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ORIGINAL ARTICLE

Dismissal protection and long-term sickness absence: Evidence from a policy change

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Research (IAB), Regensburger Str. 104,
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Email: nicole.guertzen@iab.de**Abstract**

This paper studies whether a decline in employment protection reduces workers' long-term sickness absences (of >6 weeks). We exploit exogenous variation from a German policy change that shifted the threshold exempting small establishments from dismissal protection from 5 to 10 workers. Using German register data, we find that the reform significantly reduced employees' transitions into long-term sickness during their second year after being hired. This response is due to a behavioral rather than a compositional effect and is particularly pronounced among the medium-skilled and younger males. Further results indicate that the reform did not alter the probability of involuntary unemployment after sickness.

JEL CLASSIFICATION

D02, I12, J28, J63, K31

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 (Nicholson et al., 2005; Pauly et al., 2008). 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 a higher risk of involuntary unemployment (Chadi & Goerke, 2018).

Many OECD countries implement social policies that aim to reduce these risks for individuals by providing income replacement in 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 behavior,

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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 & 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). While some studies have already focused on long-term sickness absence in the context of sick pay (e.g., Ziebarth, 2013), little is known about the effect of dismissal protection on long-term sickness absence. Given that individuals' perception of the risk of being dismissed is likely to depend on the associated income loss and the health impairment, the generosity of dismissal protection may be expected to be of greater relevance for long-term than for short-term sickness. Moreover, compared with short-term absences, employers may have larger incentives to replace and possibly dismiss long-term sick workers due to a higher loss of human capital and larger production disruptions.

The present paper attempts to fill this gap and analyzes 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, long-term sickness absences are important from a quantitative point of view, as in 2018, approximately 42% of all absence days were due to long-term sickness lasting more than 6 weeks (Meyer et al., 2019). Second, Germany is characterized 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 out in the Protection Against Dismissal Act (PADA). However, German legislation exempts small establishments with a number of employees up to a certain threshold from dismissal protection. In the course of a major labor market reform in 2004, the threshold for establishment exemption from dismissal protection was raised from 5 to 10 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 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 with this data set, which enables us to perform a precise calculation of establishment size. To better understand the underlying behavioral 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 that 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 affected by the policy change. Scoppa (2010) analyzes the 1990 policy reform in Italy that raised employment protection for workers in small firms—although not to the same level of protection as that applicable to workers in larger firms. After the reform, small firms could choose between the reemployment of affected workers or the payment of financial compensation if a dismissal was judged unfair. Overall, the results of this study point to a significant increase in sickness absence in the affected firms.

¹There is no official definition of long-term sickness. This study focuses on spells of more than 6 weeks according to the definition used by the health insurance system: The latter uses eligibility for sick pay as the threshold to distinguish between short- and long-term illnesses (see, e.g., Knieps & Pfaff, 2015; Meyer et al., 2019).

We contribute to the literature in three ways. First, our analysis exploits a reform that involved a more encompassing change in dismissal costs for small establishments (those employing more than five and up to 10 employees). In contrast to the Swedish context (Lindbeck et al., 2006; Olsson, 2009), the German reform, by relaxing employment protection regulations for small establishments, affected not only dismissals due to redundancies but also dismissals that may arise for any other reason. Most importantly, the policy change also covers dismissals due to personal incapability, a reason that is especially relevant in the context of absence behavior. Moreover, in contrast to the Italian case, small establishments in the affected size class in Germany 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 out 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 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., Bauer et al., 2007; Bauernschuster, 2013; Boeri & Jimeno, 2005; Lindbeck et al., 2006; Olsson, 2009). In our analysis, we explicitly identify the group of individuals affected by the reform. A grandfathering clause implied that the policy change was confined to workers hired by the affected establishments after the reform. Tracking the illness histories of individuals affected by the policy change enables us to address the question of whether a change in employment protection impacts particular groups of workers. 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 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 absences that arises merely from different selection of workers in establishments.

Previewing our results, we find that the reform significantly reduced employees' transitions into long-term sickness during their second year after entry into the establishment. Based on a number of selectivity analyses, we argue that this response is due to a behavioral rather than a compositional effect. First, we show that the results can neither be explained by a different selection of workers into establishments nor by employers changing their hiring behavior. Second, we also rule out dynamic selection as an explanation of our findings as the reform did not affect newly hired individuals' retention probability in the second year after establishment entry. Looking at effect heterogeneities, we find the response to be particularly pronounced among the medium-skilled and younger males, for whom we also can rule out compositional effects. Our results provide no evidence of a reform effect on the duration of long-term sickness absences, however. We also find that the reform did not alter the probability of involuntary unemployment after sickness. This is in line with findings from 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 establishments than employees themselves who respond to changes in dismissal protection. Regarding the behavioral mechanisms, we conduct a complementary analysis based on survey data in order to explore whether our main findings reflect a decline in absenteeism without being sick or an increase in presenteeism. Overall, the survey-level results do not allow us to rule out either mechanism as an explanation.

The remainder of the paper is structured as follows: In [RELATED LITERATURE](#), we give an overview of the theoretical and empirical literature regarding long-term sickness absence. [THE GERMAN INSTITUTIONAL BACKGROUND](#) illustrates the German institutional setting before [EMPIRICAL STRATEGY, DATA, AND VARIABLES](#) presents the data set and the empirical strategy. [ESTIMATION AND RESULTS](#) and [MECHANISMS](#) provide the empirical results, and [SUMMARY AND CONCLUSIONS](#) concludes.

RELATED LITERATURE

It is well established that individuals may have some discretion over their sickness behavior in the form of absenteeism² or presenteeism. Empirical studies provide extensive evidence of both types of behavior.³ To the extent that individuals may vary their sickness behavior, they are likely to trade off their utility from being absent against the financial- and employment-related costs. In certain situations, the benefits of absence may be high. This may be the case when a period of recovery from an illness is necessary or, in the case of moral hazard, if the disutility from work is large, for example, due to unfavorable working conditions (Barmby et al., 1994; Brown & 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 in an employee's absence decision. The expected costs of absence rise (i) with a lower income replacement level during a sickness episode (Brown & Sessions, 2004; Chen et al., 2020; Pichler & Ziebarth, 2017; Puhani & Sonderhof, 2010; Ziebarth & Karlsson, 2010, 2014) and (ii) with a decreasing strictness of employment protection (Brown & Sessions, 2004; Ichino & 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 than 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 positively affects sickness absences.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 lower productivity and, in the case of absenteeism, lower motivation than 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 (Chadi & Goerke, 2018; Hesselius, 2007; Markussen, 2012; Scoppa & Vuri, 2014).

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

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

⁴In addition to this strand of literature, there are studies that look at the role of other institutions and perceived job security for both types of sickness behavior. 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 benefit from dismissal protection, is completed. On the other hand, Hansen and Andersen (2008) show that a higher extent of perceived job insecurity is associated with higher levels of being present at work despite sickness.

THE GERMAN INSTITUTIONAL BACKGROUND

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 6 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 12 months, as long as these are caused by the same disease diagnosis. During this mandatory period of up to 6 weeks, the employer is obliged to provide short-term sick pay, which amounts to a replacement ratio of 100% of individuals' earnings.

After 6 weeks of illness with the same disease diagnosis, employees are entitled to long-term sick pay provided by statutory health insurance. The latter covers the majority (approximately 90%) of the German population and is mandatory for all employees subject to social security contributions whose earnings fall short of the statutory health insurance contribution limit.⁷ The replacement level for persons receiving long-term sick pay by statutory health insurance is stipulated in the German Social Code. Since its last reform in 1997, long-term sickness pay has amounted to a replacement ratio of 70% 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 of sustaining the long-term employability of individuals who are still in the labor force. Thus, unlike disability insurance schemes, long-term sick pay offers no possibility of permanently withdrawing from the labor market. The nonpermanent character of sick pay is reflected not only in the limited entitlement duration⁸ but also in two salient features of sick pay regulations. First, individuals receiving long-term sick pay may be monitored by the health insurance auditing system. The medical service run by the statutory health insurance system is entitled to audit individuals' sickness absence if the latter expresses profound suspicions about any potential abuse of the sick pay system. Such audits may be performed based on either an assessment of the documentation provided by the medical doctor who ascertained the individual's inability to work or a personal assessment of the individual's ability to work by the service's medical staff (see Görtzgen & Hank, 2018). Second, individuals who experience 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.

Dismissal protection regulation

Compared to those in 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

⁵This 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 starting from the first day of illness.

⁶An exception concerns illness during the first 4 weeks after an employee begins working for a new employer. During this period, employers are not obliged to provide sick pay, such that employees receive sick pay from their health insurance.

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

⁸The maximum duration of long-term sick pay for the same disease is 78 weeks within a period of 3 years.

more than 6 months who are employed by an establishment with a certain minimum number of employees (currently more than 10 full-time-equivalent employees). Establishments operating up to 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 judgment 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 must meet some conditions, such as the employee having a negative long-term health prognosis.⁹ For employers, such justification requirements are associated with costs.

Moreover, establishments are typically required to inform the works council, where such a worker representative body exists, about a dismissal. Consultation with the works council is mandatory for both individual and collective redundancies. The latter generally requires 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 in 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 in Court. Overall, these considerations highlight that any dismissal subject to the PADA—due either to uncertainty about which dismissals are considered just or to sanctions or severance payments—is likely to be much costlier than a comparable dismissal outside the scope of the PADA.

Key to our analysis is that the PADA applies only to establishments exceeding a stipulated establishment size. Over the last decades, the threshold for applicability has changed several times, from 5 to 10 full-time-equivalent employees (FTEs) in October 1996, back to five FTEs in January 1999, and then back again to 10 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

In what follows, we exploit the 2004 reform to identify the effect of dismissal protection on long-term sickness absences. This policy reform was part of the so-called AGENDA 2010, a large reform package implemented between 2003 and 2005. This package aimed at reducing Germany's high structural unemployment in the early 2000s. While the agenda included a considerable number of labor market and social policy changes, only the PADA reform exhibited variation across establishment size classes. Thus, our analysis of the causal effect of weaker dismissal protection should not be confounded by other elements of the AGENDA 2010. 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 in the threshold from 5 to 10 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 1, 2004). This suggests that neither the affected employees nor the affected establishments could anticipate the exact details of the reform and change their behavior accordingly (Bauernschuster, 2013).

⁹Note that this is different from regulations in other countries, such as Norway, where individuals enjoy special dismissal protection during long-term sickness (Fevang et al., 2014).

¹⁰Table A.5 in the [Appendix S1](#) describes how the establishment size is calculated on the basis of the PADA.

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

EMPIRICAL STRATEGY, DATA, AND VARIABLES

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 [Dismissal protection regulation](#), this reform raised the threshold up to which establishments are exempted from dismissal protection from 5 to 10 full-time-equivalent workers. Due to transitory regulations that (normally) granted dismissal protection to those already employed in an establishment before 2004, the reform affected only employees *entering* an establishment with more than 5 to 10 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, one with more than 10 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 of January 1, 2001 to June 30, 2003 or January 1, 2004 to June 30, 2006.¹² As we observe the treatment and control groups 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 is that time trends would be the same for both the treatment and control groups in the absence of the treatment (Blundell & Costa Dias, 2009). Furthermore, the stable unit treatment value assumption (SUTVA) states that 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 that we could track the same individuals 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 of 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:

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

In [Equation \(1\)](#), 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, that is, 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. Furthermore, in the 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 & Miller, 2015).

¹²For more details on the definition of establishment entry, see Table A.6 in the [Appendix S1](#).

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 constrain their size up to the threshold value of five FTE workers before the reform. After the reform, they may have expanded up to the new threshold size of 10 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 from 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 of June 30 (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 B1 in the [Appendix S2](#)).

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 1% 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 yet retired (for details, see Hochfellner et al., 2012). The data provide longitudinal information on individuals' entire pension-relevant biographies up to 2007. The 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 sectors and thus excludes only civil servants and self-employed individuals. As a consequence, the insurance covers more than 90% of the entire population for whom all past pension-relevant periods have been recorded.

The Pension Register provides information on all pension-relevant periods, that is, periods for which contributions were paid (such as employment, long-term illness and unemployment) and periods without contributions that were still creditable for 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, and monthly earnings, which can be calculated by exploiting information on pension credit points gained from social security employment. Table A1 in the [Appendix S1](#) 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 illness spells subject to sick pay covered by mandatory health insurance. As spelled out earlier, the latter comes into effect after a period of 6 weeks of absence and may cover spells during either employment or unemployment.¹³ The recorded sickness spells may also cover long-term rehabilitation measures aimed at reintegrating long-term ill individuals into the labor market. A potential concern is that sickness spells recorded by the Pension Register may also include caring periods for ill children below age 12. However, these periods are capped

¹³Note that our data therefore do not allow us to explore the effects on individuals' own short-term spells. Given that individuals may exhibit many short spells, the latter would also be interesting to look at in the context of dismissal protection.

at a maximum length of 10 days/year/child.¹⁴ In our empirical analysis, we 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 Labor Market Biographies provide further time-varying individual information on educational status (three categories) and an establishment identifier.¹⁵ The latter allows us to identify newly hired employees and to gain information on tenure at the current employer. Table A3 in the [Appendix S1](#) provides a more detailed description of the variables gained from the Employment Statistics Register.

Sample selection and descriptives

As spelled out earlier, we define workers entering an establishment with up to 10 FTE workers as our treatment group, whereas the control group consists of individuals entering an establishment with a size slightly above the threshold, that is, with more than 10–20 FTE workers. We carry out a somewhat more precise calculation of establishment size than that in previous studies using the number of workers—regardless of their working time—on a particular date. Unlike previous studies (see, e.g., Bauer et al., 2007; Bauernschuster, 2013), we approximate the number of FTE workers and take into account annual fluctuations in the workforce.¹⁶ Calculating the establishment size as precisely as possible is crucial for correctly assigning individuals to either the treatment or the control group in our differences-in-differences setup. 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 imprecision. 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 12–20 (control group) FTE employees.¹⁷ We further ensure that the establishments remain in the same size group during the period when a worker is employed in this establishment.¹⁸

The descriptive statistics reflect some systematic differences in the gender composition as well as the occupational and industry structure across treated and controlled individuals before and after the reform (see Tables B1 and B2 in the [Appendix S2](#)). This highlights the importance of including these observables as controls in 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 major differences concerning individuals' employment and illness histories across treated and controlled individuals.

¹⁴Given that the pension insurance records episodes of illness after a period of 6 weeks (42 days), the duration of illness spells usually amounts to 42 plus the number of days recorded by the Pension Insurance. For spells up to 10 days, we are not able to distinguish whether they arise from caring periods for ill children (and therefore represent true short spells) or whether they represent an insured individual's own long-term sickness. For example, a spell of 5 days may either represent a 5-day caring period for ill children or an illness spell with a duration of 47 days.

¹⁵Note that the legal definition of "establishment" does not exactly match the establishments identified by the establishment identifier of the Establishment History Panel (on the definitions of an establishment, see Table A.4 in the [Appendix S1](#)). However, according to the establishment panel—a representative survey of establishments in Germany—a large majority of establishments are independent companies without any other places of business (see Figure A.1 in the [Appendix S1](#)). We can expect these establishments to be covered by both the legal definition and the definition in the administrative data.

¹⁶For details on how we calculate the establishment size, see Table A.6 in the [Appendix S1](#).

¹⁷In [Robustness checks](#), we conduct robustness checks with respect to this restriction.

¹⁸The extent to which this restriction may bias our results depends on whether the reform caused establishments to self-select into certain size classes. In Figure B.1 in [Appendix S2](#), we provide evidence that the distribution of establishments across size classes remained broadly unaltered over the observation period.

ESTIMATION AND RESULTS

Incidence and duration of long-term sickness

Descriptive results

Figure 1 shows the cumulative incidence of sickness for the treatment and the control groups during the first 2 years after establishment entry. In the prereform period, the evolution of this outcome exhibits no major differences across treated and controlled individuals. In the postreform 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 than the control group. The figures also show that the transition into a long-term sickness episode is a rather rare event; only 4.6% and 3.7% of individuals in

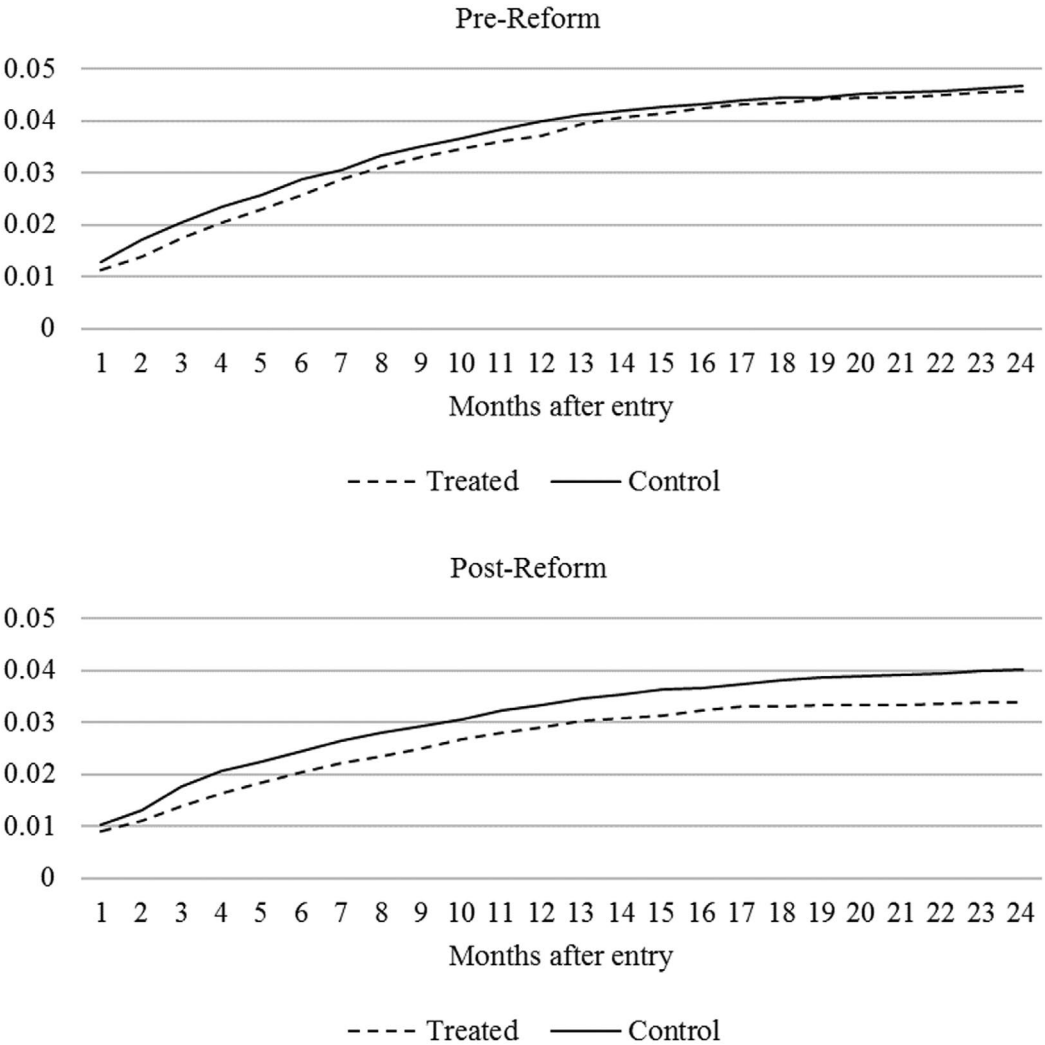


FIGURE 1 Cumulative incidence of long-term sickness. The treatment (control) group consists of workers working in establishments of 6–9 (12–20) FTE employees who entered the establishment 3 years before or 3 years after the reform. We calculate the share of workers having at least one long-term sickness period until the respective month after entry. Source: BASiD, own calculations.

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.

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 a worker experiencing a transition into sickness in the first and 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 already ill at establishment entry, resulting in a sample of 27,967 observations.¹⁹ 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 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 8845 observations. We address these issues in [Selection analysis](#).

We estimate four models, which are incrementally augmented by different sets of explanatory variables. The first model is the 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 (daily wage, working time, occupational status, and occupational sector), and year dummies. The third model also includes establishment characteristics, in particular the location of the establishment (West vs. East Germany) and 10 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 nonemployment spells, and the number of establishment changes.

For the *first year* after a worker enters the establishment, the multivariate analyses do not provide any evidence of a reform effect on the incidence of long-term sickness episodes (see [Table 1](#)). The coefficient on the interaction term is negative and becomes very small and insignificant after we control for differences in observables. The results for the reform's medium-run effect—the effect on the probability of experiencing a long-term sickness spell in the *second year* after establishment entry—are shown in [Table 2](#). 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 experiencing a transition into sickness in the second year is 2.4%, this effect is fairly large. The group effect is positive but insignificant. The last column in [Table 2](#) 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 for either the first or the second year, thereby supporting the parallel trend assumption.²⁰

Robustness checks

In this section, we explore whether the results from [Table 2](#) are robust to several sensitivity checks (for an overview, see [Table C1](#) in the [Appendix S3](#)). First, we exclude illness spells

¹⁹A total of 160 individuals in our sample (0.6%) entered the establishment while 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.

²⁰We also carried out another placebo test for 2002. Even though the coefficient of the interaction term for the second year is significant at the 10% level, it has a different sign (positive) than we would expect if the parallel trend assumption was violated (results not shown, but available upon request). Overall, we can therefore conclude that the parallel trend assumption is justified.

TABLE 1 Regression results on the transition into long-term sickness in the first year after entry.

	Model 1	Model 2	Model 3	Model 4	Placebo
Post × Treat	−0.008** (0.003)	−0.002 (0.004)	−0.003 (0.004)	−0.002 (0.004)	0.002 (0.004)
Treat	0.001 (0.003)	−0.003 (0.003)	−0.002 (0.003)	−0.002 (0.003)	−0.003 (0.003)
Female		0.008*** (0.002)	0.010*** (0.003)	0.008*** (0.003)	0.007*** (0.002)
Age		0.001*** (0.000)	0.001*** (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*** (0.003)	−0.009*** (0.003)	−0.004 (0.003)	−0.002 (0.003)
<i>Qualification, reference: Medium-skilled</i>					
Low-skilled		−0.015*** (0.003)	−0.012*** (0.003)	−0.008*** (0.003)	−0.012*** (0.003)
High-skilled		−0.011*** (0.003)	−0.010*** (0.003)	−0.007** (0.003)	−0.009*** (0.003)
Cumulative wages		−0.000 (0.000)	0.000 (0.000)	0.001*** (0.000)	0.001*** (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*** (0.003)	−0.019*** (0.003)	−0.015*** (0.003)	−0.019*** (0.003)
<i>Occupational activity, reference: Craftsman</i>					
Agrar		−0.000 (0.008)	0.007 (0.009)	0.008 (0.009)	−0.002 (0.009)
Salary		−0.016*** (0.004)	−0.013*** (0.004)	−0.011*** (0.004)	−0.013*** (0.004)
Sale		−0.016*** (0.004)	−0.013*** (0.005)	−0.013*** (0.005)	−0.012** (0.005)
Clerical		−0.014*** (0.004)	−0.011** (0.004)	−0.008* (0.004)	−0.009** (0.004)
Service		−0.010*** (0.003)	−0.005 (0.004)	−0.004 (0.004)	−0.006 (0.004)
Residence of establishment: West Germany			−0.028*** (0.004)	−0.015*** (0.004)	−0.018*** (0.004)
Cum. sickness duration				0.000 (0.000)	−0.001 (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)
No. of establishment changes				0.001*** (0.000)	0.001*** (0.000)

(Continues)

TABLE 1 (Continued)

	Model 1	Model 2	Model 3	Model 4	Placebo
No. of sickness spells				0.010*** (0.001)	0.013*** (0.001)
No. of unemployment spells				0.002** (0.001)	0.003*** (0.001)
No. of employment spells				−0.002** (0.001)	−0.003*** (0.001)
No. of nonemployment spells				−0.000 (0.001)	0.000 (0.001)
Industry dummies			✓	✓	✓
Year dummies		✓	✓	✓	✓
Constant	0.036*** (0.001)	0.071*** (0.004)	0.082*** (0.007)	0.058*** (0.008)	0.073*** (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

Note: The table shows results of a linear probability model estimating the probability of a transition into sickness 0–12 months after establishment entry; *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels. 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. Number of treated (control) observations: 11,224 (16,743). 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 that the dismissal protection reform took place in 2003.

Source: BASiD, own calculations.

lasting no longer than 10 days, as these spells may also result from leave periods due to the sickness of a child. During our observation period, 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 10 days per year. Therefore, we cannot infer from the data whether these short sickness periods arise from individuals' own sick days or from spells of caring for their ill children (see [Data](#)). 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 C2 in the [Appendix S3](#): When we exclude 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 (potentially child-related) recorded sickness spells lasting no longer than 10 days. The coefficients of the other estimates are all comparable in magnitude to those in [Table 2](#) and significant at the 5% level at least. However, the estimates in column (6) suggest that the placebo estimate using the alternative control group is weakly significant and of the same order of magnitude as the baseline estimate. This indicates that new hires in very small establishments are less suited as control individuals compared to our baseline control group (new hires in establishments with 12–20 FTE employees).

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 of which mechanisms drive this result is still open. On the one hand, the established effect

TABLE 2 Regression results on transitions into long-term sickness in the second year after entry.

	Model 1	Model 2	Model 3	Model 4	Placebo
Post × Treat	−0.014*** (0.005)	−0.014** (0.007)	−0.014** (0.007)	−0.013** (0.007)	−0.001 (0.007)
Treat	0.008 (0.005)	0.008 (0.005)	0.008 (0.005)	0.008 (0.005)	0.005 (0.005)
Individual characteristics	-	✓	✓	✓	✓
Employment-related characteristics	-	✓	✓	✓	✓
Year dummies	-	✓	✓	✓	✓
Establishment characteristics	-	-	✓	✓	✓
Individual employment and sickness history	-	-	-	✓	✓
Constant	0.024*** (0.002)	0.059*** (0.007)	0.055*** (0.010)	0.038*** (0.012)	0.037*** (0.012)
Observations	8845	8845	8845	8845	9188
R^2	0.001	0.018	0.019	0.030	0.021

Note: The table shows the results of a linear probability model estimating the probability of a transition to sickness 13–24 months after establishment entry; *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels. 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. Number of treated (control) observations: 3399 (5446). 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 S1](#). The placebo regression hypothetically assumes that dismissal protection reform took place in 2003.

Source: BASiD, own calculations.

might result from a true behavioral effect on the part of newly hired individuals who adapted their sickness behavior to the weaker dismissal protection regulations. On the other hand, the change in sickness absence might arise from a different selection of workers in establishments. First, individuals with a high propensity toward long-term illness might systematically select themselves into establishments with stricter employment protection. Second, due to the weaker dismissal protection, employers in the affected size class might have altered their hiring behavior. Relatedly, Bauernschuster (2013) shows that the reform considered here had a positive effect on hiring rates. In addition to increasing their hiring rates, employers might have become less selective in their hiring behavior and more likely to hire individuals with a higher long-term sickness propensity (Olsson, 2009). Note that such an effect would run counter to potential selection mechanisms on the workers' side. At the same time, less cautious hiring behavior might also affect the propensity to hire workers with less experience. These are often young workers who, at the same time, exhibit a lower long-term sickness propensity. 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 analyze whether the reform affected the probability of hiring an individual who had at least one long-term sickness period before entering the establishment.²¹ Second, we explore whether individuals with unfavorable health conditions opt out of searching for a job in small establishments with weaker employment protection. To do this, we again analyze whether the reform affected the probability of hiring an

²¹In doing so, we impose the assumption that individuals' long-term sickness propensity 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.

individual who had at least one long-term sickness period before entering the establishment but now with a treatment (control) group that consists of workers entering an establishment of 12–20 (22–30) FTE workers. The idea behind this test is to check whether the reform induced individuals with potentially unfavorable health conditions to select themselves into the size class directly bordering the real treatment group. Third, we analyze whether the reform affected the propensity to hire younger workers below age 40. Given that the propensity to make risky hires might vary across different employers, we perform all analyses separately for shrinking/nongrowing 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). Furthermore, as we find a significant reform effect on the probability of experiencing a long-term sickness spell in the *second* year after establishment entry, a compositional change could be relevant in particular for the sample of workers for which we identify the effect, that is, workers with a tenure of at least 1 year. Thus, we also perform all selection analyses for this restricted sample. The differences-in-differences estimates of the effects on the health composition are shown in Table C3 (6–9 vs. 12–20) and Table C.4 (12–20 vs. 22–30) in the [Appendix S3](#). The results with respect to the age composition are shown in Table C.5 in the [Appendix S3](#). The bottom panels of the tables show the estimates based on the restricted samples (tenure ≥ 1 year). Overall, the results show that the estimated reform effects on the composition of newly hired workers are insignificant at any conventional level and small in size.²² This leads us to conclude that the results provide little evidence of any compositional selection effects for either the full or the restricted sample.²³

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 because, on the one hand, the reform may have induced treated individuals to leave their employer earlier than in the prereform setting. On the other hand, weaker employment protection regulations may also have caused establishments to dismiss sick and therefore less productive employees faster 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 S3](#)). 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 2](#) is driven by neither a compositional nor a dynamic selection effect.

Heterogeneous effects

The perceived costs of less generous employment protection are likely to vary with individuals' labor market attachment and households' dependency on the affected individuals' labor earnings. To address differences in labor market attachment, we distinguish between gender and skill groups. To further capture potential differences in the dependency on the affected individuals' labor earnings, we break down the estimates by age classes (distinguishing individuals

²²Regarding the estimation with a different control and treatment group (see Table C.4), the reform effect for individuals in growing establishments (column (4)) who have a tenure of at least 1 year is positive and quite substantial in size. Note, however, that this effect is still not significantly different from zero and applies only to a subsample. For the entire sample, there is no indication of compositional selection effects.

²³The results are also insignificant when estimating the propensity to hire younger workers below age 25 (results not shown but available upon request).

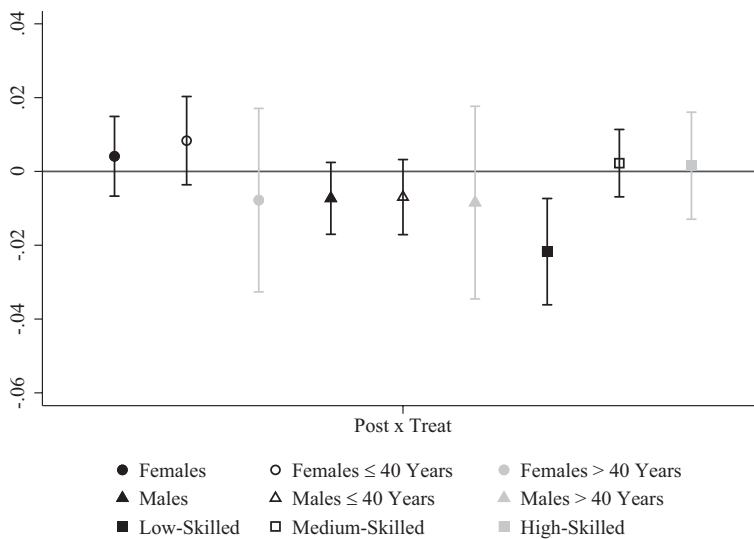


FIGURE 2 Transition into long-term sickness: heterogeneous effects (first year). The figures show the coefficients of the differences-in-differences estimations with 90% confidence intervals stratified by gender and qualification for the first year after establishment entry. Number of observations: females: 11,920; females ≤40 years: 9430; females >40 years: 2490; males: 16,047; males ≤40 years: 12,620; males >40 years: 3427; low-skilled: 4977; medium-skilled: 20,077; high-skilled: 2913.
Source: BASiD, own calculations.

aged below 41 years or older than 40 years).²⁴ Figures 2 and 3 show the results for the different groups for the first and second years after establishment entry.

For the low-skilled, the estimates point to a significantly negative reform effect already in the first year. For males, we also obtain a negative reform effect of about 1 percentage point in the first year, which is, however, very imprecisely estimated. Turning to the second year, the estimates shown in Figure 3 indicate that the negative reform appears to be driven by men aged below 41 years.²⁵ The effect for this subgroup is larger in magnitude (about 2 percentage points) than the result in the baseline specification. Overall, the results suggest that younger male workers in particular respond to the change in dismissal protection. As to skill groups, the reform appears to have a particularly negative effect on the medium-skilled in the second year after establishment entry. Low-skilled workers also exhibit a negative reform effect which is very imprecisely estimated. Note that these results are broadly consistent with the evidence provided by Ziebarth (2013) suggesting that prime-age workers and those in the bottom part of the earnings distribution 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 on male workers' earnings. Turning to the high-skilled, this group even exhibits a positive coefficient, which borders the 10% significance level.

To explore whether the heterogeneous effects might be driven by composition effects, we repeat the specifications from Table C.3 separately by subgroups. The results are shown in Tables C.7 and C.8 in the Appendix S3. For the high-skilled, the estimates in Table C.7 do not point to any positive reform effects on the probability of hiring high-skilled workers who

²⁴As we do not have information on individuals' household composition, we rely on this distinction, which aims to capture potential mothers (Müller & Strauch, 2017) and male workers' probability of having a breadwinner status.

²⁵Note that this result renders it unlikely that our effects on short-term spells are mainly driven by caring periods for ill children, as these periods were more frequently recorded for females.

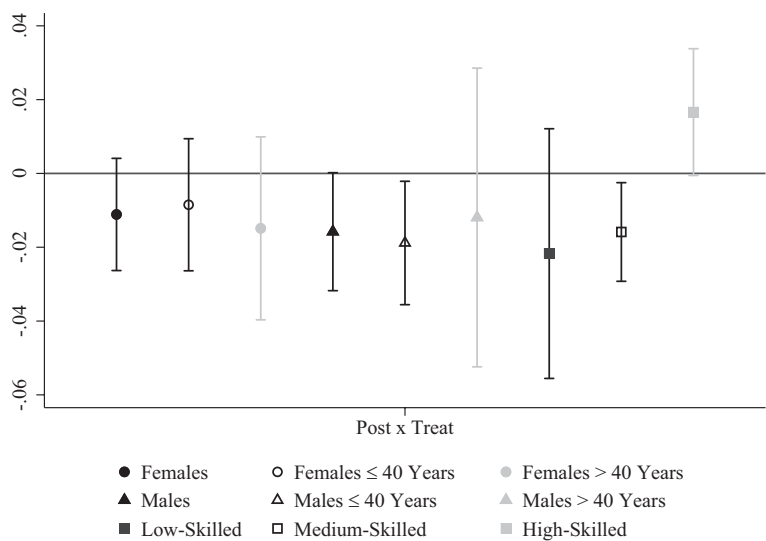


FIGURE 3 Transition into long-term sickness: heterogeneous effects (second year). The figures show the coefficients of the differences-in-differences estimations with 90% confidence intervals stratified by gender and qualification for the second year after establishment entry. Number of observations: females: 4352; females ≤40 years: 3283; females >40 years: 1069; males: 4493; males ≤40 years: 3301; males >40 years: 1192; low-skilled: 1086; medium-skilled: 6601; high-skilled: 1158.
Source: BASiD, own calculations.

are negatively selected in terms of their long-term sickness histories. This suggests that the counterintuitive positive effect on the incidence of long-term sickness cannot be explained by a different selection of high-skilled into the establishments after the reform. While we do not want to overinterpret the positive coefficient from [Figure 3](#) given its small underlying sample size, this result suggests that high-skilled workers are likely to respond differently to the reform presumably due to their higher labor market attachment and better outside options in the labor market. With the exception of the low-skilled, for whom the coefficient in the first year is positive and significant, the estimates for the other subgroups are small and not significant at any conventional levels. This suggests that the pronounced effects for the medium-skilled and younger males represent behavioral effects.

Duration of long-term sickness

Next, we analyze whether the reform also affected the duration of sick leave. The distribution and the mean values of the cumulative sickness days among those individuals who experienced at least one sickness spell after entering an establishment of the relevant size class suggest no major visible postreform change (see [Figure B.2](#) and [Table B.3](#) in the [Appendix S2](#)).

In our multivariate differences-in-differences analyses, we use the cumulative number of long-term sickness days as the dependent variable and again estimate the reform effects for the full sample and the restricted sample of individuals who have a tenure of at least 1 year. The estimations support the descriptive results (see [Table C.9](#) in the [Appendix S3](#)). There are no significant differences across the two groups. The coefficients on the interaction terms are negative but only slightly significant in the first model without any covariates. When we add the covariates, the coefficients on the interaction terms become insignificant. This result is robust to several robustness checks similar to those in [Robustness checks](#) (see [Tables C.10](#) and

C.11 in the [Appendix S3](#)).²⁶ 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.²⁷

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 policy change and a worker's 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.²⁸ Our dependent variable is an indicator variable for transitioning into involuntary unemployment 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 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, that is, those who have not yet left their initial employer. In doing so, we 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 S1](#)). To ensure that we indeed observe *involuntary* unemployment, we count only transitions into unemployment spells lasting longer than 4 weeks as transitions into involuntary unemployment.

Descriptive results

[Figure 4](#) shows nonparametric 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. [Figure 4](#) first indicates that unemployment durations are longer for both treatment and control individuals after the reform.²⁹ Note that part of the increase in unemployment durations may be attributed to the fact that the Pension Register does not allow a consistent definition of involuntary unemployment. The structural break observed in the data arises from a reform of the means-tested welfare benefit system that merged former social assistance and unemployment assistance benefits into one unified benefit in 2005. Prior to 2005, only a fraction of individuals receiving

²⁶As a further robustness check, we also retrieved differences-in-differences estimates using the marginal effects on the censored outcome from a Tobit regression, to account for left-censoring of the dependent variable. The differences-in-differences estimates corresponding to those in column (4) of Table C.9 are -0.277 (-1.098) for the full (restricted) sample with standard errors of 0.660 (1.216), respectively. The placebo estimates (column (5)) are 0.652 (0.863) with standard errors of 0.582 (1.130), respectively.

²⁷We repeated the specifications from Table C.9 after conditioning on long-term sick individuals. The effects are also insignificant and become smaller in absolute value, suggesting that those who became long-term sick do not exhibit different durations after the reform.

²⁸We consider only individuals with sickness periods lasting no longer than 78 weeks in 3 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 arises due merely to a substitution of sick pay with unemployment benefits.

²⁹The longer postreform unemployment durations are also reflected in the descriptive statistics for the whole sample (Tables B.1 and B.2 in the [Appendix S2](#)).

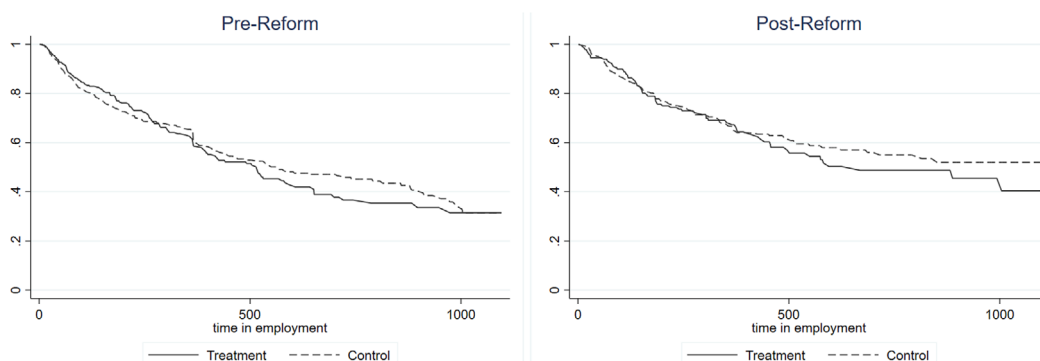


FIGURE 4 Transition into unemployment after long-term sickness—before and after reform. The figure shows the transitions into involuntary unemployment as a function of the time in the relevant employment. The treatment (control) group consists of workers working in establishments with 6–9 (12–20) FTE employees who entered the establishment 3 years before or 3 years after the reform and who have at least one sickness spell during their employment in this establishment. Number of individuals: 1161. [Colour figure can be viewed at wileyonlinelibrary.com]

Source: BASiD, own calculations.

means-tested welfare benefits were counted as involuntarily unemployed (for details, see Table A.3 in the [Appendix SI](#)). In the next section, we conduct robustness checks with respect to this structural break.

[Figure 4](#) further shows that by approximately 3 years after having started a long-term sickness spell, a fraction of approximately 35% are still employed at the same employer in both the treatment and control groups 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 increases for both groups, with the difference being somewhat larger for the control group.

Regression results

[Figure 5](#) shows the average marginal effects of 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 controlled individuals, which supports the descriptive evidence from [Figure 4](#). The magnitude and significance of the time effects are displayed in row (2) of [Table 3](#). The figures indicate that in the third and fourth quarters, the negative effects are significantly different from zero at the 1 and 5% level, respectively. The estimated differences in the marginal effects between treated and controlled individuals are displayed in the first row of [Table 3](#). For the first and third quarters, 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 significantly higher probabilities of becoming unemployed than their control counterparts.

To explore whether the structural break in the definition of involuntary unemployment affects our estimates, we confine our sample to individuals who had been employed for at least 1 year prior to entering a new employer, as these individuals were not affected by the different definitions of involuntary unemployment prior to 2005. The results shown in row (5) of [Table C.13](#) in the [Appendix S3](#) are similar to those reported in [Table 3](#), suggesting no major significant reform effect on the probability of entering unemployment.

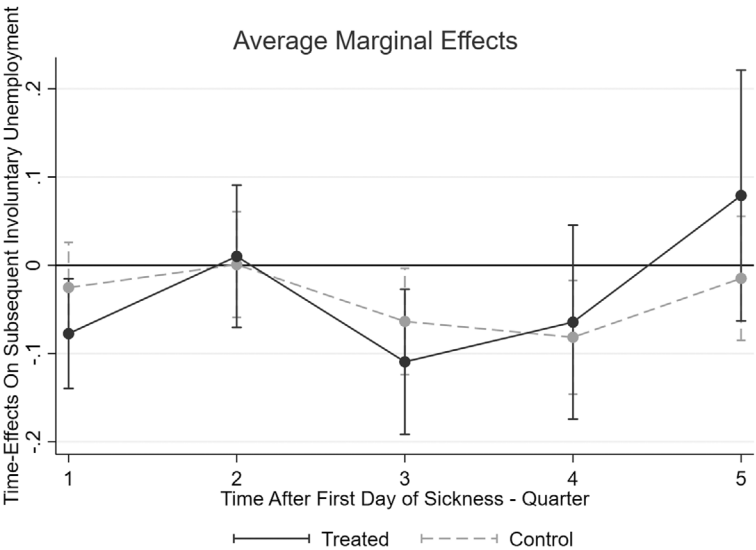


FIGURE 5 Average marginal time effects on the transition into unemployment after long-term sickness. The figure shows the average marginal time effects with 90% confidence intervals on the probability of involuntary unemployment after sickness for the treatment and control groups estimated in a time-discrete logit model. The treatment (control) group consists of workers working in establishments of 6–9 (12–20) FTE employees who entered the establishment 3 years before or 3 years after the reform and who have at least one sickness spell during their employment in this establishment. Source: BASiD, own calculations.

TABLE 3 Differences-in-differences estimations on transition into unemployment after long-term sickness.

Time after the first day of sickness (Quarter)	1	2	3	4	5
Post × Treat	−0.052 (0.048)	0.009 (0.061)	−0.046 (0.061)	0.017 (0.077)	0.094 (0.096)
Post	−0.045* (0.024)	0.004 (0.029)	−0.081*** (0.030)	−0.075** (0.036)	0.022 (0.042)
Treat	−0.008 (0.024)	0.023 (0.031)	0.0194 (0.032)	0.056 (0.040)	0.057 (0.047)

Note: 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% levels. 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: 2489. Source: BASiD, own calculations.

The results are also robust across several robustness checks similar to those in [Robustness checks](#) (see Tables C.12 and C.13 in the [Appendix S3](#)). We wish to note, however, that the estimates are selective in that they condition on a worker experiencing a long-term sickness spell. Given that the reform negatively affected the incidence of long-term sickness, this may imply that treated individuals experiencing 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, to the extent that

individuals who fall long-term sick after the reform are primarily those who are “truly” sick, there is also some scope for a positive selection in terms of moral hazard attributes.³⁰ Depending on which kind of unobservable factor is more or less decisive for employers' dismissal decisions, these selection mechanisms may cause either an upward or a downward bias in our estimates on the reform effects on unemployment transitions.

MECHANISMS

What is still unanswered is what *type of sickness behavior* caused the effect that we identify on the incidence of long-term sickness episodes: Do our results reflect a decline in *absenteeism without being sick*; that is, did treated workers stay away from work more frequently without being sick before the reform when they were protected? Alternatively, 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 add further substance to our findings, we additionally analyze German survey data providing information on absenteeism and presenteeism. The *BiBB/BA Employment Survey of the Working Population on Qualification and Working Conditions in Germany* is a repeated cross-sectional survey of approximately 20,000 employees in Germany. The survey is representative of the German working population and contains, among other things, information on individuals' health status and health behavior. More precisely, the survey of 2012 contains questions on presenteeism and absence (for details, see Tables D.1 and D.2 in the [Appendix S4](#)). Using this information, we generate dummy variables measuring the incidence and length of presenteeism and absence periods.³¹ More precisely, we generate a dummy variable equal to one for an individual reporting more than 0, 5, 10, or 15 working days of presenteeism or absence per year.³² To distinguish between employees with and without dismissal protection, we use information on establishment size and introduce a dummy variable equal to one for workers in establishments with more than 20–49 employees and zero for workers in establishments with five to nine employees.³³ This yields a sample of 2549 observations. The descriptive statistics show that there are some systematic differences in observables between the two groups (see Table D.3 in the [Appendix S4](#)). This highlights the importance of including these variables as controls in our regressions. However, in terms of subjective health status, individuals with and without dismissal protection do not seem to differ significantly.

To analyze the association between dismissal protection and both presenteeism and absenteeism, we run probit regressions that control for observables, such as sociodemographic information, working strains, and subjective health status (for a similar analysis, see Hirsch et al., 2017). [Figure 6](#) shows the average marginal effects of dismissal protection (as measured by establishment size) on the incidence of different durations of absence and presenteeism episodes. For absence, the marginal effect is initially positive and significant. More precisely, individuals subject to dismissal protection have a 7.9-percentage-point higher probability of being absent at least once a year (for details, see Table D.4 in the [Appendix S4](#)). This association is

³⁰Note that whether the average propensity of moral hazard decreases or increases after the reform also depends on the question which combinations of health and moral hazard characteristics drive individuals' utility and absence behavior and how they are jointly distributed. For instance, to the extent that individuals with quite low levels of sickness but high levels of disengagement would have been absent prereform and due to higher perceived costs of absence would not fall long-term sick postreform, this could also raise the average “work ethic” among absentees postreform.

³¹With the data at hand, we cannot explicitly measure employee's absenteeism behavior when they are *not* sick. However, we can measure the incidence and length of actual absences controlling for individuals' health status.

³²Due to a limited number of observations and an increasing measurement error in the higher distribution of sickness durations, we cannot explicitly consider episodes of long-term presenteeism or absenteeism lasting more than 6 weeks.

³³To ensure that we compare individuals with and without dismissal protection, we do not use workers in establishments with 10–19 employees as the control group. Due to measurement error, this group could also include workers without dismissal protection.

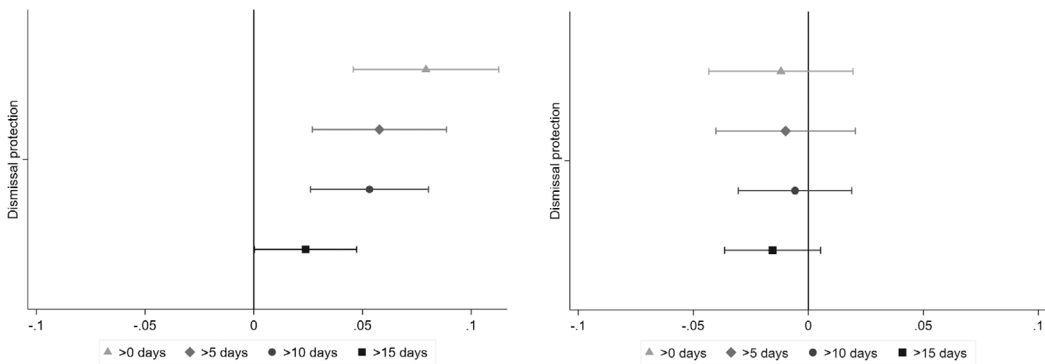


FIGURE 6 Marginal effects of dismissal protection on absence and presenteeism. The left figure shows the association between dismissal protection and absences of more than 0, 5, 10, or 15 working days per year (dummy variables) with 90% confidence intervals. The differences between the marginal effects are not significant except for the difference in 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 episodes 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 controls for gender, age, household situation, qualifications, 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 S4](#).
Source: BiBB/BA Employment Survey 2012, own calculations.

highly significant. However, for the incidence of longer absence periods, the marginal effect of dismissal protection decreases (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 episodes. 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 episodes and decreases with longer durations of absenteeism episodes. If one were to extrapolate this pattern to long-term sickness spells of more than 6 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 spells of longer durations (>15 days) is of the same order of magnitude for both absenteeism and presenteeism and is statistically indistinguishable from zero for presenteeism. Thus, the only conclusion that can be drawn from this complementary exercise is that neither mechanism can be ruled out as an explanation.

SUMMARY AND CONCLUSIONS

This paper empirically analyzes 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 shifted the threshold exempting small establishments from dismissal protection from 5 to 10 workers. We first show that loosening dismissal protection led to a decrease in the incidence of long-term sickness among treated individuals, that is, those hired by establishments affected by the reform, relative to their control counterparts. Second, we provide evidence that this negative effect stems from a behavioral change among treated employees rather than from a compositional

effect that may arise from 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 weaker dismissal protection on the sickness absence rate at the establishment level and which attributes this effect to behavioral 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 6 weeks decreased by 1.3 percentage points among treated individuals during the second year after establishment entry. Relative to the rather low mean transition rate into sickness during the second year, the effect represents a decline of approximately 54%. As to effect heterogeneities, we find the response to be particularly pronounced among the medium-skilled and younger males. Overall, our results are consistent with the PADA reform having a large impact on 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 not 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 establishments than employees who have reacted to the changes in dismissal protection regulations. Our findings also indicate that the regulations of the PADA, which allow 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 analyze the association between dismissal protection and presenteeism and absence 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. While our analyses together reveal that dismissal protection affects long-term sickness behavior, the evidence on the behavioral 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 behavior.

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CONFLICT OF INTEREST STATEMENT

None declared.

DATA AVAILABILITY STATEMENT

The data that support the findings of this study are available from the corresponding author upon reasonable request.

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SUPPORTING INFORMATION

Additional supporting information can be found online in the Supporting Information section at the end of this article.

Appendices S1–S4

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