Dismissal Protection and Long-Term Sickness Absence – Evidence from a Policy Change*

Nicole Gürtzgen[1,2,3] and Karolin Hiesinger[1]

[1] Institute for Employment Research (IAB), Regensburger Str. 104, 90478 Nuremberg, Germany†
[2] University of Regensburg, Department of Economics, Universitätsstr. 31, 93040 Regensburg, Germany
[3] ZEW, Postfach 103443, 68034 Mannheim, Germany

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Abstract

This paper studies whether a decline in employment protection reduces workers' long-term sickness absence (> six weeks). We exploit exogenous variation from a German policy change, which shifted the threshold exempting small establishments from dismissal protection from five to ten workers. Using German register data, we find that the reform significantly reduced transitions into long-term sickness during the second year after entry into the establishment. This response is due to a behavioural, rather than a compositional effect and is particularly pronounced among medium-skilled males. Further results indicate that the reform did not alter the probability of involuntary unemployment after sickness.

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†Address of correspondence: Nicole Gürtzgen, Institute for Employment Research, Regensburger Str. 104, 90478 Nuremberg, Germany, E-mail: nicole.guertzgen@iab.de.

1 Introduction

Long-term sickness represents a considerable burden for both affected employers and employees: For employers, a worker's long-term sickness absence can lead to productivity losses, lower competitiveness and a higher burden on healthy employees (Pauly et al., 2008; Nicholson et al., 2005). For individuals, long-term sickness - in addition to the burden of the sickness itself - may be accompanied by a loss of income, depreciation of human capital and higher risk of involuntary unemployment (Chadi and Goerke, 2018).

Many OECD countries run social policies that aim at reducing these risks for individuals, by providing income replacement taking the form of sick pay and job security via dismissal protection. Such policies may be beneficial in terms of their impact on health, as they allow individuals to recover from a severe disease, by preventing them from returning to work too early. At the same time, the generosity of these policies itself may affect workers' sickness behaviour, such as absenteeism (staying away from work without being sick) or presenteeism (attending work while being sick). While moral hazard may play a role for those who are subject to strong institutional protection (Ichino and Riphahn, 2005; Scoppa, 2010; Ziebarth, 2013), those who are only weakly protected may even seek to avoid or shorten long absences (Reichert et al., 2013). Some studies have already focussed on long-term sickness absence in the context of sick pay (e.g., Ziebarth, 2013), but the effect of dismissal protection on long-term sickness absence is still underexplored.

The present paper attempts to fill this gap and analyses the effects of dismissal protection on the incidence of long-term sickness absence along with its employment consequences in Germany.[1] Germany is a particularly interesting case for several reasons: First, in Germany sickness absence is important from a quantitative point of view, as in 2018 about 42 percent of all absence days were due to long-term sickness of more than six weeks (Meyer et al., 2019). Second, Germany is characterised by quite generous sick pay regulations and, at the same time, by fairly strict employment protection. Almost all employees are subject to the general protection against dismissal laid down in the Protection Against Dismissal Act (PADA). However, German legislation exempts small establishments below a certain threshold size of employees from dismissal protection. In the course of a major labour market reform in 2004, the threshold for establishments being exempted from dismissal protection was raised from five to ten full-time equivalent employees. Using this policy change as a natural experiment, we estimate the causal effect of dismissal protection on long-term sickness periods and its employment consequences at the individual level. To do so, we apply a differences-in-differences approach to quantify the effect of the exemption. We conduct these analyses by exploiting a unique administrative data set that combines

[1] There is no official definition of long-term sickness. This study focuses on spells of more than six weeks according to the definition of the health insurances: The latter use the eligibility for sick pay as a threshold to distinguish between short- and long-term illnesses (see for example Knieps and Pfaff, 2015; Meyer et al., 2019).

data from the German Pension Register and the Federal Employment Agency. The data set allows us to retrieve information on both employment spells and long-term illness periods of German employees who have at least one entry in their social security records. In addition, we can merge administrative establishment information to this data set that enables us to perform a quite precise calculation of establishment size. To better understand the underlying behavioural mechanisms (such as absenteeism or presenteeism), we further rationalize our findings using complementary individual survey data.

Thus far, very few studies have addressed the impact of dismissal protection on sickness absence in a quasi-experimental setting. The only studies we are aware of are analyses using policy changes in Sweden and Italy. The studies by Olsson (2009) and Lindbeck et al. (2006) exploit a policy reform in Sweden in 2001 that enabled small firms to exempt two workers from a seniority rule in case of redundancies. While Lindbeck et al. (2006) focus on the reform's effect on long-term illness spells, Olsson (2009) takes all types of illness spells into consideration. Both studies provide evidence for a significant reduction in sickness absence in firms that were affected by the policy change. Scoppa (2010) analyses the 1990 policy reform in Italy that raised employment protection for workers in small firms – albeit not to the same level of protection as in larger firms. After the reform, small firms could choose between the re-employment of affected workers or the payment of a financial compensation, if a dismissal was judged unfair. Overall, the results of this study point to a significant increase in sickness absence in affected firms.

We contribute to the existing literature in three ways: *First*, our analysis exploits a reform that involved a more encompassing change in dismissal costs for small establishments (employing more than five and up to ten employees). Other than in Sweden, the German reform, by relaxing employment protection regulations for small establishments, not only affected dismissals due to redundancies, but also dismissals that may arise from any other reasons. Most importantly, the policy change also covers dismissals due to personal incapability, a reason that is especially relevant in the context of absence behaviour. Moreover, compared to the Italian case, small establishments in the affected size class did not enjoy any exemptions from the PADA prior to the reform. As a result, the German reform implied a more pronounced change in dismissal costs than the Italian reform.

Second, we focus on the effects of dismissal protection on long-term sickness absence along with its employment consequences. Due to the strict employment protection laid down in the German PADA, dismissals of long-term sick workers are substantially less costly for employers who are not subject to the PADA. As a result, one may expect the risk of subsequent unemployment to rise with a less strict dismissal protection. Thus far, there is barely any research on how a change in dismissal protection alters the risk of subsequent unemployment after a long-term sickness episode. Given that long-term sickness entails high risks for individuals, employers and society, this research gap is notable.

Third, we estimate the effects of dismissal protection at the individual level. Most of the previous studies consider aggregate absence and job flow rates at the establishment level (e.g., Boeri and Jimeno, 2005; Lindbeck et al., 2006; Bauer et al., 2007; Olsson, 2009; Bauernschuster, 2013). In our analysis, we explicitly identify the group of individuals who were affected by the reform. A grandfathering clause implied that the policy change was confined to workers who were hired by the affected establishments after the reform. By tracking the illness histories of individuals who were affected by the policy change, we address the question whether a change in employment protection impacts on particular groups of workers, for example those who are believed to have the lowest productivity. Finally, by exploiting precise information on individuals' long-term illness histories, we explicitly account for the selection of workers with different illness histories into establishments that were subject to the reform. Doing so is especially important in our context, as the restriction of the policy change to newly hired workers might lead to a change in sickness absence that merely arises from a different selection of workers into establishments.

Previewing our results, we find that the reform significantly reduced transitions into long-term sickness during the second year after entry into the establishment. This response is due to a behavioural, rather than a compositional effect and is particularly pronounced among medium-skilled males. Our results provide no evidence of a reform effect on the duration of long-term sickness absence, though. We also find that the reform did not alter the probability of involuntary unemployment after sickness. This is in line with previous work, which fails to detect any major effects of dismissal protection on separations at the establishment level. Overall, our findings indicate that it is less the establishments than the employees themselves who respond to changes in dismissal protection. As to the behavioural mechanisms, our complementary survey-level analyses can neither rule out a decline in absenteeism nor an increase in presenteeism as an explanation.

The remainder of the paper is structured as follows: In section 2, we give an overview of the theoretical and empirical literature regarding long-term sickness absence. Section 3 illustrates the German institutional setting before section 4 presents the data set and the empirical strategy. Section 5 and 6 provide the empirical results and section 7 concludes.

2 Related Literature

It is well established that individuals may have some discretion over their sickness behaviour in the form of absenteeism[2] or presenteeism. Empirical studies provide extensive

[2]Note that there is no uniform definition of absenteeism. In a broader sense, absenteeism is defined as not showing up at work for whatever reason (Hirsch et al., 2017). "True" sickness-related absence times are included here. In a narrower sense, absenteeism is defined as absence from work for reasons *other* than sickness, often referred to as "shirking" (Brown and Sessions, 2004). In this study, we refer to the latter definition.

evidence for both types of behaviour.[3] To the extent that individuals may vary their sickness behaviour, they are likely to trade off their utility of absence against the financial and employment-related risks. In certain situations, the benefits of absence may be high. This may be the case when recovering from an illness is necessary or, in case of moral hazard, if the disutility from work is large, e.g., due to unfavourable working conditions (Barmby et al., 1994; Brown and Sessions, 1996; Hirsch et al., 2017). However, the costs of absence may also be large, if the (duration of the) absence period raises the probability of dismissal or is accompanied by a loss of income.

The institutional context, in particular sick pay and dismissal protection regulations, may play a crucial role for an employee's absence decision. The expected costs of absence rise (i) with a lower income replacement level during a sickness episode (Brown and Sessions, 2004; Puhani and Sonderhof, 2010; Ziebarth and Karlsson, 2010, 2014; Pichler and Ziebarth, 2017; Chen et al., 2020) and (ii) with a decreasing strictness of employment protection (Brown and Sessions, 2004; Ichino and Riphahn, 2005; Lindbeck et al., 2006; Olsson, 2009; Scoppa, 2010). Thus, due to higher anticipated costs of absence, individuals without or with only weak institutional protection may exhibit less frequent and shorter absence periods compared to individuals who are strongly protected by social policy institutions. As spelled out earlier, Lindbeck et al. (2006) and Olsson (2009) support this hypothesis, by providing evidence for a significant negative impact of weaker dismissal protection regulations on sickness absence rates. Scoppa (2010) shows that stricter dismissal protection affects sickness absence positively.[4]

In addition to its impact on sickness absence, employment protection legislation may affect the incidence of unemployment after a long-term sickness spell. Employees with long sickness-related employment interruptions may signal a lower productivity, and, in case of absenteeism, a lower motivation compared to workers who are continuously present at work. Employers may therefore have the incentive to dismiss those employees whom they consider to have the lowest productivity. In line with this, a number of studies have documented a positive relationship between sickness absence and subsequent unemployment spells (Hesselius, 2007; Markussen, 2012; Scoppa and Vuri, 2014; Chadi and Goerke, 2018).

[3]For evidence of absenteeism see e.g., Riphahn and Thalmaier (2001); Chatterji and Tilley (2002); Frick and Malo (2008) and of presenteeism see e.g., Reichert et al. (2013); Arnold and de Pinto (2015); Arnold (2016); Hirsch et al. (2017).

[4]In addition to this strand of literature, there are also studies that look at the role of other institutions and perceived job security for both types of sickness behaviour. For example, Ichino and Riphahn (2005) explore the relationship between probation periods and sickness absence, using data from an Italian bank. The authors show that absence times increase once the probation period, after which employees become subject to dismissal protection, was completed. On the other hand, Hansen and Andersen (2008) show that a higher extent of perceived job insecurity is associated with higher levels of presence despite sickness.

3 The German Institutional Background

3.1 Sick Pay Regulation

In Germany, if an employee falls sick, he or she needs to hand in a medical certificate no later than the fourth day of absence.[5] During the first six weeks of an illness episode employees are entitled to short-term sickness pay, to be paid by the employer.[6] The maximum mandatory duration of sick pay may also derive from accumulating several shorter illness spells within the last twelve months, as long as these are caused by the same disease diagnosis. During this mandatory period of up to six weeks the employer is obliged to provide short-term sick pay, which amounts to a replacement ratio of 100 per cent of individuals' earnings.

After six weeks of illness with the same disease diagnosis, employees are entitled to long-term sick pay provided by the statutory health insurance. The latter covers the majority (about 90 per cent) of the German population and is mandatory for all employees subject to social security contributions whose earnings fall short of the contribution limit of the statutory health insurance.[7] The replacement level for persons receiving long-term sick pay by the statutory health insurance is stipulated in the German Social Code. Since the last reform in 1997, long-term sickness pay has amounted to a replacement ratio of 70 per cent of gross earnings up to the (health insurance) social security contribution limit.

In general, long-term sick pay regulations in Germany pursue the overall aim to sustain the long-term employability of individuals who are still in the labour force. Thus, unlike disability insurance schemes, long-term sick pay offers no possibility to permanently withdraw from the labour market. The non-permanent character of sick pay not only reflects itself in a limited entitlement duration[8], but in two additional salient features of sick pay regulations. First, individuals receiving long-term sick pay may be monitored by the health insurance's auditing system. The medical service run by the statutory health insurance is entitled to audit individuals' sickness absence, if the statutory health insurance expresses profound suspicions about any potential abuse of the sick pay system. Such audits may be performed either based on an assessment of the documentation provided by the medical doctor who ascertained the individual's inability to work, or based on

[5] This statutory time limit is stipulated in the German Continued Remuneration Act (*Entgeltfortzahlungsgesetz*). Note that the time limit for notification defines a maximum period as the law permits employers to require a medical certificate already starting from the first day of illness.

[6] An exception concerns illness during the first four weeks after entering a new employer. During this period employers are not obliged to provide sick pay, such that employees receive sick pay from their health insurance.

[7] Civil servants and self-employed are in general exempted from social security contributions. Civil servants and the self-employed individuals as well as employees subject to social security contributions whose earnings exceed that threshold may choose between the statutory health insurance or a private health insurance. Under the latter, employees stipulate the level of their long-term sick pay individually.

[8] The maximum duration of long-term sick pay for the same disease is 78 weeks within a period of three years.

a personal assessment of the individual's ability to work by the service's medical staff (see Gürtzgen and Hank, 2018). Second, individuals who experienced a long-term illness episode are generally entitled to conclude a reintegration agreement with their employer with the general objective of a (possibly stepwise) reintegration into their former job.

3.2 Dismissal Protection Regulation

Compared to other western countries, dismissal protection regulations in Germany are quite strict (OECD, 2004). General protection against unfair dismissals (*allgemeiner Kündigungsschutz*) is provided by the PADA. The PADA applies to all workers with a tenure of more than six months, who are employed by an establishment with a certain minimum number of employees (currently ten full-time equivalent employees). Establishments operating below the stipulated threshold size may dismiss any worker as long as the less restrictive requirements of the German Civil Code (*Bürgerliches Gesetzbuch*) are met.

According to the more stringent employment protection provisions of the PADA, dismissals are justified in three cases only: first, in case of personal misconduct, second, as a result of the operational requirements of the employer, and, third, in case of personal incapability. While the judgement of individuals' (in)capability is often based on their absence times such as long-term illness episodes (e.g., Nott, 2016), just dismissals on the grounds of illness require some conditions, such as a negative long-term health prognosis, to be met.[9] For employers, such a justification is associated with costs.

Moreover, establishments are typically required to inform the works council about a dismissal, if such a worker representation exists. Consultation with the works council is mandatory for both individual and collective redundancies. The latter generally require the negotiation of a "social plan" with the works council. Such a plan may, for example, stipulate severance payments and the selection of employees who are laid off. Severance payments may also result from settlements after individual dismissals out of or at the Labour Court - either because employers are not able to prove that the requirements for a legal dismissal are met or because they want to prevent workers from suing them at Court. Overall, these considerations highlight that any dismissal subject to the PADA – either due to insecurity about which dismissals are considered just or due to sanctions or severance payments – is likely to be much more costly than a comparable dismissal outside the scope of the PADA.

Key to our analysis is that the PADA only applies to establishments exceeding a stipulated establishment size. Over the last decades, the threshold for applicability has changed several times, from five to ten full-time equivalent employees (FTEs) in October

[9] Note that this is different from regulations in other countries, such as Norway, where individuals enjoy a special dismissal protection while being long-term sick (Fevang et al., 2014).

1996, back to five FTEs in January 1999 and then in the course of the Hartz reforms back again to ten FTEs in January 2004.[10] For the latter reform, it is important to note that those workers who were already employed in affected establishments (normally) did not lose their protection.[11]

With regard to anticipation effects, the former Chancellor Gerhard Schröder announced a general reform of employment protection in a government declaration in March 2003. However, the change of the threshold from five to ten FTEs was not part of this declaration. The final dismissal protection reform along with the stipulation of the threshold and the details of its calculation was not approved until December 23, 2003, just shortly before the reform came into effect (on January 1st, 2004). This suggests that neither the affected employees nor the affected establishments could anticipate the exact details of the reform and change their behaviour accordingly.

4 Empirical Strategy, Data and Variables

4.1 Empirical Strategy

To estimate the causal effect of dismissal protection on our outcome variables, we exploit the reform of dismissal protection in 2004 as a natural experiment. As pointed out in section 3.2, this reform raised the threshold below which establishments are exempted from dismissal protection from five to ten full-time equivalent workers. Due to transitory regulations that (normally) guaranteed dismissal protection to those who were already employed in an establishment before 2004, the reform affected only employees *entering* an establishment with more than five to ten FTE workers. We define this group of workers as our treatment group and compare their outcomes of interest to those of a control group comprised of individuals entering an establishment slightly above the threshold, that is with more than ten to 20 FTE workers. An "establishment entry" is defined as the first employment spell subject to social insurance contributions in an establishment of the relevant size class within the time period 1 January 2001 and 30 June 2003 or within 1 January 2004 and 30 June 2006, respectively.[12] As we observe the treatment and control group before and after the reform, we are able to apply a differences-in-differences approach, by comparing the differences in our outcomes of interest across both groups before and after the reform. The identifying assumption of this approach requires that time trends be the same for both treatment and control group in the absence of the treatment (Blundell and Costa Dias, 2009). Further, the SUTVA assumption states that

[10] Table A.5 in the Appendix describes how the establishment size is calculated on the basis of the PADA.

[11] Under some circumstances, even individuals employed in affected establishments before 2004 may lose their dismissal protection. This may occur when the number of incumbent employees (workers already employed before 2004) falls below the threshold that determined applicability of the PADA until 2004 (five FTEs).

[12] For more details on the definition of "establishment entry" see Table A.6 in the Appendix.

the treatment of one individual must not influence other individuals' potential outcomes (and vice versa) (Rubin, 1980).

Moreover, the definition of the groups implies that the group composition may change over time as it is rather unlikely to track the same individual before and after the reform. For this reason, we need to control for differences in relevant observable characteristics across both groups before and after the reform. In doing so, we take into account, among other things, individuals' previous sickness and employment histories. While we still have to assume that there are no unobservable characteristics affecting the group composition after the reform, this procedure enables us to account for a potential selection on individuals' observable health status into establishments that were either affected or not affected by the reform.

Under these assumptions, we estimate the average treatment effect on the treated (ATT) in a linear regression framework using the following equation:

$$Y_i = \alpha + \beta T_i + \gamma G_i + \tau_{DID}(T_i * G_i) + \eta X_i + \epsilon_i \tag{1}$$

In eq. (1), the differences-in-differences estimator τ_{DID} is given by the coefficient on the interaction term of the group dummy G_i (indicating whether an individual belongs to the treatment or control group) and the time dummy T_i (indicating whether an individual is observed before or after the reform). Y_i is the outcome variable, i. e. the incidence and duration of sickness periods and the risk of becoming involuntarily unemployed after sickness. β accounts for common time effects, γ captures the group effects and ϵ_i reflects the error term. Additionally, we add a vector of control variables X_i capturing observable individual and establishment characteristics. Further, in case of correlated errors within establishments, default robust standard errors would overstate the precision of the estimation and we therefore display standard errors adjusted for clustering at the establishment level (Cameron and Miller, 2015).

To rule out that establishments might have self-selected themselves into the different size classes, we have to check whether there are any "threshold effects" with regard to changes in the establishment size distribution. Because of the threshold regulation, small establishments may have had the incentive to stay below the threshold value of five FTE workers before the reform. After the reform, they may have expanded up to the new threshold size of ten FTE workers without being affected by the PADA (see also Priesack, 2015). To test for such threshold effects, we calculate the annual share of establishments by FTE size categories between 1999 and 2010 using data of the Establishment History Panel (BHP). This cross-sectional data set contains all establishments in Germany with at least one employee liable to social security on the yearly reference date June 30th (Schmucker et al., 2018). Overall, the distribution of establishments according to FTE size categories

remained broadly unaltered over the observation period suggesting that threshold effects do not play a major role (see Figure B.1 in the Appendix).

4.2 Data

Our empirical analysis is based on longitudinal German register data (*BASiD*). The data combine information from the German Pension Register with data from the German Federal Employment Agency. The *BASiD* data set is a stratified random one-percent sample of all individuals from the early 1940s to the early 1990s birth cohorts, who have at least one entry in their social security records and who have not retired yet (for details see Hochfellner et al., 2012). The data provide longitudinal information on individuals' entire pension relevant biographies up to the year 2007. Individual work histories cover the period from the year individuals were aged 14 until the age of 67. In Germany, statutory pension insurance is mandatory for all employees in the private and public sector, thus only excluding civil servants and self-employed individuals. As a consequence, the insurance covers more than 90 per cent of the entire population for whom all past pension-relevant periods have been recorded.

The Pension Register provides information on all pension relevant periods, i. e. periods for which contributions were paid (such as employment, long-term illness and unemployment) as well as periods without contributions, which were still creditable for the pension insurance. The latter refers to activities for which an individual receives pension credits. These are periods of school or university attendance after the age of 15, periods of training and apprenticeship and periods of caring. Apart from individual information on employment status, the Pension Register provides information on age, gender as well as monthly earnings, which can be calculated by exploiting information on pension credit points gained from social security employment. Table A.1 in the Appendix contains a more detailed description of the individual characteristics provided by the Pension Register. As to our main outcome of interest, the Pension Register allows us to retrieve information on all spells of illness that are subject to sick pay covered by the mandatory health insurance. As spelled out earlier, the latter comes into effect after a period of six weeks of absence and may cover either spells of employment and unemployment. The recorded sickness spells may also cover long-term rehabilitation measures aimed at reintegrating long-term ill individuals into the labour market. A potential concern is that sickness spells recorded by the Pension Register may also include caring periods for ill infants below the age of twelve. However, these periods are capped at a maximum length of ten days per year/per child. In our empirical analysis, we will address this potentially confounding effect in a robustness check.

Starting from 1975 (in Western Germany), employment spells subject to social security contributions from the Pension Register can be merged with data from the German

Federal Employment Agency, namely the Integrated Labour Market Biographies and the Establishment History Panel. The Integrated Labour Market Biographies provide further time varying individual information on educational status (three categories) and an establishment identifier.[13]. The latter allows us to identify newly hired employees and enables us to gain information on tenure at the current employer. Table A.3 in the Appendix provides a more detailed description of the variables gained from the Employment Statistics Register.

4.3 Sample Selection and Descriptives

As spelled out earlier, we define workers entering an establishment of up to ten FTE workers as our treatment group, whereas the control group consists of individuals entering an establishment slightly above the threshold, that is with more than ten to 20 FTE workers. We carry out a somewhat more precise calculation of establishment size compared to previous studies which use the number of workers - regardless of their working time - on a particular set date. Unlike previous studies, we approximate the number of full-time equivalent workers and take into account annual fluctuations of the workforce.[14] Calculating establishment size as precise as possible is crucial for correctly assigning individuals to either the treatment or the control group in our differences-in-differences set-up. However, we do not have sufficient information on individuals' exact weekly working hours in our data. Our calculation of the establishment size that is relevant for the applicability of the PADA may therefore still suffer from some imprecisions. To allow for a certain measurement error, we therefore exclude entries into establishments with a size close to the threshold. Thus, we restrict our sample to individuals entering an establishment of six to nine (treatment group) and twelve to 20 (control group) FTE employees, respectively. We further ensure that the establishments remain in the same size group during the period a worker is employed in this establishment.

The descriptive statistics reflect some systematic differences in the gender composition as well as the occupational and industry structure across treated and control individuals before and after the reform (see Tables B.1 and B.2 in the Appendix). This highlights the importance of including these observables as controls into our regressions. The differences in industry affiliation (and to some extent occupations) clearly reflect heterogeneous establishment size distributions across different industries. Note, however, that there are no

[13]Note that the legal definition of "establishment" does not match exactly with the establishments identified by the establishment identifier of the Establishment History Panel (on the definitions of "establishment" see Table A.4 in the Appendix). However, according to the establishment panel – a representative survey of establishments in Germany – , a large majority of establishments is an independent company without any other places of business (see Figure A.1 in the Appendix.). We can expect these establishments to be covered by both the legal definition and the definition in the administrative data.

[14]For details on how we calculated the establishment size see Table A.6 in the Appendix.

major differences concerning individuals' employment and illness histories across treated and control individuals.

5 Estimation and Results

5.1 Incidence and Duration of Long-Term Sickness

5.1.1 Descriptive Results

Figure 1 shows the cumulative incidence of sickness for the treatment and the control group during the first two years after establishment entry. In the pre-reform period, the evolution of this outcome exhibits no major differences across treated and control individuals. In the post-reform period, the cumulative incidence of sickness is lower for both groups. The graphs seem to diverge slightly across both groups, with the treatment group exhibiting a larger decline in the cumulative incidence of sickness after the reform as compared to the control group. The figures also show that the transition into a long-term sickness episode is a rather rare event; only 4.6 per cent and 3.7 per cent of individuals in our baseline sample experienced at least one transition into a long-term sickness episode during the first 24 months after entry into the establishment before and after the reform, respectively.

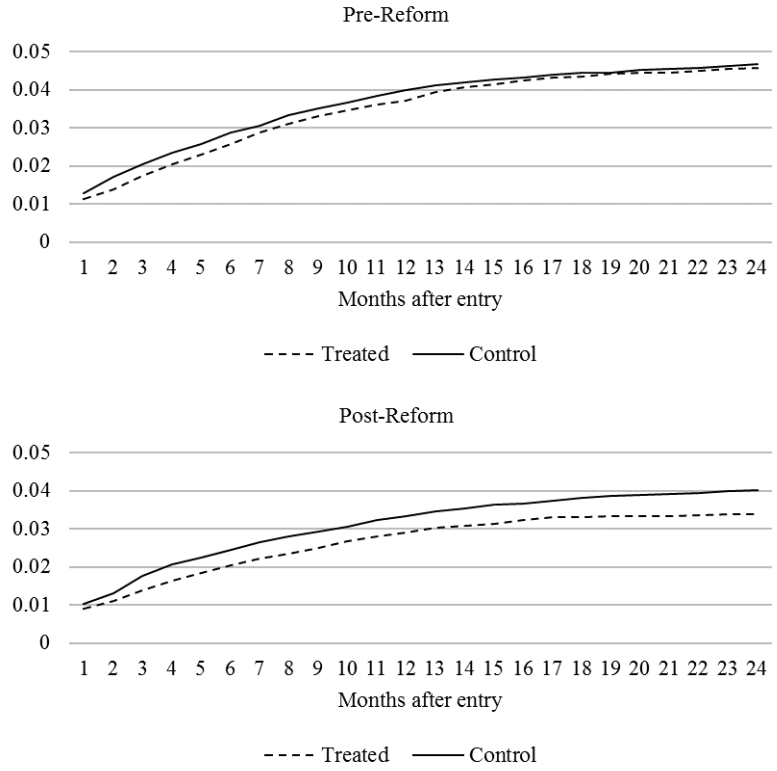
5.1.2 Regression Results

To estimate the reform's effect on the incidence of long-term sickness in the short and medium run, we look at the probability of experiencing a transition into sickness in the first and in the second year after entering an establishment. For this, we have to ensure that the individuals are at risk of experiencing such a transition. Thus, to calculate the probability of a transition into sickness in the *first* year after entry, we exclude those who were already ill at establishment entry resulting in a sample of 27,967 observations.[15] Note that looking at the probability of a transition into sickness in the *second year* raises selectivity issues, as this outcome can be derived only for those individuals with a sufficient tenure at the new employer. This is also reflected in our sample size for the second year outcome, which is reduced to a total of 8,845 observations.

We estimate four models, which are stepwise augmented by different sets of explanatory variables. The first model is the basic differences-in-differences-model without any controls. The second model includes individual characteristics (gender, age, age squared, nationality, qualification, and cumulative earnings), employment-related characteristics (the daily wage, working time, occupational status, and occupational sector), as well as year dummies. The third model also includes establishment characteristics, in particular

[15] 160 individuals in our sample (0.6%) enter the establishment while being already ill. Most of these workers fell sick shortly before entering the establishment and the duration of most of these sickness spells is rather short.

Figure 1: Cumulative Incidence of Long-Term Sickness



Note. The treatment (control) group consists of workers working in establishments of 6-9 (12-20) FTE employees who entered the establishment three years before or three years after the reform. We calculate the share of workers having at least one long-term sickness period until the respective month after entry.
Source: BASiD, own calculations.

the location of the establishment (West vs. East Germany) and ten industry dummies. Finally, the fourth model further adds information on individuals' employment and sickness histories, accounting for the duration and number of previous long-term sickness episodes, employment, unemployment and non-employment spells as well as the number of establishment changes.

For the *first* year after entering the establishment, the multivariate analyses do not provide any evidence of a reform effect on the incidence of having experienced a long-term sickness episode (see Table C.1 in the Appendix). The coefficient on the interaction term is insignificantly negative, but close to zero and remains unaltered after controlling for differences in observables. The results for the reform's medium run effect - the effect on the probability of having experienced a long-term sickness spell in the *second* year after establishment entry - are shown in Table 1. According to the specification incorporating all control variables, treated individuals exhibit a 1.3 percentage point lower incidence of long-term sickness. This effect remains largely constant across all specifications. Given that the overall probability of having experienced a transition into sickness in the second year is 2.4 per cent, this effect is fairly large. The group effect is positive, but insignificant.

In contrast, the time effect is negative and significant (except for the basic model) and becomes larger in magnitude after adding more control variables. The last column in Table 1 shows estimates from placebo regressions, which hypothetically assume that the dismissal protection reform took place in 2003. The placebo estimates do not provide any evidence of significant effects on our outcome both, for the first and the second year, thereby supporting the parallel trend assumption.

Table 1: Regression Results Transition Into Long-Term Sickness in the Second Year After Entry

	Model 1	Model 2	Model 3	Model 4	Placebo
Post x Treat	-0.012*	-0.014**	-0.014**	-0.013**	-0.001
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Post	-0.002	-0.013**	-0.013**	-0.015**	0.015**
	(0.004)	(0.006)	(0.006)	(0.006)	(0.006)
Treat	0.006	0.008	0.008	0.008	0.005
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Individual Characteristics	-	✓	✓	✓	✓
Employment-Related Characteristics	-	✓	✓	✓	✓
Year Dummies	-	✓	✓	✓	✓
Establishment Characteristics	-	-	✓	✓	✓
Individual Employment and Sickness History	-	-	-	✓	✓
Constant	0.025***	0.059***	0.055***	0.038***	0.037***
	(0.003)	(0.007)	(0.010)	(0.012)	(0.012)
Observations	8,845	8,845	8,845	8,845	9,188
R^2	0.001	0.018	0.019	0.030	0.021

Notes: The table shows results of a linear probability model estimating the probability of a transition to sickness 13 to 24 months after establishment entry; *, ** and *** denote statistical significance at the 10%, 5% and 1% level. Standard errors are in parentheses and are adjusted for clustering at the establishment level. The treatment (control) group consists of workers entering an establishment of 6-9 (12-20) FTE workers. All control variables are measured at the date of entry into the establishment. For definition and calculation of the variables see Tables A.1, A.2 and A.3 in the Appendix. The placebo regression hypothetically assumes the dismissal protection reform to take place in 2003.
Source: BASiD, own calculations.

5.1.3 Robustness Checks

In this section, we explore whether the results from Table 1 are robust to several sensitivity checks (for an overview see Table C.2 in the Appendix): First, we exclude illness spells lasting no longer than ten days, as these spells may also result from leave periods due the sickness of a child. Second, we explore whether our results are robust to using a different control group, in particular individuals working in establishments with 0.5 to four FTE employees. Third, we also include individuals entering establishments with a size close to the threshold. The fourth and fifth robustness checks are combinations of the previous checks. The results are shown in Table C.3 in the Appendix: When excluding short illness spells (columns (1) and (4)), the effects are slightly smaller in magnitude, but still significant at the 10% level. This suggests that part of the overall effect is also due to a decline in short (potentially child-related) sickness spells. The coefficients of the other estimates are all comparable in magnitude to those in Table 1 and at least significant at the 5% level. Finally, in the sixth column, we present results from placebo regressions for

2003 using the alternative control group. Again, these results do not provide any evidence of a significant placebo effect one year prior to the reform.

5.1.4 Selection Analysis

As shown above, our analyses point to a significant reform effect on transitions into long-term sickness during the second year after establishment entry. However, the question which mechanisms drive this result is still open. On the one hand, the established effect might result from a true behavioural effect of newly hired individuals who adapted their sickness behaviour to weaker dismissal protection regulations. On the other hand, the change in sickness absence might also arise from a different selection of workers into establishments. First, individuals with a high propensity of being long-term sick might systematically select themselves into establishments with stricter employment protection. Second, due to weaker dismissal protection, employers in the affected size class might alter their hiring behaviour. Related to that, Bauernschuster (2013) shows that the reform considered here had a positive effect on hiring rates. In addition to increasing their hiring rates, employers might become less selective in their hiring behaviour and might be more likely to hire individuals with a higher propensity of becoming long-term sick (Olsson, 2009). Note that such an effect would run counter potential selection mechanisms on the workers' side. At the same time, a less cautious hiring behaviour might also affect the propensity to hire workers with less experience. These are often young workers who, at the same time, exhibit a lower propensity of becoming long-term sick. To address such potential compositional effects, we next explore whether the reform changed the selection of workers into establishments of different size classes. To do so, we first analyse whether the reform affected the probability of hiring an individual who had at least one long-term sickness period before entering the establishment.[16] Second, we also analyse whether the reform affected the propensity of hiring young workers below the age of 25. Given that the propensity of risky hiring might vary across different employers, we perform both analyses separately for shrinking/non-growing and growing establishments. The underlying notion is that growing establishments may be more inclined to take on such risky hires (e.g., Coad et al., 2014). The results of the differences-in-differences estimations are shown in Table C.4 and Table C.5 in the Appendix. The estimated reform effects on the composition of newly hired workers are throughout small and insignificant at any conventional level. As to the age composition, growing establishments even exhibit a negative (albeit insignificant) coefficient. Given that the reform should especially cause growing employers to hire more younger workers, this leads us to conclude that the results provide no evidence of any compositional selection effects.

[16] In doing so, we impose the assumption that individuals' propensity of falling long-term sick is highly correlated with their past sickness histories. Strictly speaking, we cannot fully rule out that individuals *anticipating* a long-term sickness episode select themselves in establishments with stricter employment regulations.

A further more dynamic selection issue could arise from the fact that the reform might have affected newly hired individuals' probability of still being employed (and, therefore, of still being at risk of falling sick) during the second year after establishment entry. This issue arises as, on the one hand, the reform may have induced treated individuals to leave their employer earlier as compared in the pre-reform setting. On the other hand, weaker employment protection regulations may also have caused establishments to faster dismiss sick and therefore less productive employees among the treated individuals. To further investigate this issue, we next explore whether the reform affected newly hired individuals' probability of still being employed by their initial employer during the second year after establishment entry (see Table C.6 in the Appendix). The insignificant coefficient of the interaction term provides no evidence for a reform effect. Along with our earlier results pointing to no compositional effects in terms of health observables, this leads us to conclude that our established reform effect from Table 1 is neither driven by a compositional nor by a dynamic selection effect.

5.1.5 Heterogeneous Effects

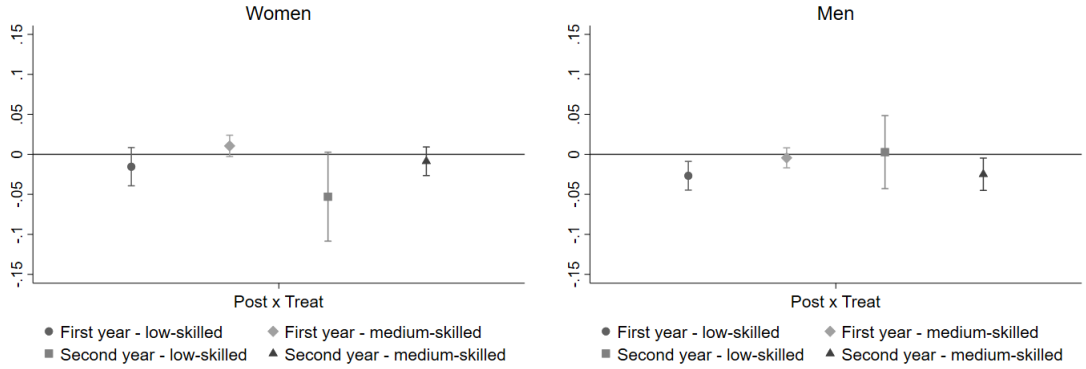
As the effects could vary across different groups of workers, we next address heterogeneous effects. To do so, we distinguish between gender and different skill groups. Due to sample size limitations, we are unable to perform separate analyses for high-skilled employees, though. Figure 2 shows the results for the different groups for the first and second year after entering the establishment, respectively. For low-skilled men, the estimates point to a significantly negative reform effect already in the first year. In the second year, the reform appears to have a particularly negative effect on medium-skilled men. The effect for this subgroup is larger in magnitude (2.5 percentage points) compared to the baseline specification. Overall, the results suggest that in particular male workers respond to the change in dismissal protection.[17] Note that this result is broadly consistent with the evidence provided by Ziebarth (2013), suggesting that middle-aged workers and those in the bottom part of the earnings distribution are found to react to a decline in sick pay. As in Ziebarth (2013), a potential explanation for our result might relate to male workers' male breadwinner status and a greater dependency of household incomes of male workers' earnings.

5.1.6 Duration of Long-Term Sickness

Next, we analyse whether the reform also affected the duration of sick leave. We restrict our sample to those individuals who experienced at least one sickness spell after entering an establishment of the relevant size class and calculate the cumulative duration of all sickness spells during this employment spell. This results in a sample of 1,213 individuals.

[17] Note, however, that the reform effects for low-skilled women are considerable in size, too, albeit not significant at any conventional level.

Figure 2: Transition Into Long-Term Sickness: Heterogeneous Effects



Notes: The figures show the coefficients of the differences-in-difference estimations with 90% confidence intervals stratified by gender and qualification. The corresponding regression tables can be found in the Appendix (Tables C.7 and C.8).
Source: BASiD, own calculations.

The distribution and the mean values of the cumulative sickness days suggest that there appears to be no major visible post-reform change (see Figure B.2 and Table B.3 in the Appendix).

Because of the right skewed distribution, we use the log of the number of long-term sickness days as our dependent variable in the multivariate differences-in-differences-analyses. The estimations support the descriptive results (see Table C.9 in the Appendix). There are neither differences across both groups nor time effects. The coefficients on the interaction terms are negative, but not significant at any conventional level either. This result is robust to several robustness checks similar to those in Section 5.1.3 (see Tables C.10 and C.11 in the Appendix). With regard to heterogeneous effects, we do not find any effect when stratifying our sample by gender and skill groups. Overall, these results indicate that weaker dismissal protection affects the *incidence* but not the *duration* of long-term sickness periods.

5.2 Involuntary Unemployment after Long-Term Sickness

In what follows, we examine whether the reform was associated with a higher risk of unemployment after long-term sickness. More precisely, we estimate the association between the reform's policy change and the probability of becoming involuntarily unemployed after starting a long-term sickness episode. We restrict the sample to individuals having at least one long-term sickness period after entering the new employment relationship.[18] Our dependent variable is an indicator variable for a transition into involuntary unemployment

[18]We only consider individuals whose sickness periods lasts no longer than 78 weeks in three years (this exclusion affects only 9 observations). After 78 weeks of sickness, sick pay expires and the individual becomes subject to unemployment benefits. In these cases, we can no longer distinguish between a true transition into involuntary unemployment and unemployment that merely arises due to a substitution of sick pay by unemployment benefits.

after having started a long-term sickness spell. This dummy variable takes on the value of unity if a transition into involuntary unemployment takes place and zero otherwise. As we will estimate a time-discrete logit model, we measure this indicator for each quarter after the start of a long-term sickness spell for those individuals who are still at risk, i. e. those who have not yet left their initial employer. In doing so, we do not only consider direct transitions from sickness into unemployment, but also allow individuals to return to work after their long-term sickness period. To distinguish between voluntary and involuntary unemployment, we exploit the fact that unemployment benefits may be temporarily suspended in case of voluntary quits (see also Table A.3 in the Appendix). To further ensure that we indeed observe *involuntary* unemployment, we only count transitions into unemployment spells lasting longer than four weeks as transitions into involuntary unemployment.

5.2.1 Descriptive Results

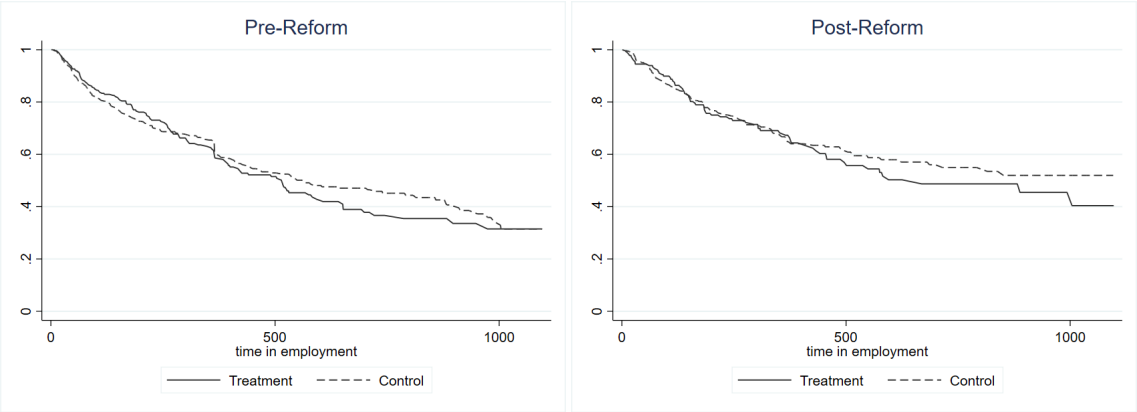
Figure 3 shows non-parametric estimates of the Kaplan-Meier survival curves based on involuntary unemployment exit hazards. Survival refers to the initial state of being employed at the same employer after having started a long-term sickness spell. The survival curves are broken down by treatment and control individuals before and after the reform.

The figures show that by about three years after having started a long-term sickness spell a fraction of about 35 per cent is still employed at the same employer both in the treatment and control group prior to the reform. The control group appears to exhibit slightly higher survival rates in the second half of the maximum observed duration of the employment spell. After the reform, the fraction remaining employed has increased for both groups, with the difference being somewhat larger for the control group.

5.2.2 Regression Results

Figure 4 shows the average marginal effects from estimating a multivariate time-discrete logit model. The figure illustrates that up to quarter four the time effect on experiencing a transition into involuntary unemployment is negative for both, treated and control individuals, which supports the descriptive evidence from Figure 3. The magnitude and significance of the time effects is displayed in row (2) of Table 2. The figures indicate that in the third and fourth quarter, the negative effects are significantly different from zero. The estimated differences in the marginal effects between treated and control individuals are displayed in the first row of Table 2. For the first and third quarter, the estimates are negative and not significant at any conventional levels. For the remaining quarters, the estimates exhibit their expected positive sign, but are again very imprecisely estimated. Overall, these results fail to provide clear evidence that individuals who are employed in establishments subject to weaker dismissal protection and who have fallen sick exhibit

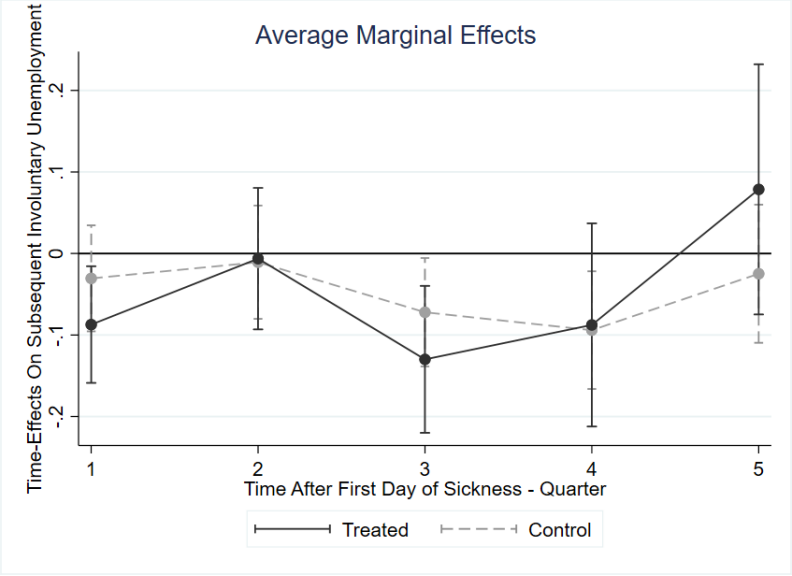
Figure 3: Transition Into Unemployment After Long-Term Sickness - Before and After Reform



Notes: The figure shows the transitions into involuntary unemployment as a function of time in relevant employment. The treatment (control) group consists of workers working in establishments of 6-9 (12-20) FTE employees who entered the establishment three years before or three years after the reform and who have at least one sickness spell during their employment in this establishment. Number of individuals: 1,161.
Source: BASiD, own calculations.

significantly higher probabilities of becoming unemployed as compared to their control counterparts.

Figure 4: Average Marginal Time Effects Transition Into Unemployment After Long-Term Sickness



Notes: The figure shows the average marginal time effects with 90% confidence intervals on the probability of involuntary unemployment after sickness for the treatment and control group estimated in a time-discrete logit model. The treatment (control) group consists of workers working in establishments of 6-9 (12-20) FTE employees who entered the establishment three years before or three years after the reform and who have at least one sickness spell during their employment in this establishment.
Source: BASiD, own calculations.

These results are robust to several robustness checks similar to those in Section 5.1.3

Table 2: Differences-in-Differences Estimations Transition Into Unemployment After Long-Term Sickness

Time After First Day of Sickness (Quarter)	1	2	3	4	5
Post x Treat	-0.057	0.004	-0.058	0.006	0.103
	(0.045)	(0.060)	(0.064)	(0.081)	(0.106)
Post	-0.052	-0.009	-0.095***	-0.091**	0.016
	(0.034)	(0.037)	(0.035)	(0.043)	(0.050)
Treat	-0.007	0.019	0.022	0.059	0.061
	(0.023)	(0.031)	(0.034)	(0.042)	(0.052)

Notes: The table shows the differences-in-differences estimations on the probability of involuntary unemployment after sickness (average marginal effects) for each quarter after the first day of sickness (time-discrete logit model). ** and *** denote statistical significance at the 5% and 1% level. Standard errors are in parentheses and are adjusted for clustering at the establishment level. The specifications control for individual characteristics, employment-related characteristics, establishment characteristics and the individual sickness and employment history. Number of observations: 2,489.
Source: BASiD, own calculations.

(see Tables C.12 and C.13 in the Appendix). We wish to note, though, that the estimates are selective in that they condition on having experienced a long-term sickness spell. Given that the reform negatively affects the incidence of long-term sickness, this may imply that treated individuals who experience such a spell are, on average, unobservably different from those with a long-term sickness episode prior to the reform. To the extent that individuals who - despite enjoying no employment protection - fall (long-term) sick after the reform are those with particular severe diseases, treated long-term sick individuals are likely to be negatively selected in terms of health unobservables. On the other hand, as long as individuals who fall sick after the reform are characterised by less moral hazard behaviour, these individuals are likely to reflect a positive selection in terms of work attitude unobservables. Depending on which kind of unobservable factor is more or less decisive for employers' dismissal decisions, these selection mechanisms may either cause an upward or downward bias of our estimates on the reform effects on unemployment transitions.

6 Mechanisms

What is still unanswered, is what *type of sickness behaviour* caused our established effect on the incidence of long-term sickness episodes: Do our results reflect a decline in *absenteeism without being sick*, i. e. did treated workers stay away from work more frequently without being sick before the reform, when they were protected? Or do our findings reflect an increase in *presenteeism*, as the reform induced more treated workers to attend work despite being sick for fear of dismissal? To further substantiate our findings, we additionally analyse German survey data providing information on absenteeism and presenteeism. The *BiBB/BAuA Employment Survey of the Working Population on Qualification and Working Conditions in Germany* is a repeated cross-sectional survey of about 20,000 em-

ployees in Germany. The survey is representative of the German Working Population and contains - among other things - information on individuals' health status and health behaviour. More precisely, the survey of 2012 contains questions on presenteeism and absence (for details see Tables D.1 and D.2 in the Appendix). Using this information, we generate dummy variables measuring the incidence and length of presenteeism and absence periods.[19] More precisely, we generate a dummy variable being equal to one for an individual reporting more than zero, five, ten or 15 working days of presenteeism or absence per year, respectively.[20] To distinguish between employees with and without dismissal protection, we use information on establishment size and introduce a dummy variable being equal to one for workers in establishments with more than 20 to 49 employees and zero for workers in establishments with five to nine employees. This yields a sample of 2,549 observations. The descriptive statistics show that there are some systematic differences in observables between the two groups (see Table D.3 in the Appendix). This highlights the importance of including these variables as controls into our regressions. However, in terms of subjective health status, individuals with and without dismissal protection do not seem to differ significantly.

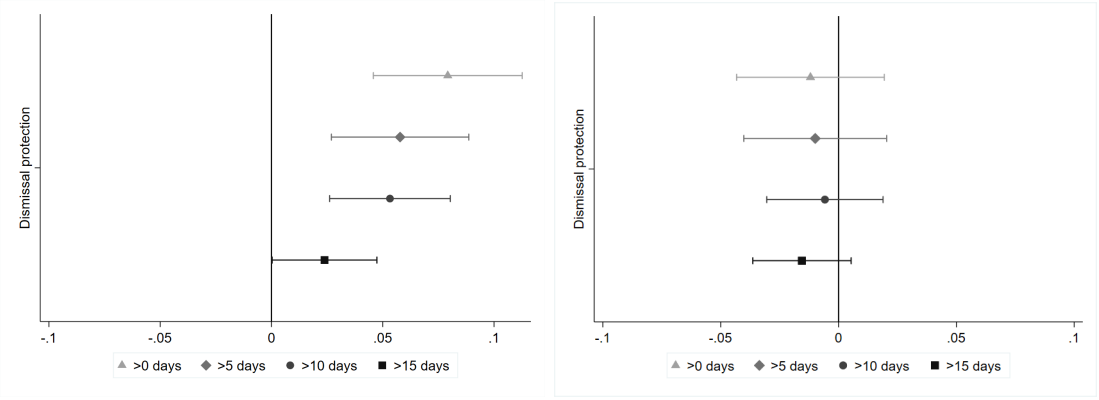
To analyse the association between dismissal protection and both, presenteeism and absenteeism, we run probit regressions that control for observables, such as socio-demographic information, working strains and the subjective health status (for a similar analysis see Hirsch et al., 2017). Figure 5 shows the average marginal effects of dismissal protection (as measured by establishment size) on the incidence of different durations of absence and presenteeism. For absence, the marginal effect is initially positive and significant. More precisely, individuals subject to dismissal protection have a 7.9 percentage points higher probability of being absent at least once a year (for details see Table D.4 in the Appendix). This association is highly significant. However, for the incidence of longer absence periods, the marginal effect of dismissal protection gets smaller (and eventually becomes insignificant). For presenteeism, the marginal effect of dismissal protection is negative and increases in magnitude for the incidence of longer periods of presenteeism. The marginal effects and their differences across different durations are, however, insignificant for all considered durations.

Overall, these findings provide no clear evidence of which of the two competing mechanisms - an increase in presenteeism or a decline in absenteeism - is more relevant for explaining our results. On the one hand, the duration-dependent pattern of the size of the marginal effects suggests that the latter becomes larger for longer durations of presenteeism

[19] With the data at hand, we cannot explicitly measure absenteeism behaviour *without* being sick. However, we can measure the incidence and length of actual absence controlling for individuals' health status.

[20] Due to a limited number of observations and an increasing measurement error in the higher distribution of sickness durations, we cannot explicitly consider long-term presenteeism or absenteeism of more than six weeks.

Figure 5: Marginal Effects of Dismissal Protection on Absence and Presenteeism



Notes: The left figure shows the association between dismissal protection and absence of more than 0, 5, 10 or 15 working days per year (dummy variables) with 90% confidence intervals. The differences between the marginal effects are not significant except for the difference of the marginal effect of >10 days and >15 days. This difference is significant at the 1% level. The right figure shows the association between dismissal protection and presenteeism of more than 0, 5, 10 or 15 working days per year (dummy variables). The differences between the marginal effects are not significant. The presented effects are average marginal effects estimated by a probit model with 90% confidence intervals and controlled for gender, age, household situation, qualification, health status, income, tenure, working hours, job satisfaction, straining working conditions and branch of industry. For a detailed description of the sample and the variables, see Tables D.1 and D.2 in the Appendix.
Source: BiBB/BAuA Employment Survey 2012, own calculations.

and becomes smaller with longer durations of absenteeism. If one were to extrapolate this pattern to long-term sickness spells of more than six weeks, this might support the view that it is rather presenteeism that explains the established negative effect in our main analysis. On the other hand, the marginal effect of establishment size on the incidence of longer durations (>15 days) is of the same order of magnitude for both, absenteeism and presenteeism, and is statistically not indistinguishable from zero for presenteeism. Thus, the only conclusion that can be drawn from this complementary exercise is that neither mechanism can be excluded as an explanation.

7 Summary and Conclusions

This paper empirically analyses the impact of a change in dismissal protection on the incidence and duration of long-term sickness along with its consequences for involuntary unemployment after long-term sickness episodes. We exploit a German reform in 2004 that has shifted the threshold exempting small establishments from dismissal protection from five to ten workers. We first show that loosening dismissal protection led to a decrease in the incidence of long-term sickness among treated individuals, i. e. those who were hired by establishments affected by the reform, relative to their control counterparts. Second, we provide evidence that this negative effect stems from a behavioural change among treated employees, rather than from a compositional effect that may arise from a different selection of workers into establishments. This result is in line with the study by Olsson (2009), which provides evidence of a negative effect of a weaker dismissal protection on

the sickness absence rate at the establishment level and which attributes this effect to behavioural changes.

In quantifying the magnitude of the reform effect for the whole sample, we find that the incidence of long-term sickness spells lasting longer than six weeks decreased by 1.3 percentage points among treated individuals during the second year after establishment entry. Compared with a rather low mean transition rate into sickness during the second year, the effect represents a decline by about 54 per cent. Overall, our results are consistent with the PADA reform having had a large impact on the perceived job insecurity among treated workers. The pronounced policy change for exempted establishments along with its impact on perceived job security might explain the relatively large effect on sickness transitions established by our study. The reform did neither affect the duration of long-term sickness spells, nor was it associated with a higher risk of becoming involuntarily unemployed after long-term sickness. In accordance with other studies, which fail to establish any effect of dismissal protection on separations (e.g., Bauer et al., 2007), our results suggest that it appears to be less the establishments than the employees themselves who react to changes in dismissal protection regulations. Our findings also indicate that the regulations of the PADA, which allow for dismissals in case of personal incapability, do not appear to prevent establishments from dismissing individuals for reasons of severe and longer illness episodes.

To identify the underlying mechanisms, we analyse the association between dismissal protection and presenteeism and absence, respectively, using cross-sectional representative German survey data. However, our complementary analysis provides no clear evidence of whether the results reflect an increase in presenteeism or a decline in absenteeism. Taken together, while our analyses reveal that dismissal protection affects long-term sickness behaviour, the evidence on the behavioural mechanisms is less clear-cut. Given that absenteeism and presenteeism impose high costs on both, employers and employees, this highlights the need for future research on the underlying sources of long-term sickness behaviour.

Declarations of Interest

None declared.

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(Online)-Appendix

Appendix A: Data Description BASiD

Table A.1: Description of Individual and Employment-Related Characteristics

Variable	Definition/Categories
Nationality	*Foreign*: Dummy with value 1 for nationality that is not German, Reference: German nationality. We correct missing and inconsistent data following the suggested imputation procedure of Drews et al. (2007).
Educational Status	*Low-skilled*: No degree or highschool degree (Reference category) *Medium-skilled*: Completed vocational training *High-skilled*: Technical college degree or university degree
Missing Education	Missing and inconsistent data on education from the Employment Statistics Register are corrected according to the imputation procedure described in Fitzenberger et al. (2006). This procedure relies, roughly speaking, on the assumption that individuals cannot lose their educational degrees.
Earnings	*Daily Wage*: Daily wage is generated from fixed period pay referring to the original duration of employment (Hochfellner et al., 2011). *Cumulative Earnings*: Gross cumulative earnings are retrieved from credit points to the German Pension Insurance. One credit point corresponds to the average of yearly earnings of all gainfully employed workers in Germany. For each spell observed in the data, earnings are thus obtained by multiplying the recorded credit points per spell with the average of earnings as documented in the Appendix 1 to the German Social Act *SGB VI*. Credit points are reported up the contribution limit of the German social security system.
Working Time	*Working Full-time*: Dummy with value 1 for working full-time, Reference: working part-time.
Occupation	*Occupational Status*: White-collar worker, Reference: Blue-collar worker *Occupational Activity*: Classification of occupational activities according to the 3-digit code of the German classification of occupations 1988 (KldB 1988). Groups: Agrar, Salary, Sale, Clerical, Service, Reference: Craftsman.

Table A.2: Description Establishment Characteristics

Variable	Definition/Categories
Location	*West Germany*: Dummy with value 1 for establishments located in West Germany, Reference: East Germany. Berlin is counted as part of West Germany.
Industry	Industry dummies according to the classification of economic activities (3-digit). Groups: Energy/Mining, Manufacturing, Construction, Wholesale, Traffic/Communication, Banking/Insurance, Other Services, Public Administration, Public Sector, Reference: Agrar/Fishery.

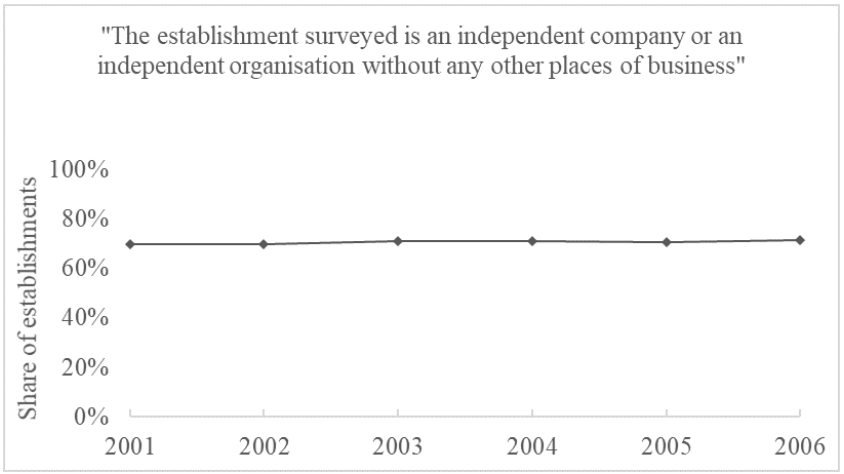
Table A.3: Description of Labour Market States

Labour Market States
Employment: Employment spells include continuous periods of employment (allowing for gaps of up to four weeks) subject to social security contributions (excluding minor employment and periods of apprenticeship). Further, we ensure that a daily wage is reported that exceeds a certain threshold (7 Euro).
Unemployment Unemployment spells include periods of unemployment with transfer receipt. A spell of unemployment in the *Pension Register* requires individuals to be registered as unemployed *and* to obtain public transfers. The latter include benefits such as unemployment insurance, and - prior to 2005 - the means-tested social assistance and unemployment assistance benefits. After 2004, unemployment and social assistance were merged to one unified benefit, also known as "unemployment benefit II" (ALG II). As the latter targets only employable individuals, a spell involving the receipt of ALG II automatically fulfills the requirements to be recorded as unemployed in the *Pension Register*. Prior to 2005, spells with social assistance benefits fulfill these requirements only if individuals were registered as unemployed. Otherwise these spells are recorded as non-employment spells. As a consequence, the *Pension Register* does not permit a consistent definition of un- and non-employment prior to and after 2005.
Distinction between Un- and Non-Employment According to the procedure proposed by Lee and Wilke (2009), involuntary unemployment is defined as comprising all continuous periods of transfer receipt. Gaps between such unemployment periods or gaps between transfer receipt and a new employment spell may not exceed four weeks, otherwise these periods are considered as non-employment spells (involving voluntary unemployment or an exit out of the social security labour force). Similarly, gaps between periods of employment and transfer receipt or job search are treated as involuntary unemployment as long as the gap does not exceed six weeks, otherwise the gap is treated as non-employment.
Sickness spells Periods of illness recorded by the *BASiD* data generally refer to spells of long-term sickness. These spells refer to employees who have been absent for more than six weeks.

Table A.4: On the Definitions of Establishments

Definitions of Establishments
Legal Definition of "Establishment": The PADA does not contain an own definition of the term "establishment". For this, the definition of § 1 BetrVG applies. According to this definition, an organizational unit is considered an establishment if the unit decides largely independently on working conditions and organisational issues and carries out personnel matters such as hirings and dismissals autonomously.
Definition of "Establishment" in the Administrative Data: An establishment is a regionally and economically delimited unit in which employees work. An establishment may consist of one or more branch offices or workplaces belonging to one company (Schmucker et al., 2018).

Figure A.1: Share of Individual Establishments



Notes: The graph shows the share of establishments that are an independent company or an independent organisation without any other places of business. The survey is representative of all establishments in Germany (Ellguth et al., 2014).
Source: IAB Establishment Panel, 2001-2006

Table A.5: Calculation of Establishment Size According to PADA

Weighting Procedure	Working Time	Weighting Factor
	$>$ 30 hours/week	1
	$<$ 30 hours/week	0.75
	$<$ 20 hours/week	0.50
Excluded groups of workers	Apprentices family members without a working contract freelance collaborators	
Temporal frame	The threshold for applicability of the PADA is typically not based upon the establishment size at a certain point in time, but is rather derived from the number of workers who are "normally" employed by an establishment. Thus, both past and future developments of the workforce need to be taken into account.	

Table A.6: Description of Group Assignment

Variables for Group Assignment

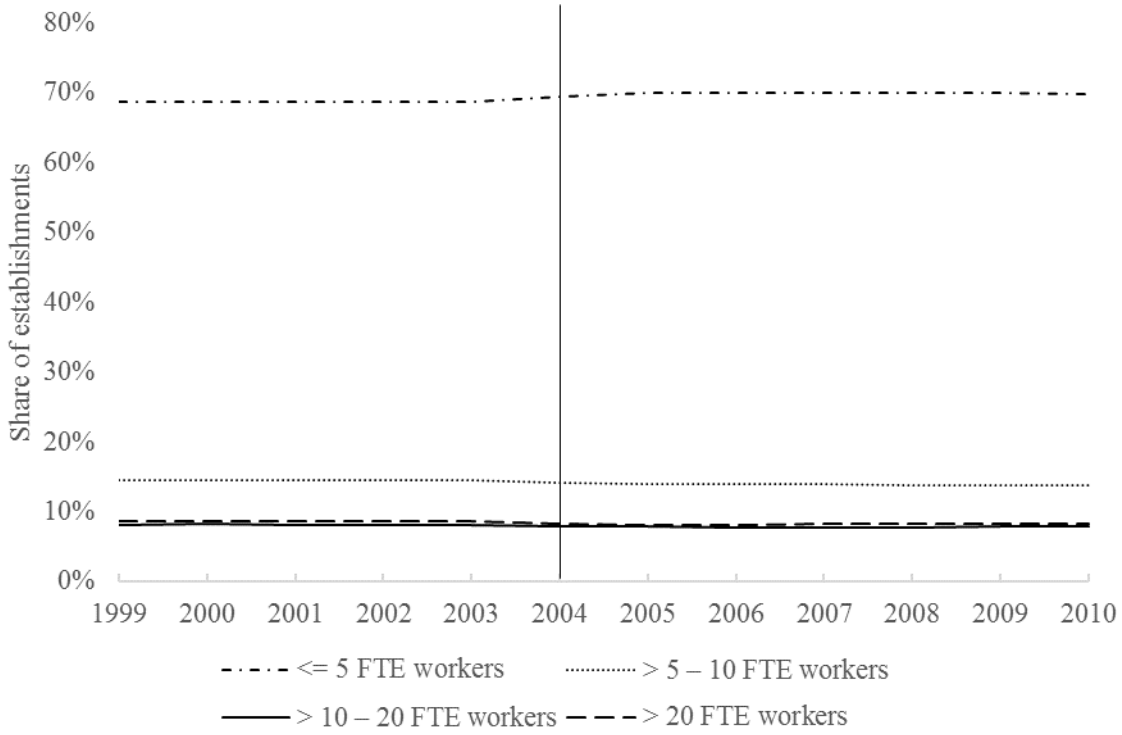
Entry in Establishment: First employment spell subject to social insurance contributions in an establishment of relevant size between 1.1.2001 and 30.6.2003 or 1.1.2004 and 30.6.2006, respectively (for definition of employment see Table A.3). Establishments in the shipping and aircraft transportation sector are excluded as they are subject to a specific legislation. We exclude individuals who were previously marginally employed or employed as an apprentice by the same employer. We further exclude recalls up to three years.

Establishment Size: Number of full-time equivalent workers according to the PADA as described in Table A.5: Workers working full-time are counted as one worker, workers in "mini part-time" ($<$ 18 hours per week) or part-time without further specification as well as marginally employed workers are weighted by a factor of 0.5, workers in "midi part-time" ($>=$ 18 hours per week) are weighted by a factor of 0.75.[a] Further, we exclude apprentices. Based on the daily-exact number of FTE workers, the annual average of the establishment size is calculated to account for past and future developments of the workforce. We assign workers entering in establishments with 6-9 (12-20) FTE workers to the treatment (control) group. We ensure that the establishment remains in the same size category during the time a worker is employed in this establishment.

[a]Note that the hours grid is not entirely identical to that of the PADA which applies the threshold of 20 hours per week to distinguish between "mini part-time" and "midi part-time" workers.

Appendix B: Descriptives

Figure B.1: Establishment Distribution by FTE Size Categories, 1999 to 2010



Notes: The establishment size is calculated using the number of full-time equivalent workers as stipulated in the PADA (see Section 3.2): Apprentices are excluded from the calculation; workers working full-time are counted as one worker; workers working in "mini-part-time" (< 18 hours) and workers in marginal employment are weighted by the factor 0.5, workers working in "midi-part-time" (>= 18 hours) are weighted by the factor 0.75.
Source: Establishment History Panel (BHP) 1999-2010, own calculations.

Table B.1: Descriptive Statistics I

	Pre-Reform					
	(1) Treatment Group		(2) Control Group			
	Mean	S. D.	Mean	S. D.	Mean (2)-(1)	
Individual Characteristics						
Female	0.445	0.497	0.416	0.493	-0.029	***
Age	31.923	9.813	32.076	9.929	0.153	
Age^2	96.961	125.702	99.030	9.929		
Foreign	0.261	0.439	0.271	0.444	0.009	
Qualification						
Low-skilled	0.192	0.394	0.195	0.397	0.003	
Medium-skilled	0.720	0.449	0.705	0.456	-0.015	*
High-skilled	0.088	0.284	0.100	0.300	0.012	**
Cum. Earnings (in 10,000 EUR)	11.367	14.495	11.998	15.776	0.631	**
Employment-Related Characteristics						
Daily Wage	52.117	28.303	54.772	29.259	2.655	***
Working Full-time	0.835	0.371	0.844	0.363	0.009	
Occupational Status						
Blue-collar	0.497	0.500	0.500	0.500	0.003	
White-collar	0.323	0.468	0.334	0.472	0.011	
Others	0.179	0.383	0.166	0.372	-0.013	**
Occupational Activity						
Agrar	0.028	0.166	0.025	0.156	-0.003	
Craftsman	0.292	0.455	0.316	0.465	0.024	***
Salary	0.082	0.274	0.091	0.287	0.009	*
Sale	0.119	0.323	0.099	0.299	-0.019	***
Clerical	0.153	0.360	0.169	0.375	0.016	***
Service	0.327	0.469	0.300	0.458	-0.027	***
Establishment Characteristics						
Location: West Germany	0.857	0.350	0.855	0.352	-0.002	
Industry						
Agrar/Fishery	0.030	0.170	0.023	0.150	-0.006	**
Energy/Mining	0.001	0.034	0.002	0.039	0.000	
Manufacturing	0.086	0.280	0.116	0.320	0.030	***
Construction	0.113	0.316	0.088	0.284	-0.025	***
Wholesale	0.201	0.400	0.177	0.381	-0.024	***
Traffic/Communication	0.073	0.260	0.074	0.262	0.001	
Banking/Insurance	0.010	0.101	0.010	0.101	0.000	
Other Services	0.278	0.448	0.311	0.463	0.033	***
Public Administration	0.031	0.173	0.036	0.186	0.005	*
Public Sector	0.014	0.119	0.010	0.101	-0.004	**
Individual Employment and Sickness History						
Cum. Sickness Duration	1.869	5.649	1.823	5.130	-0.046	
Cum. Unemployment Duration	11.694	34.873	12.178	36.981	0.484	
Cum. Employment Duration	97.939	101.785	99.945	107.715	2.006	
Cum. Non-Employment Duration	34.044	54.511	33.291	52.220	-0.753	
# of Establishment Changes	4.953	5.147	5.094	5.416	0.141	
# of Sickness Spells	1.046	2.536	1.057	2.577	0.011	
# of Unemployment Spells	1.854	2.319	1.896	2.414	0.042	
# of Employment Spells	4.748	4.580	4.829	4.839	0.080	
# of Non-Employment Spells	2.168	2.597	2.186	2.670	0.019	
# of Individuals in Baseline Sample	5,970		9,059			

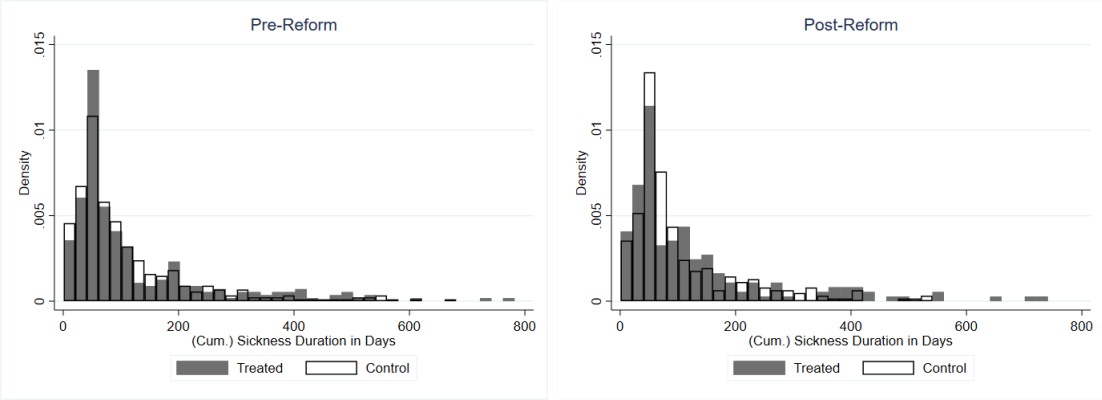
Notes: The table reports descriptive statistics of relevant characteristics of the treatment and control group before the reform. The treatment (control) group consists of employees working in establishments of 6-9 (12-20) FTE employees who entered the establishment between 1.1.2001 and 30.6.2003. *, ** and *** denote statistical significance of the difference in the mean between the treatment and control group at the 10%, 5% and 1% level (t-test). For the definition and construction of the variables see Tables A.1, A.2 and A.3 in the Appendix. All control variables are measured at the date of entry into the establishment. All durations are measured in months.
Source: BASiD, own calculations.

Table B.2: Descriptive Statistics II

	Post-Reform					
	(1) Treatment Group		(2) Control Group			
	Mean	S. D.	Mean	S. D.	Mean (2)-(1)	
Individual Characteristics						
Female	0.445	0.497	0.409	0.492	-0.037	***
Age	33.008	9.909	32.925	9.949	-0.083	
Age^2	98.235	135.405	98.993	134.836		
Foreign	0.259	0.438	0.248	0.432	-0.011	
Qualification						
Low-skilled	0.156	0.363	0.161	0.368	0.005	
Medium-skilled	0.744	0.437	0.716	0.451	-0.028	***
High-skilled	0.100	0.301	0.123	0.328	0.022	***
Cum. Earnings (in 10,000 EUR)	13.650	17.015	14.526	18.192	0.877	***
Employment-Related Characteristics						
Daily Wage	52.661	29.826	56.276	32.444	3.615	***
Working Full-time	0.839	0.367	0.837	0.369	-0.002	
Occupational Status						
Blue-collar	0.503	0.500	0.515	0.500	0.012	
White-collar	0.326	0.469	0.313	0.464	-0.013	
Others	0.171	0.376	0.172	0.377	0.001	
Occupational Activity						
Agrar	0.032	0.176	0.027	0.161	-0.005	*
Craftsman	0.299	0.458	0.313	0.464	0.014	*
Salary	0.085	0.278	0.102	0.302	0.017	***
Sale	0.115	0.319	0.096	0.294	-0.020	***
Clerical	0.154	0.361	0.166	0.372	0.011	*
Service	0.315	0.464	0.297	0.457	-0.017	**
Establishment Characteristics						
Location: West Germany	0.852	0.356	0.853	0.354	0.002	
Industry						
Agrar/Fishery	0.033	0.179	0.026	0.159	-0.007	**
Energy/Mining	0.003	0.053	0.004	0.064	0.001	
Manufacturing	0.088	0.284	0.114	0.317	0.025	***
Construction	0.124	0.330	0.089	0.285	-0.035	***
Wholesale	0.218	0.413	0.181	0.385	-0.038	***
Traffic/Communication	0.064	0.245	0.079	0.269	0.014	***
Banking/Insurance	0.012	0.111	0.012	0.109	0.000	
Other Services	0.283	0.451	0.328	0.470	0.045	***
Public Administration	0.117	0.321	0.117	0.322	0.000	
Public Sector	0.056	0.231	0.050	0.218	-0.006	
Individual Employment and Sickness History						
Cum. Sickness Duration	1.931	5.431	1.972	5.761	0.041	
Cum. Unemployment Duration	23.380	51.192	22.348	50.833	-1.031	
Cum. Employment Duration	110.245	105.068	109.666	106.368	-0.579	
Cum. Non-Employment Duration	34.292	52.265	32.937	50.920	-1.355	
# of Establishment Changes	5.677	5.561	5.868	6.596	0.191	*
# of Sickness Spells	1.008	2.503	1.010	2.520	0.001	
# of Unemployment Spells	2.235	2.665	2.232	2.671	-0.003	
# of Employment Spells	4.994	4.993	5.025	5.116	0.031	
# of Non-Employment Spells	2.150	2.602	2.138	2.744	-0.013	
# of Individuals in Baseline Sample	5,310		7,788			

Notes: The table reports descriptive statistics of relevant characteristics of the treatment and control group after the reform. The treatment (control) group consists of employees working in establishments of 6-9 (12-20) FTE employees who entered the establishment between 1.1.2004 and 30.6.2006. *, ** and *** denote statistical significance of the difference in the mean between the treatment and control group at the 10%, 5% and 1% level (t-test). For the definition and construction of the variables see Tables A.1, A.2 and A.3 in the Appendix. All control variables are measured at the date of entry into the establishment. All durations are measured in months.
Source: BASiD, own calculations.

Figure B.2: Distribution of Cumulative Sickness Days - Before and After Reform



Notes: The treatment (control) group consists of workers working in establishments of 6-9 (12-20) FTE employees who entered the establishment three years before or three years after the reform and who have at least one sickness spell during their employment in this establishment. Taking into account that sickness is reported after six weeks in our data, we calculate the entire number of absence days, by setting the start date of sickness 42 days before the start date of sickness reported in the data. 19 (25) observations are censored as these persons are still ill at the end of the observation period on 31 December 2003 (2006).
Source: BASiD, own calculations.

Table B.3: Average Sickness Duration in Days

	Treated			Control			
	Pre	Post	Diff	Pre	Post	Diff	DiD
All Sickness Spells	121.1	127.1	6.0	108.6	105.8	-2.9	8.9
Excluding Spells $\leq$ 10 days	138.8	146.2	7.3	126.2	123.7	-2.6	9.9
All Sickness Spells (ln)	4.3	4.4	0.0	4.3	4.3	0.0	-0.0
Excluding Spells $\leq$ 10 days (ln)	4.5	4.5	0.0	4.5	4.5	0.0	0.0

Notes: The table shows the mean values of (ln) long-term sickness duration in days. We sum up all long-term sickness days during the relevant employment period (cumulative duration). Taking into account that sickness is reported after six weeks in our data, we calculate the entire number of absence days, by setting the start date of sickness 42 days before the start date of sickness reported in the data. The differences are not significant at any conventional level. The treatment (control) group consists of workers employed by establishments of 6-9 (12-20) FTE employees who entered the establishment three years before or three years after the reform and who experience at least one long-term sickness spell during their employment in this establishment.
Source: BASiD, own calculations.

Appendix C: Further Results and Robustness Checks

Table C.1: Regression Results Transition Into Long-Term Sickness in the First Year After Entry

	Model 1	Model 2	Model 3	Model 4	Placebo
Post x Treat	-0.002	-0.002	-0.003	-0.002	0.002
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Post	-0.007**	-0.010**	-0.012***	-0.013***	-0.015***
	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)
Treat	-0.002	-0.003	-0.002	-0.002	-0.003
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Female		0.008***	0.010***	0.008***	0.007***
		(0.002)	(0.003)	(0.003)	(0.002)
Age		0.001***	0.001***	0.000	0.000
		(0.000)	(0.000)	(0.000)	(0.000)
Age2		-0.000**	-0.000**	0.000	0.000
		(0.000)	(0.000)	(0.000)	(0.000)
Foreign		-0.013***	-0.009***	-0.004	-0.002
		(0.003)	(0.003)	(0.003)	(0.003)
Qualification, Reference: Medium-skilled					
Low-skilled		-0.015***	-0.012***	-0.008***	-0.012***
		(0.003)	(0.003)	(0.003)	(0.003)
High-skilled		-0.011***	-0.010***	-0.007**	-0.009***
		(0.003)	(0.003)	(0.003)	(0.003)
Cumulative Wages		-0.000	0.000	0.001***	0.001***
		(0.000)	(0.000)	(0.000)	(0.000)
Daily Wage		-0.000***	-0.000***	-0.000***	-0.000***
		(0.000)	(0.000)	(0.000)	(0.000)
White-collar, Reference: Blue-collar		-0.020***	-0.019***	-0.015***	-0.019***
		(0.003)	(0.003)	(0.003)	(0.003)
Occupational Activity, Reference: Craftsman					
Agrar		-0.000	0.007	0.008	-0.002
		(0.008)	(0.009)	(0.009)	(0.009)
Salary		-0.016***	-0.013***	-0.011***	-0.013***
		(0.004)	(0.004)	(0.004)	(0.004)
Sale		-0.016***	-0.013***	-0.013***	-0.012**
		(0.004)	(0.005)	(0.005)	(0.005)
Clerical		-0.014***	-0.011**	-0.008*	-0.009**
		(0.004)	(0.004)	(0.004)	(0.004)
Service		-0.010***	-0.005	-0.004	-0.006
		(0.003)	(0.004)	(0.004)	(0.004)
Residence of Establishment: West Germany			-0.028***	-0.015***	-0.018***
			(0.004)	(0.004)	(0.004)
Cum. Sickness Duration				0.000	-0.001
				(0.000)	(0.005)
Cum. Unemployment Duration				0.000*	0.000*
				(0.000)	(0.000)
Cum. Employment Duration				-0.000***	-0.000***
				(0.000)	(0.000)
Cum. Nonemployment Duration				0.000*	0.000*
				(0.000)	(0.000)
# of Establishment Changes				0.001***	0.001***
				(0.000)	(0.000)
# of Sickness Spells				0.010***	0.013***
				(0.001)	(0.001)
# of Unemployment Spells				0.002**	0.003***
				(0.001)	(0.001)
# of Employment Spells				-0.002**	-0.003***
				(0.001)	(0.001)
# of Non-Employment Spells				-0.000	0.000
				(0.001)	(0.001)
Industry Dummies			✓	✓	✓
Year Dummies		✓	✓	✓	✓
Constant	0.039***	0.071***	0.082***	0.058***	0.073***
	(0.002)	(0.004)	(0.007)	(0.008)	(0.008)
Observations	27,967	27,967	27,967	27,967	29,373
R^2	0.000	0.013	0.017	0.030	0.034

Notes: The table shows results of a linear probability model estimating the probability of a transition into sickness 0 to 12 months after establishment entry; *, ** and *** denote statistical significance at the 10%, 5% and 1% level. Standard errors are in parentheses and are adjusted for clustering at the establishment level. The treatment (control) group consists of workers entering an establishment of 6-9 (12-20) FTE workers. All control variables are measured at the date of entry into the establishment. For the definition and construction of the variables see Tables A.1, A.2 and A.3. The placebo regression hypothetically assumes the dismissal protection reform to take place in 2003.

Source: BASiD, own calculations.

Table C.2: Overview Robustness Estimations Transition Into Long-Term Sickness

Nr.	Description
(1)	Transitions into short sickness periods (less than 10 days) are excluded as these periods may be due to sickness of a child. The health insurance covers the loss of income in case of illness of an individual's child as long as these days of sickness do not exceed ten days per year. Therefore, we cannot infer from the data whether these short sickness periods arise from individuals' own sick days or from those of caring for their ill children.
(2)	Workers in establishments with 0.5-4 FTE workers are used as control group. The individuals of this control group were not subject to the PADA before and after the reform.
(3)	The treatment (control) group consists of more than 5-10 (11-20) FTE workers.
(4)	Short sickness periods for the alternative control group of 0.5-4 FTE workers are excluded.
(5)	The treatment (control) group consists of more than 5-10 (0.5-<5) FTE workers.
(6)	2003-placebo regression using the alternative control group of 0.5-4 FTE workers.
(7)	Probit estimations. *Note:* The marginal effects remain largely unaltered in the non-linear model specifications (results not shown).

Table C.3: Robustness Estimations Transition Into Long-Term Sickness in the Second Year After Entry

	(1)	(2)	(3)	(4)	(5)	(6)
Post x Treat	-0.011*	-0.012**	-0.013***	-0.010*	-0.014***	-0.010
	(0.006)	(0.006)	(0.005)	(0.005)	(0.004)	(0.006)
Post	0.021***	-0.007	-0.011**	-0.002	-0.006	0.001
	(0.005)	(0.004)	(0.005)	(0.004)	(0.004)	(0.005)
Treat	0.006	0.012***	0.004	0.008**	0.010***	0.010**
	(0.004)	(0.005)	(0.004)	(0.004)	(0.003)	(0.005)
Individual Characteristics	✓	✓	✓	✓	✓	✓
Employment-Related Characteristics	✓	✓	✓	✓	✓	✓
Year Dummies	✓	✓	✓	✓	✓	✓
Establishment Characteristics	✓	✓	✓	✓	✓	✓
Individual Employment and Sickness History	✓	✓	✓	✓	✓	✓
Constant	-0.006	0.043***	0.046***	0.027***	0.039***	0.027***
	(0.010)	(0.008)	(0.009)	(0.007)	(0.007)	(0.007)
Observations	8,845	14,548	15,360	14,548	21,172	14,684
R^2	0.022	0.024	0.028	0.022	0.024	0.021

Notes: The table shows results of linear probability models estimating the probability of a transition into sickness 13-24 months after establishment entry. The robustness checks are described in Table C.2. *, ** and *** denote statistical significance at the 10%, 5% and 1% level. Standard errors are in parentheses and are adjusted for clustering at the establishment level. For the definition and construction of the variables see Tables A.1, A.2 and A.3.
Source: BASiD, own calculations.

Table C.4: Selection Analysis I: Individual Characteristics at the Time of Entry

	Individual Illness History			
	(1)	(2)	(3)	(4)
	All sickness periods	Long sickness periods	shrinking est.	growing est.
Post x Treat	-0.003	0.001	-0.006	-0.000
	(0.009)	(0.009)	(0.014)	0.012
Post	-0.028***	-0.031***	-0.022*	-0.034***
	(0.008)	(0.008)	(0.013)	(0.011)
Treat	-0.002	-0.002	-0.004	-0.000
	(0.006)	(0.006)	(0.009)	(0.008)
Individual Characteristics	✓	✓	✓	✓
Employment-Related Characteristics	✓	✓	✓	✓
Year Dummies	✓	✓	✓	✓
Establishment Characteristics	✓	✓	✓	✓
Individual Employment History	✓	✓	✓	✓
Constant	0.212***	0.168***	0.210***	0.210***
	(0.015)	(0.014)	(0.022)	(0.020)
Observations	28,127	28,127	11,813	16,314
R^2	0.367	0.352	0.370	0.368

Notes: The table shows results of a linear probability model estimating the probability of having had at least one sickness period at the time of establishment entry; *, ** and *** denote statistical significance at the 10%, 5% and 1% level. Standard errors are in parentheses and are adjusted for clustering at the establishment level. (1) includes all sickness periods, (2) excludes short sickness periods (less than 10 days) as these periods may be due to sickness of a child, (3) and (4) include all sickness periods; in (3) we confine our sample to establishments with a yearly growth rate smaller than or equal to zero, in (4) we analyse the effects for establishments with a yearly growth rate greater than zero. To calculate the yearly growth rate of an establishment, we compare the number of FTE workers at the beginning of a calender year (usually in January) with the number of FTE workers at the end of the same calender year (usually in December). For the definition and construction of the variables see Tables A.1, A.2 and A.3. In contrast to the other analyses, we do not include the individual illness history as control variable.
Source: BASiD, own calculations.

Table C.5: Selection Analysis II: Individual Characteristics at the Time of Entry

	Age (<25 Years)		
	(1)	(2)	(3)
	All estab.	shrinking estab.	growing estab.
Post x Treat	-0.002	0.007	-0.010
	(0.006)	(0.009)	(0.008)
Post	0.002	-0.000	0.004
	(0.006)	(0.009)	(0.007)
Treat	0.003	-0.006	0.011*
	(0.004)	(0.006)	(0.006)
Individual Characteristics	✓	✓	✓
Employment-Related Characteristics	✓	✓	✓
Year Dummies	✓	✓	✓
Establishment Characteristics	✓	✓	✓
Individual Employment and Sickness History	✓	✓	✓
Constant	0.112***	0.116***	0.107***
	(0.009)	(0.013)	(0.012)
Observations	28,127	11,813	16,314
R^2	0.678	0.674	0.683

Notes: The table shows results of a linear probability model estimating the probability of being younger than 25 years at the time of entry; *, ** and *** denote statistical significance at the 10%, 5% and 1% level. Standard errors are in parentheses and are adjusted for clustering at the establishment level. (1) includes all establishments, in (3) we confine our sample to establishments with a yearly growth rate smaller than or equal to zero, in (4) we analyse the effects for establishments with a yearly growth rate greater than zero. To calculate the yearly growth rate of an establishment, we compare the number of FTE workers at the beginning of a calender year (usually in January) with the number of FTE workers at the end of the same calender year (usually in December). For the definition and construction of the variables see Tables A.1, A.2 and A.3.
Source: BASiD, own calculations.

Table C.6: Selection Analysis III: Probability of Retention One Year After Entry

	(1)
Post x Treat	-0.006
	(0.013)
Post	0.015
	(0.011)
Treat	-0.019**
	(0.009)
Individual Characteristics	✓
Employment-Related Characteristics	✓
Year Dummies	✓
Establishment Characteristics	✓
Individual Employment and Sickness History	✓
Constant	0.281***
	(0.020)
Observations	21,218
R^2	0.123

Notes: The table shows results of a linear probability model estimating the probability of being in the establishment one year after entry; *, ** and *** denote statistical significance at the 10%, 5% and 1% level. Standard errors are in parentheses and are adjusted for clustering at the establishment level. We exclude individuals entering an establishment less than one year before 2004 and before 2006, respectively. For the definition and construction of the variables see Tables A.1, A.2 and A.3. We restrict the sample to persons entering the establishment at least one year before the observation period ends.
Source: BASiD, own calculations.

Table C.7: Heterogeneous Effects: Transition Into Long-Term Sickness in the First Year After Entry

	Female		Male	
	Low-skilled	Medium-skilled	Low-skilled	Medium-skilled
Post x Treat	-0.015	0.011	-0.027**	-0.004
	(0.015)	(0.008)	(0.011)	(0.008)
Post	-0.014	-0.017**	-0.005	-0.013*
	(0.014)	(0.009)	(0.008)	(0.007)
Treat	-0.002	-0.012**	0.017**	-0.001
	(0.010)	(0.006)	(0.009)	(0.005)
Individual Characteristics	✓	✓	✓	✓
Employment-Related Characteristics	✓	✓	✓	✓
Year Dummies	✓	✓	✓	✓
Establishment Characteristics	✓	✓	✓	✓
Individual Employment and Sickness History	✓	✓	✓	✓
Constant	0.054*	0.088***	0.051**	0.046***
	(0.028)	(0.017)	(0.025)	(0.012)
Observations	1,982	8,700	2,995	11,377
R^2	0.035	0.033	0.041	0.031

Notes: The table shows results of a linear probability model estimating the probability of a transition into sickness 0 to 12 months after establishment entry; *, ** and *** denote statistical significance at the 10%, 5% and 1% level. Standard errors are in parentheses and are adjusted for clustering at the establishment level. The treatment (control) group consists of workers entering an establishment of 6-9 (12-20) FTE workers. All controls are measured at the date of entry into the establishment. For the definition and construction of the variables see Tables A.1, A.2 and A.3.
Source: BASiD, own calculations.

Table C.8: Heterogeneous Effects: Transition Into Long-Term Sickness in the Second Year After Entry

	Female		Male	
	Low-skilled	Medium-skilled	Low-skilled	Medium-skilled
Post x Treat	-0.053	-0.009	0.003	-0.025**
	(0.034)	(0.011)	(0.028)	(0.012)
Post	0.027	-0.023**	0.002	-0.015
	(0.024)	(0.009)	(0.027)	(0.012)
Treat	0.024	0.002	0.012	0.013
	(0.023)	(0.008)	(0.022)	(0.010)
Individual Characteristics	✓	✓	✓	✓
Employment-Related Characteristics	✓	✓	✓	✓
Year Dummies	✓	✓	✓	✓
Establishment Characteristics	✓	✓	✓	✓
Individual Employment and Sickness History	✓	✓	✓	✓
Constant	0.087	0.089***	0.010	0.013
	(0.074)	(0.023)	(0.041)	(0.021)
Observations	500	3,356	586	3,245
R^2	0.076	0.050	0.077	0.038

Notes: The table shows results of a linear probability model estimating the probability of a transition to sickness 12 to 24 months after establishment entry; *, ** and *** denote statistical significance at the 10%, 5% and 1% level. Standard errors are in parentheses and are adjusted for clustering at the establishment level. The treatment (control) group consists of workers entering an establishment of 6-9 (12-20) FTE workers. All control variables are measured at the date of entry into the establishment. For the definition and construction of the variables see Tables A.1, A.2 and A.3.
Source: BASiD, own calculations.

Table C.9: Regression Results Duration of Long-Term Sickness

	Model 1	Model 2	Model 3	Model 4	Placebo
Post x Treat	-0.022	-0.023	-0.032	-0.010	-0.031
	(0.118)	(0.114)	(0.114)	(0.113)	(0.100)
Post	0.034	0.051	0.088	0.080	0.006
	(0.069)	(0.090)	(0.093)	(0.093)	(0.085)
Treat	0.088	0.097	0.098	0.087	0.073
	(0.074)	(0.071)	(0.071)	(0.070)	(0.064)
Individual Characteristics	-	✓	✓	✓	✓
Employment-Related Characteristics	-	✓	✓	✓	✓
Year Dummies	-	✓	✓	✓	✓
Establishment Characteristics	-	-	✓	✓	✓
Individual Employment and Sickness History	-	-	-	✓	✓
Constant	4.253***	4.206***	4.320***	4.137***	4.109***
	(0.047)	(0.071)	(0.122)	(0.151)	(0.138)
Observations	1,213	1,213	1,213	1,213	1,273
R^2	0.002	0.085	0.096	0.114	0.102

Notes: The table shows results of a linear regression estimating the ln number of sickness days. We sum up all long-term illness days during the relevant employment period (cumulative duration). Taking into account that sickness is reported after six weeks in our data, we calculate the entire number of absence days, by setting the start date of sickness 42 days before the start date of sickness reported in the data. *, ** and *** denote statistical significance at the 10%, 5% and 1% level. Standard errors are in parentheses and are adjusted for clustering at the establishment level. The treatment (control) group consists of workers working in establishments of 6-9 (12-20) FTE employees who entered the establishment three years before or three years after the reform and who have at least one sickness spell during their employment in this establishment. All controls are measured at the date of entry into the establishment. For the definition and construction of the variables see Tables A.1, A.2 and A.3. The placebo regression hypothetically assumes the dismissal protection reform to take place in 2003.
Source: BASiD, own calculations.

Table C.10: Overview Robustness Estimations Duration of Long-Term Sickness

Nr.	Description
(1)	Transitions into short sickness periods (less than 10 days) are excluded as these periods may be due to sickness of a child. The health insurance covers the loss of income in case of illness of an individual's child as long as these days of sickness do not exceed ten days per year. Therefore, we cannot infer from the data whether these short sickness periods arise from individuals' own sick days or from those of caring for their ill children.
(2)	Restricts the sample to sickness transitions in the first year after entry.
(3)	Restricts the sample to sickness transitions in the second year after entry.
(4)	Uses workers in establishments with 0.5-4 FTE workers as control group.
(5)	The treatment (control) group consists of workers in establishments with 5-10 (11-20) FTE workers.

Table C.11: Robustness Estimations Duration of Long-Term Sickness

	(1)	(2)	(3)	(4)	(5)
Post x Treat	-0.024	-0.047	0.091	0.027	-0.053
	(0.121)	(0.128)	(0.256)	(0.111)	(0.081
Post	0.084	0.057	-0.169	0.057	0.034
	(0.098)	(0.107)	(0.188)	(0.083)	(0.070)
Treat	0.074	0.091	0.126	-0.101	0.058
	(0.075)	(0.080)	(0.158)	(0.071)	(0.051)
Individual Characteristics	✓	✓	✓	✓	✓
Employment-Related Characteristics	✓	✓	✓	✓	✓
Year Dummies	✓	✓	✓	✓	✓
Establishment Characteristics	✓	✓	✓	✓	✓
Individual Employment and Sickness History	✓	✓	✓	✓	✓
Constant	4.337***	4.234***	4.003***	4.636***	4.175***
	(0.168)	(0.179)	(0.341)	(0.155)	(0.113)
Observations	952	871	192	1,228	1,869
R^2	0.127	0.139	0.209	0.102	0.078

Notes: The table shows results of a linear regression estimating the log number of sickness days. The robustness checks are described in Table C.10. *, ** and *** denote statistical significance at the 10%, 5% and 1% level. Standard errors are in parentheses and are adjusted for clustering at the establishment level. The treatment (control) group consists of workers working in establishments of 6-9 (12-20) FTE employees who entered the establishment three years before or three years after the reform and who have at least one sickness spell during their employment in this establishment. For the definition and construction of the variables see Tables A.1, A.2 and A.3.
Source: BASiD, own calculations.

Table C.12: Overview Robustness Estimations Transition Into Unemployment After Long-Term Sickness

Nr.	Description
(1)	Takes also unemployment spells within <30 days after job loss into account.
(2)	Uses workers in establishments with 0.5-4 FTE workers as control group.
(3)	The treatment (control) group consists of 5-10 (11-20) FTE workers.
(4)	Restricts the sample to workers having had at least one long sickness spell (>10 days) during their relevant employment.
(5)	Restricts the sample to individuals being employed one year (at least 355 days) before entering the establishment of interest.

Table C.13: Robustness Estimations Transition Into Unemployment After Long-Term Sickness

	n	Time After First Day of Sickness (Quarter)	1	2	3	4	5
(1)	2,489	Post x Treat	-0.064	0.022	-0.059	0.012	0.110
			(0.047)	(0.061)	(0.065)	(0.083)	(0.108)
(2)	3,275	Post x Treat	-0.038	0.010	-0.071	-0.039	0.099
			(0.044)	(0.057)	(0.064)	(0.081)	(0.097)
(3)	4,088	Post x Treat	-0.029	0.004	-0.013	0.019	-0.039
			(0.036)	(0.043)	(0.048)	(0.058)	(0.064)
(4)	1,912	Post x Treat	-0.048	-0.019	-0.020	0.027	0.092
			(0.051)	(0.070)	(0.077)	(0.098)	(0.112)
(5)	551	Post x Treat	0.032	-0.005	-0.005		
			(0.072)	(0.099)	(0.118)		

Notes: The table shows robustness checks for the differences-in-differences estimations on the probability of involuntary unemployment after sickness (average marginal effects) for each quarter after the first day of sickness (time-discrete logit model). The robustness checks are described in Table C.12. *, ** and *** denote statistical significance at the 10%, 5% and 1% level. Standard errors are in parentheses and are adjusted for clustering at the establishment level. All estimations are controlled for individual characteristics, employment-related characteristics, establishment characteristics, the individual sickness and employment history, and include year dummies.
Source: BASiD, own calculations.

Appendix D: Additional Survey Data Analyses

Table D.1: BiBB/BAuA Employment Survey: Description of Data and Sample

BiBB/BAuA Employment Survey 2012: The *BiBB/BAuA Employment Survey of the Working Population on Qualification and Working Conditions in Germany 2012* is a representative survey among employees in Germany. The participants are at least 15 years old and work at least ten hours per week. The survey realised a response rate of 44.3 percent yielding a representative cross-sectional sample of 20,036 individuals from the active labour force population. The survey data provides information on both the incidence and extent of sickness absence and presenteeism, subjective health status, tenure, stressful working conditions, qualification and professional field as well as socio-demographic variables (for more details see (Rohrbach-Schmidt and Hall, 2013)).

Sample: We restrict our estimation to employees working in establishments from five to nine and 20 to 49 workers. Note that the BiBB/BAuA Employment Survey only collects information on how many individuals are employed by an establishment, regardless of their working time. Trainees are also counted. This means that establishment size cannot be exactly calculated according to the regulations of the PADA (see Table A.5). Thus, the establishment size that is relevant for the applicability of the PADA is likely to be smaller than the information on establishment size available by the BiBB/BAuA Employment Survey. To ensure that we compare individuals with and without dismissal protection, we use employees working in larger establishments as a comparison group. We exclude civil servants as they enjoy special employment protection. We omit individuals working more than 60 hours a week as well as individuals older than 65 years. After these exclusions, we obtain a sample of 2,549 observations with complete data on all relevant covariates.

Table D.2: BiBB/BAuA Employment Survey: Description of Variables

Variable	Definition (Survey Question)/Categories
Dismissal Protection	Dummy variable with value 0 for individuals in establishments with five to nine employees (not protected) and value 1 for individuals working in establishments with 20 to 49 employees (protected).
Presenteeism	*In the last 12 months, did you ever go to work although you should better have called in sick due to your state of health?* If the answer is "yes": *How many workdays was that all in all?* Dummy variables with value 1 for (1) at least one workday of presenteeism, (2) at least five workdays of presenteeism, (3) at least ten workdays of presenteeism and (4) at least 15 days of presenteeism.
Sickness Absence	*Did you stay home sick or have you called in sick in the last 12 months?* If the answer is "yes": *How many workdays was that all in all?* Dummy variables with value 1 for (1) at least one workday of absence, (2) at least five workdays of absence, (3) at least ten workdays of absence and (4) at least 15 days of absence.
Education	*What is your highest general school leaving certificate? Low-skilled*: No degree or highschool degree (Reference category) *Medium-skilled*: Completed vocational or professional training *High-skilled*: Technical college degree or university degree
Subjective Health Status	*How would you describe your general state of health? Answer categories: excellent, very good, good, not so good, bad*; Reference category: good.
Income	*What is your gross monthly income?*; measured in 100 EUR.
Working Hours	*What are the weekly working hours in your occupational activity according to the agreement with your employer, excluding overtime?*; working hours >=61 are excluded.
Job Satisfaction	*And now, as an overall summary: How satisfied are you with your entire occupational activity? Answer categories: very satisfied, satisfied, less satisfied, not satisfied.*; Dummy Variable with value 0 for "less satisfied" and "not satisfied" and value 1 for "very satisfied" and "satisfied".
# of Working Strains	Following Kroll (2011), we cluster working strains into three categories with seven items for each category. *Physical Strains*: E.g., exposure to cold, heat, moisture, humidity, or draughts, handling of hazardous substances *Psychical Strains*: E.g., working under strong pressure of time or performance, repetitive tasks, work is disturbed or interrupted *Social Strains*: E.g., emotionally straining situations, perceived importance of work, being part of a community If the answer to a certain strain is positive, the individuals were further asked: *Is that stressful for you?*. Following Hirsch et al. (2017), we sum up those strains by which individuals feel stressed.

Table D.3: BiBB/BAuA Employment Survey: Descriptive Statistics

	(1) Without DP		(2) With DP			
	Mean	S. D.	Mean	S. D.	Mean (2)-(1)	
Sickness Absence (Incidence)	0.465	0.499	0.558	0.497	0.094	***
Presenteeism (Incidence)	0.605	0.489	0.605	0.489	0.000	
Female	0.676	0.468	0.569	0.495	-0.106	***
Age	44.580	10.870	45.574	10.765	0.993	**
Partner in Household (Dummy)	0.616	0.487	0.623	0.485	0.007	
Child(ren) in Household (Dummy)	0.351	0.478	0.314	0.464	-0.037	*
Education						
Low-skilled	0.150	0.357	0.233	0.423	0.083	***
Medium-skilled	0.687	0.464	0.606	0.489	-0.081	***
High-skilled	0.163	0.370	0.161	0.367	-0.002	
Health status						
Excellent	0.090	0.286	0.075	0.263	-0.015	
Very good	0.227	0.419	0.214	0.410	-0.013	
Good	0.537	0.499	0.545	0.498	0.007	
Not so good	0.127	0.333	0.141	0.348	0.014	
Bad	0.019	0.138	0.025	0.157	0.006	
Income in 100 EUR	19.902	21.142	24.948	22.741	5.046	***
Tenure (in years)	10.668	9.447	12.945	10.679	2.277	***
Working Hours	31.619	10.049	33.786	8.992	2.167	***
Occupational Status: White-collar	0.853	0.355	0.810	0.392	-0.042	***
Job Satisfaction	0.932	0.252	0.909	0.288	-0.023	**
# of Straining Working Conditions						
# of Physical Strains	0.871	1.574	1.032	1.831	0.161	**
# of Psychical Strains	1.286	1.777	1.527	1.857	0.242	***
# of Social Strains	0.230	0.643	0.294	0.698	0.064	**
Branch of Industry						
Industry Sector	0.049	0.215	0.128	0.335	0.080	***
Craft Sector	0.211	0.408	0.135	0.342	-0.076	***
Commerce Sector	0.173	0.379	0.154	0.361	-0.020	
Other Services	0.279	0.449	0.234	0.423	-0.045	**
Another Sector	0.083	0.276	0.062	0.241	-0.021	**
Public Service Sector	0.205	0.404	0.287	0.453	0.082	***
Observations	882		1,667			

Notes: The table reports descriptive statistics for relevant characteristics of individuals with and without dismissal protection (DP) (according to establishment size). *, ** and *** denote statistical significance of the difference in the mean between the treatment and control group at the 10%, 5% and 1% level (t-test). For the definition and construction of the variables see Table D.2. *Source:* BiBB/BAuA Employment Survey 2012, own calculations.

Table D.4: Determinants of Absence and Presenteeism (Marginal Effects)

	Sickness Absence	Presenteeism
Dismissal protection	0.079***	-0.012
	(0.020)	(0.019)
Female	0.040*	0.094***
	(0.023)	(0.021)
Age	-0.006***	-0.006***
	(0.001)	(0.001)
Partner in household (Dummy)	0.009	0.017
	(0.020)	(0.019)
Child(ren) in household (Dummy)	-0.015	-0.009
	(0.022)	(0.021)
Education, Reference: Low-skilled		
Medium-skilled	0.010	0.039
	(0.028)	(0.026)
High-skilled	-0.018	0.020
	(0.035)	(0.032)
Health status, Reference: Good		
Excellent	-0.192***	-0.222***
	(0.036)	(0.032)
Very Good	-0.107***	-0.167***
	(0.024)	(0.021)
Not so Good	0.156***	0.203***
	(0.030)	(0.031)
Bad	0.267***	0.233***
	(0.075)	(0.083)
Income in EUR	0.000	-0.000
	(0.000)	(0.000)
Tenure	-0.001	0.000
	(0.001)	(0.001)
Working Hours	0.003***	0.002*
	(0.001)	(0.001)
Occupational Status: White-collar (Dummy)	0.028	0.018
	(0.028)	(0.027)
Job Satisfaction	-0.103**	-0.067
	(0.040)	(0.042)
Number of Straining Working Conditions		
# of Physical Strains	0.008	0.038***
	(0.007)	(0.007)
# of Psychical Strains	-0.001	0.036***
	(0.006)	(0.006)
# of Social Strains	0.023	0.046***
	(0.016)	(0.017)
Branch of Industry, Reference: Public Service Sector		
Industry Sector	-0.090**	0.006
	(0.037)	(0.034)
Craft Sector	-0.091***	0.025
	(0.033)	(0.031)
Commerce Sector	-0.139***	-0.016
	(0.031)	(0.029)
Other Services	-0.083***	-0.021
	(0.027)	(0.025)
Another Sector	-0.043	-0.034
	(0.041)	(0.038)
Observations	2,549	2,549

Notes: The table shows the average marginal effects from probit regressions. *, ** and *** denote statistical significance at the 10%, 5% and 1% level. For a detailed description of the variables, see Table D.2.
Source: BiBB/BAuA Employment Survey 2012, own calculations.