	-0.068**	0.084
	(0.031)	(0.012)	(0.034)	

Note: *, ** and *** denote significance at the 10%, 5% and 1% level, respectively.
Source: German Socio-Economic Panel (GSOEP), own calculations.

Table 13
Effects on subjective health indicators (whole sample)

	OLS	OLS – switch on	OLS – switch off	FE	FE – switch on	FE – switch off
Health at Present						
Age 20-64	-0.008	-0.000	-0.023	-0.007	-0.001	-0.011
	(0.012)	(0.013)	(0.014)	(0.009)	(0.010)	(0.009)
n	23,702	16,351	13,451	23,702	16,351	13,451
Age 20-55	-0.012	-0.005	-0.025	-0.005	0.001	-0.011
	(0.013)	(0.014)	(0.015)	(0.009)	(0.010)	(0.009)
n	21,451	14,865	11,959	21,451	14,865	11,959
Age 20-40	0.002	0.018	-0.035	0.001	0.009	-0.007
	(0.020)	(0.021)	(0.022)	(0.014)	(0.015)	(0.013)
n	12,097	8,552	6,338	12,097	8,552	6,338
Satisfaction with Health						
Age 20-64	-0.022*	-0.020	-0.026**	-0.012	-0.009	-0.012
	(0.011)	(0.012)	(0.012)	(0.007)	(0.007)	(0.007)
n	23,702	16,351	13,451	23,702	16,351	13,451
Age 20-55	-0.020	-0.018	-0.023*	-0.007	-0.004	-0.008
	(0.012)	(0.014)	(0.014)	(0.008)	(0.008)	(0.008)
n	21,451	14,865	11,959	21,451	14,865	11,959
Age 20-40	-0.010	-0.005	-0.024	-0.007	-0.005	-0.008
	(0.018)	(0.020)	(0.018)	(0.011)	(0.012)	(0.011)
n	12,097	8,552	6,338	12,097	8,552	6,338

Note: *, ** and *** denote significance at the 10%, 5% and 1% level, respectively. Both indicators range between 0 and 1 with 1 indicating very food health. *Health at Present* is coded in 5, *Satisfaction with Health* in 10 different values.
Source: German Socio-Economic Panel (GSOEP), own calculations.

Table 14
Effects on subjective health indicators for the sample of people with positive number of doctor visits

	OLS	OLS – switch on	OLS – switch off	FE	FE – switch on	FE – switch off
Health at Present						
Age 20-64	-0.012	-0.008	-0.021	-0.005	0.001	-0.006
	(0.018)	(0.019)	(0.020)	(0.011)	(0.012)	(0.011)
n	15,664	10,790	8,906	15,664	10,790	8,906
Age 20-55	-0.015	-0.012	-0.024	-0.007	0.001	-0.011
	(0.020)	(0.021)	(0.022)	(0.012)	(0.012)	(0.011)
n	13,903	9,631	7,724	13,903	9,631	7,724
Age 20-40	-0.017	0.002	-0.054	-0.008	0.003	-0.020
	(0.032)	(0.032)	(0.033)	(0.018)	(0.020)	(0.016)
n	7,637	5,411	3,977	7,637	5,411	3,977
Satisfaction with Health						
Age 20-64	-0.028*	-0.029*	-0.027	-0.011	-0.007	-0.013
	(0.016)	(0.017)	(0.017)	(0.009)	(0.010)	(0.009)
n	15,664	10,790	8,906	15,664	10,790	8,906
Age 20-55	-0.025	-0.027	-0.023	-0.004	0.001	-0.008
	(0.018)	(0.020)	(0.019)	(0.010)	(0.010)	(0.010)
n	13,903	9,631	7,724	13,903	9,631	7,724
Age 20-40	-0.015	-0.012	-0.021	-0.002	0.005	-0.011
	(0.028)	(0.031)	(0.026)	(0.015)	(0.016)	(0.014)
n	7,637	5,411	3,977	7,637	5,411	3,977

Note: *, ** and *** denote significance at the 10%, 5% and 1% level, respectively. Both indicators range between 0 and 1 with 1 indicating very food health. *Health at Present* is coded in 5, *Satisfaction with Health* in 10 different values.
Source: German Socio-Economic Panel (GSOEP), own calculations.

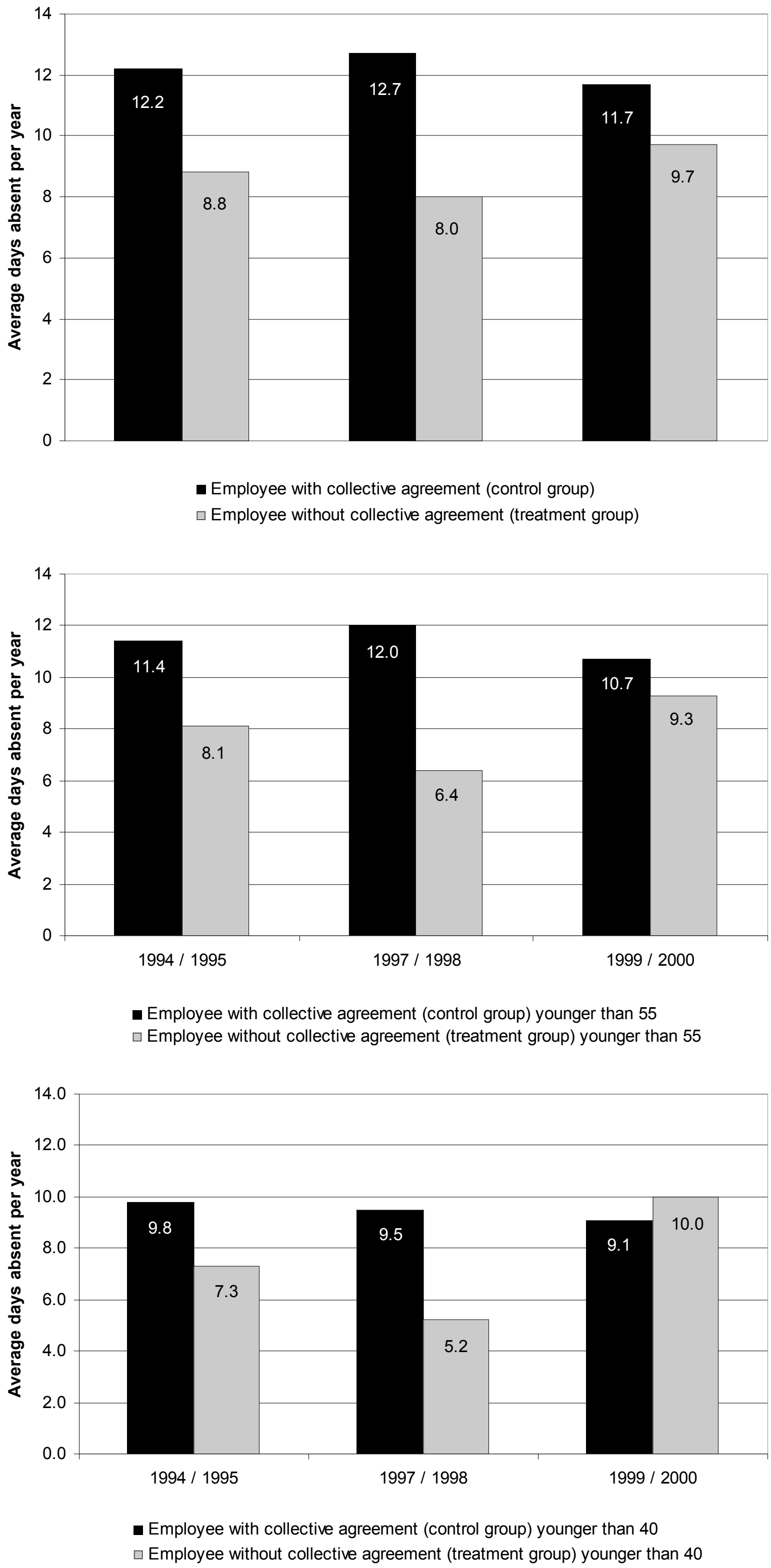

Fig. 1. Number of days absent over time by treatment status and age group

Note: We only observe the total number of days absent by calendar year, not the length of single spells of absence. The sample includes only firm stayers.

Source: German Socio-Economic Panel (GSOEP), own calculations.

Appendix

Table A1
Sample selection

Year	Sample size (including all years)	Individual is in the sample this year	Individual is also in the sample the following year	Including only employed persons between 20 and 64 years of age	No missings for questions on absence	No missings for questions on income and other explanatory variables
1994	56,150	13,417	12,520	6,288	6,040	5,134
1995	56,150	13,768	12,851	6,526	6,278	5,576
1997	56,150	13,283	12,180	5,931	5,658	4,964
1998	56,150	14,670	13,373	6,394	6,160	5,428
1999	56,150	14,085	13,035	6,443	6,196	5,263
2000	56,150	24,586	21,233	10,083	9,690	8,527
n	336,900	93,809	85,192	41,665	40,022	34,892

Source: German Socio-Economic Panel (GSOEP), own calculations.

Table A2
Selection of treatment and control groups

Year	Treated	Control	Movers	Rest	*n*
1994	1,021	3,702	0	411	5,134
1995	1,206	4,322	0	48	5,576
1997	585	2,691	962	726	4,964
1998	471	2,353	1,104	1,500	5,428
1999	845	2,956	0	1,462	5,263
2000	775	2,775	0	4,977	8,527
n	4,903	18,799	2,066	9,124	34,892

Note: To be part of either the treatment or control group in this study, an individual must have answered the question on collective bargaining in 1995. Hence, the number of observations is highest for both treated and control individuals in 1995. Panel attrition then works both backward and forward in time. So that observations can be classified into treatment and control, a worker must not have changed employer between 1996 and 1998 (i.e., until the end of the treatment period). Workers that have changed (termed “movers”) are deleted from the sample. If, however, an individual answered the question on collective bargaining coverage in 1995 but changed employer before 1995 or in or after 1999, we retain that employee in the sample. The last column, labeled “rest,” includes workers who did not answer the question on collective bargaining in 1995, meaning that they cannot be classified as either treated or control and are therefore deleted from the sample. The allocation to the treatment or control group here is based on the 1995 information on collective bargaining coverage. It should also be noted that misclassification outside the treatment period is harmless because neither the treatment nor the control group was treated either before or after the repeal of the reform. Thus, keeping all persons who answered the 1995 question on collective bargaining coverage may improve precision in the repeated cross-section difference-in-differences estimates. In the fixed-effects estimates, the coefficient on treatment is driven only by observations present at least once in the treatment period and at least once in a non-treatment period.

Table A3
Estimation results for firm stayers

	OLS	NEGBIN	FE
No collective agreement	-0.97	-0.94	-
	(0.79)	(0.65)	-
Year 1995	0.57	0.79*	0.11
	(0.58)	(0.47)	(0.50)
Year 1997	-0.02	0.31	0.19
	(1.18)	(0.67)	(0.85)
Year 1998	1.74	1.25*	1.10
	(1.27)	(0.70)	(0.85)
Year 1999	-0.07	0.15	-0.45
	(0.81)	(0.59)	(0.66)
Year 2000	0.98	0.40	-
	(0.95)	(0.59)	-
No coll. agreem. × Year of reform	-2.03	-2.09**	-1.64
	(1.34)	(0.91)	(1.02)
Hourly wage	-1.45	-0.06*	-1.42
	(0.94)	(0.10)	(1.4)
Unemployment rate	0.05	-1.34	-0.02
	(0.18)	(0.80)	(0.24)
Civil status indicators			
Age	-0.14	-0.27	-2.39***
	(0.39)	(0.22)	(0.68)
Age squared	0.00	0.00	0.04***
	(0.00)	(0.00)	(0.01)
Married	0.30	-0.06	-1.32
	(1.44)	(0.80)	(1.58)
Female	2.11	-3.49	-
	(9.89)	(6.33)	-
Children younger than 16	-2.16**	-1.10*	0.86
	(0.87)	(0.63)	(1.31)
Female × children younger than 16	3.45***	1.72*	0.58
	(1.31)	(1.15)	(1.70)
Female × married	-2.25	-1.69	-1.49
	(1.81)	(0.98)	(2.30)
Female × age	-0.10	0.33	1.70**
	(0.55)	(0.35)	(0.87)
Female × age squared	0.00	0.00	-0.02**
	(0.01)	(0.00)	(0.01)
Education			
High school degree	-1.64**	-1.01*	-0.57
	(0.70)	(0.55)	(1.24)
University degree	-1.37*	-2.04***	4.57**
	(0.73)	(0.56)	(2.21)
Job and firm characteristics			
Temporary work contract	0.50	-0.07	-0.51
	(1.75)	(1.09)	(2.04)
Working full time	3.94***	3.24***	1.94
	(0.81)	(0.52)	(1.43)
Blue-collar worker	4.05***	4.24***	2.94*
	(0.83)	(0.64)	(1.68)
Civil servant	3.17	3.24**	-0.73
	(2.14)	(1.36)	(2.37)

Table A3 (continued)

	OLS	NEGBIN	FE
Citizenship			
German	-2.11*	-1.45*	1.85
	(1.14)	(0.79)	(2.89)
West-Germany	0.66	-0.95	-2.65
	(1.41)	(0.93)	(2.45)
Firm size (ref. 1–19)			
Firm size (20–199)	2.41***	1.95***	0.90
	(0.77)	(0.68)	(1.28)
Firm size (200–1,999)	2.16**	2.23***	-0.69
	(0.86)	(0.73)	(1.40)
Firm size (>2,000)	3.50***	3.32***	-0.12
	(0.88)	(0.79)	(1.71)
Tenure (ref. <1 year)			
Tenure (1–3 years)	-0.09	0.02	3.36**
	(1.64)	(1.45)	(1.42)
Tenure (3–5 years)	1.26	1.31	4.91***
	(1.65)	(1.61)	(1.39)
Tenure (5–10 years)	0.04	0.66	3.98***
	(1.57)	(1.53)	(1.36)
Tenure (10–15 years)	-0.05	-0.36	3.70**
	(1.65)	(1.44)	(1.45)
Tenure (15–20 years)	-0.35	0.00	4.15***
	(1.69)	(1.53)	(1.59)
Tenure (>20 years)	0.25	-0.05	5.68***
	(1.73)	(1.52)	(1.82)
Industry (ref. manufacturing)			
Agriculture, hunting and forestry	-2.92*	-0.76	-2.54
	(1.52)	(1.56)	(2.20)
Mining and quarrying	5.58	7.80	21.05
	(7.77)	(8.64)	(20.18)
Electricity, gas and water supply	-1.82	-0.70	-2.89
	(1.21)	(1.07)	(2.14)
Construction	0.99	1.05	0.95
	(1.10)	(0.83)	(2.04)
Wholesale and retail trade	0.26	0.51	-0.14
	(0.88)	(0.81)	(1.83)
Transport and communication	4.74**	3.44**	1.96
	(2.18)	(1.34)	(3.95)
Financial intermediation	-0.39	-0.75	-1.33
	(0.89)	(0.78)	(1.88)
Real estate and business activities	0.70	1.22	0.94
	(1.05)	(0.96)	(1.57)
Public administration and defence	0.69	1.33	-1.15
	(1.39)	(0.86)	(1.91)
Education	0.21	0.93	2.09
	(1.49)	(1.10)	(2.61)
Health and social work	2.93**	2.59***	0.39
	(1.20)	(0.95)	(1.77)
Other social and personal service	2.50*	2.88*	1.89
	(1.30)	(1.59)	(1.58)

Table A3 (continued)

	OLS	NEGBIN	FE
Health at present (ref. satisfactory)			
Very poor	38.61***	21.60***	28.18***
	(6.40)	(4.04)	(5.29)
Poor	11.62***	6.70***	8.29***
	(1.53)	(1.02)	(1.61)
Good	-2.46***	-2.63***	-1.02*
	(0.57)	(0.43)	(0.56)
Very good	-5.05***	-4.67***	-2.20***
	(0.86)	(0.56)	(0.77)
Satisfaction with health (ref. satisfactory)			
Very poor	7.60	4.84**	1.27
	(6.43)	(2.33)	(6.28)
Poor	4.37**	2.31***	4.24***
	(1.78)	(0.85)	(1.55)
Good	-1.93***	-2.12***	-1.73**
	(0.56)	(0.41)	(0.69)
Very good	-1.17	-1.56**	-1.13
	(0.84)	(0.61)	(0.79)
n	23,702	23,702	23,702
R^2	0.11		0.04

Note: *, ** and *** denote significance at the 10%, 5% and 1% level, respectively.
Source: German Socio-Economic Panel (GSOEP), own calculations.