Essays on the Economics of Sickness Absence, Working Conditions and Health in the Labor Market

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Introduction

Demographic change towards an aging society represents an increasing challenge in industrialized countries. In recent decades, the share of the population aged 65 years and over has nearly doubled in OECD countries, increasing from less than 9 percent in 1960 to more than 17 percent in 2019 (OECD 2021). Declining fertility rates and longer life expectancy have led to older people making up an increasing proportion of the population in OECD countries.

The German labor market is strongly affected by demographic change: Between 2001 and 2022, the share of employees subject to social security contributions aged 55 and above increased substantially, rising from just 9.7 percent to 22.9 percent (Federal Employment Agency 2022*a*). Getting older is often accompanied by age-related illnesses that are long-lasting (Meyer & Mok 2019). Furthermore, the share of individuals with (severe) disabilities increases dramatically with age, as disabilities mostly develop over the course of a lifetime. Thus, the issue of including workers with health impairments is growing in importance. The share of health-impaired individuals is already remarkable: In Germany, about 8 million individuals (9.5 percent of the population) are classified as having a permanent physical, mental or psychological health restriction involving severe disability. In the working age group between 15 and 65 years, 3.1 million individuals are considered severely disabled (Federal Statistical Office 2022).

At the same time, the onset of a long-term sickness or disability may start a process of marginalization from the labor market. Studies constantly show that a health shock significantly and permanently reduces the probability of continued employment and has negative effects on income (e.g., Garcia-Gomez 2011, Garcia-Gomez et al. 2013, Halla & Zweimüller 2013, Heinesen & Kolodziejczyk 2013, Dobkin et al. 2018). Depending on the severity, length, and type of the health shock, the probability of employment decreases by 4 to 36 percentage points one year after the health shock (Heinesen & Kolodziejczyk 2013, Jones et al. 2018). Furthermore, long-term sick leave increases the risk of unemployment, even after taking the health status into account (Hesselius 2007, Hultin et al. 2012). In terms of a specific health shock, namely the onset of disability, studies also document significant negative effects on employment (Charles 2003, Oguzoglu 2010, Polidano & Vu 2015, Jones & McVicar 2020). Lechner & Vazquez-Alvarez (2011) find out that in Germany becoming disabled reduces an individual's employment probability by 9 to 13 percentage points two years after the onset of disability, depending on the degree of disability.

In view of a current shortage of skilled workers, which is likely to get worse in the coming years, promoting inclusion and maintaining employability of workers despite illness is key. Working conditions play a decisive role in the extent to which health-impaired people are able to (return to) work. Working conditions – the circumstances under which employees perform their work – are essentially determined by the employer and the legal framework. Labor market institutions form an important part of the legal framework as they refer to the structures, regulations, and organizations that shape the interactions between employers, employees, and the government within a given labor market.[1]

Labor market institutions relevant to promoting inclusion and maintaining employability of sick workers often have to fulfill several in parts contradictory goals. First, they have to ensure that individuals who become sick do not suffer economic hardship, and thus provide coverage for potential income losses (e.g., in the form of sick pay or disability pension). Second, they must guarantee special protection for sick or disabled individuals who stay in or return to the labor market (e.g., in the form of employment protection). Third, they aim to promote labor market participation of sick or disabled workers, both from a labor supply perspective (e.g., in the form of an entitlement to social benefits for individuals, which is limited in duration) and from a labor demand perspective (e.g., in the form of antidiscrimination legislation or employment quotas for firms). In this way, institutions create intended and unintended incentives for employees to stay attached to the labor market and for firms to adjust their workforce. Thus, understanding both the effects of health shocks and the effects of health-related institutions for individuals and firms is crucial. This understanding may help to design effective policies in order to balance social protection and employment promotion of health-impaired individuals in an era of demographic change.

This thesis aims to shed light on the consequences of health shocks and the role of working conditions in the form of health-related institutions for including and retaining

[1] In general, institutions are defined as formal and informal rules, including the mechanisms of their enforcement, that organize political, economic and social interaction, constraining the behavior of individuals or organizations in transactions. In that sense, institutions can be seen as the "rules of the games" in society (North 1991). *Labor market* institutions are typically understood as policy measures or collective organizations that impact the process of employment and wage determination. Examples include works councils, minimum wages, employment protection and unemployment insurance (Holmlund 2014).

health-impaired workers in the German labor market. It starts with an analysis of how dismissal protection affects workers' long-term sickness absences and the probability of involuntary unemployment after sickness (Chapter 1). Thereafter, Chapter 2 investigates whether the disabled worker quota in Germany and its noncompliance fine affect firm demand for disabled workers. Finally, Chapter 3 studies the individual effects of disability onset on labor market outcomes.

Synopsis

Despite their joint field of research, the three essays vary in some respects. To highlight differences and commonalities, Table I.1 compares the three studies by illustrating the different institutions considered, empirical strategies, underlying data, and outcomes.

All three essays share a common institutional setting, namely the German labor market in the first and partly second decade of the 21st century. Analyzing the effects of health-related labor market institutions in this setting is interesting for at least two reasons: First, as explained above, Germany is a country strongly affected by demographic change. Thus, promoting inclusion and maintaining employability of sick or disabled workers may be particularly relevant for German labor market policies. Second, compared to other OECD countries, Germany is a country characterized by quite strict employment protection and regulations to promote sick or disabled workers (OECD 2010, 2020). Therefore, it provides an interesting setting for analyzing intended and unintended effects of relevant institutions in the labor market. For this, Chapter 1 and Chapter 3 look at effects on labor market outcomes such as absence, employment and earnings at the individual level, while Chapter 2 studies effects on labor demand at the firm level. In all, I discuss the relevance of health impairments and relevant institutions for the two important players on the labor market – employees and employers.

The focus on a severe health limitation is a further common thread in all three studies. While Chapter 1 deals with long-term sickness, Chapter 2 and Chapter 3 focus on severe disability. As long-term sickness often precedes disability, the analyses in Chapter 1 can be considered as starting point. In line with the studies by Gjesdal et al. (2004) and Kivimäki et al. (2004), I find that long-term sickness – approximated by duration and number of nonemployment spells – significantly correlates with future disability onset. More precisely, male individuals with a future disability onset have on average 0.53 more nonemployment spells and 1.71 more months in nonemployment two years before the

Table I.1: Overview of Dissertation Chapters

Dimensions \ Chapter	**Chapter 1** Dismissal Protection and Long-Term Sickness Absence – Evidence from a Policy Change	**Chapter 2** To Include or Not to Include? Firm Employment Decisions with Respect to the German Disabled Worker Quota	**Chapter 3** Disability and Labor Market Performance
Co-Author(s)	Nicole Gürtzgen	–	Matthias Collischon & Laura Pohlan
Discussion Paper	Gürtzgen & Hiesinger (2020) ZEW-DP No. 20-040 IAB-DP No. 22/2020	Hiesinger (2022) IAB-DP No. 25/2022	Collischon et al. (2023) IZA-DP No. 16100
Institutional Context	German labor market		
Health Dimension	Long-term sickness	Disability	
Main Outcomes	Incidence and duration of long-term sickness	Number of disabled workers in a firm	Earnings, employment
Further Outcomes	Involuntary unemployment after sickness	Firm size density, firm growth, marginal/regular employment, wages and productivity	Nonemployment, unemployment occupational and establishment switches
Main Institution(s) Considered	Dismissal protection	Disabled worker quota, noncompliance fine	–
Relevant Threshold Regulation	Exemption of dismissal protection for small establishments	Employment obligation to employ one additional disabled worker	–
Relevant Threshold	Establishment size: 5/10 FTE employees	Firm size: 40 employees	–
Further Relevant Institution(s)	Sick pay	–	Social benefits after health shocks
Empirical Strategy	Differences-in-differences design	Threshold design	Propensity score matching & event-study design
Unit of Observation	Individuals	Firms	Individuals
Heterogeneities (Differentiation Level)	Skill groups, gender	High/low-wage firms, industries	Skill groups, age groups, degree of disability, gender
Data	BASiD	BsbM, BHP	BsbM, IEB, BHP
Complementary Survey Data	BiBB/BAuA Employment Survey	–	PASS
Observation Years	2001–2007	2004–2011	2005–2013

Notes: BASiD = Biographical Data of Selected Insurance Agencies in Germany; BHP = Establishment History Panel; BsbM = Employment Statistics for Severely Disabled People; FTE = Full-time-equivalent; IEB = Integrated Employment Biographies; PASS = Panel Study Labour Market and Social Security disability onset in comparison to individuals without a disability onset.[2] Furthermore, Hultin et al. (2012) show that long-term sickness absence may start a marginalization from the labor market as it increases the risk of disability pension and unemployment. Therefore, early intervention is important to maintain and promote the employability and the labor market attachment of sick workers. The institutions that become relevant as soon as an employee becomes ill for a longer period of time are long-term sick pay and dismissal protection. To this end, Chapter 1 first analyzes the effects of the more general labor market institution of dismissal protection, thus complementing existing analyses on long-term sick pay (e.g., Ziebarth 2013). Chapter 2 and Chapter 3 then discuss the role of institutions specifically aiming at the (re-)integration and protection of *sick* individuals, namely the disabled worker quota, the noncompliance fine and social benefits after health shocks (e.g., disability pension). While the first two essays explicitly address the causal effects of labor market institutions, Chapter 3 estimates the effects of a health shock, but also addresses the role of relevant institutions in this context.

Chapter 1 and Chapter 2 use a threshold in firm size to study the effects of institutions. A labor law threshold indicates the number of employees above which a firm is subject to labor law regulations. Small firms are often exempt from certain regulations. The legal justification for these exemptions is the special need to protect small and medium-sized firms as they often have lower financial resources, are administratively less resilient and are more dependent on individual workers (Koller 2010*a*). The German labor law contains approximately 160 threshold regulations. Table I.2 provides an overview of selected and currently valid threshold regulations in Germany for small and medium-sized firms.[3] The table illustrates the substantial variation in legal regulations regarding labor law thresholds and their measurement: Not only do numerous thresholds exist, but there are also considerable differences in reference points (establishment, firm or employer),

[2] For this, I regress a binary indicator variable for future disability onset on the cumulative duration and number of nonemployment spells up to two years before potential disability onset using the same sample of employed individuals as in Chapter 3 (see Table 3.3.1), but restricted to men (number of observations: 5,704,118). Note that I cannot clearly identify long-term sickness periods in the administrative data of the Federal Employment Agency. However, nonemployment spells include periods of long-term sickness of more than six weeks (besides, for example, periods of self-employment or child-rearing). By excluding women, the influence of child-rearing periods is reduced. In this sense, periods of nonemployment can be understood as a proxy for long-term sickness. The estimation controls for individual (age, education, nationality, job requirement level) and establishment characteristics (industry, size, region, median wages) and the individual employment history (cumulative duration in employment and in establishment). The coefficients for the cumulative duration of nonemployment and the cumulative number of nonemployment spells are both significant at the 1 percent level. *Source*: BsbM and IEB, years of potential disability onset: 2005–2013, own calculation.

[3] For a more comprehensive overview of the German threshold regulations and their different measurements, see Koller (2010*a*) and Koller (2010*b*).

reference period (e.g., normally employed workers or the annual average of the monthly number of workers), excluded employee groups (e.g., apprentices), and the measurement of the thresholds (per capita or full-time-equivalents).

Table I.2: Overview of Selected Threshold Regulations in Germany

Threshold (No. of Employees)	Regulation	Reference Point	Reference Period	Threshold Measure	Apprentices Included[a]	Law
≥ 5	Possibility to establish a works council	Establishment	Number of workers normally employed	Per capita	Yes	§9 BetrVG
> 10	Entitlement to dismissal protection	Establishment	Number of workers normally employed	Full-time-equivalents	No	§23 KSchG
> 15	Entitlement to part-time employment	Employer (Firm)	Number of workers normally employed	Per capita	No	§8 TzBfG
≥ 20	Employment obligation to employ one disabled worker	Employer (Firm)	Annual average of monthly number of employees	Per capita	No	§154 SGB IX
> 20	Establishment of an occupational health and safety committee	Employer (Firm)	Annual average number of employees	Full-time-equivalents	Yes	§11 AsiG
> 20	Nomination of a safety officer	Firm	Number of workers normally employed	Per capita	Yes	§22 SGB VII
> 30	No reimbursement payments of sick pay by health insurances any more	Employer (Firm)	Number of workers normally employed	Full-time-equivalents	No	§1 AAG
≥ 40	Employment obligation to employ two disabled workers	Employer (Firm)	Annual average of monthly number of employees	Per capita	No	§154 SGB IX
≥ 60	Obligation to report a mass layoff to the employment agency	Establishment	Number of workers normally employed	Per capita	Yes	§17 KSchG
≥ 60	Employment obligation to fill at least 5 percent of positions with disabled workers	Employer (Firm)	Annual average of monthly number of employees	Per capita	No	§154 SGB IX
≥ 200	Paid leave of absence of works counselor	Establishment	Number of workers normally employed	Per capita	Yes	§38 BetrVG

Notes: [a]The group of apprentices is only one example of a potentially excluded employee group. Other groups of employees (e.g., freelance collaborators or temporary workers) are also included or excluded depending on the law.
Status: Legislation in force in June 2023.
Source: Own illustration based on Koller (2010*a*) and Koller (2010*b*).

However, despite the large number and legal relevance of threshold regulations, empirical evidence on their effects is scarce. So far, most of the studies analyzing thresholds in German labor law find no or only marginal effects of these regulations.[4] In contrast, Garicano et al. (2016) show substantial firm reactions to the threshold of 50 employees in France, at which the number of effective labor laws increases remarkably. One reason for this discrepancy might be that the existing studies for Germany could only roughly approximate the often complex-to-calculate number of workers in a firm or establishment

[4] While Bauer et al. (2007) and Bauernschuster (2013) analyze the impact of a change in the threshold of dismissal protection, Wagner et al. (2001) and Koller et al. (2007) study the threshold effects of the German disabled worker law. Furthermore, Koller et al. (2010) and Backes-Gellner & Mohrenweiser (2010) analyze the threshold with respect to paid leave of absence of a works council member.

due to data restrictions and thus probably suffer from measurement error. This error can have a strong impact on the analyses as firm size determines the treatment status.

The first two essays of this thesis try to overcome this limitation by using administrative data sets that allow a somewhat more precise calculation of firm size compared to previous studies: In Chapter 1, the number of normally employed full-time-equivalent workers is approximated by taking the working time into account and by accounting for annual fluctuations in the workforce when calculating the threshold of dismissal protection (see Table I.2). In Chapter 2 (as well as in Chapter 3), I use a novel data set – the Employment Statistics for Severely Disabled People (BsbM) – that is based on the notification procedure used to control compliance with the disabled worker quota. Thus, this data set encompasses firm size information that aligns with the defined criteria for firm size as specified in the German disabled worker law. It allows me to revisit previous findings which are mainly based on establishment-level survey data.

Generally, the data used is a major asset of all three essays: They are all based on large administrative individual and firm-level data sets from the German Federal Employment Agency and the German Pension Register. Thus, it is possible to precisely measure individual labor market states and transitions between them and – as described above – to precisely calculate firm size with regard to threshold regulations. Furthermore, the large data sets allow for a more detailed analysis of intended and unintended effects of institutions by digging deeper into potential mechanisms and exploiting heterogeneous effects for different employee and firm groups, e.g., different age and skill groups or high- and low-wage firms. Furthermore, AKM-style fixed effects are used as a proxy for employee and firm productivity in Chapter 2 and Chapter 3. In this way, unobserved heterogeneity is taken into account (Abowd et al. 1999). Chapter 1 and Chapter 3 are additionally supplemented with an analysis of survey data, namely the BiBB/BAuA Employment Survey and the Panel Study Labour Market and Social Security (PASS), to better understand underlying mechanisms and to provide further descriptive evidence on characteristics which are not captured by administrative records.

In all of the three studies, the aim is to identify causal relations in a quasiexperimental setting. For this, different empirical methods are applied, namely a differences-in-differences approach (Chapter 1), a threshold design which is closely related to a regression discontinuity design (Chapter 2) and a propensity score matching combined with an event-study analysis (Chapter 3).

The following section provides summaries of the three essays, with two being co-authored and one being single-authored. All papers have been published as discussion papers and were submitted and went under review in a journal related to labor economics.

Executive Summary of Essays

As spelled out above, many OECD countries implement institutions that aim to reduce economic risks for health-impaired individuals by providing protection against job and income losses via sick pay and dismissal protection. Generally, such institutions may be beneficial for individuals' health as they provide the opportunity to recover from a severe disease. At the same time, the extent of the benefits may influence both employee and employer behavior. First, employees' sickness behavior in the form of absenteeism (staying away from home without being sick) or presenteeism (attending work despite being sick) may play a role. On the one hand, employees who are subject to strong employment protection may be prone to moral hazard behavior and increase their absences in the form of absenteeism (Ichino & Riphahn 2005, Scoppa 2010, Ziebarth 2013). On the other hand, the fear of job loss could cause those with weak protection to shorten or avoid their absences in the form of presenteeism (Reichert et al. 2013). Second, employers may have an incentive to dismiss employees with high absences and for whom, at the same time, institutional protection is comparatively low. Some studies have already focused on long-term sickness absence in the context of sick pay (e.g., Ziebarth 2013). However, little is known about the impact of dismissal protection on long-term sickness absence and the associated labor market consequences. The few existing studies analyze changes in dismissal protection regulation in Italy and Sweden. For Italy, Scoppa (2010) provides evidence of a positive effect of stricter dismissal protection on sickness absences. For Sweden, Lindbeck et al. (2006) and Olsson (2009) show that weaker dismissal protection regulations negatively affect sickness absence rates.

In Chapter 1 of this thesis, my co-author Nicole Gürtzgen and I study whether a decline in employment protection reduces workers' sickness absences of more than six weeks. Building on the previous studies by Lindbeck et al. (2006), Olsson (2009) and Scoppa (2010), we analyze a German reform that involved a more pronounced change in dismissal costs for establishments than the Swedish or Italian reform. More specifically, we exploit exogenous variation from a policy change in 2004 that shifted the threshold exempting small establishments from dismissal protection from five to ten full-time-equivalent (FTE) workers (see Table I.2). Due to transitory regulations that granted dismissal protection to those who were already employed in an establishment before 2004, the reform affected only employees who *entered* an establishment with more than five to ten FTE workers. Thus, in applying a differences-in-differences design, we define the affected group of workers as our treatment group and compare the outcomes of this group to those of workers entering an establishment slightly above the threshold. As our main outcome variables, we focus on the incidence and duration of long-term sickness. For our analyses, we use combined register data of the Federal Employment Agency and the Pension Register (BASiD), which allows us to identify sickness periods subject to long-term sick pay (i.e. sickness absences of more than six weeks).

As our main result, we show that the reform significantly reduced employees' transitions into long-term sickness during their second year after being hired. In terms of effect size, treated individuals exhibit a 1.3 percentage point lower incidence of long-term sickness. Given that the overall probability of experiencing a transition into long-term sickness in the second year after being hired is 2.4 percent, this effect is quite large. However, we do not discover a significant effect on long-term sickness incidence in the first year after being hired and on the duration of long-term sickness. We further perform a number of selectivity analyses to test whether our main result is driven by a true behavioral effect or by a different selection of workers into establishments. The results of these analyses provide no evidence of any compositional selection effects. The absence of composition effects leads us to conclude that the identified reform effect is driven by newly hired individuals who adapted their sickness behavior to the weaker dismissal protection regulations.

Heterogeneity analyses suggest that this adaption is particularly pronounced among medium-skilled men. In a next step, we examine whether the reform was associated with a higher risk of unemployment after long-term sickness using a time-discrete logit model. However, we do not find any evidence for such an association. This finding is in line with results from previous studies, which fail to detect any major effects of dismissal protection on separations at the establishment level (e.g., Bauer et al. 2007). Overall, our results indicate that it is less the establishments than the employees themselves who respond to changes in dismissal protection. Last, we attempt to better understand the behavioral mechanisms underlying the decrease in sickness incidence as it may either reflect a decline in absenteeism or an increase in presenteeism. For this, we additionally use German survey data – the BiBB/BAuA Employment Survey – providing information on absenteeism (in the form of absence days) and presenteeism. Our descriptive analysis show that individuals subject to dismissal protection have a higher probability to be absent at least once a year. However, for longer absence periods as well as for presenteeism, our analysis cannot reveal any significant correlations. As a consequence, we cannot rule out either mechanism (a decline in absenteeism or an increase in presenteeism) as an explanation for our main result.

In addition to general labor market institutions such as dismissal protection, some institutions explicitly aim to improve the employment of health-impaired individuals, in particular individuals with severe disabilities. In all OECD countries, individuals with disabilities experience low levels of employment and high unemployment rates reflecting their considerable labor market disadvantages (OECD 2022). As a consequence, many countries have implemented policies to better promote the integration of disabled individuals into the labor market. Along with antidiscrimination legislation, compulsory employment quotas for disabled workers are one of the most common disability policies. They are used in many OECD countries such as Austria, France, Italy, Spain, Poland, and Germany (OECD 2010). The aim of such a quota is to create an incentive for employers to hire and to retain workers with disabilities. Firms that do not meet the quota have to pay a noncompliance fine. However, even though employment quotas and noncompliance fines are widely used, surprisingly little is known about their intended and unintended effects. For Austria, Lalive et al. (2013) show that the disabled worker quota positively affects the firms' demand for disabled workers in firms located at quota thresholds. However, firms may manipulate employment to avoid the noncompliance fine and purposely stay – bunch – below the quota thresholds. The few existing studies find no or only small bunching effects (Wagner et al. 2001, Koller et al. 2006, Lalive et al. 2013, Mori & Sakamoto 2018, Szerman 2022). Still, there is a remarkable scarcity of research with regard to firms' responses to disability quotas.

Chapter 2 of this thesis therefore aims to contribute to the literature by examining the intended and unintended effects of the German disabled worker quota in great detail. For this, I again exploit a threshold regulation in German labor law. The threshold regulation I focus on is as follows: Firms with at least 20 but fewer than 40 employees are required to employ at least one disabled worker, whereas firms with 40 or more employees must employ at least two disabled workers (see Table I.2). Firms that do not comply with this obligation must pay a fine which increases with the extent of their noncompliance. To analyze the firms' behavior around the 40-employee threshold, I use unique administrative firm data from the German Employment Agency that is taken in the process of administrating firm compliance with the disabled worker quota.

As explained above, I aim to estimate both the intended and unintended effects of the quota. The intended effect is the threshold effect on the number of disabled workers in a firm, whereas the unintended effect describes the extent to which firms manipulate their firm size and bunch below the threshold. For identifying these effects, I closely follow Lalive et al. (2013) and apply a so-called threshold design. Although closely related to a regression discontinuity design, the threshold design explicitly addresses the violation of the identifying assumption, namely the manipulation of the firm size. This manipulation is likely to be relevant in this context, as firms just below the threshold face an increase in labor costs at the threshold. This increase depends on their initial number of disabled employees: Firms below the 40-employee threshold with (1) zero or (2) exactly one disabled worker(s) face a (higher) noncompliance fine when crossing the threshold.[5] I refer to these potential bunching firms as (1) *noncompliers* and (2) *perfect compliers*, respectively. As *noncompliers* face the highest costs at the threshold, I expect bunching to be more pronounced among this type of firm.

My analyses reveal the following key results: When ignoring the bunching, the estimation of the intended effect shows that firms positively respond to the threshold and employ 0.388 more disabled workers when they are located just above the threshold. However, I also provide evidence for the unintended, or bunching, effect: Some firms purposely stay below the 40-employee threshold and adjust their workforce accordingly to avoid the (increased) fine. Firms just below the threshold have lower employment growth and a higher share of marginally employed workers – a group of workers that does not count toward the calculation of firm size. Furthermore, significant discontinuities in wages and productivity suggest that adjusting the workforce may be more costly among bunching firms. When distinguishing between the different types of firms, I show that bunching is indeed particularly pronounced among those firms that face the highest costs at the threshold, the *noncompliers*. Based on the estimates about the extent to which firms bunch, I am able to provide a lower bound for the unbiased threshold effect of 0.201. Thus, although being somewhat smaller than the naive estimated threshold effect, the bounded effect still suggests that the quota promotes employment of disabled workers.

Last, I analyze heterogenous effects for high- and low-wage firms and for different industries. As the relative importance of the noncompliance fine differs substantially between low- and high-wage firms, I find that both threshold and bunching effects are larger among low-wage firms. Furthermore, I identify a comparatively large bunching effect for the construction sector – an industry characterized by a high share of physically demanding tasks. I find the main results to be robust in several robustness checks such as placebo and donut estimations and estimations for the next threshold of the quota, namely the 60-employee threshold (see Table I.2). In sum, I provide evidence for both intended and unintended effects of the noncompliance fine. On the one hand, the fine is effective as it incentivizes firms to employ (more) severely disabled individuals. On the other hand, the unintended effects may be detrimental to overall employment, as the fine incentivizes firms below the threshold to slow down employment growth and to substitute away from regular employment.

[5] When discussing the additional costs at the threshold, I rule out that it is the hiring of a disabled worker that causes the firms to cross the threshold.

However, disability policies may not only be relevant from a labor demand perspective. It is also crucial to understand the labor market consequences of becoming disabled from a labor supply perspective, as most of severe health limitations or disabilities occur during the working life. Thus, a key policy challenge is to maintain and promote individual employability after disability onset. While the literature provides evidence of adverse labor market effects of disability onset (see, e.g., Charles 2003, Jenkins & Rigg 2004, Lechner & Vazquez-Alvarez 2011, Polidano & Vu 2015, Jones et al. 2018, Meyer & Mok 2019, Jones & McVicar 2020), little is known about other underlying mechanisms apart from working time (Charles 2003, Polidano & Vu 2015) or the receipt of unemployment or other replacement benefits (Lechner & Vazquez-Alvarez 2011).

Thus, to fill this research gap, we analyze the effects of disability onset on labor market outcomes and potential mechanisms at the individual level in Chapter 3 of this thesis, which is co-authored by Matthias Collischon and Laura Pohlan. Again, we use administrative records of the notifying procedure used to control compliance with the disabled worker quota, the Employment Statistics of Severely Disabled People (BsbM). This data enables us to identify severely disabled workers in the social security data of the Federal Employment Agency, the Integrated Employment Biographies (IEB). Using the combined data set, we contribute to the literature in essentially two ways: First, as we are the first to use administrative data to quantify the effects of disability onset, with which we are able to better address challenges of previous studies, which are based on survey data. The response rate in surveys may, for example, be influenced by health or employment status. In addition, disability status, unemployment and wages as stigmatized or sensitive characteristics may be misreported in surveys. Second, the large number of variables and observations over a long time horizon enables us to study effect heterogeneities and underlying mechanisms of the adverse labor market effects of disability onset in more detail. To do so, we exploit information on different reasons for being out of the labor force (e.g., replacement benefits or death) and information on employer or occupational switches.

Our empirical approach is as follows: First, we restrict our sample to individuals who were employed five years before (potential) disability onset in order to identify a severe and sudden health shock. Second, we use propensity score matching techniques to address potential nonrandom selection into treatment. For this, we match disabled individuals to nondisabled coworkers two years before the measured date of disability onset as the process of registering for disability status takes time. The matching is based on the predisability employment history as well as on a broad array of observable individual and establishment characteristics. Third, we use the generated matching weights in an event-study design and compare labor market performance for the disabled and nondisabled group until five years after disability onset. As main outcome variables, we focus on two aspects of labor market performance, namely employment and labor earnings.

Our results provide evidence for lasting negative impacts on both aspects: The number of days in employment decreases for the disabled compared to the nondisabled, even two years before our measured date of the disability onset. The number of employment days continues to fall after the disability onset up to a total decline of 59 days per year five years after onset. Annual labor earnings also decrease substantially until five years after onset. For those who stay in employment, we observe a reduction of daily wages of approximately 7 percentage points in the fifth year after onset. With respect to mechanisms, we identify transitions to nonemployment as an important channel for our employment outcome: One year after onset, the number of nonemployment days increases by 36 days per year and the probability of being nonemployed increases by 10 percentage points in comparison to those of the control group. After five years, the effects amount to 15 percentage points and 55 days, respectively. In contrast, the effect on the unemployment status is quite small – a result which is also documented by the study of Lechner & Vazquez-Alvarez (2011). By exploiting information on individuals' reasons for being out of the labor force, we are able to show that the three most mentioned reasons – end of employment, receipt of replacement benefits and death – do in fact play a role in the transition to permanent nonemployment after disability onset. For those who stay in employment, working part-time and occupational switches are important adjustment channels. We find occupational switches to be relevant both in a horizontal and in a vertical dimension: Five years after disability onset, the probability of switching to a less physically (psychosocially) demanding job increases by 2.4 (1.7) percentage points, and the probability to switch to a job with a lower job requirement level increases by 1.6 percentage points relative to the probability in the control group. In contrast, establishment changes play only a minor role.

With regard to heterogeneous effects, we show that the negative labor market effects of disability onset are more pronounced among severely disabled, older and low-skilled individuals, which is largely in line with the literature (Charles 2003, Jenkins & Rigg 2004, Lechner & Vazquez-Alvarez 2011, Polidano & Vu 2015, Jones et al. 2018). In a next step, we use AKM fixed effects as a proxy of productivity and show that restricting our sample to pre-disability employment does not lead to a significant positive selection. Last, we enrich our analyses by complementary analyses based on survey data, namely the Panel Study Labour Market and Social Security (PASS). These analyses confirm the absence of a positive sample selection and provide further descriptive insights on characteristics of disabled workers, which are not captured by administrative records.

Chapter 1

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

*Abstract**

This paper studies whether a decline in employment protection reduces workers' long-term sickness absences (of more than six weeks). We exploit exogenous variation from a German policy change that 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 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 medium-skilled males. Further results indicate that the reform did not alter the probability of involuntary unemployment after sickness.

Keywords: dismissal protection, long-term sickness, involuntary unemployment, differences-in-differences, administrative data, small establishments
JEL Codes: D02, I12, J28, J63, K31

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1.1 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 (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 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, 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.

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 2021, approximately 46 percent of all absence days were due to long-term sickness lasting more than six 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 below a certain threshold from

[1] There is no official definition of long-term sickness. This study focuses on spells of more than six 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).

dismissal protection. In the course of a major labor market reform in 2004, the threshold for establishment exemption 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 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 quasiexperimental 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 – albeit 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.

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 ten employees). In contrast to the Swedish context, 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., Boeri & Jimeno 2005, Lindbeck et al. 2006, Bauer et al. 2007, Olsson 2009, Bauernschuster 2013). 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 into establishments.

Previewing our results, we find that the reform significantly reduced employees' transitions into long-term sickness during their second year after being hired. Based on a number of selectivity analyses, we argue that this response is due to a behavioral rather than a compositional effect. Moreover, we find the response to be particularly pronounced among medium-skilled males. 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, our complementary survey-level analyses do not allow us to rule out either a decline in absenteeism or an increase in presenteeism as an explanation.

The remainder of the paper is structured as follows: In Section 1.2, we give an overview of the theoretical and empirical literature regarding long-term sickness absence. Section 1.3 illustrates the German institutional setting before Section 1.4 presents the data set and the empirical strategy. Sections 1.5 and 1.6 provide the empirical results, and Section 1.7 concludes.

1.2 Related Literature

It is well established that individuals may have some discretion over their sickness behavior in the form of absenteeism or presenteeism.[2] Empirical studies provide evidence of both types of behavior being relevant.[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, e.g., 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, Puhani & Sonderhof 2010, Ziebarth & Karlsson 2010, 2014, Pichler & Ziebarth 2017, Chen et al. 2020) 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 regu-

[2] Note 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.

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

lations 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 (Hesselius 2007, Markussen 2012, Scoppa & Vuri 2014, Chadi & Goerke 2018).

1.3 The German Institutional Background

1.3.1 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 percent of individuals' earnings.

After six 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 percent) of the German population and is mandatory for all employees subject to social security contributions whose earnings fall short of the statutory health

4 In 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 & 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 & Andersen (2008) show that a higher extent of perceived job insecurity is associated with higher levels of being present at work despite sickness.

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 starting from the first day of illness.

6 An exception concerns illness during the first four 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.

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 percent 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 duration[8] 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 Gürtzgen & 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.

1.3.2 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 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

7 Civil 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.

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

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 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 in the Labor 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 five to ten full-time-equivalent (FTE) employees in October 1996, back to five FTE employees in January 1999 and then back again to ten FTE employees 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,

[9] Note 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).

[10] Table 1.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 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 (five FTE employees).

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 five to ten FTE workers 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).

1.4 Empirical Strategy, Data and Variables

1.4.1 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 1.3.2, 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 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, 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

[12] For more details on the definition of establishment entry, see Table 1.A.6 in the appendix.

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 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:

$$Y_i = \alpha + \beta T_i + \gamma G_i + \tau_{DID}(T_i * G_i) + \eta X_i + \epsilon_i \tag{1.1}$$

In equation (1.1), 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. 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).

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 to 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 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

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 1.B.1 in the appendix).

1.4.2 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 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 percent 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) 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 1.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 illness spells subject to sick pay covered by mandatory health insurance. As spelled out earlier, the latter comes into effect after a period of six weeks of absence and may cover spells during either employment or unemployment. 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 twelve. However, these periods are capped at a maximum length of ten days per year/per child. 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 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 to gain information on tenure at the current employer. Table 1.A.3 in the appendix provides a more detailed description of the variables gained from the Employment Statistics Register.

1.4.3 Sample Selection and Descriptives

As spelled out earlier, we define workers entering an establishment with up to ten 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 to 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, we approximate the number of full-time-equivalent workers and take into account annual fluctuations in the workforce.[14] 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 twelve to 20 (control group) FTE employees. We further ensure that the establishments remain in the same size group during

13 Note 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 1.A.4 in the appendix). 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 1.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 calculate the establishment size, see Table 1.A.6 in the appendix.

the period when a worker is employed in this establishment.[15]

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 1.B.1 and 1.B.2 in the appendix). 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 control individuals.

1.5 Estimation and Results

1.5.1 Incidence and Duration of Long-Term Sickness

Descriptive Results

Figure 1.5.1 shows the cumulative incidence of sickness for the treatment and the control groups during the first two years after establishment entry. In the prereform period, the evolution of this outcome exhibits no major differences across treated and control 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 percent and 3.7 percent 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.

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 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

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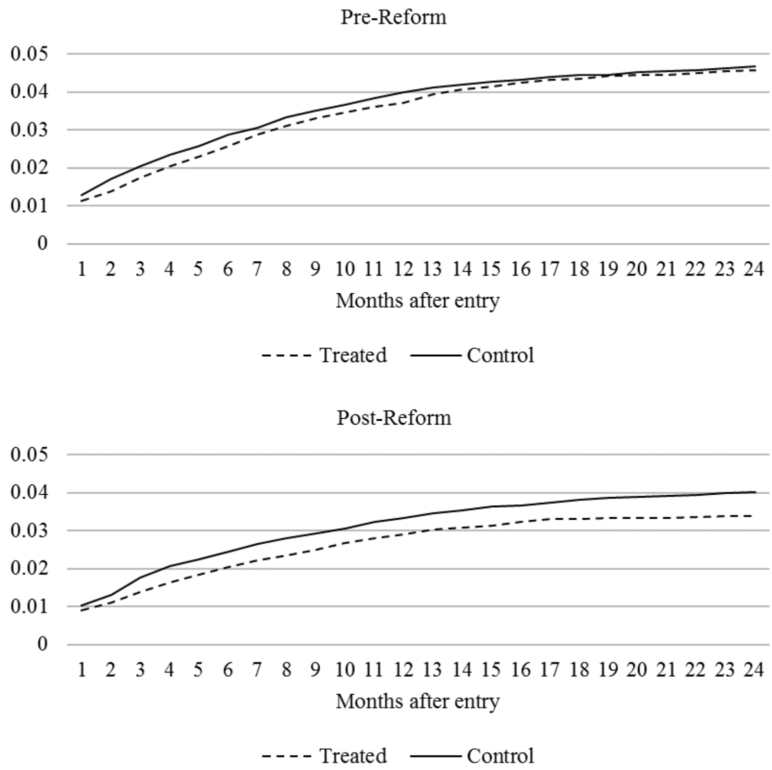
15 The extent to which this restriction may bias our results depends on whether the reform caused establishments to self-select into certain size classes. In Figure 1.B.1, we provide evidence that the distribution of establishments across size classes remained broadly unaltered over the observation period.

Figure 1.5.1: Cumulative Incidence of Long-Term Sickness



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. We calculate the share of workers having at least one long-term sickness period until the respective month after entry.
Source: BASiD, own calculations.

already ill at establishment entry, resulting in a sample of 27,967 observations.[16] 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 8,845 observations. We address these issues in Section 1.5.1.

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 ten industry dummies. Finally,

[16] A total of 160 individuals in our sample (0.6 percent) 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.

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.C.1 in the appendix). The coefficient on the interaction term is insignificantly negative but close to zero and remains unaltered 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 1.5.1. 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 experiencing a transition into sickness in the second year is 2.4 percent, this effect is fairly large. The group effect is positive but insignificant. In contrast, the time effect is negative and significant (except for in model (1)) and becomes larger in magnitude after we add more control variables. The last column in Table 1.5.1 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 for either the first or the second year, thereby supporting the parallel trend assumption.

Table 1.5.1: Regression Results on Transitions into Long-Term Sickness in the Second Year after Entry

	Model 1	Model 2	Model 3	Model 4	Placebo
Post x Treat	-0.014***	-0.014**	-0.014**	-0.013**	-0.001
	(0.005)	(0.007)	(0.007)	(0.007)	(0.007)
Treat	0.008	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	0.024***	0.059***	0.055***	0.038***	0.037***
	(0.002)	(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. Significance levels: ** $p < 0.05$, *** $p < 0.01$. 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 1.A.1, 1.A.2 and 1.A.3 in the appendix. The placebo regression hypothetically assumes that dismissal protection reform took place in 2003.
Source: BASiD, own calculations.

Robustness Checks

In this section, we explore whether the results from Table 1.5.1 are robust to several sensitivity checks (for an overview, see Table 1.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 to the sickness of a child. 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 spells of caring for their ill children (see section 1.4.2). 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 1.C.3 in the appendix: When we exclude short illness spells (columns (1) and (4)), the effects are slightly smaller in magnitude but still significant at the 10 percent 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 1.5.1 and significant at the 5 percent level at least. 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.

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 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 into 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.[17] 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 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 young workers below age 25. 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, i.e., workers with a tenure of at least one 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 1.C.4 (6–9 vs. 12–20) and Table 1.C.5 (12–20 vs. 22–30) in the appendix. The results with respect to the age composition are shown in Table 1.C.6 in the appendix. The bottom panels of the tables show the estimates based on the restricted samples (tenure $\geq$ 1 year). Overall, the results show that the estimated reform effects on the composition of newly hired workers are throughout small and insignificant at any conventional level.[18] Regarding the age composition, growing establishments even

[17] In 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.

[18] Regarding the estimation with a different control and treatment group (see Table 1.C.5), the reform effect for individuals in growing establishments (column (4)) who have a tenure of at least one 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

exhibit a negative (albeit insignificant) coefficient (see Table 1.C.6, column (3)). For the restricted sample of individuals in growing establishments with a tenure of at least one year, the negative coefficient is quite large (see bottom panel of Table 1.C.6, column (3)). However, given that the reform should have caused growing employers in particular to hire more younger workers, this leads us to conclude that the results provide no evidence of any compositional selection effects for either the full or the restricted sample.

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 1.C.7 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 1.5.1 is driven by neither a compositional nor a dynamic selection effect.

Heterogeneous Effects

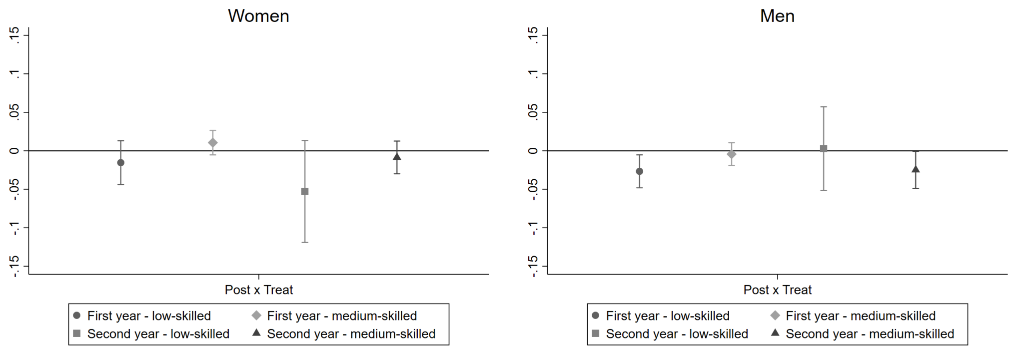
The perceived costs of less generous employment protection are likely to vary with individuals' labor market attachment and households' dependency on the affected individuals' labor earnings. To address potential heterogeneous effects, we distinguish between gender and skill groups. Due to sample size limitations, however, we are unable to perform separate analyses for high-skilled employees. Figure 1.5.2 shows the results for the different groups for the first and second years after establishment entry. 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) than the result in the baseline specification. Overall, the results suggest that male workers in particular respond to the change in dismissal protection.[19] Note that this result is broadly consistent with the evidence provided by Ziebarth (2013) suggesting that middle-

compositional selection effects.

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

aged 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.

Figure 1.5.2: Transition into Long-Term Sickness: Heterogeneous Effects



Notes: The figures show the coefficients of the differences-in-differences estimations with 95% confidence intervals stratified by gender and qualification. The corresponding regression tables can be found in the appendix (Tables 1.C.8 and 1.C.9).
Source: BASiD, own calculations.

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 1.B.2 and Table 1.B.3 in the appendix).

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 one year. The estimations support the descriptive results (see Table 1.C.10 in the appendix). There are neither differences across the two groups nor time effects. 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 section 1.5.1 (see Tables 1.C.11 and 1.C.12 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.

1.5.2 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 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.[20] 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, i.e., 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 1.A.3 in the appendix). To further ensure that we indeed observe *involuntary* unemployment, we count only transitions into unemployment spells lasting longer than four weeks as transitions into involuntary unemployment.

Descriptive Results

Figure 1.5.3 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 1.5.3 first indicates that unemployment durations are longer for both treatment and control individuals after the reform.[21] 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

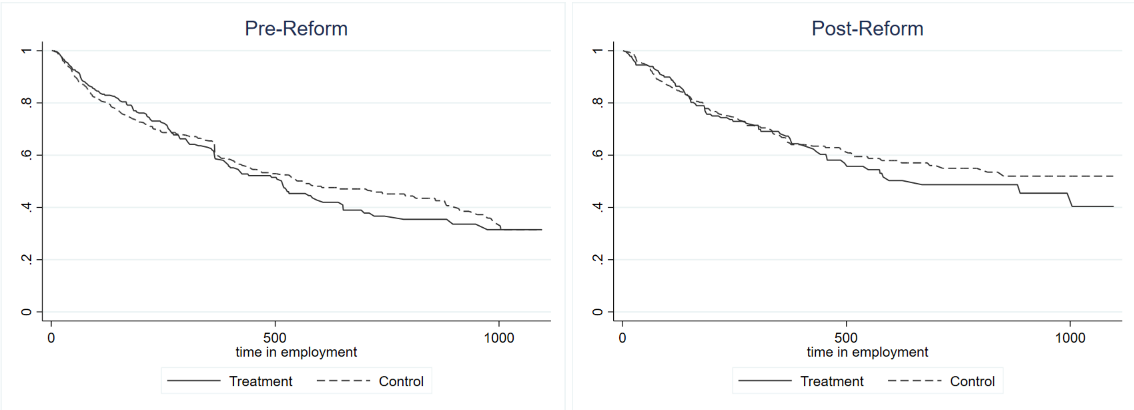
[20] We consider only individuals with sickness periods lasting no longer than 78 weeks in three years (this exclusion affects only 9 observations). After 78 weeks of sickness, sick pay expires, and the individual becomes subject to unemployment benefits. In these cases, we can no longer distinguish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

[21] The longer postreform unemployment durations are also reflected in the descriptive statistics for the whole sample (Table 1.B.1 and Table 1.B.2 in the appendix).

2005, only a fraction of individuals receiving means-tested welfare benefits were counted as involuntarily unemployed (for details, see Table 1.A.3 in the appendix). In the next section, we conduct robustness checks with respect to this structural break.

Figure 1.5.3 further shows that by approximately three years after having started a long-term sickness spell, a fraction of approximately 35 percent 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.

Figure 1.5.3: Transition into Unemployment after Long-Term Sickness – before and after Reform

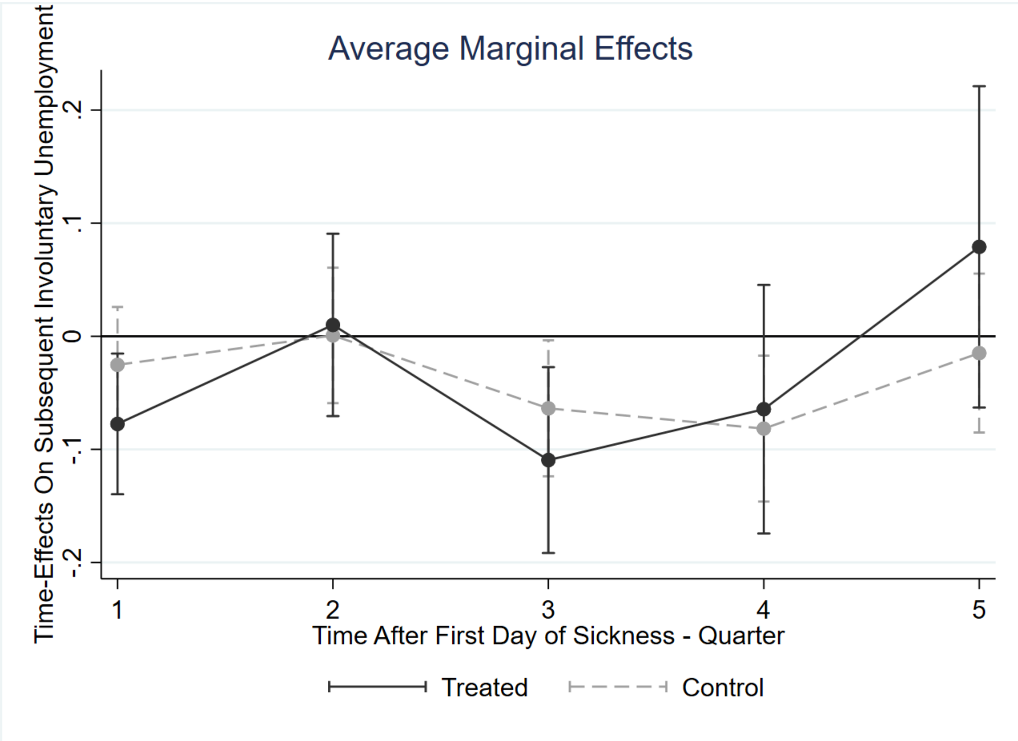


Notes: The figure shows the transitions into involuntary unemployment as a function of the time in the relevant employment. The treatment (control) group consists of workers working in establishments with 6–9 (12–20) FTE employees who entered the establishment three years before or three years after 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.

Regression Results

Figure 1.5.4 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 1.5.3. The magnitude and significance of the time effects is displayed in row (2) of Table 1.5.2. The figures indicate that in the third and fourth quarters, 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 1.5.2. 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.

Figure 1.5.4: Average Marginal Time Effects on the Transition into Unemployment after Long-Term Sickness



Notes: The figure shows the average marginal time effects with 90 percent 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 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.

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 one 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 1.C.14 in the appendix are similar to those reported in Table 1.5.2, suggesting no major significant reform effect on the probability of entering unemployment.

The results are also robust across several robustness checks similar to those in Section 1.5.1 (see Tables 1.C.13 and 1.C.14 in the appendix). 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 sick-

Table 1.5.2: Differences-in-Differences Estimations on Transition into Unemployment after Long-Term Sickness

Time after First Day of Sickness (Quarter)	1	2	3	4	5
Post x Treat	-0.052	0.009	-0.046	0.017	0.094
	(0.048)	(0.061)	(0.061)	(0.077)	(0.096)
Post	-0.045*	0.004	-0.081***	-0.075**	0.022
	(0.024)	(0.029)	(0.030)	(0.036)	(0.042)
Treat	-0.008	0.023	0.0194	0.056	0.057
	(0.024)	(0.031)	(0.032)	(0.040)	(0.047)

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). Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. 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.

ness, 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, as long as individuals who fall sick after the reform are characterized by less moral hazard behavior, these individuals are likely to reflect 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 cause either an upward or a downward bias in our estimates on the reform effects on unemployment transitions.

1.6 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*; i.e., 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/BAuA 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 1.D.1 and 1.D.2 in the appendix). Using this information, we generate dummy variables measuring the incidence and length of presenteeism and absence periods.[22] More precisely, we generate a dummy variable equal to one for an individual reporting more than zero, five, ten or 15 working days of presenteeism or absence per year.[23] 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 to 49 employees and zero for workers in establishments with five to nine employees.[24] 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 1.D.3 in the appendix). 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.

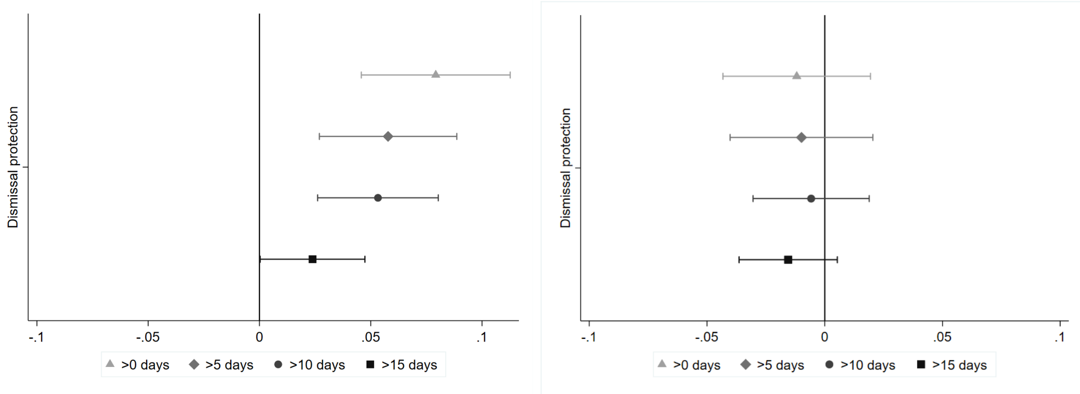
To analyze the association between dismissal protection and both presenteeism and absenteeism, we run probit regressions that control for observables, such as sociodemographic information, working strains and subjective health status (for a similar analysis, see Hirsch et al. 2017). Figure 1.6.1 shows 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 1.D.4 in the appendix). This association is 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.

[22] With 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.

[23] Due 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 six weeks.

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

Figure 1.6.1: Marginal Effects of Dismissal Protection on Absence and Presenteeism



Notes: 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). 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 percent 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 1.D.1 and 1.D.2 in the appendix.
Source: BiBB/BAuA Employment Survey 2012, own calculations.

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 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 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.

1.7 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 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 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 six 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 percent. 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 the establishments than the employees themselves 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.

1.8 Appendix

1.A.1 Appendix A: Data Description BASiD

Table 1.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 high school 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 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 earnings as documented in Appendix 1 of 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 1.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.

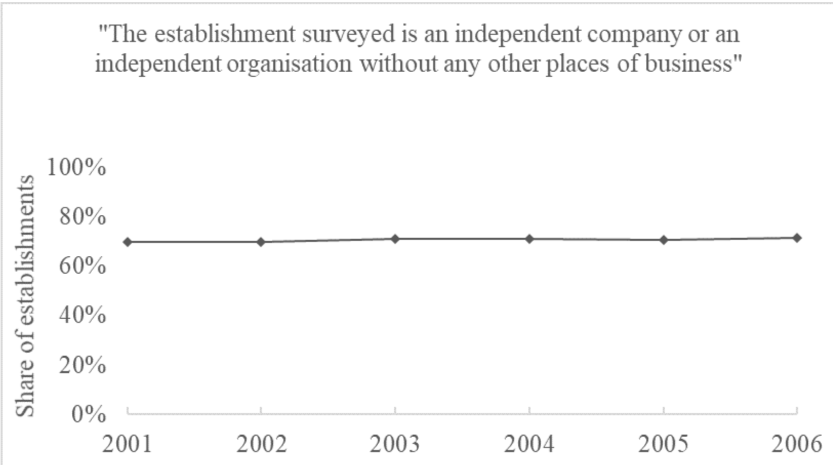
Table 1.A.3: Description of Labor Market States

Labor Market States
Employment Employment spells include continuous periods of employment (allowing gaps of up to four weeks) subject to social security contributions (excluding minor employment and periods of apprenticeship). Further, we ensure that the daily wage reported exceeds a certain threshold (7 EUR).
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 – 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 for being recorded as unemployed in the Pension Register. Spells prior to 2005 with social assistance benefits fulfill these requirements only if individuals were registered as unemployed. Otherwise these spells are recorded as nonemployment spells. As a consequence, the Pension Register does not permit a consistent definition of unemployment and nonemployment prior to and after 2005.
Distinction between Unemployment and Nonemployment According to the procedure proposed by Lee & 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 nonemployment spells (involving voluntary unemployment or an exit from the social security labor 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 nonemployment.
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 1.A.4: On the Definitions of Establishments

Definitions of Establishments
Legal Definition of Establishment The PADA does not contain its own definition of the term establishment. For this, the definition of § 1 BetrVG applies. According to this definition, an organizational unit is an establishment if the unit decides largely independently on working conditions and organizational issues and carries out personnel tasks 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 1.A.1: Share of Individual Establishments



Notes: The graph shows the share of establishments that are an independent company or an independent organization 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 1.A.5: Calculation of Establishment Size According to the PADA

Weighting Procedure	Working Time	Weighting Factor
	$>$ 30 hours/week	1
	$\leq$ 30 hours/week	0.75
	$\leq$ 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 1.A.6: Description of Group Assignment

Variables for Group Assignment

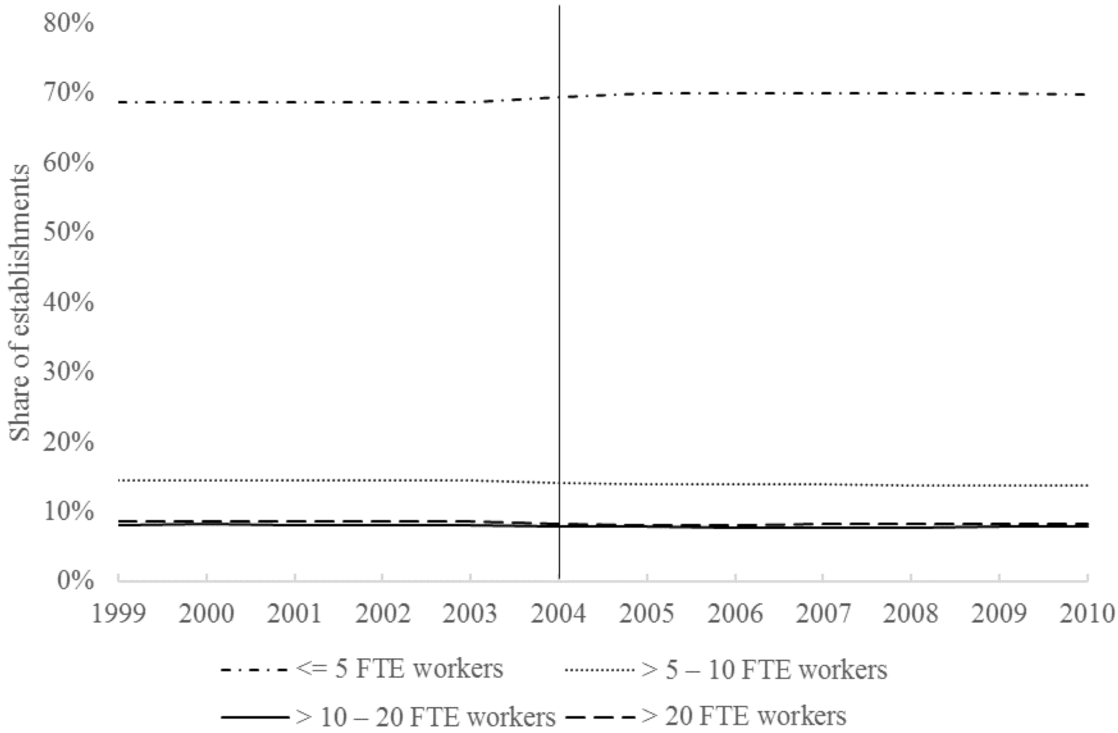
Entry in Establishment First employment spell subject to social insurance contributions in an establishment of relevant size between 1.1.2001 and 30.6.2003 or 1.1.2004 and 30.6.2006, respectively (for definition of employment, see Table 1.A.3). Establishments in the shipping and aircraft transportation sector are excluded, as they are subject to specific legislation. We exclude individuals who were previously marginally employed or employed as an apprentice by the same employer. We further exclude recalls within up to three years.

Establishment Size Number of full-time-equivalent workers according to the PADA as described in Table 1.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.

1.B.2 Appendix B: Descriptives

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



Notes: The establishment size is calculated from the number of full-time-equivalent workers as stipulated in the PADA (see section 1.3.2). Apprentices are excluded from the calculation; workers working full-time are counted as one worker; workers working "mini-part-time" (< 18 hours) and workers in marginal employment are weighted by the factor 0.5; workers working "midi-part-time" (>= 18 hours) are weighted by the factor 0.75.
Source: Establishment History Panel (BHP), 1999–2010, own calculations.

Table 1.B.1: Descriptive Statistics I

	Prereform					
	(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. Nonemployment 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 Nonemployment Spells	2.168	2.597	2.186	2.670	0.019	
# of Individuals in Baseline Sample	5,970		9,059			

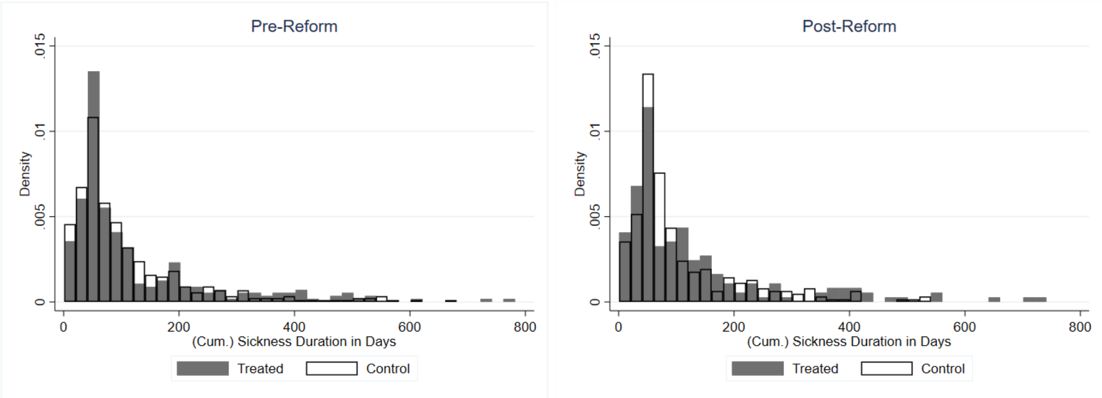
Notes: The table reports descriptive statistics of relevant characteristics of the treatment and control group before the reform. The treatment (control) group consists of employees working in establishments of 6–9 (12–20) FTE employees who entered the establishment between 1.1.2001 and 30.6.2003. *, ** and *** denote statistical significance of the difference in means between the treatment and control groups at the 10%, 5% and 1% levels (t test). For the definition and construction of the variables, see Tables 1.A.1, 1.A.2 and 1.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 1.B.2: Descriptive Statistics II

	Postreform					
	(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. Nonemployment 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 Nonemployment 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 groups 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 means between the treatment and control groups at the 10%, 5% and 1% levels (t test). For the definition and construction of the variables, see Tables 1.A.1, 1.A.2 and 1.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 1.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 the sickness spell 42 days before the start date of the absence reported in the data. Nineteen (25) observations are censored, as these persons were still ill at the end of the observation period on December 31, 2003 (2006).
Source: BASiD, own calculations.

Table 1.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 durations in days. We sum up all long-term sickness days during the relevant employment period (cumulative duration). Taking into account that sickness spells are reported after six weeks in our data, we calculate the entire number of absence days, by setting the start date of the sickness spell 42 days before the start date of the absence 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.
Source: BASiD, own calculations.

1.C.3 Appendix C: Further Results and Robustness Checks

Table 1.C.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 x Treat	-0.008**	-0.002	-0.003	-0.002	0.002
	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)
Treat	0.001	-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 Nonemployment Spellss				-0.000	0.000
				(0.001)	(0.001)
Industry Dummies			✓	✓	✓
Year Dummies		✓	✓	✓	✓
Constant	0.036***	0.071***	0.082***	0.058***	0.073***
	(0.001)	(0.004)	(0.007)	(0.008)	(0.008)
Observations	27,967	27,967	27,967	27,967	29,373
R-squared	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. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. 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 1.A.1, 1.A.2 and 1.A.3. The placebo regression hypothetically assumes that the dismissal protection reform took place in 2003.
Source: BASiD, own calculations.

Table 1.C.2: Overview: Robustness Estimations on the Transition into Long-Term Sickness

No.	Description
(1)	Transitions into short sickness periods (less than 10 days) are excluded from our baseline specification as these periods may be due to sickness of a child. The health insurance system 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 in this control group were not subject to the PADA before and after the reform.
(3)	The treatment (control) group consists of establishments with more than 5–10 (11–20) FTE workers.
(4)	Short sickness periods for the alternative control group with 0.5–4 FTE workers are excluded.
(5)	The treatment (control) group consists of establishments with more than 5–10 (0.5–<5) FTE workers.
(6)	2003-placebo regression using the alternative control group with 0.5–4 FTE workers.
(7)	Probit estimations The marginal effects remain largely unaltered in the nonlinear model specifications (results not shown, but available upon request).

Table 1.C.3: Robustness Estimations on the 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)
Treat	0.006	0.012***	0.004	0.008**	0.010***	0.011**
	(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	0.020**	0.043***	0.046***	0.027***	0.039***	0.032***
	(0.009)	(0.008)	(0.009)	(0.007)	(0.007)	(0.008)
Observations	8,845	14,548	15,360	14,548	21,172	14,684
R^2	0.022	0.024	0.025	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 1.C.2. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses and are adjusted for clustering at the establishment level. For the definition and construction of the variables, see Tables 1.A.1, 1.A.2 and 1.A.3.
Source: BASiD, own calculations.

Table 1.C.4: Selection Analysis Ia: Individual Characteristics at the Time of Entry (6–9 vs. 12–20 FTE workers)

	Individual Illness History – Full Sample			
	(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
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***	0.209***
	(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
	Individual Illness History – Individuals $>$ 1 Year Tenure			
	(1)	(2)	(3)	(4)
	All sickness periods	Long sickness periods	Shrinking est.	Growing est.
Post x Treat	-0.006	-0.004	0.011	-0.019
	(0.016)	(0.015)	(0.025)	(0.021)
Treat	-0.007	-0.004	-0.012	-0.003
	(0.011)	(0.011)	(0.018)	(0.014)
Individual Characteristics	✓	✓	✓	✓
Employment-Related Characteristics	✓	✓	✓	✓
Year Dummies	✓	✓	✓	✓
Establishment Characteristics	✓	✓	✓	✓
Individual Employment History	✓	✓	✓	✓
Constant	0.202***	0.150***	0.235***	0.174***
	(0.026)	(0.026)	(0.042)	(0.032)
Observations	8,845	8,845	3,587	5,258
R^2	0.386	0.372	0.391	0.388

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. The treatment (control) group consists of workers entering an establishment of 6–9 (12–20) FTE workers. Significance level: *** $p < 0.01$. 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; and (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 analyze 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 calendar year (usually in January) with the number of FTE workers at the end of the same calendar year (usually in December). For the definition and construction of the variables, see Tables 1.A.1, 1.A.2 and 1.A.3. In contrast to in the other analyses, here, we do not include the individual illness history as control variable.
Source: BASiD, own calculations.

Table 1.C.5: Selection Analysis Ib: Individual Characteristics at the Time of Entry (12–20 vs. 22–30 FTE workers)

	Individual Illness History – Full Sample			
	(1)	(2)	(3)	(4)
	All sickness periods	Long sickness periods	Shrinking est.	Growing est.
Post x Treat	-0.004	-0.006	-0.001	-0.006
	(0.009)	(0.009)	(0.015)	(0.011)
Treat	-0.012*	-0.009	-0.014	-0.011
	(0.006)	(0.006)	(0.010)	(0.008)
Individual Characteristics	✓	✓	✓	✓
Employment-Related Characteristics	✓	✓	✓	✓
Year Dummies	✓	✓	✓	✓
Establishment Characteristics	✓	✓	✓	✓
Individual Employment History	✓	✓	✓	✓
Constant	0.222***	0.169***	0.223***	0.221***
	(0.015)	(0.015)	(0.024)	(0.019)
Observations	27,478	27,478	10,384	17,094
R^2	0.368	0.352	0.376	0.365
	Individual Illness History – Individuals $>$ 1 Year Tenure			
	(1)	(2)	(3)	(4)
	All sickness periods	Long sickness periods	Shrinking est.	Growing est.
Post x Treat	0.009	-0.003	-0.019	0.024
	(0.016)	(0.016)	(0.028)	(0.020)
Treat	-0.002	0.003	0.011	-0.010
	(0.011)	(0.011)	(0.019)	(0.014)
Individual Characteristics	✓	✓	✓	✓
Employment-Related Characteristics	✓	✓	✓	✓
Year Dummies	✓	✓	✓	✓
Establishment Characteristics	✓	✓	✓	✓
Individual Employment History	✓	✓	✓	✓
Constant	0.190***	0.143***	0.179***	0.192***
	(0.026)	(0.026)	(0.041)	(0.033)
Observations	8,511	8,511	3,190	5,321
R^2	0.384	0.365	0.384	0.388

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. The treatment (control) group consists of workers entering an establishment of 12–20 (22–30) FTE workers. Significance level: *** $p < 0.01$. 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; and (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 analyze 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 calendar year (usually in January) with the number of FTE workers at the end of the same calendar year (usually in December). For the definition and construction of the variables, see Tables 1.A.1, 1.A.2 and 1.A.3. In contrast to in the other analyses, here, we do not include the individual illness history as control variable.
Source: BASiD, own calculations.

Table 1.C.6: Selection Analysis II: Individual Characteristics at the Time of Entry

	Age (<25 Years) – Full Sample		
	(1)	(2)	(3)
	All estab.	Shrinking estab.	Growing estab.
Post x Treat	-0.002	0.007	-0.010
	(0.006)	(0.009)	(0.008)
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
	Age (<25 Years) – Individuals $>$ 1 Year Tenure		
	(1)	(2)	(3)
	All estab.	Shrinking estab.	Growing estab.
Post x Treat	-0.008	0.005	-0.020
	(0.011)	(0.016)	(0.014)
Treat	0.011	0.004	0.016*
	(0.007)	(0.012)	(0.010)
Individual Characteristics	✓	✓	✓
Employment-Related Characteristics	✓	✓	✓
Year Dummies	✓	✓	✓
Establishment Characteristics	✓	✓	✓
Individual Employment and Sickness History	✓	✓	✓
Constant	0.120***	0.140***	0.105***
	(0.015)	(0.022)	(0.020)
Observations	8,845	3,587	5,258
R^2	0.663	0.667	0.665

Notes: The table shows results of a linear probability model estimating the probability of being younger than 25 years at the time of entry. The treatment (control) group consists of workers entering an establishment of 6–9 (12–20) FTE workers. Significance levels: * $p < 0.1$, *** $p < 0.01$. 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; and in (4), we analyze 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 calendar year (usually in January) with the number of FTE workers at the end of the same calendar year (usually in December). For the definition and construction of the variables, see Tables 1.A.1, 1.A.2 and 1.A.3.
Source: BASiD, own calculations.

Table 1.C.7: Selection Analysis III: Probability of Retention One Year after Entry

	(1)
Post x Treat	-0.006
	(0.013)
Treat	-0.019**
	(0.009)
Individual Characteristics	✓
Employment-Related Characteristics	✓
Year Dummies	✓
Establishment Characteristics	✓
Individual Employment and Sickness History	✓
Constant	0.251***
	(0.020)
Observations	21,218
R^2	0.122

Notes: The table shows results of a linear probability model estimating the probability of being in the establishment one year after entry; Significance levels: ** $p < 0.05$, *** $p < 0.01$. 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. For the definition and construction of the variables, see Tables 1.A.1, 1.A.2 and 1.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 1.C.8: 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)
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. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. 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 1.A.1, 1.A.2 and 1.A.3.
Source: BASiD, own calculations.

Table 1.C.9: 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)
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. Significance levels: ** $p < 0.05$, *** $p < 0.01$. 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 1.A.1, 1.A.2 and 1.A.3.
Source: BASiD, own calculations.

Table 1.C.10: Regression Results on the Duration of Long-Term Sickness

	Full Sample				
	Model 1	Model 2	Model 3	Model 4	Placebo
Post x Treat	-1.297*	-0.322	-0.366	-0.287	0.501
	(0.683)	(0.842)	(0.843)	(0.842)	(0.724)
Treat	0.931*	0.483	0.395	0.394	-0.007
	(0.549)	(0.613)	(0.615)	(0.614)	(0.533)
Individual Characteristics	-	✓	✓	✓	✓
Employment-Related Characteristics	-	✓	✓	✓	✓
Year Dummies	-	✓	✓	✓	✓
Establishment Characteristics	-	-	✓	✓	✓
Individual Employment and Sickness History	-	-	-	✓	✓
Constant	4.770***	9.766***	10.483***	6.283***	8.258***
	(0.243)	(0.695)	(1.192)	(0.355)	(1.354)
Observations	28,127	28,127	28,127	28,127	29,373
R^2	0.000	0.008	0.010	0.020	0.016
	Individuals $>$ 1 Year Tenure				
	Model 1	Model 2	Model 3	Model 4	Placebo
Post x Treat	-2.451**	-2.073	-2.105	-2.071	0.195
	(1.124)	(1.394)	(1.394)	(1.396)	(1.324)
Treat	0.908	0.859	0.802	0.777	-0.196
	(0.981)	(1.099)	(1.107)	(1.110)	(1.041)
Individual Characteristics	-	✓	✓	✓	✓
Employment-Related Characteristics	-	✓	✓	✓	✓
Year Dummies	-	✓	✓	✓	✓
Establishment Characteristics	-	-	✓	✓	✓
Individual Employment and Sickness History	-	-	-	✓	✓
Constant	5.102***	11.944***	12.399***	7.457***	13.078***
	(0.401)	(1.236)	(1.794)	(2.179)	(2.572)
Observations	8,845	8,845	8,845	8,845	9,188
R^2	0.000	0.016	0.019	0.031	0.0265

Notes: The table shows results of a linear regression estimating the 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 the sickness spell 42 days before the start date of the absence reported in the data. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. 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 1.A.1, 1.A.2 and 1.A.3. The placebo regression hypothetically assumes that the dismissal protection reform took place in 2003.
Source: BASiD, own calculations.

Table 1.C.11: Overview: Robustness Estimations on the Duration of Long-Term Sickness

No.	**Description**
(1)	Transitions into short sickness periods (those of less than 10 days) are excluded, as these periods may be due to sickness of a child. The health insurance system 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 days caring for their ill children.
(2)	The control group is workers in establishments with 0.5–4 FTE workers.
(3)	The treatment (control) group consists of workers in establishments with 5–10 (11–20) FTE workers.

Table 1.C.12: Robustness Estimations on the Duration of Long-Term Sickness

	Full Sample		
	(1)	(2)	(3)
Post x Treat	-0.128	-0.692	-0.314
	(0.829)	(0.786)	(0.607)
Treat	0.378	0.602	0.255
	(0.601)	(0.561)	0.452
Individual Characteristics	✓	✓	✓
Employment-Related Characteristics	✓	✓	✓
Year Dummies	✓	✓	✓
Establishment Characteristics	✓	✓	✓
Individual Employment and Sickness History	✓	✓	✓
Constant	4.944***	6.871***	7.210***
	(1.320)	(1.173)	(1.153)
Observations	28,127	40,650	44,377
R^2	0.0172	0.019	0.015
	Individuals > 1 Year Tenure		
	(1)	(2)	(3)
Post x Treat	-1.859	-1.760	-1.603
	(1.343)	(1.273)	(0.992)
Treat	0.876	1.358	0.089
	(1.057)	(0.987)	(0.781)
Individual Characteristics	✓	✓	✓
Employment-Related Characteristics	✓	✓	✓
Year Dummies	✓	✓	✓
Establishment Characteristics	✓	✓	✓
Individual Employment and Sickness History	✓	✓	✓
Constant	4.858**	9.266***	10.251***
	(2.050)	(1.801)	(1.927)
Observations	8,845	14,548	15,360
R^2	0.024	0.024	0.027

Notes: The table shows results of a linear regression estimating the number of sickness days. The robustness checks are described in Table 1.C.11. Significance levels: ** $p < 0.05$, *** $p < 0.01$. 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 1.A.1, 1.A.2 and 1.A.3.
Source: BASiD, own calculations.

Table 1.C.13: Overview: Robustness Estimations on the Transition into Unemployment after Long-Term Sickness

No.	**Description**
(1)	Additionally takes 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 establishments with 5–10 (11–20) FTE workers.
(4)	Restricts the sample to workers with at least one long sickness spell (>10 days) during their relevant employment.
(5)	Restricts the sample to individuals employed one year before entering the establishment of interest.

Table 1.C.14: Robustness Estimations on the 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	-0.060	0.024	-0.046	0.020	0.097
			(0.050)	(0.062)	(0.062)	(0.078)	(0.098)
(2)	3,275	Post x Treat	-0.042	0.014	-0.070	-0.031	0.107
			(0.047)	(0.058)	(0.060)	(0.077)	(0.091)
(3)	4,088	Post x Treat	-0.029	0.003	-0.007	0.022	-0.033
			(0.037)	(0.043)	(0.046)	(0.056)	(0.059)
(4)	1,912	Post x Treat	-0.040	-0.008	-0.010	0.046	0.080
			(0.054)	(0.071)	(0.075)	(0.093)	(0.101)
(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 the sickness spell (time-discrete logit model). The robustness checks are described in Table 1.C.13. Standard errors are in parentheses and are adjusted for clustering at the establishment level. All estimations control for individual characteristics, employment-related characteristics, establishment characteristics and individual sickness and employment history.
Source: BASiD, own calculations.

1.D.4 Appendix D: Additional Survey Data Analyses

Table 1.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 had a response rate of 44.3 percent, yielding a representative cross-sectional sample of 20,036 individuals from the active labor force population. The survey data provide information on both the incidence and extent of sickness absences and presenteeism, subjective health status, tenure, stressful working conditions, qualifications and professional field as well as sociodemographic variables (for more details, see Rohrbach-Schmidt & Hall 2013).

Sample We restrict our estimation to employees working in establishments with from five to nine and 20 to 49 workers. Note that the BiBB/BAuA Employment Survey collects information only on how many individuals are employed by an establishment, regardless of their working time. Trainees are also counted. This means that the establishment size cannot be exactly calculated according to the regulations of the PADA (see Table 1.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 from 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 and individuals older than 65 years. After these exclusions, we obtain a sample of 2,549 observations with complete data on all relevant covariates.

Table 1.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 have instead 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 characterized by presenteeism, (2) at least five workdays characterized by presenteeism, (3) at least ten workdays characterized by presenteeism and (4) at least 15 days characterized by presenteeism.
Sickness Absences	*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 workdays of absence.
Education	*What is your highest general school leaving certificate? Low-skilled*: No degree or high school 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 drafts, 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 1.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 means between the treatment and control groups at the 10%, 5% and 1% levels (t test). For the definition and construction of the variables, see Table 1.D.2.
Source: BiBB/BAuA Employment Survey 2012, own calculations.

Table 1.D.4: Determinants of Absences and Presenteeism Episodes (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. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. For a detailed description of the variables, see Table 1.D.2.
Source: BiBB/BAuA Employment Survey 2012, own calculations.

Chapter 2

To Include or Not to Include? Firm Employment Decisions with Respect to the German Disabled Worker Quota

Abstract[*]

This paper analyzes whether financial disincentives affect firm demand for disabled workers. In Germany, firms must pay a noncompliance fine if they do not meet their legal quota for disabled workers. I exploit a threshold in this quota: Firms with fewer than 40 employees are required to employ one disabled worker, whereas firms with 40 or more employees must employ two disabled workers. Using administrative firm data, my results suggest that firms respond partially to the threshold and employ 0.388 more disabled workers when they are located just above the threshold. The effect remains positive after correcting for bunching behavior.

Keywords: disability, employment quota, noncompliance fine, administrative data
JEL Codes: J15, J21, J23, J71, J78

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2.1 Introduction

In all OECD countries individuals with disabilities experience low levels of employment and high unemployment rates reflecting their considerable labor market disadvantages. In 2019, individuals with disabilities were 2.3 times more likely to be unemployed than individuals without disabilities (OECD 2022). Furthermore, many employers prefer to pay a fine instead of employing individuals with (severe) disabilities. In Germany, approximately 26 percent of public and private employers with 20 or more employees preferred paying a noncompliance fine to employing any worker with a disability in 2021 (Federal Employment Agency 2023). In addition to discrimination tendencies and prejudices, employers may anticipate higher costs when considering hiring an individual with a (severe) disability. Such individuals may need special workplace equipment, are often subject to special employment protection regulations, take more vacation time and, on average, show higher rates of sickness absence (Hiesinger & Kubis 2022).

To improve the integration of disabled individuals into the labor market despite these costs, many OECD countries, such as Austria, Italy, France, Italy and Spain, have implemented policy reforms, often in the form of a mandatory employment quota combined with a monetary fine in the case of noncompliance (OECD 2010). Even though employment quotas and noncompliance fines are widely used policy instruments for integrating individuals with severe disabilities into the labor market, little is known about their effectiveness.

This paper analyzes the intended and unintended effects of the employment quota for disabled workers. I exploit a threshold in the German labor law that sets the mandatory employment quota: Employers that are below the 40-employee threshold but have at least 20 employees are obliged to employ at least one disabled individual. Above this threshold, employers must employ at least two disabled individuals. My empirical analysis consists of two parts. First, I estimate the *intended* effect of the quota, which is the effect of the threshold on the number of disabled workers in a firm (the "threshold effect"). Second, I analyze whether firms manipulate their employment levels and purposely stay – or bunch – below the threshold to avoid the fine. I refer to this as the *unintended* effect of the quota. For my analysis, I follow Lalive et al. (2013) and adopt a threshold design which is closely related to a regression discontinuity design (RDD). However, as I do find evidence of bunching, the naive threshold effect may be biased. Quantifying the magnitude of the bunching effect helps me to assess this bias and to bound the threshold effect.

Understanding the intended and unintended effects of any employment quota is crucial for two reasons. First, along with antidiscrimination legislation, mandatory employment quotas are one of the most common policies for integrating disabled workers into the labor market, as they are used in many OECD countries (OECD 2010). While the effects of antidiscrimination policies have been quite well explored, there is a remarkable paucity on the effects of employment quotas on firm demand for disabled workers. Given the alleged importance of these quotas for integration, this paucity is striking. Second, my study helps to better understand the role of employment quotas and financial disincentives for labor demand in general. The threshold in the regulatory employment quota implies a sharp change in relative labor costs for different firms near the threshold. Thus, this policy allows me to study the behavior of firms facing this discontinuity. This way, I explicitly address the unintended effects of the employment quota, such as adjustments in the (nondisabled) workforce composition or changes in firm dynamics near the threshold. I further distinguish between firms that face different costs at the threshold depending on the extent of their compliance with the quota regulation.

To date, few studies have addressed the impact of an employment quota on firm dynamics and on firm demand for disabled workers. A large number of studies have looked at either the effects of antidiscrimination legislation with respect to workers with disabilities (see, for example, Acemoglu & Angrist 2001, Beegle & Stock 2003, Kruse & Schur 2003, Bell & Heitmueller 2009) or the impact of disability policies on the employment of disabled workers from a labor supply perspective (see, for example, Verick 2004, Lechner & Vazquez-Alvarez 2011, Kostøl & Mogstad 2014, Autor et al. 2019, Barnay et al. 2019). However, to the best of my knowledge, only few studies have evaluated the effect of a disabled worker quota on employment decisions from a labor demand perspective.

Mori & Sakamoto (2018) study the disabled worker quota in Japan's manufacturing industry and find that a levy-grant scheme increases the employment of disabled workers. Szerman (2022) analyzes the disabled worker quota in Brazil, where firms with 100 employees and more must fill at least two percent of their positions with individuals with disabilities. By investigating the introduction of the quota, the author shows that individuals with disabilities in local labor markets more exposed to the reform experienced larger increases in employment. The employment of workers without disabilities, in contrast, is not affected by the reform. Lalive et al. (2013) examine whether there is a discontinuity in the employment of disabled individuals between firms below and above the Austrian employment quota, which kicks in when a firm reaches 25 nondisabled workers. The authors find that firms react to the quota in two ways. First, firm demand for disabled workers increases above the threshold. Second, some firms manipulate their employment of nondisabled workers and purposely stay below the threshold. Despite this manipulation, the lower bound of the threshold effect remains positive.

Similarly, Wagner et al. (2001) and Koller et al. (2006) examine firm dynamics at quota thresholds in Germany. While Wagner et al. (2001) do not find any evidence that there is an effect on employment growth at the first threshold within the employment quota, Koller et al. (2006) find evidence that employment growth slows slightly just before the second threshold. Wagner et al. (2001) argue that according to their results, the (first) threshold in the German disabled worker law "does not seem to have the kind of strong negative influence on job dynamics in small firms that is often attributed to it in public debates" (p. 10).

In sum, the few existing studies find positive effects of disabled worker quotas on the employment of disabled workers. Results on the effects of the quota on potential firm size manipulation are, however, less clear. I extend this scarce literature on employment quotas and labor demand and study the German case in more detail. I contribute to the literature in two ways: *First*, I revisit the findings obtained by Wagner et al. (2001) and Koller et al. (2006) and analyze the German employment quota with a high-quality data set that has more precise information on firm size according to the disabled worker law and the number of disabled workers in a firm. As both Wagner et al. (2001) and Koller et al. (2006) use establishment-level survey data – and combined with administrative data in the case of Koller et al. (2006) – , these studies are based on only a small number of observations (approximately 300–400 establishments). Due to data restrictions, Wagner et al. (2001) do not have information on the number of workers with disabilities in each establishment. Furthermore, that study could only approximate the number of workers in each establishment on the basis of the disabled worker law and thus probably suffers from measurement error.

In contrast, the data used in the present study – the Employment Statistics for Severely Disabled People (BsbM) – are based on the notification procedure used by the German Federal Employment Agency to determine compliance with the employment quota. Thus, this data set contains firm size information that is consistent with the definition of firm size stipulated in the German disabled worker law.[1] The data set contains information on *all* German firms subject to the employment obligation (i.e., firms with 20 or more employees). Thus, my analyses are based on a vast number of observations. Combined with an additional administrative data set from the Federal Employment Agency, namely,

[1] Note that the definition of firm/establishment size in German labor law is not consistent with that used in the German disabled worker law. Depending on the law, the (i) reference point (e.g., establishment, firm or employer), (ii) excluded employee group (e.g., freelancers, marginally employed workers or apprentices), (iii) measure of the number of employees (e.g., per capita or full-time-equivalents) and (iv) reference period (e.g., annual average of employees or normally employed workers) differ considerably. For an overview of German threshold regulations and their measurement, see Table I.2 and Koller (2010*a*).

the Establishment History Panel (BHP), I am able to describe all German firms around the firm size thresholds and each firm's workforce in great detail.

Second, while the few existing studies provide no or only weak evidence for firms purposely staying below the threshold of the disabled worker quota, I show that in Germany bunching plays an important role.[2] In doing so, I first provide evidence for the presence of bunching in the form of a discontinuity in firm size density. I then expand the study by Lalive et al. (2013), regarding the bunching behavior of firms below the threshold, by investigating whether firms adjust their (nondisabled) employment when close to the threshold. As labor costs increase at the threshold, firms just below the threshold may avoid crossing it. This may be done, for example, by extending the number of hours worked per employee or substituting workers who are counted toward the threshold number of employees with workers who are not counted (e.g., marginally employed workers). While Lalive et al. (2013) find that firms below and above the threshold are quite similar in the Austrian case, my results for the German case reveal considerable differences between those firms with regard to firm dynamics, workforce and productivity. I further systematize the potential bunching of firms along the costs these firms face at the threshold. In so doing, I distinguish *noncompliers*, which face the highest costs at the threshold, *perfect compliers*, which face lower costs at the threshold than *noncompliers*, and *overcompliers*, which do not face any additional costs at the threshold. Analyzing the extent to which these different types of firms bunch helps to clarify the role of (additional) labor costs.

My key findings are as follows: Firms above the threshold do in fact employ more disabled workers than firms below the threshold. Furthermore, there is clear evidence of firms bunching just below the threshold. Firms purposely stay below the threshold and adjust their workforce accordingly to avoid the (increase in the) noncompliance fine. This bunching is particularly pronounced among *noncompliers*, i.e., those firms that face the largest increase in costs at the threshold. Taking this bunching into account, I assess the bias in the threshold effect and find that even though firms manipulate their employment, the lower bound on the threshold effect is still positive.

The remainder of the paper is structured as follows: Section 2.2 describes the German institutional setting, and Section 2.3 discusses the theoretical framework developed by Lalive et al. (2013) for the German context. Section 2.4 presents the data set and the em-

[2] While Szerman (2022) finds no evidence that firms bunch below the 100 employee threshold in Brazil, Mori & Sakamoto (2018) provide evidence of potential bunching effects in Japan, but only for two of the eight thresholds considered. As spelled out above, Lalive et al. (2013) and Koller et al. (2006) provide some evidence for manipulation, while Wagner et al. (2001) do not find any slowdown effect on employment growth.

pirical strategy. Section 2.5 provides the empirical results for the intended and unintended effects, and Section 2.6 concludes.

2.2 The German Institutional Background

2.2.1 The Situation for Disabled Workers

In Germany, a special independent agency (*Versorgungsamt*) grants disability status once a medical expert diagnoses a physical, mental or psychological disorder that is not typical for the age of the patient. This disorder needs to be expected to last longer than six months and needs to impair the ability of the individual to participate in social life. Depending on the extent of the impairment, the medical expert evaluates the degree of the disability ranging from 20 to 100, graduated in steps of ten. An individual is defined as "severely disabled" if his or her degree of disability is greater than or equal to 50.[3] In the labor market, individuals with a degree of disability between 30 and 50 can be treated as severely disabled when their disability restricts their opportunities to find or hold a job. The decision to obtain disability status and to report that status to an employer is generally voluntary.[4]

In 2011, approximately 7.3 million individuals (8.9 percent of the total population) in Germany were recognized severely disabled. Since then, the number has continued to increase to over 7.8 million in 2021 (9.4 percent of the total population). Data from the Federal Statistical Office from 2011 show that mainly older workers show disabilities. A total of 53.4 percent of severely disabled individuals in Germany in 2011 were 65 years or older. The vast majority of disabilities – approximately 85 percent – are caused by illness. Hence, only a small percentage of disabilities are congenital or due to war injuries, accidents or other causes.

With regard to the degree of disability, almost a quarter (24.3 percent) of severely disabled individuals were assigned the highest degree of disability (100) in 2011, while 31.4 percent had a degree of disability of 50. Physical causes – in particular organ disorders – account for the majority of disabilities (approximately 62.3 percent). A total of 11.1 percent of disabled individuals had mental or emotional disabilities, and 9.0 percent suffered from cerebral disorders (Federal Statistical Office 2013, 2022).[5]

3 An example of a disability of degree 50 is voicelessness or a lip-jaw cleft until closure of the jaw cleft.

4 The obligation to disclose disability status to the employer exists only if the disability affects the occupational activity, e.g. in such a way that the disability puts the disabled individual or his/her colleagues at risk.

5 For the remaining fraction (17.6 percent), the type of the most severe disability is not indicated.

2.2.2 The German Disabled Worker Law

The legal framework for promoting the integration of people with disabilities in the labor market in Germany is laid down in part 3 of Book IX of the Social Code, "Integration and Rehabilitation of Disabled People (SGB IX, 2001)", also called the disabled worker law (*Schwerbehindertenrecht*). Enacted in 2001, it built upon the People with Severe Disabilities Act (PSDA), which was originally implemented in 1974. In 2018, the *Bundesteilhabegesetz* replaced the former law.[6] One key element of the disability law is the *employment obligation* for public and private employers to fill at least 5 percent of their positions with severely disabled workers.[7] Many other OECD countries, such as Austria, France, Italy and Spain, use similar quota systems to enforce the employment of workers with severe disabilities (OECD 2010).

Key to my analysis is the fact that the quota system applies only to employers exceeding a stipulated size.[8] Small firms with fewer than 20 (nondisabled and disabled) employees are exempt from the employment obligation.[9] Firms with 20 to less than 40 employees must employ at least one severely disabled individual, whereas firms with 40 to less than 60 employees must employ at least two severely disabled individuals.[10] Firms with 60 or more employees must meet the 5 percent quota. Firms that do not comply with this obligation must pay a graduated noncompliance fine (*Ausgleichsabgabe*).[11] Figure 2.2.1 provides an overview of the German quota regulation and the corresponding

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6 Between 2001 and 2018, some marginal changes were made to the law. These changes include, for example, the role of the Federal Employment Agency in integrating disabled individuals into the labor market. However, these changes do not relate to the employment obligation or the noncompliance fine and therefore should not affect the empirical analysis. In 2012 and 2016, the noncompliance fine was raised. Thus, I restrict my analyses to the period prior to 2012.

7 As spelled out in the previous section, "severely disabled" means that the degree of disability is at least 50 or – in case of restrictions with regard to job opportunities – at least 30.

8 "Employers" can be natural persons or legal entities under public or private law. To measure employer size, all employees of the same employer are counted together, regardless the number of establishments or other workplaces across which they are distributed (see also Koller et al. 2006). Thus, in the following, "employer" is used synonymously with "firm".

9 The German regulation refers to total employment in a firm, including disabled and nondisabled workers. In contrast, the Austrian quota, for example, refers only to the nondisabled workers. Further, the relevant measure for the firm size is the annual average of the monthly number of employees. For details on the firm size calculation, see Table 2.A.1 in the appendix.

10 The number of disabled workers in a firm is also measured as the annual average of the monthly number of disabled employees. Thus, a firm that e.g. claims to employ one disabled individual must prove a total of at least 12 months of employment of one (or more) severely disabled individual(s) in one calendar year.

11 Note that paying the noncompliance fine does not remove the employment obligation. Thus, employers can be penalized beyond the noncompliance fine with an additionally fine of up to 10,000 EUR if they *culpably* fail to comply with the employment obligation (§238 SGB IX). However, these additional fines are rarely imposed. For example, in 2010, only two fines were imposed (German Bundestag 2011).

noncompliance fines. It shows that the fine increases with the extent of noncompliance. For example, firms with 40 to less than 60 employees have to pay a fine of 105 EUR if they employ one severely disabled worker, but 180 EUR if they employ zero disabled workers.[12] The purpose of this noncompliance fine is to provide a financial incentive for firms to fulfil the employment obligation.[13] Firms may be inclined not to employ individuals with disabilities as their employment may have additional costs that arise, for example, from the need to purchase special workplace equipment for the disabled worker. Furthermore, employees with a recognized severely disabled status are institutionally better protected in two ways. First, they are subject to special dismissal protection. If the employee has been working longer than six months in a firm, the employer needs to obtain permission for a dismissal from the local integration office. Second, a severely disabled worker receives more vacation days, i.e., an additional five days per year. Thus, in order to make complying firms better off, noncomplying firms must pay the fine.[14]

As in almost all countries with a quota system, the employment quota is generally not met in Germany. In 2011, 60.1 percent of employers with 20 or more employees did not fulfill the employment obligation and thus had to pay the noncompliance fine. Furthermore, approximately one quarter (26.2 percent) did not employ any severely disabled workers. These percentages have remaind essentially unchanged since then. In general, public employers are better at fulfilling their quotas. The share of workers with a disability is particularly low in the hotel and restaurant industry and in the agricultural sector (Federal Employment Agency 2014).

2.3 Theoretical Considerations

This section mainly discusses the behavioral framework developed by Lalive et al. (2013). The basic idea behind this framework is that a threshold determining the quota for work-

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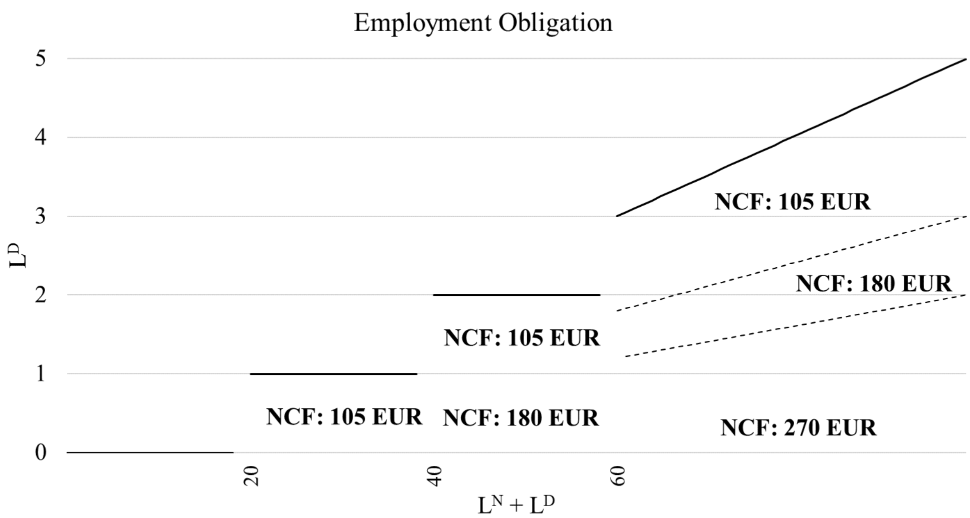
12 The monthly noncompliance fine is based on the number of unfilled positions, i.e., firms with 40 to less than 60 employees have to pay a fine of 1260 EUR/year (12*105 EUR) if the employ one severely disabled worker and 4320 EUR/year (12*2*180 EUR) if they employ zero disabled workers.

13 The fine must be paid to the integration offices and is used mainly to finance assistance for occupational rehabilitation for severely disabled individuals. In 2020, the revenue from the noncompliance fine in Germany amounted to almost 697 million EUR (Bundesarbeitsgemeinschaft der Integrationsämter und Hauptfürsorgestellen 2021).

14 Note that according to §223 SGB IX noncomplying firms can receive a 50 percent credit for the noncompliance fine by placing orders to officially recognized workshops for disabled people (*Werkstätten für behinderte Menschen*). These workshops are institutions aimed at enabling participation of severely disabled individuals in working life. Disabled individuals who, due to the nature or severity of their disabilities, are not (yet) able to return to the general labor market, receive adequate vocational training and employment opportunities.

Figure 2.2.1: Employment Obligation and Monthly Noncompliance Fines in Germany



Notes: The figure shows the legal regulations concerning the German employment quota and monthly noncompliance fines (NCF) according to §159 SGB IX during the observation period (2004–2011). L^D is the number of workers with disabilities that firms are obligated to employ; $L^N + L^D$ represents the number of employees in a firm (i.e., firm size). For firms with 60 and more employees, the noncompliance fine is based on the share of noncompliance: The fine is 270 EUR for firms with 0 to less than 2 percent of severely disabled workers, 180 EUR for firms with 2 to less than 3 percent of severely disabled workers and 105 EUR for firms with 3 to less than 5 percent of severely disabled workers. For details on the firm size calculation, see Table 2.A.1 in the appendix. The noncompliance fines increased in 2012, 2016 and 2021. The current fines are 140, 245 and 360 EUR per month and per unfilled position, respectively.
Source: Own illustration.

ers with disabilities may affect the demand not only for disabled workers but also for nondisabled workers. In what follows, I reformulate this framework for the German quota system. This framework serves to explain the bunching behavior of firms below the quota threshold T, where T refers to the total number of workers in the firm. To examine this behavior, I look at firms with $T-1$ employees and their decision to hire an additional (disabled or nondisabled) worker.[15]

2.3.1 Employment Decisions at the Quota Thresholds

Lalive et al. (2013) assume that nondisabled workers have productivity P, whereas disabled workers have productivity p which is less than P.[16] Because of antidiscrimination legislation, both disabled and nondisabled workers receive the same wage w.[17] The productivity of nondisabled workers exceeds the wage ($P > w$) in all firms, but firms differ in the value of p that obtains, i.e. $p < w$ in some firms and $p > w$ in other firms.[18] As the quota rule is based on a head count, labor L is assumed to be indivisible, while product demand Z is assumed to be continuous. Labor consists of L^N nondisabled and L^D disabled workers.

Let us first discuss this system in the *absence of a quota*. The firm's profit function can be described as

$$\pi_0(L^N, L^D) = min(PL^N + pL^D, Z) - w(L^N + L^D) \tag{2.1}$$

"Residual demand" is defined as the product demand Z minus the output produced by the L^N nondisabled workers:

$$R(Z, L^N) = Z - L^N P \tag{2.2}$$

15 Note that the framework presented below refers to the *first* threshold in Germany (20 employees) in order to explain the general logic behind the quota system. This logic also generalizes to higher thresholds. In subsection 2.3.2, I also discuss the implications of the framework for employment manipulation at the *second* threshold (40 employees).

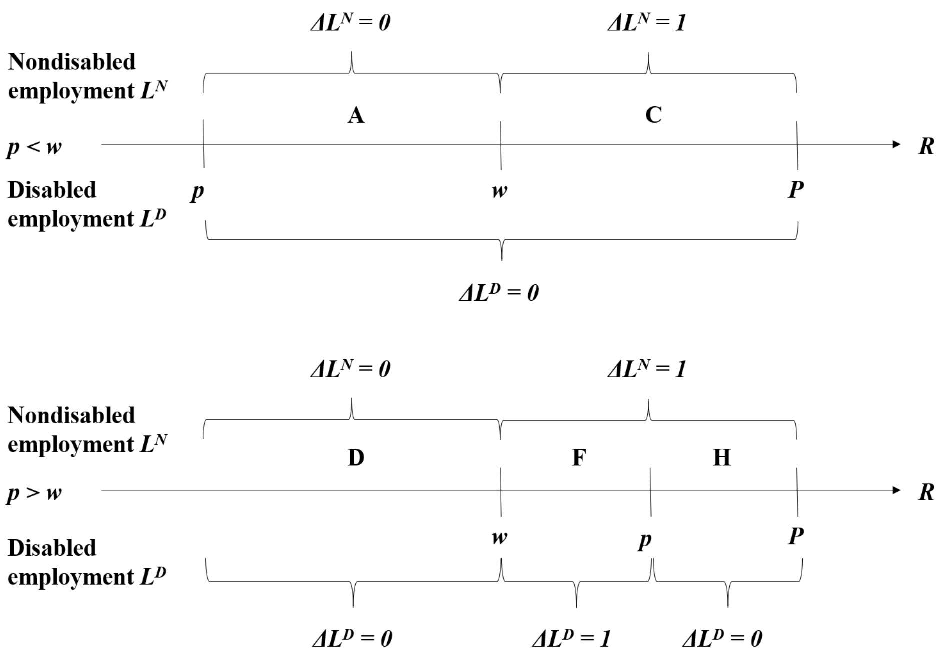
16 Assuming that $p < P$ is plausible in the German context: Survey results show that a considerable share of German establishments report a lower level of performance and resilience and a higher level of absence rates among workers with disabilities than among workers without disabilities (Hiesinger & Kubis 2022).

17 Note that I assume that firms face the same hiring costs for disabled or nondisabled workers. But as disabled individuals may have a lower labor force participation, hiring disabled workers may result in higher search costs. These additional search costs would then increase the marginal costs of hiring disabled workers. However, for the sake of simplicity, I abstract from including these additional costs in my framework.

18 Note that a workers' productivity within a firm may decline depending on the level of employment (see also footnote 15).

Figure 2.3.1 illustrates the firm's employment decisions in the absence of a quota for $p < w$ and $p > w$. In either case, the firm will not hire an additional nondisabled worker ($\Delta L^N = 0$) as long as residual demand is below the wage rate: $R(Z, L^N) < w$ (*A-* and *D-firms*). When residual demand exceeds the wage rate ($R(Z, L^N) > w$), the firm will hire an additional nondisabled worker ($\Delta L^N = 1$) (*C-firms*). However, the firm is not willing to hire a disabled worker ($\Delta L^D = 0$) as long as $p < w$: The productivity of the disabled worker is always too low to satisfy the residual demand. When $p > w$ and residual demand is in the range $R(Z, L^N) \in (w, p)$, the firm is indifferent between hiring a disabled or a nondisabled worker, as the productivity of both workers is great enough to satisfy the residual demand (*F-firms*). A firm with large residual demand $p < R(Z, L^N) < P$ will strictly prefer hiring a nondisabled worker over a disabled worker (*H-firms*).

Figure 2.3.1: No Quota



Notes: The figure shows the employment decisions of firms with $T - 1$ employees in the absence of a quota system.
Source: Own illustration based on the discussion in Lalive et al. (2013).

Now let us describe a system in the *presence of a quota*. In Germany, firms with total employment below the threshold T (i.e., $L^N + L^D < T$) do not face an employment obligation for disabled workers, whereas firms with total employment at or above T (i.e., $L^N + L^D \geq T$) need to employ at least one disabled worker. Note that the German system differs from the Austrian system presented in Lalive et al. (2013). Specifically, the quota in Germany is determined on the basis of total employment, i.e., nondisabled *and* disabled employment ($L^N + L^D$), whereas the quota system in Austria is based only on nondisabled (L^N) employment. Firms must pay a noncompliance fine τ if they do not satisfy their employment obligation. Following Lalive et al. (2013), I assume that $\tau < w$ and $\tau < P - p$.[19] The profit function in the presence of a quota given nondisabled employment L^N and disabled employment $L^D \in \{0, 1\}$ can be described as

$$\pi_1(L^N, L^D) = min(PL^N + pL^D, Z) - w(L^N + L^D) + min[L^D - 1(L^N + L^D \geq T), 0] \cdot \tau \tag{2.3}$$

$$\text{with } 1(L^N + L^D \geq T) = \begin{cases} 1 & \text{if } L^N + L^D \geq T \\ 0 & \text{if } L^N + L^D < T \end{cases}$$

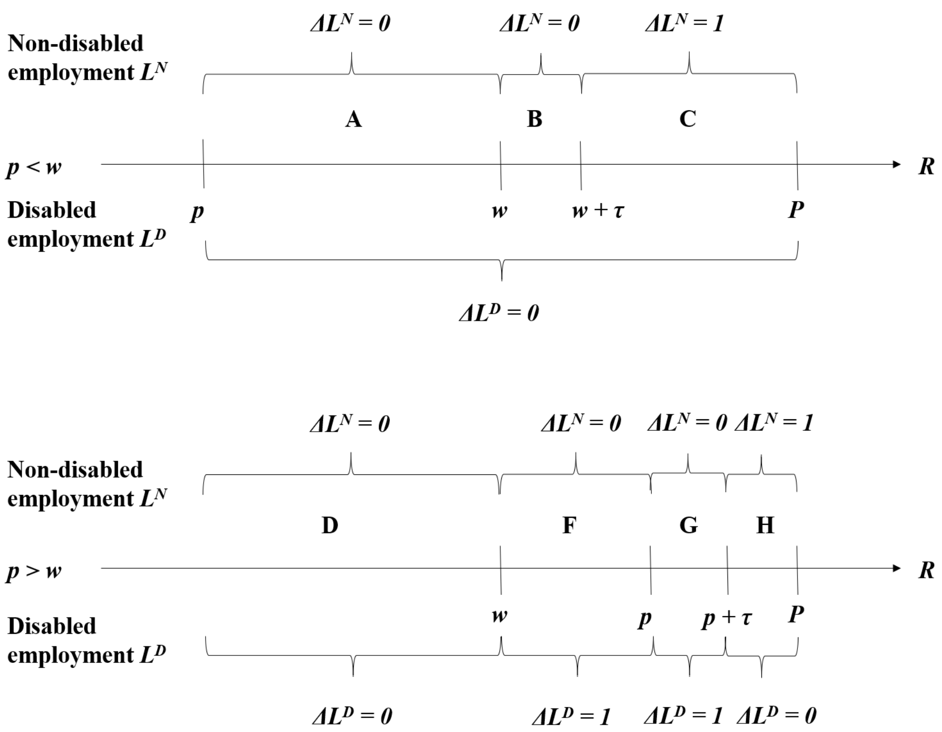
For employment decisions in the presence of a quota, I again distinguish between the cases $p < w$ and $p > w$, as shown in Figure 2.3.2. When $p < w$, firms will not hire any disabled workers, even in the presence of a quota ($\Delta L^D = 0$). However, the quota may affect nondisabled employment. A firm with residual demand R in the range $(w, w + \tau)$ will not hire an additional nondisabled worker ($\Delta L^N = 0$), whereas it would have hired that worker in the absence of the quota (see Figure 2.3.1). The marginal cost an additional nondisabled worker is now the wage w of this worker *plus* the tax τ. In the range $(w, w + \tau)$, this marginal cost is larger than the residual demand. Thus, firms with residual demand in this range, i.e., *B-Firms*, are better off setting their employment level just below the threshold, i.e., $L^N + L^D = T - 1$, to avoid the tax. Avoiding the tax by staying below the threshold – bunching – is the *unintended effect* of the quota.

For $p > w$, the decision to hire a disabled worker depends on the residual demand R. The quota rule does not affect *D-firms*, which have a low level of residual demand in the range $(0, w)$. As residual demand does not exceed the wage rate, an additional worker – disabled or not – would not produce the needed revenue. However, firms with

[19] Both assumptions are plausible in the German context. First, among firms with 20-60 employees, the noncompliance fine is only approximately 4.3-7.5 percent of gross monthly earnings. Second, the degree of disability (see Section 2.2.1) reflects the extent of the impairment caused by the disability. For severely disabled workers (those with a degree of disability of at least 50), it is plausible to assume that this impairment substantially affects their labor productivity. Furthermore, survey results show that a considerable share of German establishments report that workers with disabilities have a lower level of performance and resilience and a higher rate of absence than workers without disabilities (Hiesinger & Kubis 2022).

residual demand in the range (w, p), *F-firms*, and in the range $(p, p + \tau)$, *G-firms*, now prefer to hire a disabled worker instead of a nondisabled worker as they would have to pay the (additional) fine when hiring a nondisabled worker (without a quota, *F-firms* are indifferent between hiring a disabled or a nondisabled worker while *G-firms* prefer to hire a nondisabled worker and no disabled workers). Incentivizing firms to hire a disabled worker instead of a nondisabled worker is the aim of the quota and thus reflects its *intended effect*. *H-firms*, with residual demand in the range $(p + \tau, P)$, prefer hiring a nondisabled worker, as this worker generates higher profits despite the fine.[20]

Figure 2.3.2: Quota



Notes: The figure shows the employment decisions of firms with $T - 1$ employees in the presence of a quota system.
Source: Own illustration based on the discussion in Lalive et al. (2013).

Let us now have a closer look at manipulating firms. Lalive et al. (2013) define *manipulators* as firms that set their employment level below the threshold under the quota system but above it without such a system. The Austrian system induces *B-firms* and *G-firms*

[20] Lalive et al. (2013) also discuss employment decisions for firms at or above the threshold T (where T refers to the number of nondisabled workers), i.e., the decision to hire T or $T + 1$ workers. Those firms will hire an additional disabled worker when the residual demand is in the range $(w - \tau, w)$, as the marginal cost of hiring a disabled worker is $w - \tau$ and thus less than the residual demand. As I focus on employment decisions *below* the threshold, i.e., the decision to hire $T - 1$ or T workers, I do not discuss this case in more detail.

to manipulate employment, as the Austrian quota is defined on the basis of nondisabled employment. *B-firms* do not hire a disabled worker and set their nondisabled employment level below the threshold to avoid the tax. *G-firms* also set their nondisabled employment level below the threshold but hire a disabled worker (because he or she increases profit). In the Austrian system, *G-firms* that hire a disabled worker are still located *below* the threshold. However, in contrast to the Austrian system, the German system induces only *B-firms* to manipulate their employment. As the German quota is based on total – i.e., nondisabled and disabled – employment, *G-firms* cross the threshold when they hire a disabled worker and are located *above* the threshold. Thus, I expect potential manipulation to arise entirely from *B-firms*, for which $p < w$ and which purposely stay below the threshold to avoid the fine.

How does this manipulation bias the difference in the average number of disabled workers between firms with $T - 1$ employees and firms with T employees? Due to the quota, the composition of firms with $T - 1$ employees changes: *B-firms* would have hired an additional nondisabled worker without the quota (i.e., they would have chosen employment level T) but now bunch below the threshold in the presence of the quota. As *B-firms* are not willing to hire an additional disabled worker, the difference in the average number of disabled workers between firms below and at the threshold is *overestimated*.

Manipulating firms may further adjust their employment (Koller et al. 2006): To avoid crossing the threshold, *B-firms* may extend the number of hours worked for (incumbent) employees. Note that forcing workers to work overtime could be costly due to overtime pay. However, a firm may, for example, substitute part-time workers with full-time workers. A second option includes substituting workers who are counted when determining whether the firm is subject to the quota (e.g., regularly employed workers) with workers who are not so counted (e.g., marginally employed workers). Note that this would only be the case when the productivity of the members of these working groups is sufficient to meet the product demand. In sum, bunching may lead to different wage and employment structures in a firm.

2.3.2 Manipulation of Employment at the Second Threshold

As my empirical analysis focuses on the second threshold of 40 employees, let us now briefly discuss the marginal cost for firms with employment just below this *second* threshold (i.e., $L^N + L^D = T_2 - 1$). As shown in Figure 2.2.1, the German labor law defines two levels of the fine depending on the *initial* level of disabled employment around the second threshold (τ_1 and τ_2 with $\tau_2 > \tau_1$).[21] Thus, there are three types of firms with $L^N + L^D = T_2 - 1$ employees: First, firms with an initial level of disabled employment $L^D = 0$ already pay the noncompliance fine τ_1. When crossing the threshold, the noncompliance fine increases to $2 \cdot \tau_2 - \tau_1$, provided that it is not the hiring of disabled worker that causes the firms to cross the threshold. I refer to these firms as *noncompliers*. Second, firms with an initial level of disabled employment $L^D = 1$ are in perfect compliance with the quota rule. When crossing the threshold, these firms would be obliged to pay the noncompliance fine τ_1, again provided that they do not hire another disabled worker. I refer to these firms as *perfect compliers*. In the German context during the observation period, *perfect compliers* (*noncompliers*) face additional costs of 1260 EUR/year (3060 EUR/year) at the threshold. Finally, firms with $L^D \geq 2$ already employ more disabled workers than required by law. These firms do not face any additional costs at the threshold, as they do not have to pay a noncompliance fine regardless of whether they are below or above T_2. I refer to these firms as *overcompliers*.

What are the consequences for employment manipulation at T_2? As shown in Section 2.3.1, employment manipulation arises entirely from *B-firms*, for which $p < w$. I identify two types of manipulating firms according to the marginal cost that they face at T_2. First, *B-noncompliers* are firms with no disabled workers and with residual demand in the range $(w, w + 2 \cdot \tau_2 - \tau_1)$. Second, *B-perfect compliers* are firms that already have one disabled worker and residual demand in the range $(w, w + \tau_1)$.[22] Since the range of bunching is larger among *B-noncompliers* as shown in Figure 2.3.3, I expect bunching to be more pronounced among this type of firm than among *B-perfect compliers*.

2.4 Empirical Strategy, Data and Variables

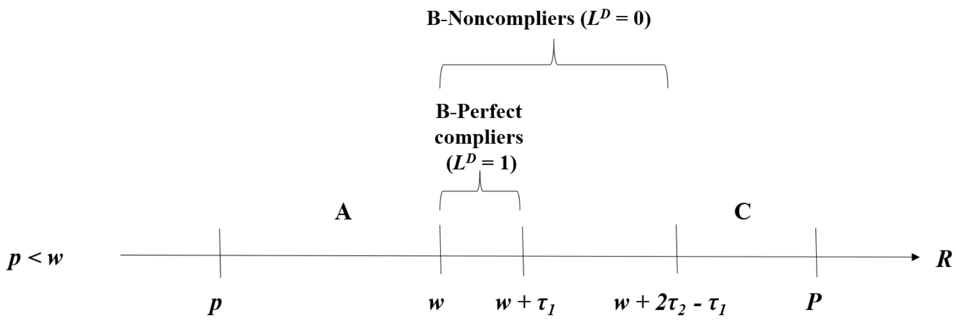
2.4.1 Data

My empirical analysis is based on two administrative data sets from the German Federal Employment Agency. The Employment Statistics for Severely Disabled People (BsbM) is an annual set of statistics that has been available since 2003 and that includes informa-

[21] Note that I treat the *initial* level of disabled employment as given and discuss only the decision of firms with $L^N + L^D = T_2 - 1$ employees to hire an *additional* disabled or nondisabled worker.

[22] The fact that I allow *perfect compliers* to bunch even though they have already hired one disabled worker may be rationalized by a decreasing marginal product. This assumption implies that beyond a given level of employment, the ratio of p to w may change. Thus, for some firms, the productivity of the first disabled worker exceeded the wage (i.e., $p > w$) when the firm had a lower employment level (for example, at the first threshold), while this is no longer the case at the second threshold (i.e., $p < w$).

Figure 2.3.3: Types of B-Firms at T_2



Notes: The figure shows the employment decisions of firms at the second threshold. *Perfect compliers* are firms with one disabled worker, and *noncompliers* are firms with no disabled workers. *A-firms* are firms that do not hire a nondisabled worker, whereas *C-firms* are firms that hire a nondisabled worker. *B-firms* are firms that would hire a nondisabled worker in the absence of a quota system but would not hire a nondisabled worker in the presence of such a system (see Figures 2.3.1 and 2.3.2).
Source: Own illustration.

tion on the employment of disabled workers in firms. Firms with 20 or more employees must annually declare (i) how many individuals they employ and (ii) how many of them are severely disabled. Thus, the information on firm size and the number of disabled workers stems directly from the notifying procedure used to control compliance with the disabled worker quota. As a consequence, the BsbM has the great advantage of providing information on firm size that is consistent with the legal definition stipulated in the disabled worker law.[23] Note that many studies that have analyzed regulations with a firm size criterion, e.g., in the context of dismissal protection, have tried to approximate the firm size stipulated in the respective law (see, for example, Wagner et al. 2001, Bauer et al. 2007, Bauernschuster 2013, Hijzen et al. 2017). Thus, these analyses often suffer from a considerable amount of measurement error, which can be ruled out in my case. In addition to some basic information about the firm, such as region and industry, the BsbM contains an identifier for the establishment or the main establishment in the case of multiestablishment firms.

This identifier allows me to merge additional information from the establishment data of the Federal Employment Agency, namely, the Establishment History Panel (BHP) (Schmucker et al. 2018). Since I consider only small businesses with up to a maximum of 80 employees, the vast majority of cases, each firm consists of one establishment.[24] This

23 For details on the definition of firm size according to the disabled worker law, see Table 2.A.1 in the appendix.

24 According to the establishment panel, a representative survey of establishments in Germany, a vast majority of establishments are independent companies without any other places of business. This is particularly true for small establishments, thus minimizing the error from treating establishments as single firms. For details, see Figure 2.A.1 in the appendix. In the case of multiestablishment firms,

allows me to merge the firm data with the establishment data. The Establishment History Panel provides annual detailed information on each establishment's workforce, e.g., regarding skills or employment type (marginal, part-time, full-time), as of the reference date, June 30th.

2.4.2 Empirical Strategy

The employment obligation for firms, which varies according to the firm size thresholds, provides a natural application for a "threshold design" (Lalive et al. 2013).[25] I use the second threshold and compare the various outcome variables, most importantly the number of workers with disabilities, of firms just below and just above the threshold of 40 employees.[26] The key assumption for identifying the effect of the quota is that firm demand for disabled workers would be continuous in the absence of the employment obligation. This assumption is reasonable, as no rules – other than the disabled worker quota – take effect when firms change their employment levels around the thresholds.[27] However, as the noncompliance costs rise sharply at the thresholds, firms may indeed choose to manipulate their employment levels in the presence of the disabled worker quota and purposely stay below the threshold to avoid this additional fine.

Therefore, following Lalive et al. (2013), my empirical analysis consists of two parts. First, I estimate the intended threshold effect, which is the (naive) effect of the threshold regulation on the number of disabled workers. Second, I report the unintended bunching effect, which is the effect of the threshold regulation on firm size. The bunching effect thus indicates the maximum number of firms at the threshold that manipulate their size. Taking this potential bunching effect into account, I am able to bound the threshold effect.

the establishment information in the Establishment History Panel (i.e., the information on the employment, wage and skill structures) refers only to the main establishment. Thus, for multiestablishment firms, the "bunching behavior" analyses (see Section 2.5.2) would be biased if the wage, skill and employment structures in the main establishment differs substantially from the wage, skill and employment structures in the branch offices. As a robustness check, I exclude those firms that I can identify as multiestablishment firms. For more details, see Section 2.5.5.

25 Although closely related to an RDD, the threshold design has a slightly different setup than the RDD, as it explicitly addresses the manipulation of the running variable (firm size in my case) and thus a violation of the identifying assumption of an RDD. The estimation techniques are, however, very similar.

26 Due to data limitations, I cannot exploit the first threshold of 20 employees, as the BsbM data set covers only firms affected by the employment obligation, i.e., firms with 20 or more employees. Furthermore, I do not focus on the third threshold of 60 employees for two reasons. First, the assumption that a firm consists of only one establishment (see Section 2.4.1) is more plausible for smaller firms. Second, there are additional labor law rules with thresholds (apart from the disabled worker quota) for firms with at least 60 employees (see Section 2.5.5 and Table I.2). Thus, I cannot ensure that the effects that I find for this threshold are due solely to the disabled worker quota.

27 For an overview of German threshold regulations, see Table I.2 and Koller (2010*a*).

To estimate the *threshold effect*, I rely on graphical analyses to provide some first intuition. For this, I plot the local averages of the number of disabled employees per firm size category. In my case, the firm size categories are defined by the total numbers of employees in a firm. I complement this nonparametric analysis with local polynomial regressions using the following baseline equation:

$$Y_i = \beta_0 + D_i\,\beta_1 + (1 - D_i)\,c_i\,\beta_2^- + D_i\,c_i\,\beta_2^+ + X_i\,\beta_3 + \epsilon_i, \tag{2.4}$$

where Y_i is the outcome variable, i.e., the number of disabled workers in firm i. D_i is a treatment dummy indicating whether the firm is above the critical threshold of 40 employees and thus obliged to employ one additional severely disabled worker according to the law. c_i is the running variable that the cutoff is based on (firm size).[28] c_i is defined as deviation from the treatment cutoff, i.e., $c_i = C_i - C$, where C denotes the cutoff.[29] The superscripts '$-$' and '$+$' indicate whether the coefficient relates to the left- or right-hand side of the threshold. X_i represents a vector of control variables capturing predetermined observable firm characteristics. Including these predetermined characteristics helps to reduce the sampling variability in the estimator (Calonico et al. 2019). ϵ_i represents the error term. All coefficient estimates are obtained from a local linear regression that weights all observations by their deviations from the cutoff using a triangular kernel.[30]

The aim of the analysis is to extrapolate the counterfactual number of disabled workers in firms at the threshold in the absence of the noncompliance fine. Equation (2.4) assumes a linear functional form (with polynomial order po=1). To assess the sensitivity of estimates to the functional form, I add higher-order polynomials to the linear model. In doing so, I additionally use polynomials in the running variable of order 2, 3 and 4.[31] Regarding the bandwidth – the window of relevant observations around the threshold – I choose a mean square error optimal (MSE-optimal) bandwidth for each side of the threshold (Calonico et al. 2020, Cattaneo & Vazquez-Bare 2016).[32] Furthermore, as I use

[28] Note that this running variable is discrete and takes on 40 distinct values (mass points) between 20 and 59 employees. However, as the number of observations per mass point is sufficiently large (approximately 7,000-30,000 observations per mass point), I can apply the continuity approach presented above (Cattaneo, Irobo & Titiunik 2018*b*).

[29] In my case, C_i refers to the total number of (disabled and nondisabled) employees in firm i.

[30] This weight is optimal in the MSE-optimal context (Cattaneo, Irobo & Titiunik 2018*a*).

[31] Equation 2.4 for any polynomial order po is: $Y_i = \beta_0 + D_i\,\beta_1 + (1-D_i)\,r_{po}\,(c_i)\,\beta_2^- + D_i\,r_{po}\,(c_i)\,\beta_2^+ + X_i\,\beta_3 + \epsilon_i$ with $r_{po}(c) = (c_i, c_i^2, \ldots, c_i^{po})$. β_2^- and β_2^+ are conformable vectors with dimension po (Calonico et al. 2017).

[32] The form of the MSE-optimal bandwidth is $h_{MSE} = C_{MSE} \cdot n^{-1/(2po+3)}$ (Cattaneo & Vazquez-Bare 2016). n indicates the sample size available, and po indicates the polynomial order. The constant

pooled cross-sectional firm data, I display standard errors adjusted for clustering at the firm level.[33]

To consistently estimate the effect of the threshold on the number of disabled workers in the firm as just described, I have to assume that firms do not manipulate their firm size in order to purposely stay below the threshold. However, as some firms face an increase in labor costs at the threshold due to the increased noncompliance fine, this assumption may possibly be violated (see Section 2.3). Thus, I explicitly address the question of how the manipulation of employment level may bias the estimated naive threshold effect. To do so, I first check whether manipulation is present by graphically inspecting firm size density. The intuition behind this test is that bunching should be reflected as a discontinuity in the firm size distribution at the threshold (see McCrary 2008). Due to the increased labor costs at the threshold, a negative discontinuity in the firm size density can be expected at the threshold. I also formally check for the presence of bunching (Cattaneo et al. 2020). Furthermore, again following Lalive et al. (2013), I quantify the effect on the firm size density – the *bunching effect* – to assess the bias in the estimated naive threshold effect. For this, I use an equation similar to equation (2.4) but with firm size density (in percentage terms) as the outcome variable. Again, I estimate different specifications, including specifications with different polynomial orders.

To shed additional light on the bunching behavior of firms, I further inspect alternative outcome variables by replacing the dependent variable in equation (2.4) with each of the alternative outcome variables. These variables include information on firm workforce composition, firm productivity and firm dynamics. Firms just below the threshold that manipulate their employment levels may substitute regular (full-time) employed workers with workers whose employment does not count toward their firms size for purposes

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C_{MSE} involves several known and unknown values that depend on objects such as the kernel function, the parameter of interest, po, the asymptotic bias and variance of the estimator, and whether additional predetermined covariates are included in the estimation. h_{MSE} is constructed by forming a preliminary estimator $\hat{C}_{MSE}$ of the unknown constant C_{MSE}, which leads to the estimated bandwidth $\hat{h}_{MSE} = \hat{C}_{MSE} \cdot n^{-1/2p+3}$. Thus, the selected bandwidth around the threshold T takes the form $[T - \hat{h}_{MSE}, T + \hat{h}_{MSE}]$. As a consequence, only observations within this bandwidth are used. This estimator is data-driven and objective. Note, however, that one cannot directly use the MSE-optimal point estimator for inference. The bandwidth $[T - \hat{h}_{MSE}, T + \hat{h}_{MSE}]$ is selected for MSE-optimal point estimation. Thus, bias and variance are balanced in a manner that makes inference invalid by construction when the same observations and estimator are used. I use an inference approach proposed by Calonico et al. (2014) which is based on bias correction for the point estimate. Hence, the *robust* confidence intervals are fully compatible with the use of the observations in the selected MSE-optimal bandwidth and are still valid (Cattaneo & Vazquez-Bare 2016).

33 Furthermore, following the guide for multiway clustering by Cameron & Miller (2015), I provide standard errors clustered at the firm level *and* at the discrete values of the running variable (firm size) for my main specification in column (5) of Table 2.5.1. To do so, I calculate the standard errors using the following equation: $se_{twoway} = \sqrt{se_1^2 + se_2^2 - se_{1\cap2}^2}$ (see the notes to Table 2.5.1).

of the quota, such as marginally employed, part-time workers (<18 hours/week) or apprentices. Such substitution effects would be reflected in differences in the workforce composition and in firm productivity below and above the threshold. Another alternative outcome variable is employment growth, as manipulating firms may have lower employment growth just below the threshold.[34] As I expect different bunching behavior among different types of firms below the threshold, I always distinguish between *noncompliers*, *perfect compliers* and *overcompliers* (see Section 2.3).

In summary, my empirical approach explicitly takes potential violations of the key assumptions of a standard regression discontinuity design into account. Specifically, my approach accounts for the fact that observations just below and just above the threshold may indeed be different with regard to workforce composition, productivity and dynamics. However, with regard to predetermined covariates such as region, industry and firm age, the observations below and above the threshold should not differ substantially. I report on these predetermined covariates for firms located around the threshold of 40 employees in Table 2.4.1 and test whether differences between firms below and above the threshold are substantial. Testing for balance in the predetermined covariates is important to ensure that firms just below the threshold represent an appropriate control group for treated firms just above the threshold. Furthermore, I include those predetermined covariates as control variables in my main estimations.

2.4.3 Sample and Descriptive Statistics

As described earlier, I focus on the second threshold of 40 employees in my main analysis. My baseline sample consists of firms with 29 to 51 employees (according to the BsbM) in the years 2004 to 2011, resulting in 319,939 firm-year observations.[35]

Table 2.4.1 reports predetermined firm characteristics (firm age, region and industry) for firms around the threshold of 40 employees. By construction, firms above the threshold have more employees than control firms and are, on average, older. Firms below and above the threshold also differ with regard to their industrial and geographical distribution. However, note that even though all differences are statistically significant at the 1 percent level, most of them are small in size. In addition, as a scale-free measure of balancing, I report the standardized differences in means (Δ_X) between firms below and

[34] I define employment growth with a dummy variable equal to 1 if a firm has more employees (according to the BsbM) in $t+1$ than in t and equal to 0 otherwise.

[35] Note that restricting the sample to firms with 29 to 51 employees is only relevant for describing the predetermined characteristics of the firms. In the analysis, the sample differs across the different specifications, as I calculate the MSE-optimal bandwidth for each estimation.

above the threshold in column (4) (see, e.g., Austin 2011).[36] As there is no generally agreed criterion for how small the standardized difference must be to provide balance, I rely on the rule of thumb of $\Delta_X < |0.1|$ as proposed by Austin (2011). With the exception of firm size, all control variables meet the criterion. Thus, the inspection of differences in predetermined characteristics suggests that firms below the threshold represent a basically appropriate control group for firms above the threshold. Nevertheless, to control for remaining differences in predetermined firm characteristics, I include these characteristics as controls in the main estimations.

Table 2.4.1: Descriptive Statistics for Firm Characteristics

	Below Threshold 29-39 Employees (1) Mean	Above Threshold 40-51 Employees (2) Mean	Difference (3) t test	Difference (4) Standardized Difference
Firm Size	33.64	45.18	11.53***	3.487
Age of Establishment	18.80	19.17	0.370***	0.033
Region: East Germany	0.171	0.171	-0.001***	-0.002
Industry Shares				
Agriculture	0.022	0.016	-0.006***	-0.042
Energy/Mining	0.009	0.012	0.003***	0.031
Manufacturing	0.245	0.270	0.025***	0.057
Construction	0.096	0.082	-0.014***	-0.050
Wholesale	0.182	0.172	-0.009***	-0.024
Traffic/Communication	0.066	0.062	-0.004***	-0.017
Banking/Insurance	0.010	0.014	0.004***	0.034
Other Services	0.188	0.175	-0.013***	-0.035
Public Administration	0.137	0.158	0.021***	0.058
Public Sector	0.045	0.039	-0.005***	-0.027
# of Firm-Year Observations	202,583	117,356	319,939	

Notes: The table presents descriptive statistics for the characteristics of firms around the 40-employee threshold. Significance level: *** $p < 0.01$.
Source: BsbM and BHP 2004–2011, own calculations.

2.5 Results: Intended and Unintended Effects

2.5.1 Intended Effect: Demand for Disabled

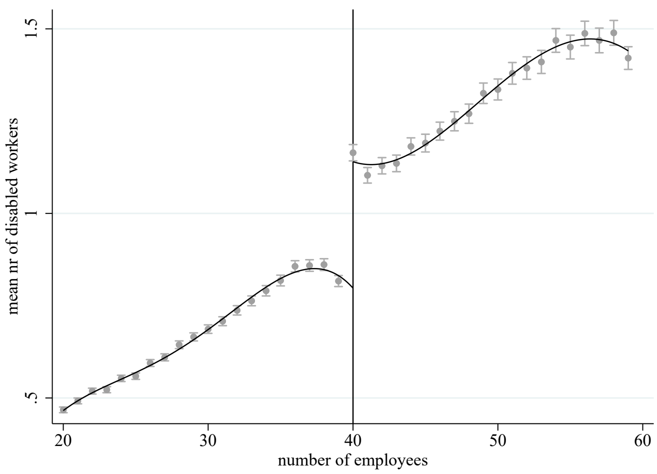
Graphical Illustration

Let us now turn to the graphical illustration of a potential discontinuity at the second threshold of 40 employees. Figure 2.5.1 displays the mean number of disabled workers by

36 The standardized difference is defined as $\Delta_X = \left(\bar{X}_1 - \bar{X}_0\right) / \left((S_1^2 + S_0^2)/2\right)^{0.5}$, where $\bar{X}_w$ is the sample mean of firms above ($w = 1$) or below ($w = 0$) the threshold and S_w^2 are the respective sample variances (Austin 2011). The advantage of Δ_X over the usual t-statistic is that it does not mechanically increase with the sample size, thus avoiding overstating small differences that would still be significant in a t-test.

firm size for the threshold of 40 employees. It shows that the number of disabled workers employed by firms increases with firm size in a quite linear fashion. Firms at the bottom of the observed firm size distribution, i.e., firms with 20 employees, employ on average 0.47 disabled workers, whereas firms at the top of the observed firm size distribution, i.e., firms with 59 employees, employ on average 1.42 disabled workers. The plot shows a considerable discontinuity in the number of disabled workers at the threshold. While firms just below the threshold, i.e., firms with 39 employees, employ on average 0.817 disabled workers, firms just above the threshold, i.e., firms with 40 employees, employ 1.164 disabled workers. However, the figure also illustrates that the (linear) increase in the number of workers with disabilities slows and eventually reverses into a decline just before the threshold.

Figure 2.5.1: Number of Disabled Workers



Notes: This graph plots the average number of disabled workers by firm size around the 40-employee threshold with 95% confidence intervals. The black line approximates the functional form of the running variable (here with polynomial order po=4 and bandwidth h_{below}=20 and h_{above}=19).
Source: BsbM and BHP 2004–2011, own calculations.

Estimation of Naive Effects

Table 2.5.1 reports the econometric results for the estimated naive threshold effects. I estimate five models with different bandwidths and polynomial orders. The first model in column (1) shows the results for the basic econometric model with an MSE-optimal bandwidth on either side of the threshold, a linear functional form, and predetermined firm characteristics included as control variables. The estimated discontinuity at the threshold is 0.345. This discontinuity is significantly different from zero at the 1 percent level. Columns (2), (3) and (4) use higher-order polynomials and again estimate the optimal bandwidth below and above the threshold. The results show that the estimates are sensitive to functional form. Higher-order polynomials lead to larger threshold effects. This is not surprising, as a more flexible functional form takes the (nonlinear) developments near the threshold into account (see Figure 2.5.1). Column (5) also uses a very flexible functional form but with a fixed bandwidth of h_{below}=8 and h_{above}=9 based on the optimal bandwidth in column (4).[37] The estimated coefficients in column (4) and column (5) are very similar. I therefore adopt the model in column (5) with a threshold effect of 0.388 as my *baseline specification* for the remainder of the analysis.

In quantifying the magnitude of the naive effect, the estimates suggest that the employment obligation leads to threshold firms employing 0.388 more disabled workers. Given that the mean number of disabled workers just below the threshold is 0.817, this effect represents an increase in the number of disabled workers of 47 percent. This effect is considerably larger than the 12 percent effect that Lalive et al. (2013) found in their analysis of the Austrian case. However, Lalive et al. (2013) also found relatively small bunching effects. As suggested by graphical evidence and as shown later, bunching may be a more salient issue in the German case. The large threshold effect found in this naive analysis may be upwardly biased (see Section 2.3.1). Thus, I shed additional light on potential bunching effects and bunching behavior in the following section.

Table 2.5.1: Threshold Effects (Dep. Var.: Number of Disabled Workers)

	40-Employee Threshold				
	(1)	(2)	(3)	(4)	(5)
Effect (β_1)	0.345***	0.373***	0.386***	0.394***	0.388***
Robust CI	[0.318; 0.429]	[0.318; 0.456]	[0.320; 0.469]	[0.304; 0.492]	[0.185; 0.668]
Bandwidth h	2.20; 3.38	4.15; 5.51	6.54; 7.58	8.31; 9.46	8; 9
Polynomial Order po	1	2	3	4	4
Covariates Included	yes	yes	yes	yes	yes
# of Observations	76,271	129,228	182,727	238,306	210,306

Notes: This table presents the estimation results for the effect of the threshold on the number of disabled workers in a firm (threshold = firm size of 40 employees)(see equation (2.4)). Basic covariates include firm age, regional characteristics (federal state) and industry. The bandwidths in columns (1)-(4) reflect the MSE-optimal bandwidths calculated with the *rdrobust* command in Stata. As the running variable (firm size) is discrete, estimates are adjusted for mass points in the running variable. Standard errors are clustered at the firm level. The robust confidence interval for the main specification in column (5) with standard errors clustered at the firm level *and* discrete values for the running variable (firm size) is [0.296; 0.557]. Significance level: *** $p < 0.01$.
Source: BsbM and BHP 2004–2011, own calculations.

[37] For the choice of bandwidth in this specification, I use the estimated optimal bandwidths in column (4) as my benchmark and round up to the nearest whole number. Thus, I gain predefined and uniform bandwidths that I can use to calculate the bunching effects. Uniform bandwidths that encompass a fixed number of firms are important for calculating the lower bound on the threshold effect (see Section 2.5.3).

2.5.2 Unintended Effect: Bunching Below

Bunching Effect

This section analyzes the potential bunching effect that results from firms purposely staying below the firm size threshold. The histogram shown in Figure 2.5.2 further indicates the importance of manipulation. It shows that firm size density drops at the threshold, indicating that manipulation may indeed be an issue. I also formally test for the presence of a discontinuity in the firm size distribution (Cattaneo et al. 2020). The test results suggest that the null that there is no bunching should be rejected at the 1 percent level (see Table 2.B.1 and Table 2.B.2 in the appendix).[38] To quantify the extent of the bunching, I calculate the share of firms in each firm size category and run local regressions around the threshold with the calculated firm size density as the outcome variable. I again use different polynomial orders (2, 3 and 4) to check whether the results are sensitive to functional form.[39]

Table 2.5.2 shows the results from estimating the bunching effects. The coefficient from the model with a second-order polynomial is -1.305. The models incorporating a more flexible functional form suggest larger bunching effects. When using a very flexible functional form, i.e., with a polynomial of order 4 and a fixed bandwidth of h_{below}=8 and h_{above}=9 based on my baseline specification in Section 2.5.1, the bunching effect is -2.017. This means that approximately 2 percent of the firms around the threshold have manipulated their size. Taken together, the evidence suggests firms indeed seem to manipulate their size due to the (higher) noncompliance fine that is imposed for firms with 40 or more employees. This suggests that the large threshold effect on the number of disabled workers identified in Section 2.5.1 is upwardly biased.

Table 2.5.2: Bunching Effects (Dep. Var.: Firm Size Density)

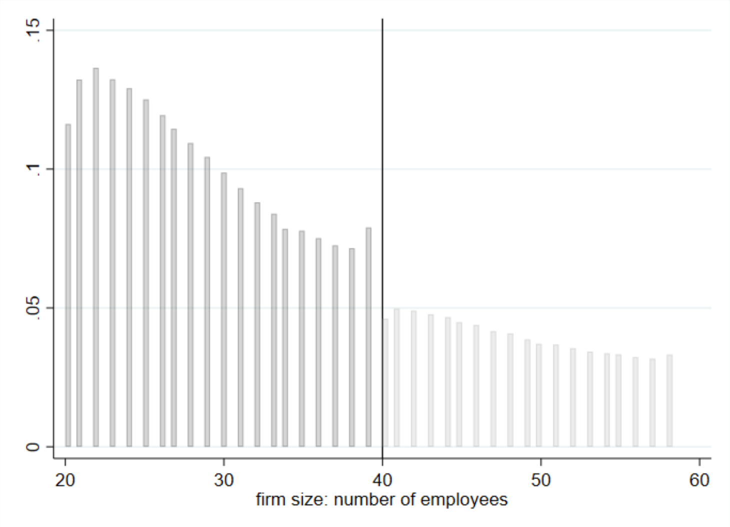
	40-Employee Threshold			
	(1)	(2)	(3)	(4)
Bunching Effect	-1.305***	-1.454***	-2.012**	-2.017
Robust CI	[-2.198; -0.604]	[-2.587; -0.535]	[-4.080; -0.212]	[-5.521; 1.702]
Bandwidth h	6.38; 7.20	8.34; 10.07	7.96; 11.03	8; 9
Polynomial Order po	2	3	4	4
# of Observations	14	19	19	16

Notes: This table shows estimation results for the effect of the 40-employee threshold on firm size density (in %). Significance levels: ** $p < 0.05$, *** $p < 0.01$.
Source: BsbM and BHP 2004–2011, own calculations.

38 I also perform the manipulation test proposed by Frandsen (2017) in the context of RDDs with a discrete running variable. This test also indicates that there is systematic manipulation of the running variable.

39 Note that it is not possible to estimate a linear specification (po=1) with the MSE-optimal bandwidth calculation in this case due to the very small number of observations.

Figure 2.5.2: Firm Size Density



Notes: Histogram indicating firm size density around the 40-employee threshold.
Source: BsbM and BHP 2004–2011, own calculations.

Bunching Behavior

The bunching effect shown above raises the question of whether firms below and above the threshold behave differently. To shed additional light on the bunching *behavior* of firms, I use the characteristics of the firm's workforce, which may be affected by bunching, as unintended outcome variables. Specifically, I examine firm and employee productivity, firm dynamics and workforce composition.

First, the graphical inspection of wages shown in Figure 2.5.3 illustrates that median wages are considerably lower in firms below the threshold. In addition to wages, I use firm and person fixed effects – also called AKM effects – as a proxy for the firm and employee productivity provided by Bellmann et al. (2020).[40] The illustration of the firm fixed effects analysis in Figure 2.B.3 in the appendix is very similar to that for wages. For person fixed effects, the graphical inspection also indicates a substantial discontinuity at the threshold (see Figure 2.B.4 in the appendix).[41] Second, the share of regularly employed workers in firms below the threshold is lower than in firms above the threshold, whereas the share of marginally employed workers is higher (see Figure 2.B.5 and 2.B.6 in the appendix). Third, firms just below the threshold have considerably lower employment growth. Table 2.5.3 reports the estimated discontinuities in the considered variables at the 40-employee threshold, again with different specifications (*po*=1 and *po*=4). The

[40] Table 2.A.3 in the appendix explains the construction of the AKM effects in more detail.

[41] I use the person fixed effects for 2003-2010 provided by Bellmann et al. (2020) for individuals in the Integrated Employment Biographies (IEB) employed in firms sized 28-51 in 2010. In this way, I obtain a baseline sample of 1,479,831 individuals.

pattern of results supports the hypothesis that some firms bunch below the threshold and adjust their workforce when facing an increase in labor costs. Specifically, firms below the threshold substitute regularly employed workers with marginally employed workers who do not count toward the calculation of firm size (see Section 2.3.1). The significant discontinuities in wages and in AKM firm and person fixed effects suggest that adjusting the workforce may be more costly among bunching firms which are likely to be less productive than other firms with similar employment levels. These discontinuities may also result from selection, as low-productivity firms may be more incentivized to bunch since they face (relatively) higher costs at the threshold (see also the discussion and analysis of the heterogeneous effects between low- and high-wage firms in Section 2.5.4). In summary, the overall picture suggests that the increase in labor costs due to the noncompliance fine at the threshold of 40 employees is highly correlated with firm dynamics, firm productivity and firm employment structures.

When distinguishing between *noncompliers*, *perfect compliers* and *overcompliers*, the results in Table 2.5.3 show that the significant coefficients are mainly driven by *noncompliers*.[42] For *overcompliers*, in contrast, the coefficients are not significantly different from zero for any of the alternative outcomes. Koller et al. (2006) analyzed employment growth for firms around the (former) threshold within the German disabled worker law in 1999 and 2000. In line with my results, those authors also found evidence of a significant and substantial decline in employment growth among firms below the second threshold that do not employ any disabled workers.[43] Taken together, the results suggest that bunching behavior is particularly pronounced among those firms just below the threshold that face the highest costs at the threshold, as discussed theoretically in Section 2.3.2.

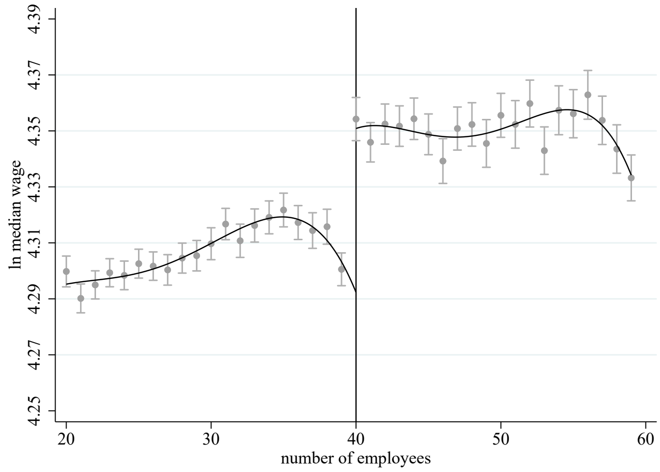
2.5.3 Bounding the Effect

As shown before, the threshold effect on the number of disabled workers in a firm identified in Section 2.5.1 is upwardly biased. In the following, I assess the upward bias in the

[42] The graphical illustrations for the different types of firms are shown in Figures 2.B.7, 2.B.10, 2.B.11 and 2.B.12 in the appendix.

[43] Koller et al. (2006) estimate a different model specification. Specifically, the authors use a probit model, with the probability of growth in t+1 as the dependent variable. Their coefficient of interest is the interaction between being just below the former threshold (i.e., having 24 employees) and employing fewer than two disabled workers (i.e., not complying with the law). The coefficient on this interaction is -1.336 and is significant at the 5 percent level. Through simulations, the authors quantify the decline in growth as approximately 22.9 percentage points. The probably best approximation to this specification is to estimate the threshold effect on employment growth for all potential B-firms (i.e., *noncompliers* and *perfect compliers*). This estimation (with po=4 and the MSE-optimal bandwidth calculation) results in a coefficient of 0.211, which is significantly different from zero at the one percent level. Thus, the effect is very similar to that found by Koller et al. (2006).

Figure 2.5.3: Wages



ln Median Wages

Note: This graph plots the ln of median wages (=median value of gross daily wages for full-time employees) by firm size around the threshold of 40 employees with 95% confidence intervals. The black line approximates the functional form of the running variable (here with polynomial order fit *po*=4 and bandwidth h_{below}=20 and h_{above}=19).
Source: BsbM and BHP 2004–2011, own calculations.

Table 2.5.3: Bunching Behavior

	po = 1	*po* = 4			
Dependent Variable	Total	Total	Noncompliers	Perfect Compliers	Over-compliers
	(1)	(2)	(3)	(4)	(5)
Sociodemographic Structure					
Share of Females	-0.003	-0.011	-0.006	-0.015	-0.012
Share of Germans	0.005***	0.012**	0.023**	0.004	-0.000
Employment Structure					
Median Wages (ln)	0.050***	0.088***	0.095***	0.048*	0.036
Firm Fixed (AKM) Effects	0.029***	0.050***	0.048***	0.036**	0.022
Person Fixed (AKM) Effects	0.039***	0.033***	0.049***	0.029	-0.005
Share of Regularly Employed Workers	0.014***	0.016***	0.019***	0.015	0.007
Share of Marginally Employed Workers	-0.015***	-0.024***	-0.037***	-0.015	-0.004
Share of Apprentices	0.000	0.002	0.009	-0.003	-0.000
Share of Full-Time Workers	0.008**	0.015	0.012	0.022*	0.020
Share of Part-Time Workers	0.003	0.008	0.014	-0.005	-0.005
Skill Structure					
Share of Low-Skilled Workers	-0.005***	-0.010	-0.016*	-0.009	0.002
Share of Medium-Skilled Workers	-0.003	0.006	0.017	0.004	-0.021
Share of High-Skilled Workers	0.009***	0.007	0.004	0.002	0.017
Firm Dynamics					
Employment Growth in t+1	0.071***	0.229***	0.353***	0.116*	0.033

Notes: This table shows the estimation results for the effect of the threshold of 40 employees on alternative outcome variables. *Noncompliers*, *perfect compliers* and *overcompliers* are firms below the threshold that employ zero, exactly one or at least two disabled worker(s), respectively. All estimations are estimated by using the MSE-optimal bandwidth for either side of the threshold. Standard errors are clustered at the firm level. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
Source: BsbM and BHP 2004–2011 (2004-2010 for firm fixed effects, 2010 for person fixed effects), own calculations.

threshold effect and provide a lower bound on the effect, again following the strategy used by Lalive et al. (2013). For this, I refer to my baseline specification in which h_{below}=8, h_{above}=9 and po=4 for both the bunching and the threshold effects. The bunching effect of -2.017 identified in Section 2.5.2 informs us about the absolute number of bunching firms, suggesting that 2.017 percent of the 210,306 firms considered within the fixed bandwidth manipulate their employment levels. Hence, there are 2,121 (=(0.02017*210,306)/2) employment manipulators in total.[44] As both types of firms, i.e., *noncompliers* and *perfect compliers*, may be *B-Firms* according to Section 2.3.2, I expect firms of each type to bunch below the threshold.

To assess how many of the 2,121 bunching firms are *B-perfect compliers*, I restrict my sample to firms that employ at least one disabled worker and estimate the bunching and threshold effects for this subsample of 121,382 observations. The result is an estimated bunching effect of -1.413 and an estimated threshold effect of 0.262 (see also Figure 2.B.1 and Table 2.B.3 in the appendix). This result suggests that 858 of the 2,121 bunching firms are *B-perfect compliers* and 1,263 firms are *B-noncompliers*.[45]

To bound the threshold effect, I hypothetically reassign all potential bunching firms from a firm size of 39 employees to a firm size 40 while keeping the number of disabled workers constant (i.e., a total of 1,263 firms would still employ zero disabled workers, and 858 firms would still employ one disabled worker). I then recalculate the raw difference in the mean number of disabled workers among firms with 39 employees and among those with 40 employees. This yields a difference of 0.161. The original raw difference in the mean number of disabled workers in those firms was 0.348, so the bias amounts to 0.348-0.161=0.187. Using this bias calculation to bound the naive threshold effect of 0.388 suggests that the lower bound of the effect is 0.201. Thus, even after taking potential bunching into account, I still obtain a positive threshold effect. Taken together, my estimates suggest that the employment quota indeed induces firms to employ more disabled workers, but depending on the extent of bunching, the true threshold effect may

[44] The following example illustrates why this number is divided by two: Imagine 100 firms on either side of the threshold. Now assume that ten firms bunch and purposely stay below the threshold. Now, there are 110 firms below and 90 firms above the threshold. The resulting difference in the number of firms is 20 – twice the number of bunching firms (Lalive et al. 2013).

[45] As a robustness check, I restrict my sample to firms that employ at least two disabled workers. As these firms (*overcompliers*) do not face additional costs at the threshold when they are below the threshold, bunching should not occur. In fact, Figure 2.B.2 in the appendix suggests that *bunching below the threshold* is not relevant for *overcompliers*. According to the formal test by Cattaneo et al. (2020), firm size density *increases* significantly for *overcompliers* at the threshold of 40 employees. This is plausible, as it is in line with the institutional regulations under which firms above the threshold are obliged to employ two and more disabled workers. Thus, the share of these firms increases at the threshold.

be considerably smaller than the naive effect.

2.5.4 Heterogeneous Effects

I now turn to a discussion of the potential heterogeneity in the bunching and treatment effects based on the theoretical considerations described in Section 2.3. Specifically, I differentiate between low- and high-wage firms and analyze different industries.

Low-Wage and High-Wage Firms

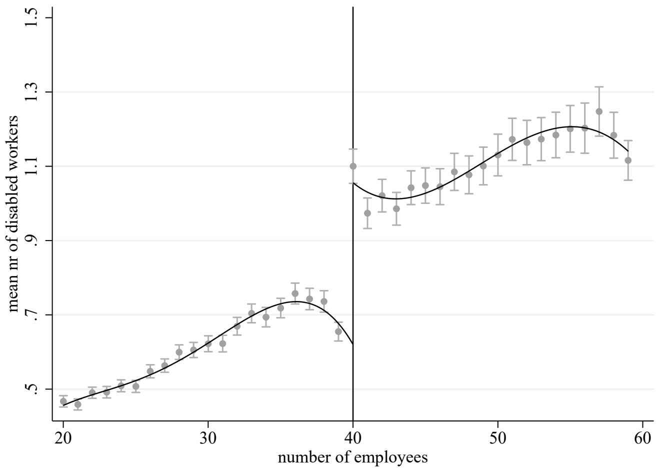
First, with regard to low- and high-wage firms, the share of the noncompliance fine τ relative to wages is substantially higher for low-wage firms than for high-wage firms: Among firms with 20-59 employees, the relative shares of the noncompliance fine are approximately 7.1 percent (τ_1) and 12.2 percent (τ_2) of wages among firms in the first quartile of the wage distribution (low-wage firms).[46] In contrast, these shares are only approximately 3.0 percent (τ_1) and 5.1 percent (τ_2) of wages among firms in the fourth quartile (high-wage firms). Thus, the (relative) importance of the noncompliance fine differs considerably between these groups of firms. For a given wage w and disabled worker productivity p, a relatively larger noncompliance fine τ leads to an increase in the number of *B-firms* among firms for which $p < w$ and thus a larger bunching effect among low-wage firms. Likewise, a relatively larger fine leads to an increase in the number of *G-firms* among firms for which $p > w$. As a consequence, I also expect a larger threshold effect among low-wage firms (see Section 2.3.1). For the empirical analysis, I group the firms based on quartiles of the wage distribution. The graphical analysis shown in Figure 2.5.4 suggests that the threshold effect among firms in the first quartile of the wage distribution is larger than that among firms in the fourth quartile of the wage distribution. The estimated threshold and bunching effects shown in Table 2.5.5 confirm this notion: The threshold effect is substantially larger among low-wage firms. The naive threshold effect among low-wage firms is 0.588, compared to 0.235 among high-wage firms, and the lower bound of this effect is 0.301 among low-wage firms, compared to 0.072 among high-wage firms. Furthermore, bunching is present among both types of firms but is also more pronounced among low-wage firms. In summary, these results support the hypothesis that the threshold and bunching effects are larger among low-wage firms.

Effects by Industry

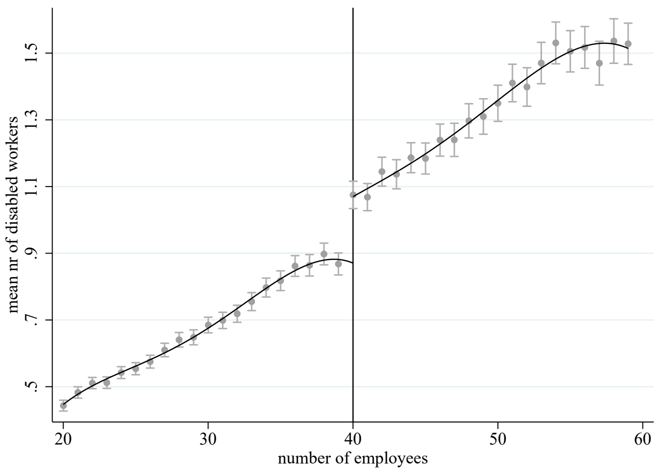
I next estimate the bunching and threshold effects stratified by industry. The results displayed in Table 2.B.6 show that the largest (naive) threshold effects are found in the in-

[46] The wage distribution is based on the median value of gross daily wages for full-time employees.

Figure 2.5.4: Mean Number of Disabled Workers



(A) Low-Wage Firms



(B) High-Wage Firms

Notes: These graphs plot the mean number of disabled workers in (A) low-wage firms and (B) high-wage firms by firm size around the threshold of 40 employees with 95% confidence intervals. Low-Wage (high-wage) firms are firms in the 1st (4th) quartile of the wage distribution. The black line approximates the functional form of the running variable (here with polynomial order fit po=4 and bandwidth h_{below}=20 and h_{above}=19).
Source: BsbM and BHP 2004–2011, own calculations.

Table 2.5.4: Heterogeneous Effects by Firm Wages

	Observations	Threshold Effect (TE)		Bunching Effect	Lower Bound
		po = 1	po = 4	po = 4	of TE
	(1)	(2)	(3)	(4)	(5)
Firms in 1st Quartile of Wage Distribution					
MSE-Optimal Bandwidth		0.473***	0.588***	-2.963**	
Fixed Bandwidth	61,462	0.307***	0.590***	-2.598*	0.310
Firms in 2nd Quartile of Wage Distribution					
MSE-Optimal Bandwidth		0.369***	0.398***	-1.913**	
Fixed Bandwidth	57,386	0.273***	0.392	-2.126	0.199
Firms in 3rd Quartile of Wage Distribution					
MSE-Optimal Bandwidth		0.307***	0.334***	-1.643**	
Fixed Bandwidth	59,242	0.236***	0.318	-1.519*	0.151
Firms in 4th Quartile of Wage Distribution					
MSE-Optimal Bandwidth		0.178***	0.233***	-0.826	
Fixed Bandwidth	74,540	0.126***	0.235***	-1.099**	0.072

Notes: This table shows the estimation results for the threshold effects (dependent variable: mean number of disabled workers in a firm) around the 40-employee threshold and the bunching effects (dependent variable: firm size density in %) stratified by firms' median daily wage (quartiles). Basic covariates include firm age, regional characteristics (federal state) and industry. Standard errors are clustered at the firm level. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The fixed bandwidth is the MSE-optimal bandwidth from the estimation with polynomial order po=4, rounded up to the nearest whole number.
Source: BsbM and BHP 2004–2011, own calculations.

dustries agriculture/fishery, construction and traffic/communication industries. Bunching is particularly pronounced in construction, traffic/communication and the public sector. When bounding the threshold effects by following the bounding exercise in Section 2.5.3, the largest lower bounds emerge in the agriculture/fishery, construction and other services sector. Industries may be a proxy for different workplace characteristics. First, assuming that wages are equal between disabled and nondisabled workers, as done in Section 2.3.1, is accurate mainly for firms in industries with high levels of collective bargaining coverage. However, the results displayed in Table 2.B.6 do not indicate a clear pattern in terms of the level of collective bargaining coverage: On the one hand, there are no significant threshold effects among firms in energy/mining and banking/insurance – two sectors with a relatively high level of collective bargaining coverage (Ellguth & Kohaut 2004, 2012).[47] On the other hand, there are substantial threshold (and bunching) effects among firms both in industries with high collective bargaining coverage (such as construction and public administration) and in industries with low collective bargaining coverage (such as other services and traffic/communication). Second, industries may have different working conditions, in particular different shares of physically demanding tasks. As physical disabilities still account for the majority of disabilities (see Section 2.2.1), the share of physically demanding tasks in an industry may serve as a proxy for the average productivity gap between disabled and nondisabled workers. Sectors in which the

[47] Note that collective bargaining coverage also depends on establishment size. According to Ellguth & Kohaut (2012), 53 (39) percent of West German establishments with 10 to 49 (50 to 199) employees were not covered by a collective bargaining agreement in 2011.

average productivity of a disabled worker differs substantially from that of a nondisabled worker (i.e., $p \ll P$) probably have a higher share of firms in which $p < w$ (and thus a higher share of bunching (*B*-)firms). The large bunching effect in the construction sector (-2.562) is in line with these considerations, as this industry is characterized by a high share of physically demanding tasks (Kroll 2011).

Table 2.5.5: Heterogeneous Effects by Industry

	Observations	Threshold Effect (TE)		Bunching Effect	Lower Bound
		po=1	*po*=4	*po*=4	of TE
	(1)	(2)	(3)	(4)	(5)
Agriculture/Fishery					
MSE-Optimal Bandwidth		0.226*	0.591**	-1.194***	
Fixed Bandwidth	4,356	0.218*	0.647	-1.519	0.420
Energy/Mining					
MSE-Optimal Bandwidth		0.235	0.279	-1.554	
Fixed Bandwidth	2,392	0.209	0.298	-1.591	-
Manufacturing					
MSE-Optimal Bandwidth		0.331***	0.337**	-1.704**	
Fixed Bandwidth	63,118	0.228***	0.336	-1.654	0.137
Construction					
MSE-Optimal Bandwidth		0.255***	0.536**	-2.517*	
Fixed Bandwidth	18,336	0.229***	0.536	-2.562	0.413
Wholesale					
MSE-Optimal Bandwidth		0.301***	0.372***	-1.821**	
Fixed Bandwidth	46,454	0.198***	0.372*	-1.724*	0.205
Traffic/Communication					
MSE-Optimal Bandwidth		0.352***	0.506***	-2.785*	
Fixed Bandwidth	16,763	0.257***	0.513*	-2.476*	0.241
Banking/Insurance					
MSE-Optimal Bandwidth		0.101	0.081	0.187	
Fixed Bandwidth	3,151	0.039	-0.009	1.072	-
Other Services					
MSE-Optimal Bandwidth		0.324***	0.428***	-2.053**	
Fixed Bandwidth	36,172	0.278***	0.569*	-2.046	0.362
Public Administration (PA)					
MSE-Optimal Bandwidth		0.334***	0.396***	-1.191	
Fixed Bandwidth	37,661	0.271***	0.377	-1.258**	0.186
Public Sector (w/o PA)					
MSE-Optimal Bandwidth		0.355***	0.371*	-2.903*	
Fixed Bandwidth	11,666	0.265***	0.349	-1.818*	0.110

Notes: This table shows the estimation results for the threshold effects (dependent variable: mean number of disabled workers in a firm) around the 40-employee threshold and the bunching effects (dependent variable: firm size density in %) stratified by industry. Basic covariates include firm age and regional characteristics (federal state). Standard errors are clustered at the firm level. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The fixed bandwidth is the MSE-optimal bandwidth from the estimation with polynomial order *po*=4, rounded up to the nearest whole number. For "energy/mining" and "banking/insurance" industries, no significant threshold effects were identified and thus no lower bounds were calculated.
Source: BsbM and BHP 2004–2011, own calculations.

2.5.5 Testing for Robustness

Placebo Estimations

To assess the credibility of my main results, I perform several robustness checks. My first test is the use of placebo thresholds. For this, I estimate the discontinuities in the number of disabled workers per firm at firm sizes where there should be no discontinuities. Figure 2.5.5 shows the estimated discontinuities for the specification with *po*=4 and an optimal bandwidth of firm sizes 28-51 (including the true threshold at a firm size of 40).[48] The pattern of estimates displays a clear-cut peak at the true threshold. For some placebo thresholds, e.g., firm sizes of 28, 41, 42, 45 or 46 employees, the 95 percent confidence interval does not include zero. This is in contrast to the graphical illustration in Figure 2.5.1, which suggests that there are no discontinuities at these firm sizes. Specifications with different polynomial orders show that although there are significant discontinuities at some placebo thresholds, the robustness of these estimates seems to be low: While the estimated discontinuity at the true threshold is positive and highly significant in all specifications, the significance of the coefficients for the placebo thresholds varies considerably depending on the specification. Furthermore, in terms of magnitude, the coefficient at the true threshold is substantially larger than the coefficients at the placebo thresholds in most cases (see Figures 2.B.13, 2.B.14 and 2.B.15 in the appendix). Taken together, the overall pattern confirms the credibility of the estimated discontinuity at the true threshold of 40.

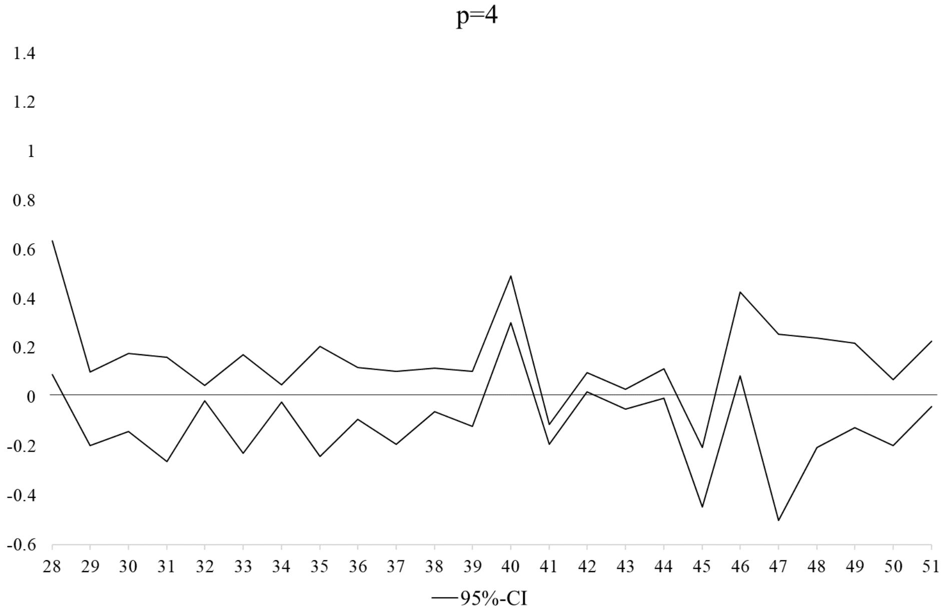
Donut Estimations

Firms close to the threshold may be particularly prone to manipulate their firm size. Thus, as a further robustness check, I exclude observations close to the threshold and calculate the bunching and threshold effects using the remaining sample. Table 2.5.6 shows the results of these donut estimations. Note that I now use a linear specification, as the overall relationship between firm size and the number of disabled workers – when excluding the nonlinear developments near the threshold – appears to be linear.[49] Let us first turn to the threshold effects. Compared to the coefficients from the baseline specifications, the coefficients from the donut estimations are smaller but still highly significant. This confirms the notion that part of the estimated naive threshold effect is biased by firms bunching below the threshold. Regarding bunching, the estimations show that the bunching effects are considerably smaller than in my baseline estimations, indicating that firm

48 Note that the German labor law has additional regulations with other thresholds, which may also be relevant for the employment of disabled workers. One example is the threshold of 30 employees with regard to insurance for continued payment ("Entgeltfortzahlungsversicherung") (see also Table I.2). In Germany, employees are entitled to sick pay that is paid by their employer during the first six weeks of an illness. Health insurance reimburses employers for some of these costs through the insurance for continued payment. This insurance is obligatory for employers who do not employ more than 30 employees. For an overview of the German regulations with thresholds, see Koller (2010*b*).

49 The point estimates for the threshold effect with polynomial order *po*=4 are larger compared to the estimates for the threshold effect with polynomial order *po*=1 shown in Table 2.5.6. However, the statistical power of these estimates is reduced. Results not shown, but available on request.

Figure 2.5.5: Placebo Thresholds



Notes: The graph shows the 95% confidence intervals of the estimates of placebo thresholds (po=4, MSE-optimal bandwidth on either side of the threshold and controlling for predetermined covariates). All thresholds except for the 40-employee threshold are placebo thresholds. The 95% confidence interval refers to the robust CI estimated with the *rdrobust* command in Stata. As the point estimates could be outside the robust CIs, only the interval boundaries are shown. Standard errors are clustered at the firm level.
Source: BsbM and BHP 2004–2011, own calculations.

size manipulations occur mainly among firms located directly around the threshold.

Overall, the significant threshold effects estimated for the subsamples without firms near the threshold confirm my main results: Even though bunching is present, the regulation seems to positively affect the number of disabled workers in firms.

Table 2.5.6: Donut Estimations

	Baseline Estimation	Donut Estimations: Excluding Firms of Firm Size				
		39	38,39	39,40	39-41	39-42
	(1)	(2)	(3)	(4)	(5)	(6)
			Bunching Effects			
Coefficient	-2.017	-0.608**	-0.568	-0.521*	-0.510	-0.510
Robust CI	[-5.521; 1.702]	[-1.459; -0.080]	[-2.396; 0.992]	[-1.364; 0.052]	[-1.453;0.164]	[-1.682;0.377]
# of Obs.	16	15	14	14	13	12
			Threshold Effects			
Coefficient	0.388***	0.203***	0.172***	0.164***	0.159***	0.147***
Robust CI	[0.185; 0.668]	[0.233; 0.349]	[0.155; 0.366]	[0.174; 0.301]	[0.152; 0.319]	[0.060;0.322]
Polyn. Ordner po	4	1	1	1	1	1
# of Obs.	210,306	192,965	177,260	182,616	171,457	160,455

Notes: This table shows the estimation results for the effect of the threshold on the number of disabled workers in a firm (threshold = firm size of 40 employees). The bandwidth for all estimations is h_{below}=8 and h_{above}=9. The estimates for the threshold effects are controlled for predetermined characteristics, which include firm age, regional characteristics (federal state) and industry. Standard errors are clustered at the firm level. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
Source: BsbM and BHP 2004–2011, own calculations.

Results for the 60-Employee Threshold

In what follows, I check whether a similar pattern is visible at the third threshold of 60 employees. Firms with 40 to less than 60 employees must employ at least two disabled workers, while firms with 60 or more employees are obliged to employ at least three (=5 percent) disabled workers. Note, however, that there are other threshold rules for the 60-employee threshold in the German labor law.[50] Thus, the following analyses are primarily exploratory and serve as a robustness check for the results for the 40-employee threshold.

I restrict my sample to firms around the threshold of 60 employees. Regarding the intended effect, the graphical illustration again indicates a considerable discontinuity in the mean number of disabled workers between firms below and above this threshold (see Figure 2.5.6). The histogram for the firm size distribution suggests that bunching is also present at this threshold (see Figure 2.B.16 and, for the results of the formal test, Table 2.B.4 in the appendix). Furthermore, the plots of and estimations for selected alternative outcome variables regarding the employment and wage structures as well as firm dynamics are similar to the patterns in those outcome variables near the 40-employee threshold (see Figures 2.B.17, 2.B.18, 2.B.19 and Table 2.B.5 in the appendix). Table 2.5.7 gives an overview of the formally estimated bunching and threshold effects for the 60-employee threshold. All threshold effects estimated with MSE-optimal bandwidths are significantly different from zero at least at the 10 percent level. In terms of size, the threshold effects for the 60-employee threshold are larger than those for the 40-employee threshold, while the sizes of the bunching effects are similar. This result is consistent with the results of Lalive et al. (2013), who also find larger effects at higher thresholds (albeit without evidence of bunching at higher thresholds). Repeating the bounding exercise described in Section 2.5.3 yields a lower bound of the threshold effect of 0.380.[51] In sum, the analyses for the 60-employee threshold largely confirm the results obtained for the 40-employee threshold.

Further Robustness Tests

As a further robustness check, I exclude firms that are identifiable as multiestablishment firms.[52] The results for the threshold and bunching effects and – more importantly –

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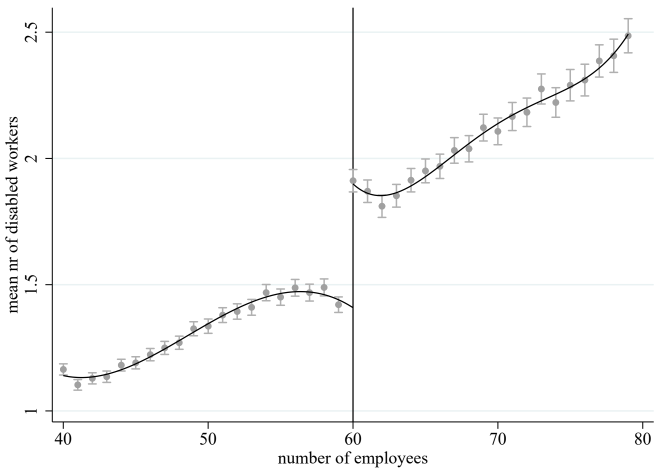
50 For example, according to the Protection Against Dismissal Act (*Kündigungsschutzgesetz*), an employer with 60 or more employees must report a layoff of 10 percent of the workforce or of more than 25 employees to the employment agency, see Table I.2.

51 Note that there are four firm types around the threshold of 60 employees: *noncompliers* with employment of disabled workers $D = 0$, *undercompliers* with $D = 1$, *perfect compliers* with $D = 2$ and *overcompliers* with $D \geq 3$. I estimate the share of *B-undercompliers* and *B-perfect compliers* among all bunching firms by restricting the sample to firms with at least one or at least two disabled workers, respectively. As a result, I find 237 *B-perfect compliers*, 765 *B-undercompliers* and 278 *B-noncompliers* among the 1,281 total bunching firms.

52 I can identify a firm as multiestablishment firms in the BsbM data as soon as it reports a disabled

Figure 2.5.6: Number of Disabled Workers



Note: This graph plots the average number of disabled workers by firm size around the threshold of 60 employees with 95% confidence intervals. The black line approximates the functional form of the running variable (here with polynomial order fit p=4).
Source: BsbM and BHP 2004–2011, own calculations.

Table 2.5.7: Threshold and Bunching Effects for the 60-Employee Threshold

	60-Employee Threshold – Bunching Effects				
	(1)	(2)	(3)	(4)	(5)
Coefficient		-1.691**	-1.905*	-2.006*	-2.161
Robust CI		[-3.482; -0.213]	[-4.202; 0.076]	[-4.528; 0.238]	[-7.084; 3.381]
Bandwidth h		6.75; 8.79	8.67; 12.53	11.51; 15.53	8; 14
Polynomial Order *po*		2	3	4	4
# of Observations		15	21	27	21
	60-Employee Threshold – Threshold Effects				
	(1)	(2)	(3)	(4)	(5)
Coefficient	0.462***	0.498***	0.529***	0.653***	0.653**
Robust CI	[0.410; 0.574]	[0.417; 0.618]	[0.420; 0.671]	[0.451; 0.902]	[0.160; 1.178]
Bandwidth h	3.48; 3.58	5.61; 6.77	7.44; 9.74	7.84; 13.63	8; 14
Polynomial Order *po*	1	2	3	4	4
Covariates Included	yes	yes	yes	yes	yes
# of Observations	43,523	73,050	101,907	118,537	118,537

Notes: This table shows the estimation results for the bunching effects (dependent variable: firm size density in %) and the threshold effects (dependent variable: mean number of disabled workers in a firm around the 60-employee threshold). Basic covariates include firm age, regional characteristics (federal state) and industry. Standard errors are clustered at the firm level. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. It is not possible to estimate the bunching effect in a linear specification (*po*=1) with the MSE optimal bandwidth calculation due to the very small number of observations.
Source: BsbM and BHP 2004–2011, own calculations.

the results regarding bunching behavior are also robust to this exclusion (see Table 2.B.6 and Table 2.B.7 in the appendix). Last, altering the specification, for example, by using different kernel weights or using a different bandwidth selector, does not alter my results, either.[53]

2.6 Conclusion

In Germany, firms with 40 or more employees are obliged to employ one additional worker with a disability. This paper analyzes the intended and unintended effects of this German employment quota for workers with disabilities. The intended effect refers to the effect of this regulatory threshold on firm demand for workers with disabilities, whereas the unintended effect refers to potential bunching below the threshold. Thus, my paper extends the literature on the effects of an increase in labor costs resulting from a disabled worker quota system.

I use this sharp increase in labor costs and adopt a threshold design, which is closely related to an RDD, to estimate these effects. However, the threshold design accounts for the fact that the running variable – firm size, in my case – is endogenous. My results indicate that the employment quota promotes the employment of disabled workers in firms located around the threshold. A naive estimate of the *intended*, or threshold, effect (when ignoring the bunching) suggests that threshold firms employ on average 0.388 more disabled workers. When analyzing the *unintended*, or bunching, effect, the results show that firms do indeed manipulate their employment due to the increase in labor costs at the threshold. The existence of bunching violates the assumptions necessary for identifying the unbiased effect of the regulatory threshold. However, based on the estimates about the extent to which firms manipulate, I am able to provide a lower bound for the threshold effect. After taking this bunching effect into account, I obtain a lower bound of 0.201, which is still positive, though considerably smaller. Thus, the German noncompliance fine does indeed increase compliance with the quota and promote the employment of disabled workers.

However, the quota also has unintended consequences that can be harmful to overall employment: Firms just below the threshold have a lower probability of increasing employment and a higher probability of substituting away from regularly employed workers.

worker who is not working in the main establishment. In this manner, I exclude 1,782 firm-year observations (of firms with 20 to 59 employees). Note, however, that this exclusion is selective in the sense that I exclude only firms employing at least one disabled worker.

[53] Results not shown but available upon request.

This is interesting, as previous research has found little evidence of firms bunching below the labor law thresholds in Germany. In view of the multitude of threshold regulations in German labor law, my findings shed new light on the relevance of such thresholds. Further research should therefore emphasize the evaluation of regulatory thresholds and firm adaptation to such regulations in other contexts.

2.7 Appendix

2.A.1 Appendix A: Definitions and Institutional Details

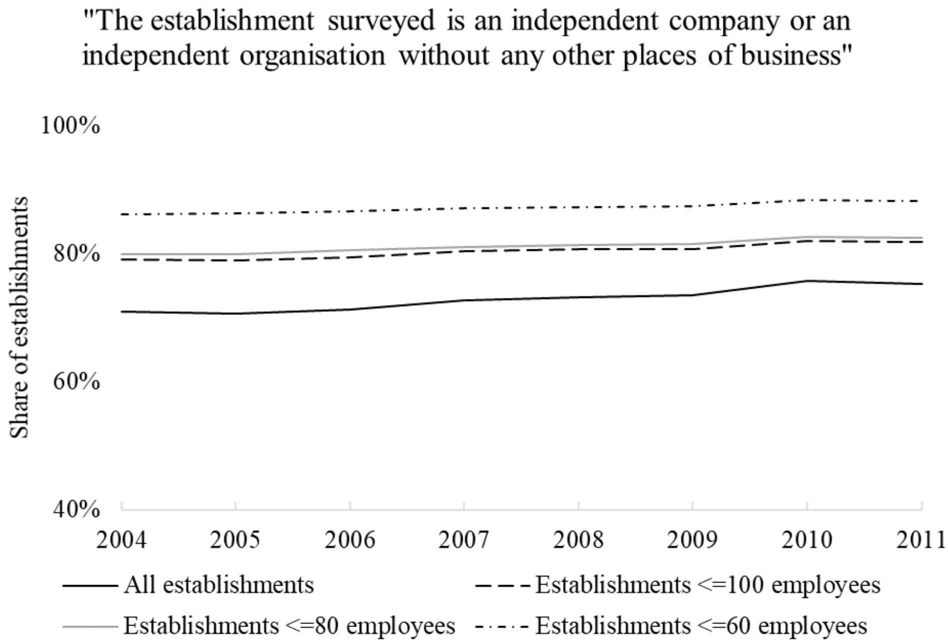
Table 2.A.1: Calculation of Firm Size According to the Disabled Worker Law (§156 and §157 SGB IX)

Excluded groups of workers	Apprentices (including special trainee positions for lawyers and teachers) Individuals who work less than 18 hours per week Individuals with a temporary contract of fewer than eight weeks Individuals whose employment is not primarily for pay (e.g., individuals whose employment is primarily for rehabilitation) Individuals participating in job creation schemes according to SGB III Individuals who are elected to their job after continuous practice
Temporal dimension	The relevant measure for the firm size is the *annual average* of the monthly number of positions. A firm that e.g. claims to employ one disabled individual must prove a total of at least 12 months of employment of one (or more) severely disabled individual(s) in one calendar year.
Calculation details	Fractions of 0.5 or more are rounded down to the nearest whole number for firms with 20 to 59 positions. Fractions of 0.5 or more are rounded up to the nearest whole number for firms with 60 or more positions.

Table 2.A.2: Additional Definitions Related to Firms/Establishments

Definitions of Firms and Establishments
Legal Definition of "Employer" (Firm) According to the Disabled Worker Law Employers can be either a natural or a legal person under public or private law as well as a company of any kind. Consequently, all employees of the same employer are included, regardless of the number of establishments or other locations over which they are distributed.
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 2.A.1: Share of Individual Establishments



Notes: This graph shows the share of establishments that are independent companies or independent organizations without any other places of business. The survey is representative of all establishments in Germany (Ellguth et al. 2014).
Source: IAB Establishment Panel, 2004–2011, own calculations.

Table 2.A.3: Person and Establishment Fixed Effects ("AKM Effects")

AKM-Effects
AKM person and establishment fixed effects stem from a wage decomposition pioneered by Abowd et al. (1999), implemented for Germany by Card et al. (2013), and updated by Bellmann et al. (2020). These effects are derived from the following wage model: $$log(wage_{it}) = \alpha_i + \Psi_{J(i,t)} + x'_{it}\beta + \epsilon_{it},$$ where the log daily wages for worker i are the sum of a time-invariant person effect α_i, a time-invariant establishment effect $\Psi_{J(i,t)}$ for the establishment at which worker i is employed at time t, plus time-varying worker characteristics $x'_{it}\beta$, which affect all workers' wages equally at all establishments, and an error component ϵ_{it}, which is assumed to be independent of the right-hand-side variables. The estimates for the person effect α_i capture time-invariant individual characteristics that are rewarded equally across employers. Likewise, the index $x'_{it}\beta$ is interpreted as measuring the time varying worker characteristics that affect the productivity of worker i in all jobs. In x_{it}, an unrestricted set of year dummies and of quadratic and cubic terms in age fully interacted with education is included. Last, the establishment effect $\Psi_{J(i,t)}$ is interpreted as a proxy for an establishment productivity, as this effect represents the proportional pay premium (or discount) that is paid by establishment j to all employees (i.e., all those with $J(i,t) = j$) (Bellmann et al. 2020, p. 7).

2.B.2 Appendix B: Further Analyses

Table 2.B.1: Manipulation Test by Cattaneo et al. (2020)

Cattaneo et al. Manipulation Test
(see Cattaneo, Jansson & Xinwei 2018, Cattaneo et al. 2020)

This test is based on a local polynomial density estimator and uses robust bias correction coupled with variance adjustments. Specifically, the manipulation test statistics in *rddensity* (Cattaneo, Jansson & Xinwei 2018) take the form

$$T_{po}(h) = \frac{\hat{f}_{+,po}(h) - \hat{f}_{-,po}(h)}{\hat{V}_{po}(h)} \qquad T_{po}(h) \sim \mathcal{N}(0,1),$$

where h is the bandwidth and po, the polynomial order. $\hat{f}_{+,po}(h)$ and $\hat{f}_{-,po}$ are the local polynomial density estimators, and $\hat{V}_{po}(h)$ represents the corresponding SE estimator.

In Table 2.B.2 and Table 2.B.4, I estimate a model with data-driven MSE-optimal bandwidth choices (h_{MSE}), $po = 2$ and a triangular kernel weight. With $q \geq po + 1$, the manipulation test takes the form of an α-level test, with the null rejected if

$$T_q^2(h_{MSE,po}) > X_1^2(1-\alpha)$$

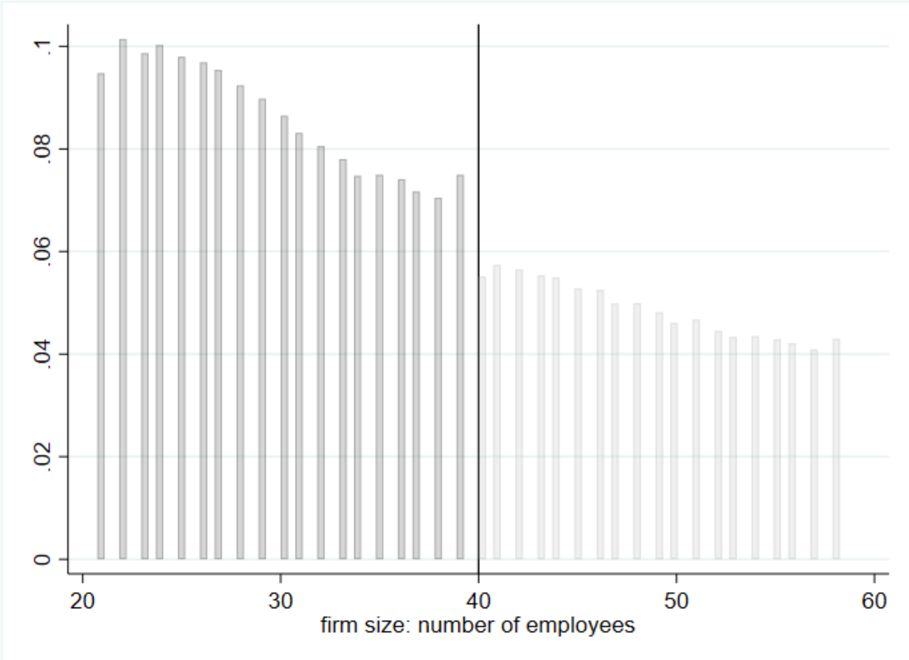
$T_q^2(h_{MSE,po})$ gives an asymptotically valid distributional approximation of $q \geq po + 1$. Thus, the possible first-order bias in the statistic T_{po}^2 is removed by using a higher-order polynomial in the estimation of the densities and adjusting the SE formulas accordingly.

Table 2.B.2: Cattaneo et al. Estimator Test Statistics

	T		P > —T—
Robust	-21.2550		0.000
# of Observations		625,664	

Notes: For details of the test statistics, see Table 2.B.1.
Source: BsbM and BHP 2004–2011, own calculations.

Figure 2.B.1: Firm Size Density for Firms with at Least One Disabled Worker



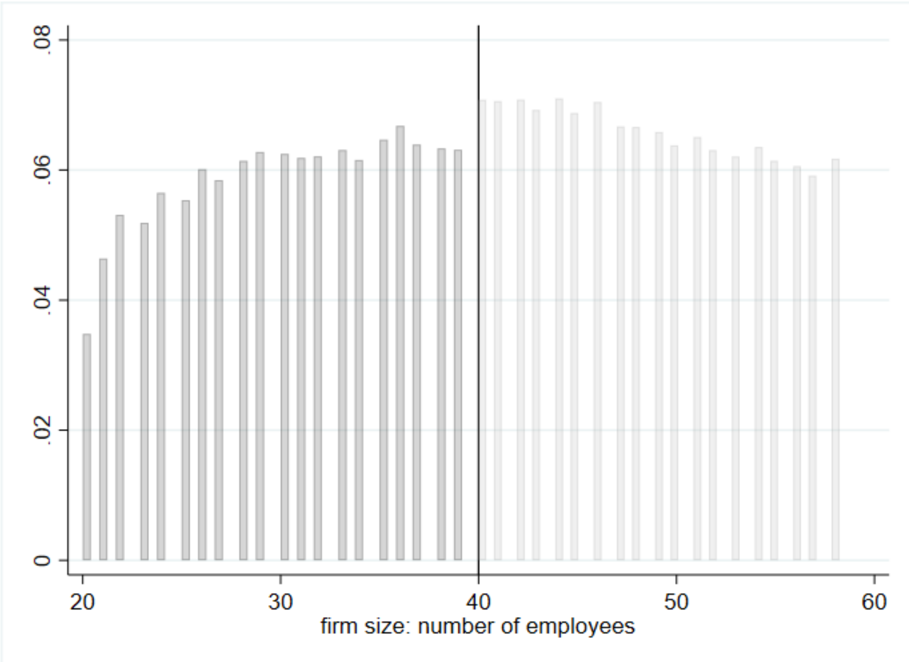
Notes: Histogram of firm size density for firms with at least one disabled worker around the 40-employee threshold.
Source: BsbM and BHP 2004–2011, own calculations.

Table 2.B.3: Effects among Firms with at Least One Disabled Worker

	Bunching Effect	Threshold Effect
Coefficient	-1.413	0.262**
Robust CI	[-3.818; 1.240]	[0.084; 0.645]
# of Observations	16	121,382

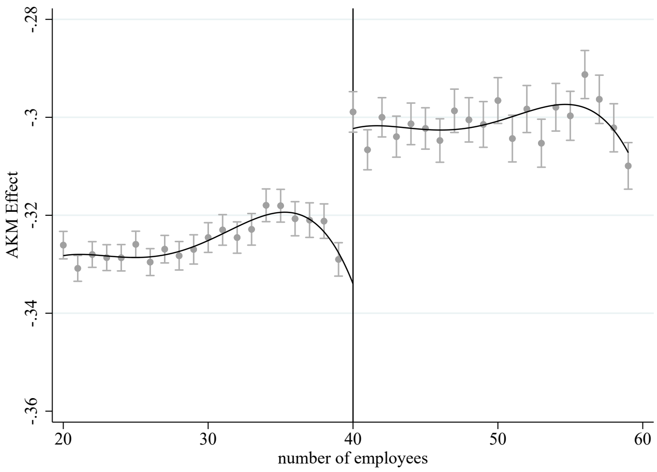
Notes: This table shows the estimation results for the threshold effects on the number of disabled workers in a firm only for firms which employ at least one disabled worker (h_{below}=8, h_{above}=9; po=4). Standard errors are clustered at the firm level. Significance level: ** $p < 0.05$.
Source: BsbM and BHP 2004–2011, own calculations.

Figure 2.B.2: Firm Size Density for Firms with at Least Two Disabled Workers



Notes: Histogram of firm size density for firms with at least two disabled workers around the 40-employee threshold.
Source: BsbM and BHP 2004–2011, own calculations.

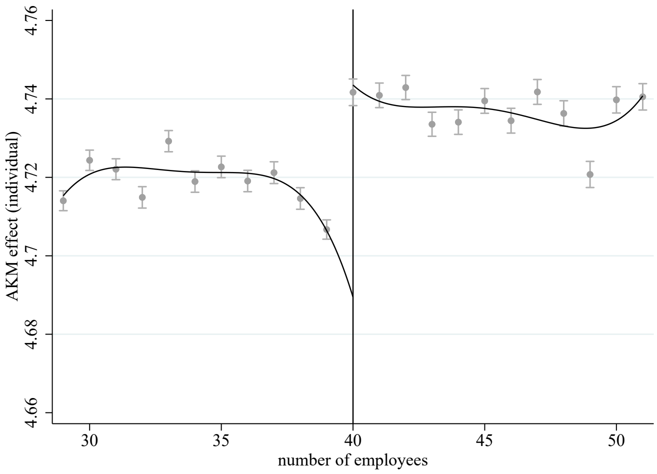
Figure 2.B.3: Firm Productivity



AKM Firm Fixed Effects

Notes: The graph plots the AKM firm fixed effects (see Table 2.A.3) by firm size around the 40-employee threshold with 95% confidence intervals. The black line approximates the functional form of the running variable (here with polynomial order fit po=4 and bandwidth h_{below}=20 and h_{above}=19).
Source: BsbM and BHP 2004-2010, own calculations.

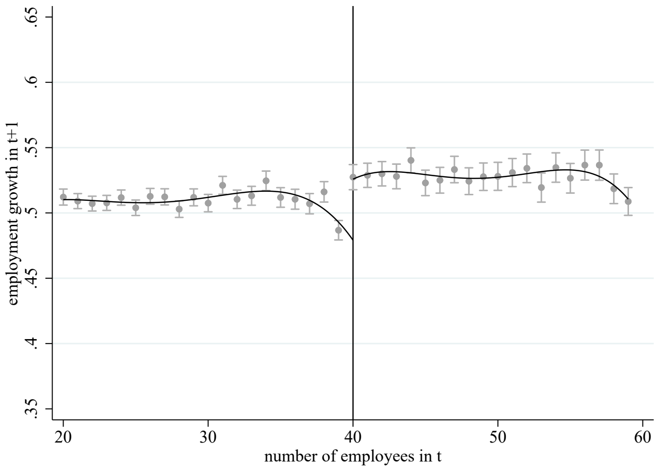
Figure 2.B.4: Employee Productivity



AKM Person Fixed Effects

Notes: This graph plots the AKM person fixed effects (see Table 2.A.3) by firm size around the 40-employee threshold with 95% confidence intervals. The black line approximates the functional form of the running variable (here with polynomial order fit po=4 and bandwidth h_{below}=12 and h_{above}=12).
Source: BsbM and Integrated Employment Biographies 2010, own calculations.

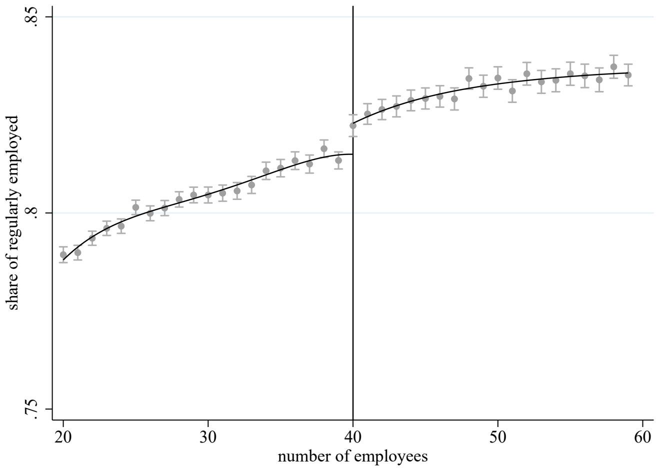
Figure 2.B.5: Firm Dynamics



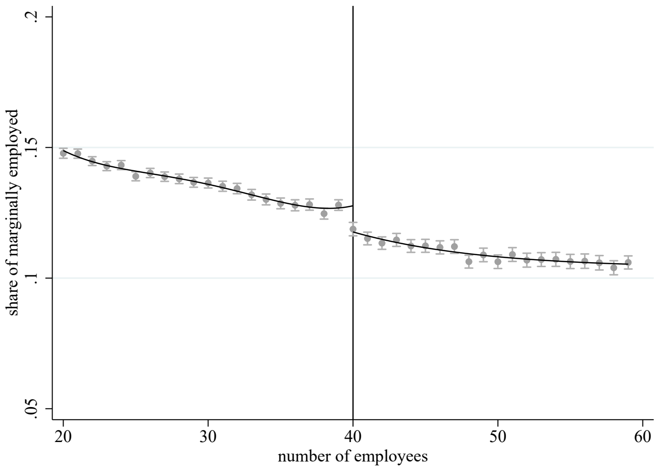
Employment Growth in t+1

Notes: This graph plots employment growth by firm size around the 40-employee threshold with 95% confidence intervals. Employment growth is defined via a dummy variable that equals 1 if a firm has more employees (according to the BsbM) in t+1 than in t and 0 otherwise. The black line approximates the functional form of the running variable (here with polynomial order fit po=4 and bandwidth h_{below}=20 and h_{above}=19).
Source: BsbM and BHP 2004–2011, own calculations.

Figure 2.B.6: Regular and Marginal Employment



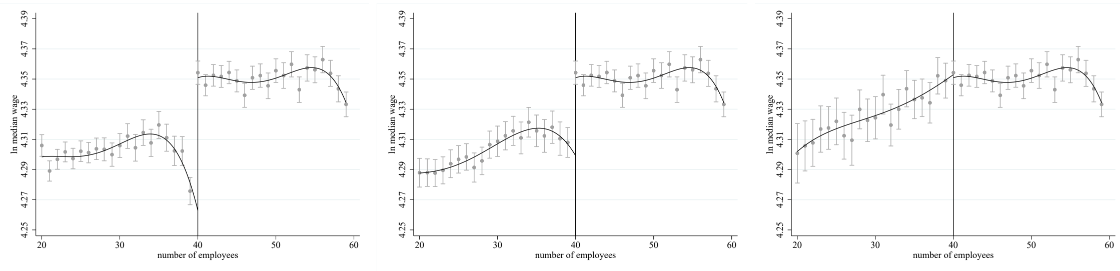
(A) Share of Regularly Employed Workers



(B) Share of Marginally Employed Workers

Notes: This graphs plot (A) the share of regularly employed workers and (B) the share of marginally employed workers by firm size around the 40-employee threshold with 95% confidence intervals. The black line approximates the functional form of the running variable (here with polynomial order fit po=4 and bandwidth h_{below}=20 and h_{above}=19).
Source: BsbM and BHP 2004–2011, own calculations.

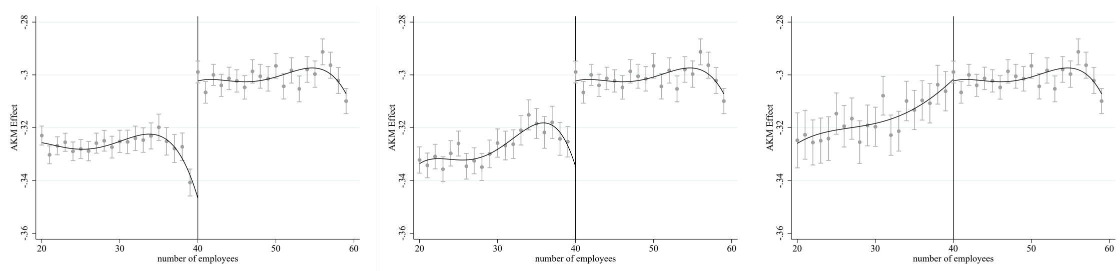
Figure 2.B.7: Median Wages: Noncompliers, Perfect Compliers and Overcompliers



Noncompliers *Perfect Compliers* *Overcompliers*

Notes: These graphs plot the ln of median wages (=median value of gross daily wages for full-time employees) by firm size around the 40-employee threshold with 95% confidence intervals separately for *noncompliers*, *perfect compliers* and *overcompliers*. The black line approximates the functional form of the running variable (here with polynomial order fit *po*=4 and bandwidth h_{below}=20 and h_{above}=19).
Source: BsbM and BHP 2004–2011, own calculations.

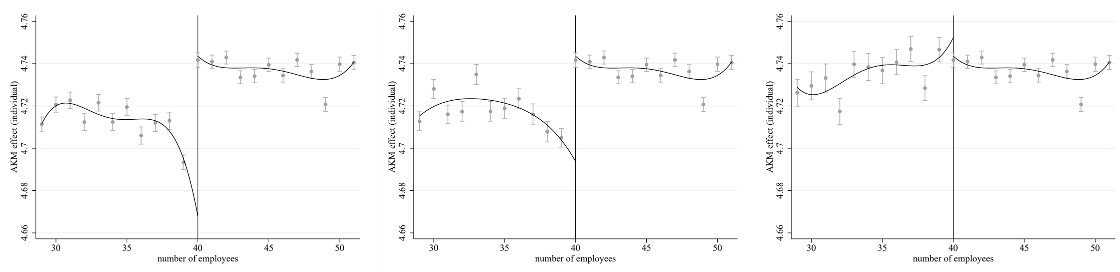
Figure 2.B.8: Firm Productivity: Noncompliers, Perfect Compliers and Overcompliers



Noncompliers *Perfect Compliers* *Overcompliers*

Notes: These graphs plot the AKM firm fixed effects (see Table 2.A.3) by firm size around the 40-employee threshold with 95% confidence intervals separately for *noncompliers*, *perfect compliers* and *overcompliers*. The black line approximates the functional form of the running variable (here with polynomial order fit *po*=4 and bandwidth h_{below}=20 and h_{above}=19).
Source: BsbM and BHP 2004-2010, own calculations.

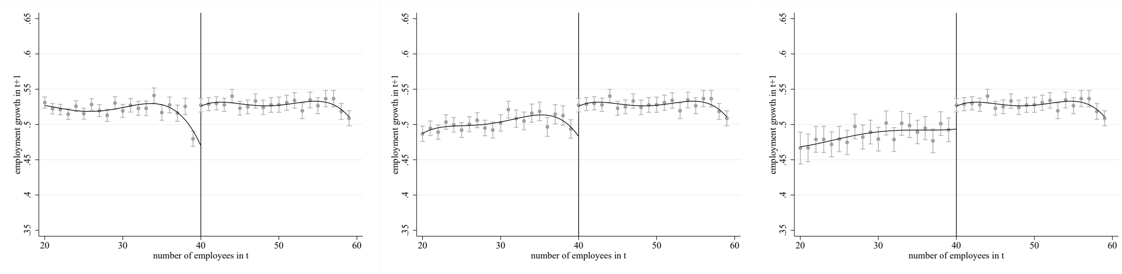
Figure 2.B.9: Employee Productivity: Noncompliers, Perfect Compliers and Overcompliers



Noncompliers *Perfect Compliers* *Overcompliers*

Notes: These graphs plot the AKM person fixed effects (see Table 2.A.3) by firm size around the 40-employee threshold with 95% confidence intervals separately for *noncompliers*, *perfect compliers* and *overcompliers*. The black line approximates the functional form of the running variable (here with polynomial order fit *po*=4 and bandwidth h_{below}=12 and h_{above}=12).
Source: BsbM and Integrated Employment Biographies 2010, own calculations.

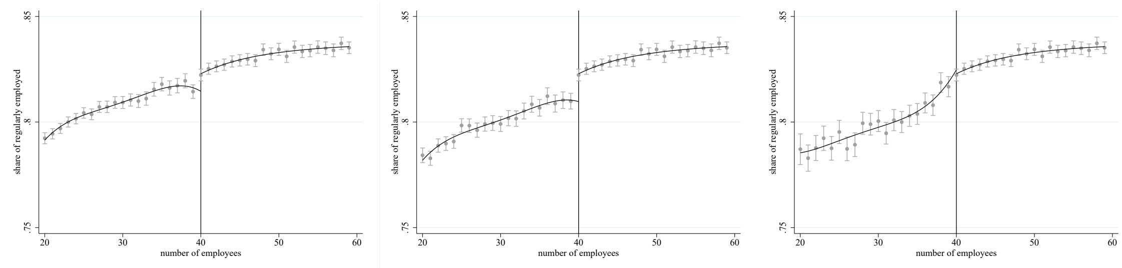
Figure 2.B.10: Employment Growth: Noncompliers, Perfect Compliers and Overcompliers



Noncompliers *Perfect Compliers* *Overcompliers*

Notes: These graphs plot employment growth by firm size around the 40-employee threshold with 95% confidence intervals separately for *noncompliers*, *perfect compliers* and *overcompliers*. Employment growth is defined as a dummy variable equal to 1 if a firm has more employees (according to the BsbM) in t+1 than in t and equal to 0 otherwise. The black line approximates the functional form of the running variable (here with polynomial order fit *po*=4 and bandwidth h_{below}=20 and h_{above}=19).
Source: BsbM and BHP 2004–2011, own calculations.

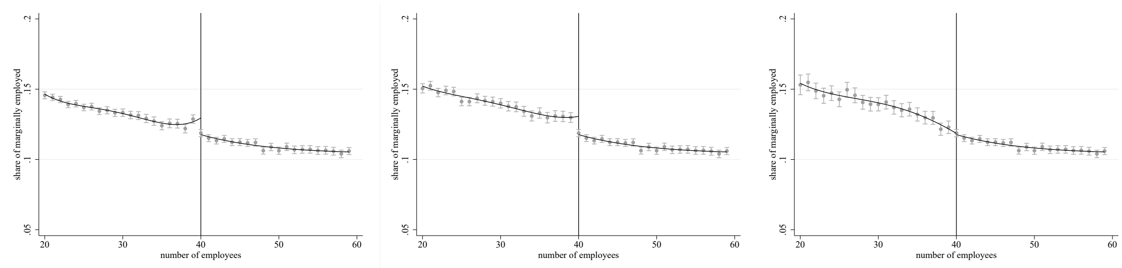
Figure 2.B.11: Share of Regularly Employed Workers: Noncompliers, Perfect Compliers and Overcompliers



Noncompliers *Perfect Compliers* *Overcompliers*

Notes: These graphs plot the share of regularly employed workers by firm size around the 40-employee threshold with 95% confidence intervals separately for *noncompliers*, *perfect compliers* and *overcompliers*. The black line approximates the functional form of the running variable (here with polynomial order fit *po*=4 and bandwidth h_{below}=20 and h_{above}=19).
Source: BsbM and BHP 2004–2011, own calculations.

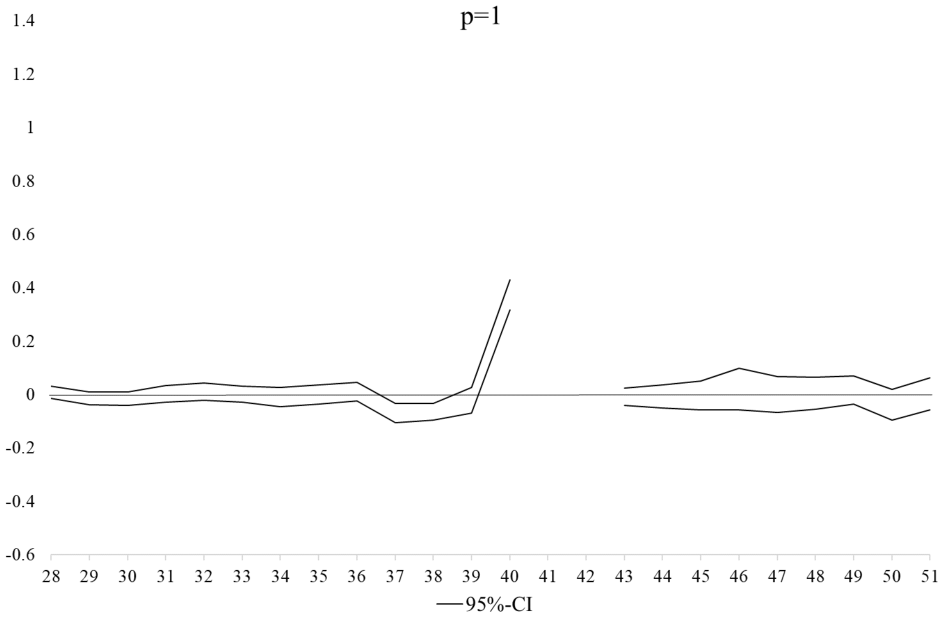
Figure 2.B.12: Share of Marginally Employed Workers: Noncompliers, Perfect Compliers and Overcompliers



Noncompliers *Perfect Compliers* *Overcompliers*

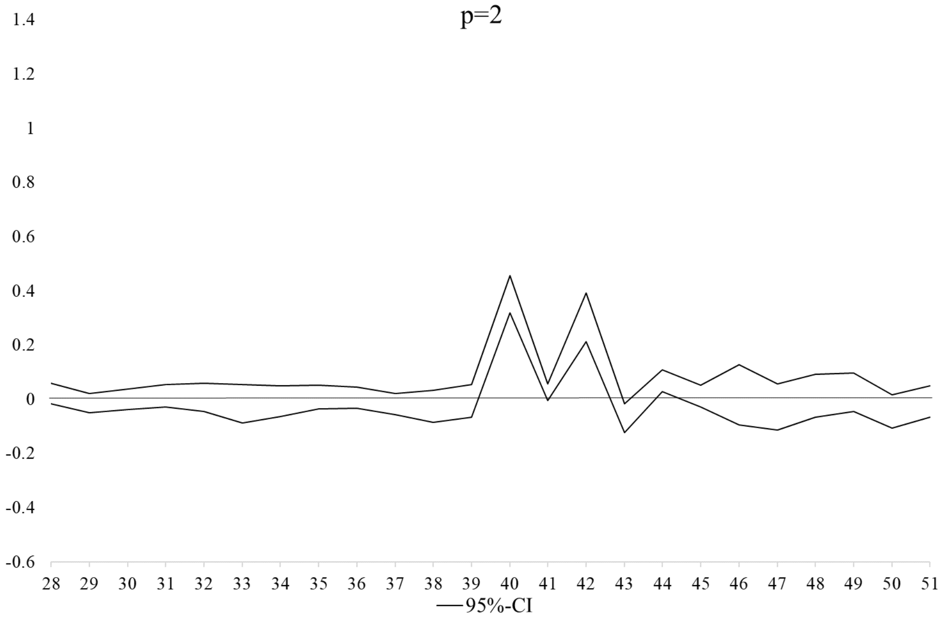
Notes: These graphs plot the share of marginally employed workers by firm size around the 40-employee threshold with 95% confidence intervals separately for *noncompliers*, *perfect compliers* and *overcompliers*. The black line approximates the functional form of the running variable (here with polynomial order fit *po*=4 and bandwidth h_{below}=20 and h_{above}=19).
Source: BsbM and BHP 2004–2011, own calculations.

Figure 2.B.13: Placebo Thresholds (Polynomial Order 1)



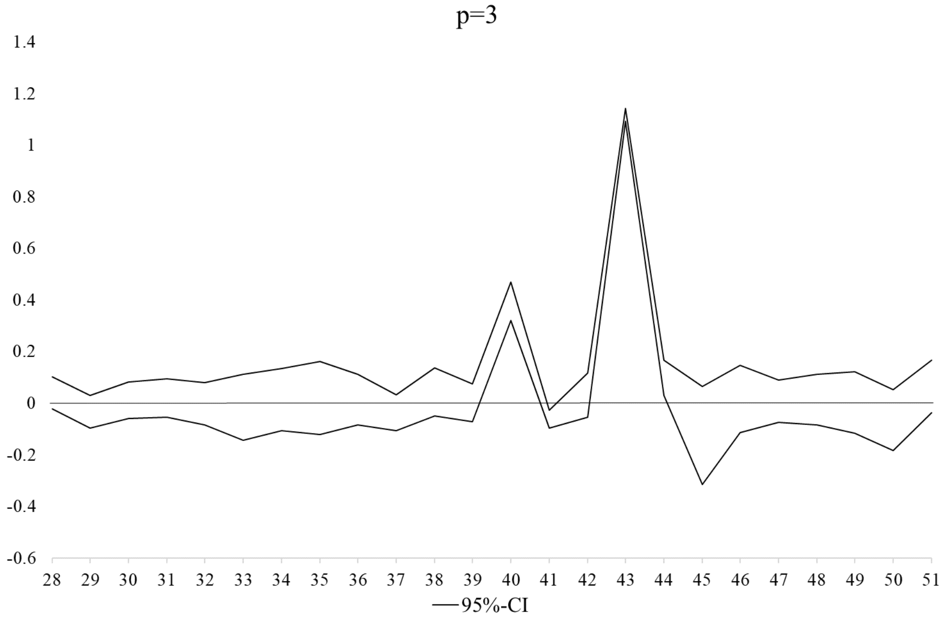
Notes: This graph shows the effects of placebo thresholds on the mean number of disabled workers for polynomial order *po*=1 and an MSE-optimal bandwidth on either side of the threshold (estimated including predetermined covariates). All thresholds except the 40-employee threshold are placebo thresholds. The 95% confidence interval is the robust CI estimated with the *rdrobust* command in Stata. As the point estimates could fall outside the robust CIs, only the interval boundaries are shown. Standard errors are clustered at the firm level. For *c*=41 and *c*=42, there were not enough observations to perform MSE-optimal bandwidth calculations.
Source: BsbM and BHP 2004–2011, own calculations.

Figure 2.B.14: Placebo Thresholds (Polynomial Order 2)



Notes: This graph shows the effects of placebo thresholds on the mean number of disabled workers for polynomial order *po*=2 and an MSE-optimal bandwidth on either side of the threshold (estimated including predetermined covariates). All thresholds except the 40-employee threshold are placebo thresholds. The 95% confidence interval is the robust CI estimated with the *rdrobust* command in Stata. As the point estimates could fall outside the robust CIs, only the interval boundaries are shown. Standard errors are clustered at the firm level.
Source: BsbM and BHP 2004–2011, own calculations.

Figure 2.B.15: Placebo Thresholds (Polynomial Order 3)



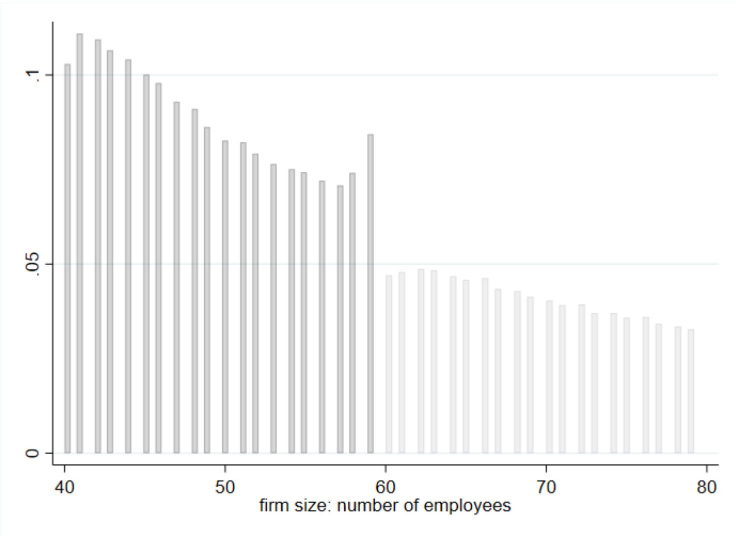
Notes: This graph shows the effects of placebo thresholds on the mean number of disabled workers for polynomial order *po*=3 and an MSE-optimal bandwidth on either side of the threshold (estimated including predetermined covariates). All thresholds except the 40-employee threshold are placebo thresholds. The 95% confidence interval is the robust CI estimated with the *rdrobust* command in Stata. As the point estimates could fall outside the robust CIs, only the interval boundaries are shown. Standard errors are clustered at the firm level.
Source: BsbM and BHP 2004–2011, own calculations.

Table 2.B.4: Cattaneo et al. Estimator Test Statistics - 60-Employee Threshold

	T		P > —T—
Robust	-22.5877		0.000
# of Observations		266,486	

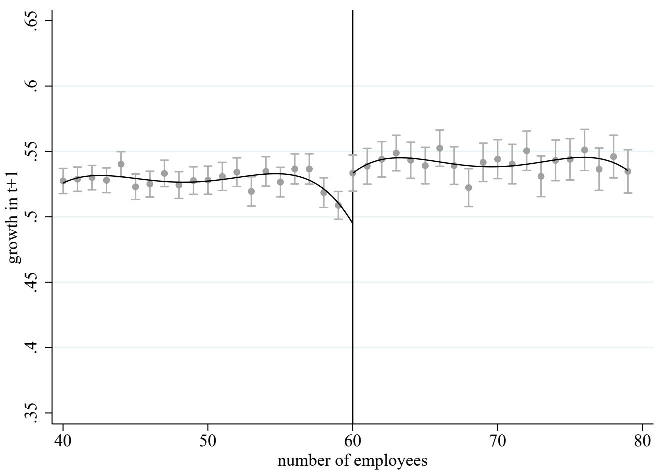
Notes: For details of the test statistics, see Table 2.B.1.
Source: BsbM and BHP 2004–2011, own calculations.

Figure 2.B.16: Firm Size Density at the 60-Employee Threshold



Notes: Histogram of firm size density around the 60-employee threshold.
Source: BsbM and BHP 2004–2011, own calculations.

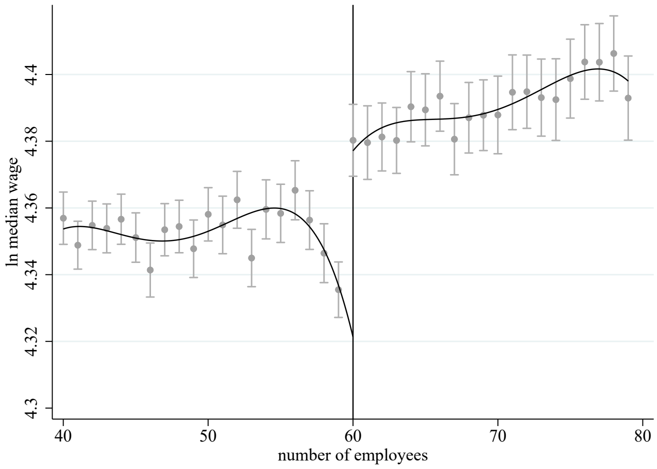
Figure 2.B.17: Firm Dynamics



Growth

Notes: This graph plots employment growth by firm size around the 60-employee threshold with 95% confidence intervals. Employment growth is defined as a dummy variable equal to 1 if a firm has more employees (according to the BsbM) in t+1 than in t and equal to 0 otherwise. The black line approximates the functional form of the running variable (here with polynomial order fit po=4 and bandwidth h_{below}=20 and h_{above}=19).
Source: BsbM and BHP 2004–2011, own calculations.

Figure 2.B.18: Median Wages



ln Median Wages

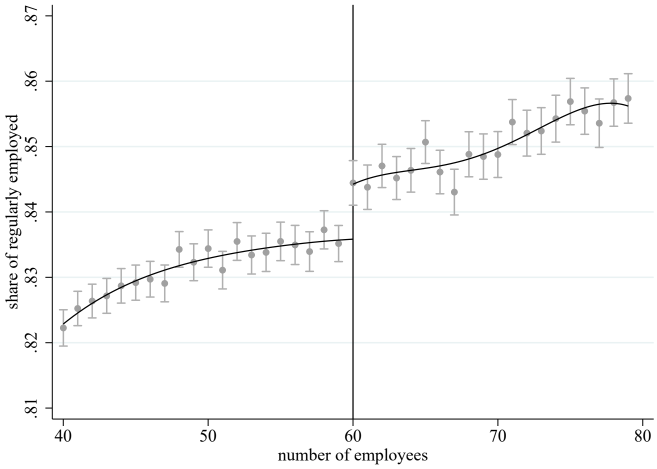
Notes: This graph plots the ln of median wages (=median value of gross daily wages for full-time employees) by firm size around the 60-employee threshold with 95% confidence intervals. The black line approximates the functional form of the running variable (here with polynomial order fit po=4 and bandwidth h_{below}=20 and h_{above}=19).
Source: BsbM and BHP 2004–2011, own calculations.

Table 2.B.5: Bunching Behavior - 60-Employee Threshold

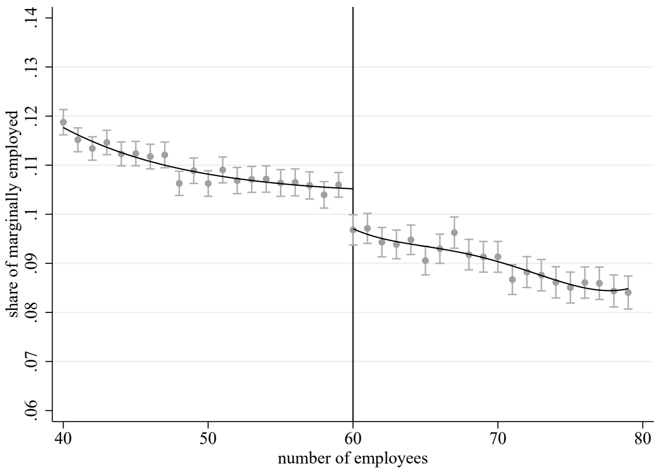
	po = 1	*po* = 4				
Dependent Variable	(1) Total	(2) Total	(3) Non-compliers	(4) Under-compliers	(5) Perfect Compliers	(6) Over-compliers
Sociodem. Structure						
Females	0.001	0.001	-0.003	-0.001	0.038	-0.019
Germans	0.003*	0.000	0.014	-0.016	0.003	-0.012
Employment Structure						
Median Wages (ln)	0.048***	0.050	0.090***	0.045	-0.029	0.010
Regularly Employed	0.009***	0.012*	0.025	-0.007	0.039*	0.006
Marginally Employed	-0.009***	-0.014**	-0.033*	-0.007	-0.019	0.002
Apprentices	-0.001	0.000	-0.002	0.007	-0.003	-0.009
Full-Time Workers	0.002	0.008	-0.005	-0.007	0.042	0.023
Part-Time Workers	0.007***	0.002	0.026	0.005	-0.013	-0.018
Skill Structure						
Low-Skilled Workers	-0.006***	-0.007	-0.009	0.000	-0.001	0.008
Medium-Skilled Workers	-0.001	-0.001	0.001	0.005	0.003	-0.005
High-Skilled Workers	0.010***	0.021	0.018	0.016	-0.012	-0.004
Firm Dynamics						
Growth	0.035**	0.031	0.120	0.069*	0.029	-0.315*

Notes: This table shows estimation results for the effects of the 60-employee threshold on alternative outcome variables. *Noncompliers*, *undercompliers*, *perfect compliers* and *overcompliers* are firms below the threshold that employ zero, exactly one, exactly two or at least three disabled worker(s), respectively. All estimations are estimated by using the MSE-optimal bandwidth on either side of the threshold. Standard errors are clustered at the firm level. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
Source: BsbM and BHP 2004–2011, own calculations.

Figure 2.B.19: Regular and Marginal Employment



(A) Share of Regularly Employed Workers



(B) Share of Marginally Employed Workers

Notes: These graphs plot (A) the share of regularly employed workers and (B) the share of marginally employed workers by firm size around the 60-employee threshold with 95% confidence intervals. The black line approximates the functional form of the running variable (here with polynomial order fit po=4 and bandwidth h_{below}=20 and h_{above}=19).
Source: BsbM and BHP 2004–2011, own calculations.

Table 2.B.6: Robustness Test Excluding Multiestablishment Firms I

	40-Employee Threshold – Bunching Effects				
	(1)	(2)	(3)	(4)	(5)
Coefficient		-1.307***	-1.455***	-2.013**	-2.018
Robust CI		[-2.199; -0.607]	[-2.584; -0.538]	[-4.075; -0.221]	[-5.509; 1.725]
Bandwidth h		6.37; 7.20	8.34; 10.08	7.95; 11.05	8; 9
Polynomial Order po		2	3	4	4
# of Observations		14	19	19	16
	40-Employee Threshold – Threshold Effects				
	(1)	(2)	(3)	(4)	(5)
Coefficient	0.345***	0.371***	0.383***	0.390***	0.383***
Robust CI	[0.316; 0.427]	[0.316; 0.452]	[0.317; 0.466]	[0.297; 0.489]	[0.190; 0.678]
Bandwidth h	2.23; 3.36	4.19; 5.36	6.56; 7.61	8.24; 9.45	8; 9
Polynomial Order po	1	2	3	4	4
Covariates included	yes	yes	yes	yes	
# of Observations	76,005	128,795	182,109	237,516	209,599
Lower Bound of Threshold Effect					0.198

Notes: This table shows the estimation results for the bunching effects (dependent variable: firm size density in %) and the threshold effects (dependent variable: mean number of disabled workers in a firm around the 40-employee threshold) without identifiable multiestablishment firms. Basic covariates include firm age, regional characteristics (federal state) and industry. Standard errors are clustered at the firm level. Significance levels: ** $p < 0.05$, *** $p < 0.01$.
Source: BsbM and BHP 2004–2011, own calculations.

Table 2.B.7: Robustness Test Excluding Multiestablishment Firms II

	(1)	(2)	(3)	(4)	(5)
	$po = 1$			$po = 4$	
Dependent Variable	Total	Total	Noncompliers	Perfect Compliers	Overcompliers
Sociodemographic Structure					
Share of Females	-0.003	-0.011	-0.006	-0.017	-0.011
Share of Germans	0.005***	0.012*	0.022**	0.005	-0.000
Employment Structure					
Median Wages (ln)	0.050***	0.085***	0.095***	0.045*	0.027
Firm Fixed (AKM) Effects	0.029***	0.050***	0.048***	0.037**	0.020
Person Fixed (AKM) Effects	0.039***	0.033***	0.048***	0.043	-0.004
Share of Regularly Employed	0.014***	0.016***	0.019***	0.016*	0.007
Share of Marginally Employed	-0.015***	-0.024***	-0.037***	-0.015*	-0.004
Share of Apprentices	0.000	0.002	0.009	-0.003	-0.001
Share of Full-Time Workers	0.008**	0.015	0.012	0.024*	0.018
Share of Part-Time Workers	0.003	0.007	0.014	-0.005	-0.005
Skill Structure					
Share of Low-Skilled Workers	-0.005***	-0.010	-0.015	-0.010	0.002
Share of Medium-Skilled Workers	-0.003	0.006	0.016	0.005	-0.021
Share of High-Skilled Workers	0.009***	0.007	0.003	0.003	0.018
Firm Dynamics					
Employment Growth in t+1	0.071***	0.228***	0.354***	0.118*	.0263

Notes: This table shows the estimation results for the effect of the threshold of 40 employees on alternative outcome variables. *Noncompliers*, *perfect compliers* and *overcompliers* are firms below the threshold that employ zero, exactly one or at least two disabled worker(s), respectively. All estimations are estimated by using the MSE-optimal bandwidth on either side of the threshold. Standard errors are clustered at the firm level. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
Source: BsbM and BHP 2004–2011 (2004–2010 for firm fixed effects, 2010 for person fixed effects), own calculations.

Chapter 3

Disability and Labor Market Performance

Abstract[*]

This paper analyzes the individual-level effects of disability onset on labor market outcomes using novel administrative data from Germany. Combining propensity score matching techniques with an event-study design, we find lasting negative impacts on employment and wages. One important mechanism is transitions to nonemployment after disability onset: newly disabled individuals' probability of becoming nonemployed increases by 10 percentage points after one year and by 15 percentage points after five years relative to that of the control group. For those who stay in employment, working part-time and switching to less physically or psychosocially demanding jobs are important adjustment paths. The negative labor market effects of disability onset are more pronounced for severely disabled, older and low-skilled individuals.

Keywords: disability, labor market outcomes, propensity score matching, event study
JEL Codes: I10, J14, J21, J71

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[*] This part is joint work with Matthias Collischon and Laura Pohlan. The paper was submitted and went under review in the *Journal of Human Resources* in May 2023.

3.1 Introduction

Disability is a widespread issue affecting the lives of millions of individuals. In 2019, one in seven working-age adults in OECD countries was identified as having a disability (OECD 2022). As aging is often accompanied by age-related chronic illnesses, this number is likely to grow in the future. Disabilities, such as physical impairments, hamper the opportunities of individuals in many domains, including the labor market.[1] Due to the sheer number of affected individuals and its implications for economic growth and societal welfare, many developed and developing countries have disability laws and acts aimed at abolishing discrimination against individuals with disabilities and eliminating barriers to their inclusion in society (United Nations 2022). These acts often include policy or institutional measures such as return-to-work programs, special protections against dismissal, disability pensions and employment quotas. Against this backdrop, an understanding of the labor market effects of becoming disabled is key, as it can support the design of effective disability policies to improve the recruitment, retention and development of disabled workers.

This paper quantifies the labor market effects of the onset of disability (defined, in our context, as the recognition of a severe disability status) and analyzes their underlying mechanisms for the first time based on administrative data. We focus on Germany, a country that is strongly affected by demographic change and where approximately 8 million individuals (9.4 percent of the population) are classified as having a permanent physical, mental or psychological health restriction involving severe disability (Federal Statistical Office 2022). Specifically, we use the Employment Statistics of Severely Disabled People (BsbM), which include annual information on the employment status of disabled workers in firms since 2003. In Germany, at least five percent of the workforce of firms with 20 or more employees must be disabled workers. These firms must declare annually which of their employees are disabled. Based on this information, we identify severely disabled individuals in the social security data of the Federal Employment Agency. Firms not meeting the quota are penalized. Thus, firms have an incentive to correctly declare their employees' disability status. Additionally, employees have an incentive to apply for disability status, as the status is associated with benefits such as additional leave days and social security benefits.[2] These are great data to track employees closely attached to the labor market who become disabled over the course of their working lives. In order to identify a severe and sudden health shock, we restrict our sample to individuals who are

[1] According to OECD (2022), people with disabilities are 40 percent less likely to be in employment than people without disabilities.

[2] We describe the institutional system in detail in the next section.

employed five years before disability onset.

We use the combined data set, which covers individuals reporting the onset of a severe disability between 2005 and 2013, to validate the survey evidence on the impact of disability onset on labor market outcomes (for survey evidence see, e.g., Charles 2003, Lechner & Vazquez-Alvarez 2011, Polidano & Vu 2015). Furthermore, the data allows us to study effect heterogeneities by individual and establishment characteristics and to investigate the potential mechanisms underlying the adverse labor market effects of disability onset. While much of the literature focuses on changes in working time, receipt of unemployment benefits or receipt of other replacement benefits (see, e.g., Charles 2003, Lechner & Vazquez-Alvarez 2011, Polidano & Vu 2015), little is known about other underlying mechanisms. In our paper, we attempt to fill this gap by exploiting information on deaths as a reason for being out of the labor force and information on employer or occupational switches. These analyses allow us to draw conclusions about which groups of disabled individuals are more likely to succeed in reintegration into the labor market and what changes in the employment relationship, for instance, with regard to working hours, the tasks performed and employer characteristics, are associated with this.

Moreover, the contribution of this paper lies in the fact that our administrative data set can be used to overcome challenges – such as selection into disability status, measurement of disability and sample size – that are poorly addressed by other empirical studies based on survey data. First, we have a considerably larger sample than the samples in previous works, with approximately 150,000 treated and more than 9 million potential control individuals for whom we have employment and wage information at daily frequency over a long time horizon. Longitudinal data from surveys, in contrast, typically enable the analysis of only 200–2,500 disability events (see, e.g., Charles 2003, Lechner & Vazquez-Alvarez 2011).

Second, to address biases through selection, such as the fact that individuals who are more likely to become disabled systematically differ in their labor market trajectories from nondisabled coworkers, we combine propensity score matching with an event-study approach. The process of registering for disability status takes time and can happen only after the actual onset of disability. Therefore, we match disabled individuals to nondisabled coworkers two years before the measured date of disability onset based on a broad array of observable characteristics, including detailed information on past labor market performance. We also take unobserved heterogeneity into account by conditioning on AKM-style measures for individual and establishment fixed effects.

Third, administrative data sources are less prone to measurement error, sample selection and panel attrition than comparable survey data sources. Disability status is a sensitive characteristic that might be misreported in surveys. Economic and psychological incentives, coupled with potential difficulties in interpreting the survey questions, are reasons for unreliable self-reports of disability status (e.g., Myers 1982, Bowe 1993, Hale 2001). Moreover, some studies document that individuals who find themselves out of the labor force tend to systematically overreport disability (see, e.g., Kreider 1999, Kreider & Pepper 2007, Lindeboom & Kerkhofs 2009), which could be explained by a so-called justification bias: People justify their labor market failures by using ill health as an excuse. In addition to the data on the treatment variable, information on wages and employment states might suffer from misreporting and selectivity issues (Pedace & Bates 2000). In surveys, short unemployment spells tend to be underreported, and unemployed persons tend to not respond to surveys at all (see, e.g., Van Den Berg et al. 2006, Pyy-Martikainen & Rendtel 2009, Lafuente 2020).

Based on our administrative data source, we document the following key results. Days in employment decrease and days in nonemployment increase for the disabled, even two years before our measured date of disability onset. One year after onset, nonemployment days increase by 36 days per year and the probability of being nonemployed increases by 10 percentage points in comparison to those of the control group. After five years, the effects amount to 15 percentage points and 55 days, respectively. Transitions to unemployment after disability onset, in contrast, do not seem to play an important role. Receipt of replacement benefits from health insurance and the end of the employment relationship are reasons listed for permanently leaving the labor force. Disabled workers remaining in the labor force experience a significant drop in daily wages: the difference from the wages of the control group amounts to 7 percentage points five years after the onset of disability. A significant share of disabled workers reduce their working time, and a rather small fraction change employers. We observe horizontal occupational switches toward less physically or psychosocially demanding jobs as well as vertical occupational switches toward jobs with a lower job requirement level. The negative labor market effects of disability onset are more pronounced for severely disabled, older and low-skilled individuals. Overall, disability onset is thus accompanied by a variety of adverse labor market outcomes.

In addition to our analyses using administrative records, we use survey data from the Panel Study Labour Market and Social Security (PASS) to provide descriptive evidence on the representativeness of our estimation sample and the types of disabilities that individuals usually face. Based on both the administrative and survey data sources, we document that the sample restrictions applied in our main analysis do not seem to strongly increase the selectivity of our sample. Moreover, the type of disability does not seem to depend on the degree of labor market attachment: approximately 90 percent of disabled individuals report physical disabilities and approximately 30 percent report psychological impairments.

Our paper represents an important contribution to the literature on the labor market integration of disabled individuals. It connects closely to studies focusing on the employment and income effects of disability onset by using comparable empirical identification strategies but relying on survey panel data and thus on assessments of the respondents. Lechner & Vazquez-Alvarez (2011) show for Germany based on the German Socio-Economic Panel (GSOEP) that becoming disabled reduces an individual's employment probability by 9 to 13 percentage points, depending on the degree of disability. The authors do not find a statistically significant relationship between the event of becoming disabled and a reduction in earnings or an increase in unemployment. Polidano & Vu (2015) also find negative impacts on employment rates, especially for full-time employment, using Australian panel data. The effects are particularly pronounced for younger individuals and individuals without post-school qualifications. The latter group has higher chances of being out of work and on income support than individuals with qualifications up to four years after disability onset. Charles (2003) concentrates on the impact of becoming disabled on the earnings of American men. The results indicate that disabled men experience sharp drops in earnings around the year of disability onset. Their earnings recover rapidly in the first two post-onset years, but a modest downward trend follows, which results in significant long-term losses of approximately 12 percent per year. Moreover, the author documents heterogeneous effects: being older at onset, nonwhite, more chronically disabled, and less educated come along with larger losses from disability and a smaller recovery. A large portion of these differences across groups appear to derive from industry affiliation after onset.

Other longitudinal studies on the impact of disability onset or health shocks confirm the negative effects on labor market participation and earnings.[3] Besides a significant and long-lasting decline in the probability of employment, Jones et al. (2018) document a decrease in life satisfaction and Meyer & Mok (2019) poorer economic conditions, as reflected by a decrease in earnings, net income, consumption and wealth at disability onset. The negative consequences are particularly pronounced for individuals with a chronic and

[3] See, e.g., Jenkins & Rigg (2004), Gannon (2005), Oguzoglu (2010), Jones & McVicar (2020) and Jolly & Wagner (2023) on the impacts of disability onset. Riphahn (1999), Garcia-Gomez (2011) and Lenhart (2019) study the effects of a deterioration of self-reported health status. Garcia-Gomez et al. (2013), Lundborg et al. (2015) and Dobkin et al. (2018) analyze the effects of acute hospitalization, while Moller Dano (2005), Crichton et al. (2011), Halla & Zweimüller (2013) and Parro & Pohl (2021) look at the impact of accidents and injuries. Moran et al. (2011), Heinesen & Kolodziejczyk (2013) and Jeon (2016) investigate the effects of surviving cancer.

severe disability condition.

Our paper also relates to analyses investigating the regulatory features of the German legislation as stipulated by the People with Severe Disabilities Act. The act was reformed in 2001, involving, among others, a substantial reduction in the generosity of the public disability insurance system. In a recent paper, Fischer et al. (2022) show that the reform significantly reduced the inflow of new benefit recipients but do not observe compensation through the private insurance market.[4] In addition, under the 2001 reform, the threshold for the applicability of the legislation for employers was increased, and the quota of positions to be filled with disabled workers was reduced. Studies evaluating the impact of this policy reform suggest that the reform was not successful in increasing the employment chances of severely disabled workers (see Verick 2004, Braakmann 2008).[5] Analyses of the disability quota threshold on firm dynamics and firm outcomes have come to different results. While Wagner et al. (2001) and Koller et al. (2007) document zero or small threshold effects on firm dynamics, Hiesinger (2022) finds significant effects on the number of employed disabled workers, firm growth, employment structure and wages.[6]

This paper is structured as follows. Section 3.2 elaborates on the German institutional context with respect to acquiring disability status and receiving social benefits after a health shock. Section 3.3 describes the data source, sample selection and empirical identification strategy. Section 3.4 presents the results of the empirical analysis, and Section 3.5 concludes.

3.2 The German Institutional Background

3.2.1 Acquiring Disability Status

In Germany, disability is defined as a physical, mental or psychological disorder that is not typical for the age of the patient and that has permanent consequences for the individual's health status. This disorder must impair the ability of the patient to participate in social life. An individual who wants to acquire a disability must go through a formal procedure

4 The impact of the generosity of the public disability insurance system on take-up rates, labor supply and the probability of returning to work outside of Germany has been studied, for instance, by French & Song (2014), Kostøl & Mogstad (2014), Autor et al. (2019) and Krekó et al. (2022).

5 Supply and demand effects of labor market disability policies such as employment quotas or wage subsidies for disabled workers have also been studied, e.g., by Barnay et al. (2019) for France, Szerman (2022) for Brazil, Baert (2016) for Belgium or Lalive et al. (2013) for Austria.

6 One possible explanation for these differences is the different data sets used in the respective studies: both Wagner et al. (2001) and Koller et al. (2007) use establishment-level survey data and hence rely on a small number of observations. In contrast, the study by Hiesinger (2022) uses an administrative data set that contains information on all German firms subject to the employment obligation.

carried out by an independent institution, the *Versorgungsamt* (§159 SGB IX). For this procedure, all medical documents related to the relevant health impairment(s) covering the preceding two years, for example, from treating physicians, must be submitted to the *Versorgungsamt*. This institution evaluates the degree of disability on a scale ranging from 20 to 100, graduated in steps of ten. An individual is defined to be "severely disabled" if his or her degree of disability is equal to or larger than 50.[7] Individuals with a degree of disability between 30 and 50 can be treated as severely disabled in the labor market if the disability restricts the possibilities of finding and holding a job.[8] As acquiring a disability status is a formal procedure involving several parties (e.g., the disabled individual, physicians, public authorities), the acquisition process takes time. Thus, there is probably a (considerable) time gap between the date of disability onset and the date of approval of disability status.[9] Once approved, the disability status is normally valid for five years.[10] Individuals are obliged to disclose their disability status to their employers only if the status affects the occupational activity in such a way that others or the individuals themselves would be at risk. Otherwise, acquiring disability status and communicating it to one's employer are voluntary. However, there are incentives for the individual worker to do so, as will be described in the following paragraphs.

The legal framework to promote the integration of people with disabilities in the labor market in Germany is laid down in part 3 of Book IX of the Social Code "Integration and Rehabilitation of Disabled People (SGB IX, 2001)", also called the Disabled Worker Law (*Schwerbehindertenrecht*). Enacted in 2001, it built upon the People with Severe Disabilities Act, which was originally implemented in 1974. In 2018, the so-called *Bundesteilhabegesetz* replaced the former law. One key element of the disability law is the employment obligation whereby public and private employers with at least 20 employees must fill at least five percent of their employment positions with severely disabled workers.[11] Many other OECD countries, such as Austria, France, Italy and Spain, use similar

7 An example of a degree of 50 is voicelessness or stunted growth of 120 to 130 cm.

8 Note that according to Lechner & Vazquez-Alvarez (2011), it is rare that individuals with an assigned degree of disability between 30 and 50 are *not* treated as severely disabled in the labor market.

9 Note that "disability onset" itself is often not a sudden change in status but a slow process (Jenkins & Rigg 2004).

10 Disability status is granted infinitely only if the severity of the disability is unchangeable or worsens over time.

11 Note that there are threshold rules with regard to the employment obligation for small firms: Firms with 20 to fewer than 40 employees must fill at least one position with a severely disabled individual per year, whereas firms with 40 to fewer than 60 employees must fill at least two positions with severely disabled individuals. Firms with 60 or more employees must meet the five percent quota. In general, one severely disabled individual is credited to one position. However, in the case of a very severe impairment due to the disability, a disabled individual may also be credited for more than one position (multiple crediting).

quota systems to mandate the employment of workers with severe disabilities (OECD 2003, 2010). The aim of this obligation is to create an incentive for employers to retain and/or hire disabled workers. Firms that do not comply with this obligation have to pay a graduated noncompliance fine (*Ausgleichsabgabe*).[12]

From workers' perspective, employees with a recognized severe disability are institutionally better protected than those with an unrecognized disability in two ways. First, they are subject to special dismissal protection. If the disabled employee has been working longer than six months in a firm, the employer needs to obtain permission for dismissal from the local integration office.[13] Second, a severely disabled worker receives more vacation days, i.e., an additional five days per year. Moreover, a recognized disability status may help an employee obtain special workplace equipment or financial assistance for occupational rehabilitation. Apart from better institutional protection in the labor market, individuals with a recognized severe disability status may receive further disadvantage compensations, e.g., in the form of reduced public transportation costs or museum admission. Thus, even though the acquisition of disability status is voluntary, the institutional framework in Germany offers many incentives to formally acquire such a status.

According to figures from the Federal Statistical Office (2022), 7.8 million individuals in Germany were considered severely disabled in 2021. Disabilities occur mainly in older people: Over one-third (34 percent) of the severely disabled individuals were 75 years and older, 45 percent were between 55 and less than 75 years old, and only 3 percent were younger than 18 years. Among the working-age group (individuals between 15 and 65 years old), 3.1 million individuals were considered severely disabled, representing approximately 6 percent of the total population in this age group. Illness is the main cause of the vast majority of disabilities (almost 90 percent). Hence, only a small share of disabilities are congenital or due to war damage, accidents or other causes. Further, physical causes, in particular organ disorders, account for the majority of disabilities (58 percent). While 14 percent of the severely disabled had mental or emotional disabilities, 9 percent suffered from cerebral disorders. For the remaining fraction (19 percent), the type of the most severe disability is not indicated. With respect to the degree of disability, 22

[12] The fine is based on the number of unfilled positions and is graduated according to the extent of noncompliance. The current fines are 140, 245 and 360 EUR per month and unfilled position. As in almost all countries with a quota system, the employment quota is generally not met in Germany. In 2021, approximately 61 percent of employers with 20 or more employees did not meet their employment obligation and thus had to pay the noncompliance fine (Federal Employment Agency 2023).

[13] In practice, the integration offices approve dismissals in most cases. For example, in 2020, 79 percent of dismissals were approved (Bundesarbeitsgemeinschaft der Integrationsämter und Hauptfürsorgestellen 2021). Nevertheless, many firms perceive the regulation as a hurdle to employing individuals with severe disabilities (Hiesinger & Kubis 2022).

percent of severely disabled individuals had the highest degree of disability (100), while 34 percent had a degree of disability of 50.

3.2.2 Social Benefits after Health Shocks

In addition to returning to employment, there are alternative ways for individuals with health impairments to receive income. In the following, we will discuss the four most relevant statutory regulations on the receipt of social benefits in Germany: sick pay, transitional benefits, unemployment benefits, and reduced earnings capacity pensions.

During the first six weeks of an illness episode, employees are entitled to short-term sick pay, which must be covered by the employer.[14] The replacement ratio amounts to 100 percent of individuals' earnings.[15] After six weeks of sickness with the same disease diagnosis, employees are entitled to long-term sick pay from the statutory health insurance fund.[16] The latter is mandatory for all employees subject to social security contributions and whose earnings fall short of the contribution limit of the statutory health insurance scheme. Thus, it covers the majority (approximately 90 percent) of the German population. The maximum duration of long-term sick pay for the same disease is 78 weeks within a period of three years, and the replacement level amounts to 70 percent of gross earnings.

After the expiration of long-term sick pay, employees who are still incapable of working can receive transitional benefits (*Übergangsgeld*). In general, this requires that former employees have contributed to the statutory pension insurance scheme and intend to participate in medical rehabilitation or vocational training measures. The statutory pension insurance takes over the transitional benefits for all rehabilitation measures that are intended to preserve the employability of individuals. The statutory accident insurance applies to individuals who have become ill as a result of an occupational accident or occupational disease. The Federal Employment Agency pays for vocational training measures that enable people with disabilities to participate in working life. Although responsibilities are not always entirely clear, recent figures show that the statutory pension insurance is most often involved: in 2020, approximately one million completed mea-

[14] The mandatory maximum duration of sick pay may also be reached if the employee accumulates several shorter illness periods within the preceding year provided that they are due to the same disease diagnosis.

[15] The German Continued Remuneration Act (*Entgeltfortzahlungsgesetz*) obliges employees who fall sick to submit a medical certificate no later than the fourth day of absence. Yet, the law permits employers to require a medical certificate starting from the first day of sickness.

[16] If an accident at work or an occupational disease caused the health impairment, the *Berufsgenossenschaften* pay an injury benefit during the period of medical rehabilitation.

sures were documented, while the Federal Employment Agency took over the transitional benefits for approximately 7 thousand individuals (Federal Employment Agency 2022*b*, German Pension Insurance 2022*b*). Transitional benefits for insured individuals without children amount to 68 percent of the last net salary (75 percent for insured individuals with children).

Individuals are entitled to receive insurance-based unemployment benefits (*Arbeitslosengeld I*) amounting to 60 percent (67 percent for claimants with children) of their previous net salary if they fulfill certain requirements. Specifically, they must have been employed and making social security contributions for at least 12 months within a certain time frame prior to becoming unemployed, and they must register as unemployed and as seeking employment at the Federal Employment Agency. Unemployed individuals can receive unemployment benefits for a maximum duration of one year; for older individuals, longer periods of benefit receipt are also possible.[17] Individuals whose qualifying period has ended but who are (still) unable to work, for instance, due to illness or disability, are also entitled to insurance-based unemployment benefits. After the expiration of insurance-based benefits, individuals can receive permanent means-tested welfare benefits (*Arbeitslosengeld II*).

In general, sick pay and transitional and unemployment benefits in Germany pursue the overall aim of sustaining the long-term employability of individuals who are still in the labor force. The nonpermanent character of these schemes is first reflected in the limited entitlement duration. Furthermore, 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.[18] Individuals who are registered as unemployed should be willing to find a job, for instance, by applying to vacancies or participating in integration measures or training courses.

Finally, we discuss statutory schemes that enable individuals to permanently withdraw from the labor market. The possibilities of receiving an old-age pension before retirement age due to unemployment or severe disability have become increasingly restricted or have been abolished altogether since the beginning of 2000. However, individuals with a degree of disability of 50 or more can apply for an old-age pension for severely disabled people before they reach the standard retirement age if they fulfill a minimum insurance period

[17] Since 2008, a maximum period of two years has been granted to individuals who are 58 years of age or older and have been employed for at least 48 months in the last five years.

[18] Individuals receiving long-term sick pay may also be monitored by the health insurance program's auditing system to prevent potential abuse of the sick pay system.

of 35 years.[19] Apart from this, individuals who fulfil a minimum insurance period of five years and made compulsory contributions during the last three years and who are unable to work for at least three hours per day can apply for a full reduced earnings capacity pension (*Erwerbsminderungsrente*) covered by the statutory pension insurance.[20] Individuals who are able to work for more than three hours but are unable to work for more than six hours per day are entitled to a partial pension. For severely disabled persons, reduced earning capacity pensions are not automatically granted. Doctors and physicians commissioned by the statutory pension insurance scheme draw up an expert opinion on the claimant's earning capacity based on submitted medical reports and, if necessary, on their own examinations. Of course, the documents and files play a decisive role both in the application for a severely disabled person's disability status certificate and for the prospect of obtaining a reduced earning capacity pension. Then, it is first examined whether the individual's earning capacity can be restored or at least improved through medical and/or occupational rehabilitation measures. If neither is possible, the reduced earnings capacity pension is usually granted for a maximum of three years and is converted to a permanent pension, at the latest, after nine years. The amount of the pension is based on the pension contributions of insured individuals and on their projected earnings until retirement age. When the individual reaches the statutory pension age, the reduced earnings capacity pension is converted to an old-age pension. Recent figures show that this institution is very relevant in Germany: in 2021, 88 thousand individuals received a partial reduced earnings capacity pension, and 1.7 million individuals received a full reduced earnings capacity pension. The average age of entry is slightly above 50. Almost 90 percent of new pensioners with reduced earnings capacity are under 60 years of age when they retire (German Pension Insurance 2022*a*,*b*).

3.3 Data, Sample and Empirical Strategy

3.3.1 Data

Our empirical analysis is based on three administrative data sets from the German Federal Employment Agency. The Employment Statistics of Severely Disabled People (BsbM)

[19] A severe disability generally allows an individual to retire before age 63. With a deduction of up to 11 percent, retirement is even possible at just over 60 years of age.

[20] The reduced earnings capacity pension is not associated with the occupation previously performed. Statutory occupational disability insurance was abolished in 2001. However, people of working age are increasingly taking out private insurance policies (according to the German Insurance Association (GDV), there were just under 17 million insurance policies covering occupational disability in 2017 (Deutsche Aktuarvereinigung e.V. 2018)).

offer annual statistics available since 2003 on the employment of disabled workers in firms. As spelled out in Section 3.2.1, firms with 20 or more employees must fill a certain share of their employment positions with workers with disabilities. Thus, firms of this size must declare annually how many employees they have and which of their employees are severely disabled.[21]

The information from the BsbM data can be merged with severely disabled workers' employment histories from the Integrated Employment Biographies (IEB) until 2013 (for detailed information on a subsample of this data set, see, e.g. Frodermann et al. 2021).[22] The IEB include detailed information on individual characteristics (such as gender, age, nationality), different labor market states (such as periods of employment and registered unemployment) and employment information (such as occupation or daily wages) of individuals in Germany with at least one entry in their social security records (starting from 1975 onward in West Germany and from 1992 onward in East Germany). Thus, periods of self-employment, civil service, and military service are not included in the data set. Further, the data also include an establishment identifier that allows us to merge further information from the establishment data of the Federal Employment Agency, namely, the Establishment History Panel (BHP) (Schmucker et al. 2018).[23] The BHP provides detailed annual information on establishments' workforce such as their skill, employment or wage structure on the reference date of June 30.

3.3.2 Sample and Variables

We restrict our sample of disabled workers to individuals for whom we observe a change in status from nondisabled to disabled during their employment in the reporting establishment.[24] Further, we exclude individuals for whom we observe more than one change

[21] Severely disabled means that an individual has a degree of disability of at least 50. As spelled out in Section 3.2.1, individuals with a degree of disability between 30 and 50 can be treated as severely disabled in the labor market if the disability restricts their possibilities of finding and holding a job. These individuals are included and account for 14.7 percent of the disabled workers in the BsbM data.

[22] The records were linked based on personal identifiers and birth dates from the Data Infrastructure Management (DIM) department of the Institute of Employment Research (IAB). 86 percent of these severely disabled individuals could be linked to the IEB. Due to data restrictions, the records can be linked only up to 2013.

[23] Note that the BsbM is a firm data set while the BHP contains information on establishments. Thus, in the case of multiestablishment firms, the establishment information of the Establishment History Panel refers only to the main establishment.

[24] As spelled out in the first paragraph of this section, the information on whether an individual is disabled stems from reports by the employer. Thus, we do not have a fixed date of "disability status acquisition" for an individual but rely on the annual report of his or her employing firm. Our sample consequently contains disabled individuals who were not reported as being disabled for at least one year in an establishment that later reported him or her as such. "Reporting" establishments are

from nondisabled to disabled in our observation period to ensure that the event of disability onset is not influenced by previous onset events.[25]

In addition to the sample of disabled workers, we draw a control sample of nondisabled workers employed in the same establishments and occupations[26] as the disabled workers. Specifically, our control sample includes individuals not identified as disabled who ever worked in one of the reporting establishments between 2005 and 2013. To ensure comparability between individuals, we restrict our control group to individuals working in the firms and occupations in which at least one disabled worker is employed. Since our focus is on the impact of disability on labor market outcomes, we restrict our sample to individuals closely attached to the labor market. In our main specification, we therefore include in the sample only individuals who have been participating in the labor force for at least five years (i.e., in t-5, t-4, t-3, t-2 and t-1) prior to potential disability onset.[27] Furthermore, we aim to rule out that establishments purposely hire individuals with a (developing) severe disability. Thus, we restrict our sample to individuals with a sufficiently long tenure in an establishment and occupation, i.e., individuals who have been employed in the reporting establishment for at least three years prior to disability onset.[28] We also restrict on individuals being in the same occupational segment one year before matching and in the year of matching. Last, as disability onset is particularly relevant at an advanced age, we exclude individuals under the age of 30 (at the time of matching). Moreover, disabled employees aged 58 years or older are not subject to the special dismissal regulations. Thus, we include only individuals younger than 56 years (at the time of matching).[29] By definition, we do not observe a disability onset event for our control individuals.

Creating a balanced panel, we observe individuals five years before and five years after potential disability onset. This leads to a sample of 148,660 disabled and a pool

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thus establishments that (1) are subject to the employment obligation (and thus employ 20 or more employees) and (2) report at least one disabled worker during the observation period.

25 This exclusion affects 14.9 percent of the individuals in the original sample.

26 The 2-digit aggregate of the German Classification of Occupations 2010 (KldB 2010) contains 14 occupational segments that are summarized based on the tasks characterizing a job (Matthes et al. 2015).

27 Note that many studies that analyze the effects of health shocks on labor market outcomes condition on employment prior to the shock (see, e.g., Lundborg et al. 2015, Jeon 2016).

28 For this, we make sure that an individual has an employment spell in the same establishment as of the reference date, June 30th, in the years t-1, t-2 and t-3. Thus, we allow for variation in employment days during the year. Nevertheless, to ensure that we consider only individuals closely attached to the reporting establishment in the sample, we further exclude individuals with fewer than 365 employment days in the reporting establishment within the two years before potential disability onset.

29 This restriction is also in line with common practice in the literature, as many studies focus on individuals aged between 30 and the late 50s (e.g. Lundborg et al. 2015, Heinesen & Kolodziejczyk 2013, Moran et al. 2011).

of 9,231,050 nondisabled observations. Table 3.3.1 on page 138 and 139 presents individual and establishment characteristics separately for treated and control individuals (columns (1) and (2)). The table suggests that 57 percent of the individuals experiencing disability onset are male, 45 percent are between 50 and 55 years old and 80 percent have a vocational training degree (are medium-skilled). Compared to the control group, the group experiencing disability onset includes more older and low- or medium-skilled individuals. Moreover, treated individuals work in smaller establishments, have a longer employment duration and earn lower wages. Interestingly, disability onset does not seem to be concentrated in specific occupations or industries.

We focus on two aspects of labor market outcomes: employment and labor earnings. For employment, we analyze the effect of disability on (1) being employed on the reference date (June 30th) and (2) the number of days in employment per calendar year. The employment status helps us compare the effect that we identify with the effects found in the literature, as this measure is widely used as an outcome variable in disability studies (see, e.g., Polidano & Vu 2015, Lechner & Vazquez-Alvarez 2011). In addition to employment status, the annual number of days in employment provides a more precise measure of labor market participation after disability onset. For labor earnings, we focus on (1) annual labor earnings (in EUR and deflated to 2015 prices, measured as the inverse hyperbolic sine (ihs) transformation to account for zeros in annual labor earnings), which can be interpreted as a measure of economic welfare, and (2) log daily wages (in EUR and deflated to 2015 prices) as an indicator of productivity (Charles 2003).[30]

To give a first impression of the outcome variables, Figure 3.3.1 shows the trend for employment days and annual earnings for disabled workers. Both outcome variables show constant development until two years before disability onset. In t-2, in particular, the number of days in employment begins to decline, indicating that disability is already relevant before the official acquisition of disability status.[31]

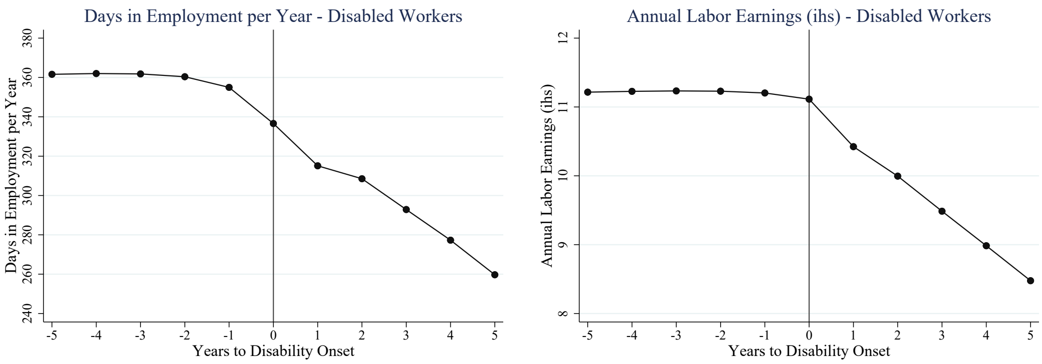
3.3.3 Empirical Strategy

The onset of disability is, in many cases, not a random event but depends, for instance, on occupational tasks and health history. To address potentially nonrandom self-selection into treatment, we apply a matching strategy, more specifically, 5-nearest-neighbor propen-

[30] Note that gross daily wages are right censored in the IEB due to the upper limit on social security contributions. However, we assume that this censoring should, if anything, result in attenuation bias, as observations in the control group should be more likely to report censored wages, which would lead to an underestimation of the magnitude of the effects of disability.

[31] As spelled out in Section 3.2.1, this is not surprising since the acquisition of disability status takes time.

Figure 3.3.1: Descriptives: Employment Days and Annual Earnings



Notes: The figure shows the trends for employment days and (ihs-transformed) annual earnings for the sample of disabled workers five years before and after disability onset. Earnings are deflated to 2015 prices.
Source: BsbM and IEB, years of disability onset: 2005–2013, n=148,660, own calculations.

sity score matching combined with exact matching. As discussed in the previous paragraph and shown by the descriptive trends of the outcome variables, the process of registering for disability status takes time and can only happen after the actual onset of disability. Thus, we split the sample by future disability status two years prior to the appearance of disability status in our data to identify the treatment and control groups.[32] Our control sample consists of individuals with a hypothetical disability onset event two years later. One nondisabled individual can therefore be used as control several times (in several calendar years and multiple times as nearest neighbor).[33]

For the matching procedure, we use a rich set of individual, establishment and predisability characteristics, i.e., variables that cannot be affected by the treatment. We match exactly on gender, age categories and calendar year. To estimate the propensity score, we match on nationality, qualification, occupation and job requirement level as individual characteristics. Among establishment characteristics, we use the number of employees, industry as well as median wage and location of the establishment (East vs. West Germany) as matching variables. Last, to match on the individual employment history, we include the cumulative duration (in months) of previous employment, tenure and nonemployment and the cumulative number of nonemployment spells as well as employment and nonemployment days (in categories) in the preceding years of disability onset (i.e.,

[32] Note that matching on observables two years before the measured date of disability onset is also in line with common practice in the literature (Polidano & Vu 2015).

[33] Of our sample of 624,439 control observations, 566,069 are unique individuals. In Section 3.4.4, we show that our results are robust in estimations applying 1-nearest-neighbor propensity score matching without replacement.

in t-5, t-4, t-3 and t-2).[34] Furthermore, we match on the logarithm of daily wages in the predisability years and on dummy variables indicating whether an individual was in a different establishment than the reporting establishment in t-5 and t-4.

Table 3.3.1 describes the matching quality. The last columns report the standardized differences in covariate means (Δ_X) between treated and (matched) control observations as a scale-free measure of balancing (see, e.g., Austin 2011, Guo & Fraser 2014).[35] Since there is no universally agreed criterion for how small the standardized difference must be to provide balance, we lean on the general rule of $\Delta_X < |0.1|$ suggested by Austin (2011). The standardized differences between treated and control observations reported are substantially reduced after matching, resulting in differences that are very close to zero and fulfill the criterion. Thus, we conclude that the matching procedure is successful in identifying a suitable control group.

In the next step, we use the generated matching weights in an event study analysis similarly to Kleven et al. (2019). We compare individuals who eventually become disabled (the treatment condition) to individuals who never experience disability (the control group). We then depict the results over time from five years prior to the onset of disability to five years afterward. This strategy allows us first to assess whether the treatment and control groups are truly comparable in their trajectories by investigating the trajectories in labor market outcomes prior to the onset of disability, which should not diverge. Second, we can observe the treatment effect and dynamics in this effect over time by investigating the trajectories after the onset of disability.

Specifically, we estimate the following equation:

$$Y_{it} = \alpha \; + \; \beta disabled_i \; + \sum_{\substack{k=-5,\\ k\neq-2}}^{5} \delta_k disabled_i \times I(t=k) \; + \sum_{\substack{k=-5,\\ k\neq-2}}^{5} \gamma_k \; I(t=k) \; + \; \omega X_{it} \; + \; \epsilon_{it}, \tag{3.1}$$

where Y_{it} is the outcome of interest (e.g., employment status or daily wage) of individual i in period $t = \{-5, \ldots, +5\}$ before or after disability onset. $disabled_i$ is an individual-constant group indicator for ever becoming disabled, $I(t = k)$ indicates the

[34] Note that we cannot match on individual sickness history because we cannot clearly identify illness periods in our data. However, nonemployment spells include periods of long-term sickness. Thus, we can assume that we approximately control for individual sickness history by including the number and duration of nonemployment spells.

[35] The standardized difference is defined as $\Delta_X = \left(\bar{X}_1 - \bar{X}_0\right) / \left((S_1^2 + S_0^2)/2\right)^{0.5}$, where $\bar{X}_w$ is the sample mean of treated ($w = 1$) or control ($w = 0$) observations and S_w^2 are the respective sample variances (Austin 2011). The advantage of Δ_X over the usual t statistic is that it does not mechanically increase with the sample size and therefore avoids exaggerating small imbalances that would still appear significant in a t test.

Table 3.3.1: Balancing of Covariates

	Treated	Control Unmatched	Control Matched	Standardized Differences	
	(1)	(2)	(3)	(1)-(2)	(1)-(3)
Male	0.570	0.609	0.570	-0.078	0.000
Age Categories					
30–34 Years	0.038	0.069	0.038	-0.140	0.000
35–39 Years	0.088	0.206	0.088	-0.337	0.000
40–44 Years	0.169	0.268	0.169	-0.241	0.000
45–49 Years	0.258	0.241	0.258	0.041	0.000
50–55 Years	0.447	0.217	0.447	0.504	0.000
Foreign	0.080	0.078	0.082	0.007	-0.008
Qualification					
Low-Skilled	0.086	0.067	0.087	0.069	-0.003
Medium-Skilled	0.801	0.745	0.800	0.135	0.003
High-Skilled	0.113	0.188	0.113	-0.211	0.000
Occupation					
Agriculture, Forestry, Horticulture	0.007	0.002	0.007	0.066	-0.002
Manufacturing	0.131	0.121	0.133	0.029	-0.005
Production Technology	0.158	0.213	0.155	-0.143	0.008
Building and Interior Construction	0.039	0.021	0.041	0.107	-0.009
Food, Gastronomy, Tourism	0.025	0.013	0.026	0.092	-0.004
Medical and Nonmedical Healthcare	0.078	0.130	0.080	-0.171	-0.008
Social Sector and Cultural Work	0.058	0.047	0.060	0.048	-0.008
Commerce and Trade	0.043	0.021	0.044	0.123	-0.008
Business Management and Organization	0.202	0.179	0.196	0.057	0.015
Business-Related Services	0.072	0.105	0.072	-0.114	0.001
IT Sector and Natural Sciences	0.046	0.062	0.045	-0.067	0.006
Safety and Security	0.018	0.008	0.018	0.093	0.005
Traffic and Logistics	0.100	0.066	0.101	0.122	-0.005
Cleaning Services	0.023	0.012	0.023	0.085	0.001
Job Requirement Level					
Unskilled/Semiskilled	0.059	0.039	0.060	0.096	-0.001
Specialist	0.750	0.726	0.753	0.057	-0.006
Complex Specialist	0.094	0.096	0.090	-0.009	0.012
Highly Complex	0.097	0.139	0.097	-0.133	-0.001
Establishment Characteristics					
Industry					
Agrarian, Fishery	0.003	0.001	0.003	0.057	-0.001
Energy, Mining	0.023	0.020	0.023	0.024	0.000
Manufacturing	0.385	0.430	0.380	-0.091	0.012
Construction	0.029	0.011	0.030	0.131	-0.004
Wholesale	0.082	0.039	0.082	0.179	0.000
Traffic, Communication	0.048	0.046	0.051	0.011	-0.011
Banking, Insurance	0.058	0.103	0.060	-0.168	-0.011
Other Services	0.075	0.057	0.076	0.075	-0.003
Public Administration (PA)	0.265	0.268	0.264	-0.007	0.003
Public Sector (w/o PA)	0.031	0.026	0.032	0.034	-0.003
Location: East Germany	0.138	0.125	0.138	0.041	0.002
Number of Employees in Firm					
20–49 Employees	0.104	0.010	0.106	0.414	-0.008
50–99 Employees	0.119	0.023	0.118	0.383	0.002
100–199 Employees	0.137	0.050	0.136	0.302	0.002
200–499 Employees	0.196	0.144	0.194	0.138	0.005
500–999 Employees	0.142	0.173	0.140	-0.085	0.005
1000+ Employees	0.303	0.600	0.306	-0.626	-0.006
Median Wages in Establishment	102.447	113.564	102.381	-0.395	0.002
Number of Observations	148,660	9,231,050	624,439		

Notes: Gender and age categories are matched exactly. In addition to the covariates shown, our matching procedure uses years (exact matching). All listed covariates are measured at t-2 (two years before (hypothetical) disability onset). We impute the education variable following Fitzenberger et al. (2006). Categories of education: (1) low-skilled: no vocational training; (2) medium-skilled: vocational training; (3) high-skilled: university or university of applied sciences. The summary statistics of the matched control observations (column (3)) are weighted by the matching weights described in Section 3.3.3.
Source: BsbM and IEB, years of disability onset: 2005–2013, own calculations.

Table 3.3.1: Balancing of Covariates *(continued)*

	Treated	Control Unmatched	Control Matched	Standardized Differences	
	(1)	(2)	(3)	(1)-(2)	(1)-(3)
Individual Employment History					
Cum. Employment Duration	263.853	233.895	263.482	0.342	0.004
Cum. Nonemployment Duration	27.053	26.549	27.149	0.012	-0.002
Tenure	162.295	161.523	162.352	0.008	-0.001
Number of Nonemployment Spells	2.881	2.180	2.908	0.261	-0.009
Days in Employment in t-5	361.603	363.191	361.470	-0.092	0.007
Days in Employment in t-4	362.013	363.891	361.907	-0.127	0.006
Days in Employment in t-3	361.808	364.116	361.734	-0.160	0.004
Days in Employment in t-2	360.383	364.237	360.545	-0.230	-0.008
Days in Nonemployment in t-5	2.480	1.328	2.603	0.086	-0.008
Days in Nonemployment in t-4	2.532	0.919	2.595	0.128	-0.004
Days in Nonemployment in t-3	3.061	0.891	3.085	0.161	-0.001
Days in Nonemployment in t-2	4.688	0.942	4.497	0.230	0.009
(ln) Daily Wages in t-5	4.458	4.557	4.451	-0.205	0.014
(ln) Daily Wages in t-4	4.486	4.590	4.479	-0.225	0.015
(ln) Daily Wages in t-3	4.512	4.621	4.505	-0.243	0.016
(ln) Daily Wages in t-2	4.528	4.646	4.521	-0.262	0.016
Different Establishment in t-5	0.090	0.078	0.090	0.043	0.000
Different Establishment in t-4	0.045	0.038	0.045	0.036	-0.001
Number of Observations	148,660	9,231,050	624,439		

Notes: Cumulative durations and tenure are measured in months. For our matching procedure, we use 3 (in t-3 and t-2) and 5 (in t-5 and t-4) categories of employment and nonemployment days. We classify the categories at the median/quartiles and generate a separate category for 365/366 employment days and zero nonemployment days. The summary statistics of the matched control observations (column (3)) are weighted by the matching weights described in Section 3.3.3.
Source: BsbM and IEB, years of disability onset: 2005–2013, own calculations.

periods around the year of disability onset and X_{it} is a set of covariates including age, gender and year dummies. ϵ_{it} is the idiosyncratic error term. Considering the matching procedure performed previously, we assign $I(t = k)$ to individuals who never become disabled based on the timing of the onset of disability of the individuals to whom they are matched. α is a regression constant, and β accounts for the level difference between disabled and nondisabled individuals in the reference period, i.e., at t-2. γ_k measures the impact of time period k relative to the reference period for the control group. δ_k is the coefficient of interest, which provides the difference between the outcomes of individuals who become disabled and those of their control group in period k and thus the treatment effect. As nondisabled individuals can be used as controls several times (in several calendar years and multiple times as nearest neighbors), we display standard errors adjusted for clustering at the individual level.

3.4 Results

3.4.1 Baseline Results

To shed more light on the dynamics over the entire 10-year observation period, we use a graphical representation of our estimation results. Specifically, we plot the coefficients of the interaction between the years to disability onset and the treatment dummy with reference year t-2.[36] Figure 3.4.1 shows the event-study results for the employment indicators. As we restrict our sample to employed individuals in the years prior to potential disability onset, the pretrends for employment status are set to zero by construction. The results imply that the probability of being employed drops by 10.3 percentage points one year after disability onset relative to that of the control group. Thereafter, the effect remains at this level for one year, possibly because some individuals return to employment after the expiration of sick pay or transitional or unemployment benefits (see Section 3.2.2). In year three after disability onset, the employment rate decreases again, resulting in an effect of -16.3 percentage points after five years. Our findings are comparable to the effect of -9 percentage points in the year of disability onset identified by Polidano & Vu (2015). Lechner & Vazquez-Alvarez (2011) identify an effect of -9.3 percentage points in the second year after disability onset, which is also quite close to our estimated effect in t+2 (-10.4 percentage points).[37]

Furthermore, the results for employment days show that the days in employment do not diverge between the treatment and control groups until two years before our measured date of disability onset. Within the two years before disability onset, the number of days in employment decreases substantially by 25 days per year relative to t-2. Days in employment continue to fall after disability onset; then, we again observe a plateau between t+1 and t+2 up to a total decline of 59 days per year in t+5.

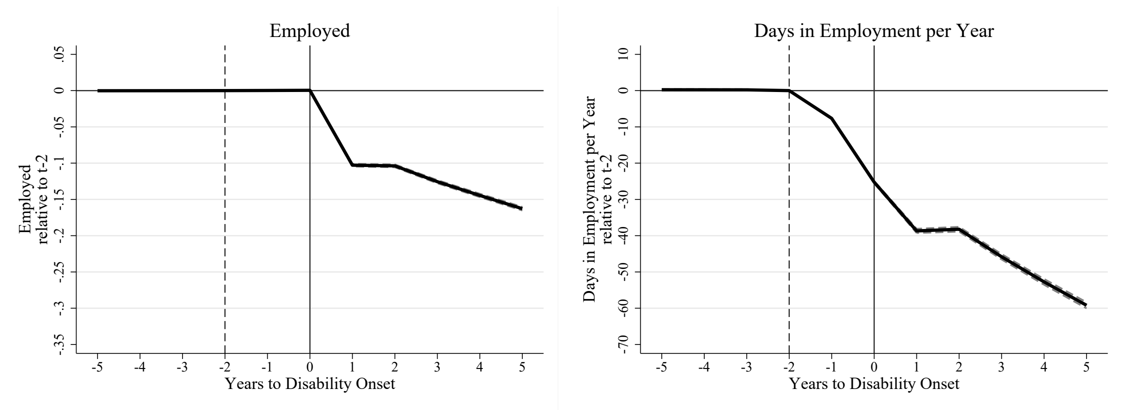
Figure 3.4.2 illustrates the event-study results for the earning variables. Again, the pretrends do not diverge between control and treated individuals. Annual labor earnings decrease slightly until disability onset before decreasing substantially until five years after onset. In the fifth year after disability onset, disabled workers experience an overall reduction in ihs-transformed annual labor earnings of -1.710 (approximately -41 percent-

[36] The development of the outcome variables of the treatment and control groups can be found in Figure 3.B.1 in the appendix.

[37] Note that our sample selection differs somewhat from that in the studies of Polidano & Vu (2015) and Lechner & Vazquez-Alvarez (2011). Specifically, we condition on employment five years prior to onset and observe only individuals employed at the time of (acquisition of) disability status (t). Thus, the individuals in our sample are probably more closely attached to the labor market. However, as discussed in Section 3.4.4, the sample restrictions tied to employment do not seem to strongly affect the selectivity of our sample.

age points in annual earnings)[38] relative to those of the control group. Note that for the estimation of this outcome variable, both individuals who stay in the labor market and those who leave the labor market are included. Furthermore, disabled workers who stay in the labor force are found to experience drops in daily wages that predate the date of disability onset. Daily wages recover one year after disability onset before they decrease again, resulting in an effect of -0.072 log-points (approximately -7 percentage points, see footnote [38]) in t+5. The decrease in earnings and wages is in line with the findings of the study by Charles (2003) of substantial long-term earnings losses among disabled men and stands in contrast to those of the study by Lechner & Vazquez-Alvarez (2011), who find only a small, if any, reduction in earnings among those who remain employed after disability onset.

Figure 3.4.1: Main Effects: Employment



Notes: The figure shows the effects of disability on employment (estimates of coefficient δ_k in equation (3.1)) with 95 percent confidence intervals after propensity score matching as described in Section 3.3.3. Number of treated (matched control) observations in t-2: 148,660 (624,439). The employment indicator is measured at the reference date June 30th.
Source: BsbM and IEB, years of disability onset: 2005–2013, own calculations.

3.4.2 Channels

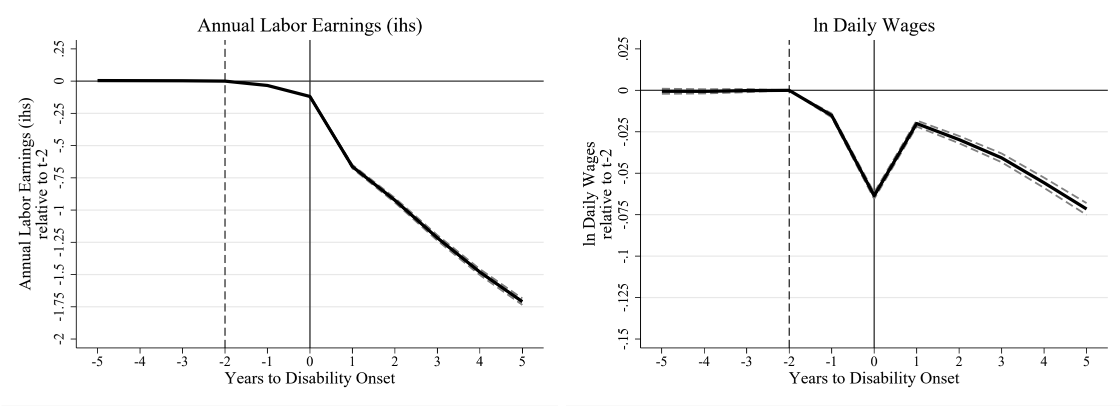
Channels for Employment Outcomes

In what follows, we aim to dig deeper into potential mechanisms that can explain our main results. To explore the mechanisms for the employment outcomes, we first focus on nonemployment status and the yearly number of nonemployment days.[39] The results

38 The estimated effect on annual earnings is calculated as: $(exp(\delta_5+\gamma_5)-1)*100-(exp(\gamma_5)-1)*100$ with δ_5, γ_5 from equation (3.1).

39 We define an individual to be unemployed as soon as he or she receives any kind of benefit receipt. We define an individual as nonemployed when there is no entry in the social security record. This

Figure 3.4.2: Main Effects: Earnings



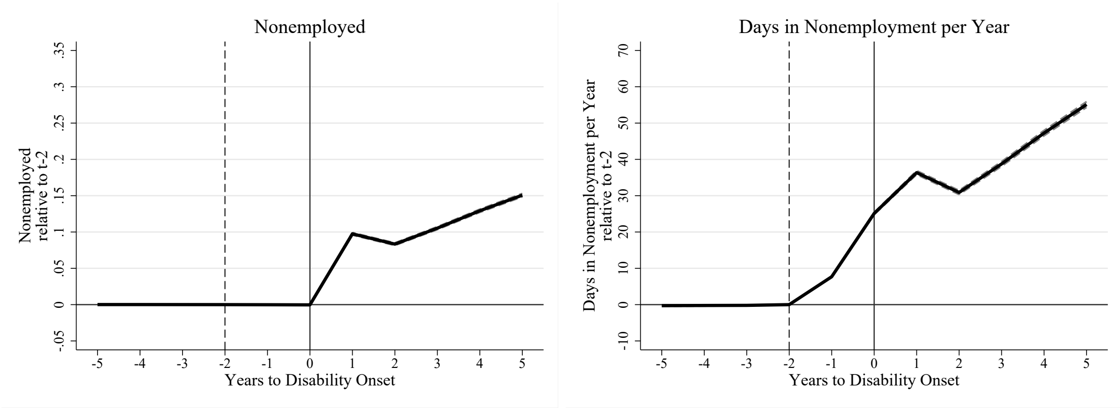
Notes: The figure shows the effects of disability on earnings (estimates of coefficient δ_k in equation (3.1)) with 95 percent confidence intervals after propensity score matching as described in Section 3.3.3. Number of treated (matched control) observations in t-2: 148,660 (624,439). Annual labor earnings are defined as the product of employment days and daily wages and are measured by an inverse hyperbolic sine (ihs) transformation. Earnings and wages are deflated to 2015 prices.
Source: BsbM and IEB, years of disability onset: 2005–2013, own calculations.

shown in Figure 3.4.3 illustrate that nonemployment is an important driver of the decline in employment: one year after disability onset, the probability of being nonemployed increases by 10 percentage points and the days in nonemployment by 36 days per year in comparison to those of the control group. The effect decreases slightly in t+2 for both the nonemployment rate and the days in nonemployment per year. This suggests that the expiration of temporary social benefits and the regaining of earning capacity favors a return to employment. However, this may be only temporary, as individuals may become ill again or transition after some time to early retirement. After five years, the effects on nonemployment amount to 15 percentage points and 55 days, respectively. In contrast, the effect on unemployment status is quite small. Compared to the outcome for the control group, days in unemployment increase slightly after the onset of disability but fall again in t+5 (see Figure 3.B.3 in the appendix). Although the individuals in our sample should be entitled to unemployment benefits, Section 3.2.2 suggests that the lower replacement rate, the limited replacement duration and the job search requirements make receipt of unemployment benefits less attractive than receipt of social benefits under other schemes in Germany. The small effect of disability onset on unemployment has also been documented by other studies (see, e.g., Lechner & Vazquez-Alvarez 2011).

In the next step, we dig deeper into the transitions into nonemployment by exploiting the information on individuals' reasons for being out of the labor force. In the employ-

means that we fill in gaps between administrative entries and periods after permanent exit from the labor market with nonemployment days.

Figure 3.4.3: Channel: Nonemployment



Notes: The figure shows the effects of disability on nonemployment (estimates of coefficient δ_k in equation (3.1)) with 95 percent confidence intervals after propensity score matching as described in Section 3.3.3. Number of treated (matched control) observations in t-2: 148,660 (624,439). The nonemployment indicator is measured at the reference date June 30th.
Source: BsbM and IEB, years of disability onset: 2005–2013, own calculations.

ment notifications, employers deregister employees when they leave the labor force and indicate a reason for the deregistration (*Abmeldegrund*). The main reasons for permanently leaving the labor force in our sample include the end of employment, receipt of replacement benefits and death.[40] The reason "end of employment" describes the regular end of an employment relationship (e.g., due to the expiration of a fixed-term contract, dismissal by the employer or employment termination by the employee).[41] A deregistration with the listed reason of "receipt of replacement benefit" means that an employee is now entitled to compensation by the statutory health insurance provider. In Germany, the statutory health insurance scheme provides compensation for (1) maternity leave (at least six weeks before and eight weeks after childbirth) and (2) long-term illness (see Section 3.2.2).[42] Finally, a deregistration with the reason "death" describes the death of an

40 Note that we use the information on deregistration only when an individual permanently leaves the labor force. Specifically, we create a dummy for each of the three reasons that take *1* only when the reason is indicated in the last observable employment spell. For individuals who do not leave the labor force permanently (i.e., for whom we observe subsequent employment or unemployment spells) and for individuals for whom the reason is not indicated, the dummy takes *0*. Thus, the reason for (subsequent) nonemployment can already be reported in the spell containing t. The share of disabled individuals who permanently leave the labor force after disability onset is 6.2 percent in our sample.

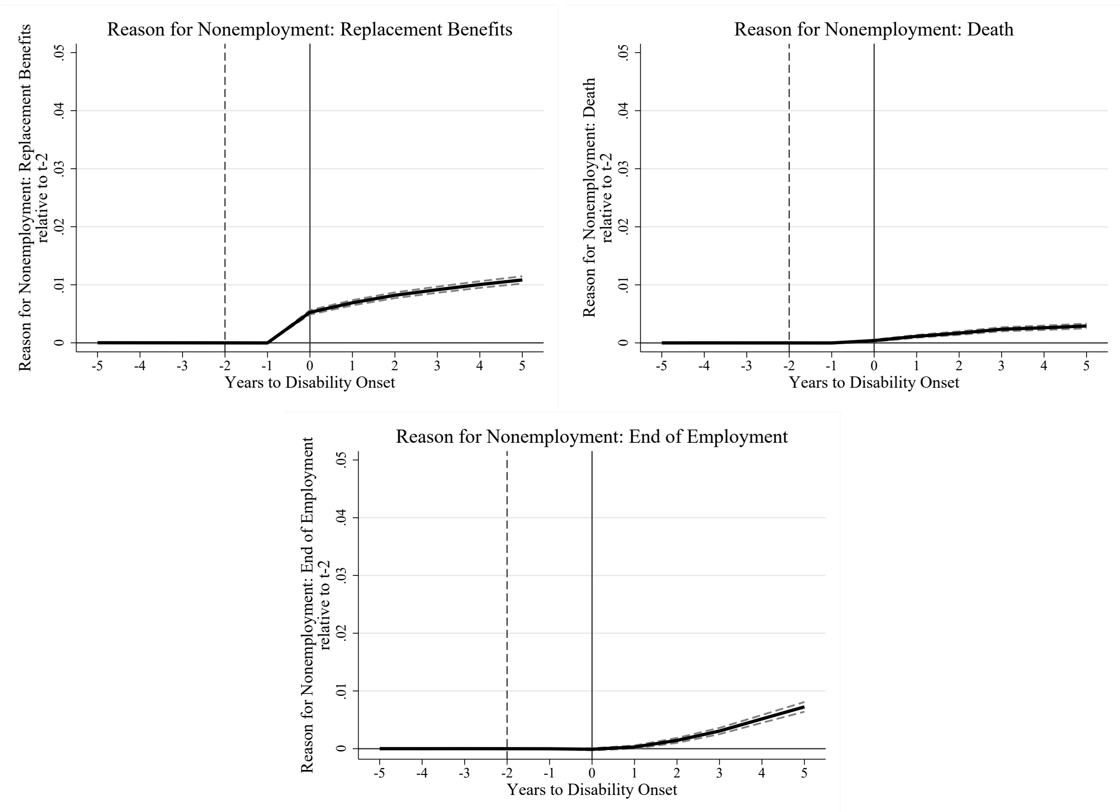
41 A worker who switches to a full reduced earnings capacity pension or an old-age pension (see Section 3.2.2) would also probably be deregistered with this reason listed (or with the reason "other"). In principle, the "end of employment" reason for deregistration could also include transitions to self-employment. At an older age, however, efforts to become self-employed typically decrease (Wasserman 2012), which should especially be the case in the group of severely disabled persons.

42 Note that this reason for deregistration only includes replacement benefits provided by the statutory health insurance provider (Müller et al. 2022). Transitional benefits by the pension insurance scheme (see Section 3.2.2) presumably correspond to the category "other deregistration reason".

employee.

Figure 3.4.4 shows the event-study results for the three main reasons listed for permanent nonemployment. All three reasons play a significant role after the onset of disability and serve to explain the mechanisms behind the increase in permanent nonemployment. In the fifth year after disability onset, the probability of deregistration with the reason "receipt of replacement benefits" increases by 1.1 percentage points, with the reason "end of employment" by 0.7 percentage points and with the reason "death" by 0.3 percentage points relative to that of the control group. Concerning magnitudes, replacement benefits seem to play the largest role (especially in the year directly after disability onset), whereas death is a comparably minor reason.

Figure 3.4.4: Channel: Reasons for Nonemployment



Notes: The figure shows the effects of disability on reasons for deregistration (permanent nonemployment) (estimates of coefficient δ_k in equation (3.1)) with 95 percent confidence intervals after propensity score matching as described in Section 3.3.3. Number of treated (matched control) observations in t-2: 148,660 (624,439).
Source: BsbM and IEB, years of disability onset: 2005–2013, own calculations.

Channels for Earnings Outcomes

As shown in the right graph of Figure 3.4.2, workers experience drops in daily wages that predate the measured date of disability onset. However, daily wages seem to rapidly recover within the first year after onset. One possible explanation for this kink could be positive selection since we observe daily wages only for individuals who stay in the labor market after disability onset. In fact, the results from a logit regression show that among disabled workers, mainly younger, well-educated and high-earning men in larger and better-paying firms remain in the workforce (see Table 3.B.1 in the appendix). However, when we restrict the sample to individuals employed in t+1 and perform the matching procedure for this sample, the kink is still present (although somewhat less pronounced; see Figure 3.B.2 in the appendix), indicating that compositional differences alone cannot explain the pattern of daily wages.

Another explanation could be the dynamics of working time around the onset of disability. As pointed out by Charles (2003), the drop in annual earnings for disabled men is caused mainly by a reduction in working hours. Further, the regulations relating to the partial reduction in earnings capacity pension (see Section 3.2.2) may provide an incentive to work part-time after disability onset. Thus, to explore the mechanisms for the earnings outcomes, we first analyze whether disability affects part-time employment.[43] The upper left graph in Figure 3.4.5 shows that for those who stay in employment after disability onset, part-time work plays an important role: in year two after disability onset, the probability of working part-time increases by 2.5 percentage points in comparison to that of the control group. The effect amounts to 4.7 percentage points in year five, which corresponds to an increase in part-time work of 27 percent of the sample mean.[44] This result is also in line with the findings by Polidano & Vu (2015) of a high prevalence of part-time employment after disability onset. Unfortunately, the administrative records do not include information on hours worked. Instead, we can only differentiate between part-time and full-time employment as described above. This restriction prevents us from examining in detail the dynamics of working time around the onset of disability. However, as supported by the findings of Charles (2003), dynamics in working hours seem to play an important role in both the initial decline and the subsequent recovery of earnings.

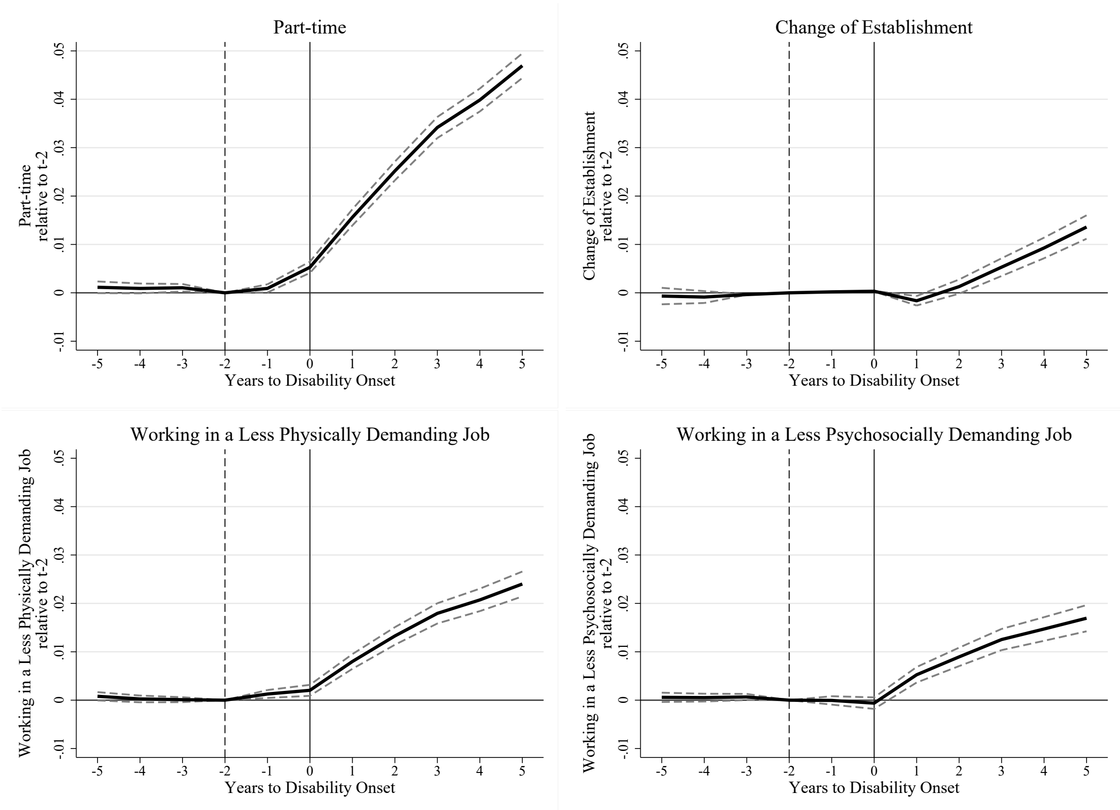
A further reason for the drop in earnings may be establishment or occupational changes. Individuals who experience a severe health shock may not be able to work in their former occupation and/or establishment. Some jobs, for example, those with physically demanding tasks, may be difficult for individuals to return to after disability onset. Some employers may not be willing or able to provide workplaces equipped to meet the special

[43] In the data, "part-time" indicates that the contractual working hours are less than the usual working hours in the establishment.

[44] The share of part-time employment in t-2 in the whole sample is 17.3 percent.

needs of disabled workers. The loss of occupation- and/or firm-specific human capital may explain the drop in daily wages. The upper right graph in Figure 3.4.5 shows that an individual's probability of changing employers does not change significantly up to two years after disability onset but increases by 1.4 percentage points after five years in comparison to that of the control group. The establishments to which individuals move are, on average, less productive than the initial establishments in which they become disabled (see Table 3.B.2 in the appendix).

Figure 3.4.5: Channel: Part-time, Establishment and Occupational Changes



Notes: The figure shows the effects of disability on part-time employment, establishment changes and changes to a less physically or psychosocially demanding job (estimates of coefficient δ_k in equation (3.1)) with 95 percent confidence intervals after propensity score matching as described in Section 3.3.3. Number of treated (matched control) observations in t-2: 148,660 (624,439). For switches to a less physically (psychosocially) demanding job, we use the Kroll index, which provides information on the extent of physical (psychosocial) demands in an occupation (Kroll 2011).
Source: BsbM and IEB, years of disability onset: 2005–2013, own calculations.

Furthermore, we analyze whether workers have a higher propensity to work in less demanding jobs after disability onset. For this, we use an index of physical and psychosocial job demands that can be merged with the occupations in our data (Kroll 2011).[45] The index describes the extent of the demandingness of each occupation on a scale ranging from

[45] For the merge, we use the 3-digit level of the German Classification of Occupations 2010.

1 (less demanding) to 10 (highly demanding). The basis for the index is a representative survey of employees, namely, the BIBB/BAuA Employment Survey, which asks about a broad range of work-related demands. Physical demands include, for example, frequent carrying of heavy loads, working in forced postures or working with noise. Psychosocial demands include, for example, high time pressure, frequent interruptions, working overtime or having no support from colleagues. The lower left (right) graph in Figure 3.4.5 shows the effects of disability on the probability of working in a less physically (psychosocially) demanding job.[46] It illustrates that horizontal occupational switches toward less physically (psychosocially) demanding occupations play some role directly after disability onset and become more frequent over time. Five years after disability onset, the probability of switching to a less physically (psychosocially) demanding job increases by 2.4 (1.7) percentage points relative to the probability in the control group.[47] We also find some evidence for vertical occupational switches.[48] While for job "upgrading" no relevant effects emerge, we find some evidence for job "downgrading": In the fifth year after disability onset, the probability of switching to a job with a lower requirement level increases by 1.6 percentage points in comparison to that of the control group (see Figure 3.B.4 in the appendix).[49]

In sum, our channel analysis shows that dynamics in working time – although insufficiently observable and approximated here by a part-time indicator – seem to play an important role in explaining the wage pattern after disability onset. We also observe switches toward less demanding jobs, both in terms of physical and psychosocial dimensions and in terms of formal requirement levels, while establishment switches play only a minor role.

3.4.3 Effect Heterogeneity

In this section, we analyze heterogeneity in the effects. To do so, we differentiate between age and skill groups and between two levels of disability (i.e., individuals with a degree

[46] We define a transition to a less physically (psychosocially) demanding job with a dummy indicating that the index of physical (psychosocial) work demands in the new occupation is lower than the index in the occupation in t-2.

[47] Note that switching to less physically and less psychosocially demanding jobs is highly correlated: 67.7 percent of individuals in our sample switching to a less physically demanding job until t+5 are also switching to a less psychosocially demanding job.

[48] We define a vertical occupational change as an upward or downward change at the 5-digit level of the German Classification of Occupations 2010. The fifth digit of this level describes the job requirement level of an occupation with categories unskilled/semiskilled, specialist, complex specialist and highly complex (Paulus & Matthes 2013).

[49] Vertical and horizontal job switches are also strongly correlated: 24.5 percent (23.5 percent) of those who "downgrade" until t+5 also switch to a less physically (psychosocially) demanding job.

of disability between 30 and less than 50 and severely disabled individuals with a degree of disability of at least 50).

Severely Disabled Individuals
As shown in the upper right graph of Figure 3.B.5 in the appendix, the effect on employment days is particularly pronounced for individuals with a degree of disability of at least 50. Five years after disability onset, those with a degree of disability of at least 50 work, on average, 65 fewer employment days per year than control individuals. In contrast, those with a degree of disability between 30 and less than 50 work only 35 fewer employment days.[50] The same pattern arises for the effect on annual labor earnings (see Figure 3.B.6 in the appendix). This finding is in line with previous studies that document larger effects on employment and earnings among individuals with more severe or chronic disabilities (see, e.g., Charles 2003, Jones et al. 2018, Lechner & Vazquez-Alvarez 2011).

Older Individuals
The lower left graphs of Figures 3.B.5 and 3.B.6 in the appendix suggest that the employment and earning effects are also more pronounced among those in the oldest age cohort, i.e., individuals aged 50–55 at the time of matching. This finding is in line with that of the study by Charles (2003) but stands in contrast to the results of Jenkins & Rigg (2004) and Polidano & Vu (2015), who find the employment impacts of disability to be most pronounced during prime age. Jenkins & Rigg (2004) and Polidano & Vu (2015) argue that disability in advanced age may be due more to a slow deterioration in health rather than to a sharp health shock. Thus, when disability onset occurs at the end of a slow deterioration in health, many labor market adjustments, such as plans for early retirement, may already be made prior to onset. In our study, we restrict our sample to individuals closely attached to the labor market prior to onset, i.e., those employed five years before disability onset. As a consequence, we probably observe sharper health shocks, as individuals experiencing a slow deterioration in health and prior labor market adjustments are not in our sample. Thus, being older at onset and suffering a sharp and probably unforeseen health shock cause larger losses from disability.

Low-Skilled Individuals
Last, as shown in the lower right graphs of Figures 3.B.5 and 3.B.6 in the appendix, low-skilled workers who become disabled experience larger employment and earning effects than the effects found for the baseline sample. Low-skilled workers show 69 fewer

[50] The results are not shown but are available on request.

employment days per year five years after disability onset than the workers in the control group. Among the high-skilled, this decline amounts to only 50 days.[51] Again, this finding is in line with the results from previous studies that consistently document larger effects among the low-skilled after a health shock (see, e.g., Charles 2003, Heinesen & Kolodziejczyk 2013, Polidano & Vu 2015, Jones & McVicar 2020). Low-skilled workers may have a higher risk of working in physically demanding jobs. As most disabilities are due to physical illness, it may be harder for the low skilled to return to these physically demanding jobs.[52] Further, as spelled out by Polidano & Vu (2015), for individuals with no vocational training who leave or lose their jobs, a lack of credentials can make it more difficult to find suitable alternative employment.[53]

Further Heterogeneities

In terms of gender, we do not identify any substantial differences between men and women (see Figures 3.B.7 and 3.B.8 in the appendix), consistent with the findings of Jenkins & Rigg (2004) and Polidano & Vu (2015). Women seem to have a slightly more pronounced decline in employment days from t-2 to t+1, but the overall decline of 59 employment days per year until t+5 is the same as for men. Moreover, the importance of part-time employment as a channel for earnings loss seems to be somewhat more pronounced for women.[54]

Last, we analyze whether it makes a difference whether individuals are employed in firms that (do not) meet the employment quota for disabled workers. On the one hand, firms that do not meet the quota at t-2 may be more inclined to retain a worker who becomes severely disabled because he or she contributes to meeting the quota. On the other hand, firms that do not meet the quota may have less employee-friendly (and, in particular, less disability-friendly) structures, which may lead to disabled workers being

[51] The results are not shown but are available on request.

[52] To check this channel, we analyze whether the employment and earnings effects are more pronounced for individuals working in physically demanding jobs. To do so, we again use the physical work exposure index from Kroll (2011) to identify physically demanding jobs. Although the effects are somewhat more pronounced among workers in these jobs, the differences are not substantial (the results are not shown but are available on request).

[53] To analyze this mechanism, we test whether establishment changes are less relevant for the low skilled. In fact, we cannot identify significant change-of-establishment effects among the low skilled (the results are not shown but are available on request). Note, however, that due to the relatively small sample of individuals with no vocational training, the confidence intervals are quite large. The point estimates do not differ substantially from those estimated for the whole sample (see the upper right graph in Figure 3.4.5).

[54] Please note that the true effect heterogeneities by gender may be masked by the fact that the approximation of an individual's past sickness history by nonemployment spells (see Section 3.3.3) may be insufficient for women, since their nonemployment histories also often include maternity and childrearing periods.

more likely to leave these firms. However, we do not find heterogeneity in the effects with regard to this aspect. If anything, the effects on employment and earnings are slightly more pronounced among individuals working in firms that do not meet the quota.[55]

3.4.4 Testing for Robustness and Selectivity

Using AKM Effects

To check the robustness of our results and the selectivity of our sample of disabled individuals, we make use of AKM effects. AKM person and establishment fixed effects stem from a wage decomposition pioneered by Abowd et al. (1999) and can serve as a proxy for establishment and employee productivity (Bellmann et al. 2020).[56] First, we perform a robustness check by including the pre-disability onset AKM effects for the 1998–2004 period as matching variables instead of the median establishment wage in t-2 and the individual daily wages in t-3, t-4 and t-5. The results are very similar to our baseline results, as shown in Figure 3.B.9 in the appendix.[57]

Second, we compare the AKM effects in samples with different restrictions: the raw sample (i.e., only individuals for whom we observe a disability onset), a sample not restricted to employment in t-5 and t-4 and the AKM-matched treated and control sample described above. We use this analysis to obtain an understanding of potential selectivity in the groups. The results displayed in Table 3.4.1 show that the differences in productivity between the samples are not pronounced (the standardized differences are below 0.1). These findings suggest that our sample restrictions tied to employment do not seem to lead to relevant positive selection. In Section 3.4.5, we discuss the issue of selectivity again using survey data.

Table 3.4.1: Productivity: Raw Sample, Unrestricted Sample, Treated, Control

	Raw Sample	Treated Unrest.	Treated	Control	Stand. Differences		
	(1)	(2)	(3)	(4)	(3)-(4)	(3)-(2)	(3)-(1)
Person Fixed Effect	4.469	4.474	4.486	4.483	0.010	0.039	0.052
Number of Observations	404,672	158,606	128,994	542,906			

Notes: The table displays productivity indicators measured by person AKM fixed effects (1998–2004) for four samples: “Raw Sample” is the raw sample (including only individuals experiencing disability onset and excluding individuals with multiple changes from nondisabled to disabled); “Treated Unrest.” is the prepared sample of disabled individuals without the 5-year pre-employment restriction (but with, e.g., restrictions on age or valid values for relevant variables). “Treated” (“Control”) is the sample of disabled (nondisabled) observations from robustness check R2 (see Tables 3.B.3 and 3.B.4), which uses AKM individual fixed and establishment fixed effects (1998–2004) and daily wages in t-2 as matching variables. In (3) and (4), the sample is restricted to individuals employed in t-5, t-4, t-3, t-2 and t-1. The construction of the AKM effects is explained in Table 3.A.1 in the appendix.
Source: BsbM and IEB, own calculations.

[55] The results are not shown but are available on request.

[56] The construction of the AKM effects is explained in Table 3.A.1 in the appendix.

[57] Note that the preonset AKM effects are not available for all individuals in our baseline sample. Thus, the sample is reduced to 128,994 disabled individuals. The effects for all four main outcomes are also shown in the second row (R1) in Tables 3.B.3 and 3.B.4.

Further Sensitivity Checks
To further check whether our results are sensitive to the choice of matching algorithm, the choice of control variables included in the propensity score function or the sample restrictions, we perform several robustness checks, the results of which are displayed in Tables 3.B.3 and 3.B.4 in the appendix. First, we use a 1-nearest-neighbor matching algorithm. The point estimates are almost identical to those from the main specification with a 5-nearest-neighbor matching algorithm. Second, to reduce the impact of extreme outliers in our matching procedure, we drop the top one percent of matching weights. Third, we do not include pre-event wages (i.e., ln daily wages in t-2, t-3, t-4 and t-5) as control variables in the propensity score function. Fourth, in contrast to how we construct our baseline sample, which is restricted to individuals permanently employed five years before disability onset, we restrict the sample to individuals employed only three years before onset (i.e., in t-3, t-2 and t-1). Fifth, we exclude the years 2005, 2008 and 2009 to ensure that our results are not driven by economic crises.[58] In sum, the results of all the robustness checks are very similar to those of the baseline model.

Finally, we randomly assign 100,000 of the observations in our control group to a placebo treatment group by randomly selecting one spell of the control observations and treating them as if disability onset happened during this spell. We then use this setup to repeat our matching procedure and estimate the coefficient δ_k of event-study equation (3.1). The results of the placebo estimations consistently show zero effects for all outcomes (see Figure 3.B.10 in the appendix).

3.4.5 Descriptive Insights from the PASS-Survey

Thus far, our results have shown that disability is accompanied by a severe and persistent deterioration of labor market outcomes over time. However, the administrative data tell us little about the selectivity of our sample restrictions with regards to the sample of eventually disabled individuals that we use in the analysis, the types of disabilities that people experience or the actual relationship between disability onset and health deterioration.

To examine these issues, we use survey data from the Panel Study Labour Market and Social Security (PASS) administered by the IAB. The PASS is a yearly panel study that has been collected since 2006. In our analysis, we use wave 15, which covers interviews up until 2021.[59] The sample consists of a sample of long-term unemployment benefit recipients and a general population survey and contains information on approximately

58 The year 2005 saw very high unemployment rates in Germany. In addition, a substantial labor market reform (the Hartz reform) was introduced. In 2008 and 2009, the global financial crisis prevailed.

59 See doi: 10.5164/IAB.PASS-SUF0621.de.en.v1

10,000 households per year. The PASS has been widely used in social science research with a special focus on topics related to health (Trappmann et al. 2019). It contains information on self-reported disability status and types of disability. Thus, it is well suited for the analysis. However, it is important to consider that the definitions of disability differ slightly from those in our main analyses, as we do not know whether the respondents in the PASS notified their employer of their disability.

In the first step, we investigate how our criterion that workers be employed five consecutive years prior to disability onset affects the selection of individuals in our sample. Due to the limited number of observations with self-reported disabilities in the PASS, we cannot exactly mimic the conditions that we apply to the administrative data, but we can nevertheless approximate the criteria. We create three definitions for counting individuals as newly disabled in our data: (1) that they are disabled now and were nondisabled one year ago, (2) that they are disabled and employed now and were nondisabled and employed one year ago, and (3) that they are disabled and employed now and were nondisabled one year ago and employed over the last three years (the definition that comes closest to the administrative records). These definitions map to the corresponding column numbers in Table 3.4.2. Applying more restrictive definitions does not alter most of the average sample characteristics shown in Panel (a) of the table by status in meaningful ways. The mean age of disabled individuals is always around 47 at the onset of disability, around 50 percent are female, and the average years of schooling are around 11.5. However, whether children are present in the household varies between 45 and 56 percent, and the gross daily wages two years prior to onset increase under the more restrictive definitions. The difference is most pronounced between the first group and the other two groups. However, this is expected, as conditioning on prior employment status leads to a sample that is more attached to the labor market by construction.

In the next step, we investigate the types of disabilities that disabled individuals experience. Using the data, we can investigate the specific handicaps that individuals face. Here, we construct three groups of limitations: (1) physical limitations, such as missing limbs or damaged organs, (2) impaired hearing or vision and (3) psychological impairments. Furthermore, PASS surveys the disability degree. The descriptive statistics are displayed in Panel (b) of Table 3.4.2. The average disability degree is 49 for all newly disabled individuals (column (1)) and 45 conditional on prior employment (columns (2) and (3)). Concerning the types of limitations that the respondents face, physical disabilities are the most common, with around 87 percent of individuals experiencing them. These are followed by psychological disabilities, which around one-third of individuals report. Finally, around 16 percent of individuals report impaired vision or hearing. Note

Table 3.4.2: Descriptives of Disabled Individuals from PASS

	(1)		(2)		(3)	
	Mean	SD	Mean	SD	Mean	SD
(a) Socioeconomic Characteristics						
Age	47.16	6.65	47.02	6.75	47.05	6.79
Female	0.48	0.50	0.48	0.50	0.49	0.50
(ln) Daily Wages in t-2 (admin)	3.43	1.18	3.72	1.08	3.76	1.08
Years of Schooling	11.31	2.41	11.45	2.43	11.56	2.51
Child in Household (0/1)	0.45	0.50	0.52	0.50	0.56	0.50
(b) Disability						
Disability Degree (30-100)	48.88	20.19	45.36	18.05	45.11	18.01
Physical Disability	0.87	0.34	0.86	0.34	0.87	0.34
Vision/Hearing Disability	0.17	0.38	0.16	0.36	0.16	0.36
Psychological Disability	0.38	0.48	0.31	0.46	0.30	0.46
Number of Observations	1,198		353		281	

Notes: All aged 30–55; for disabled individuals: minimum disability degree of 30. Physical disabilities include organ damages and cancer. Analysis samples: column (1): disabled and nondisabled one year ago; column (2): the column (1) restrictions plus currently employed and employed one year ago; column (3): the column (2) restrictions plus employed during the last three years. Numbers of cases for the measurement of ln(Daily Wages) in t-2 are available only for a subset of respondents, by column: (1) 379, (2) 224, (3) 211.
Source: PASS0621v1 merged with administrative data, own calculations.

that the disabilities are not mutually exclusive: the reported shares add up to above 140 percent in all columns, indicating that a substantial share of individuals experiences multiple limitations.[60] Nevertheless, the types of disabilities and disability degree remain largely stable across all three columns. We thus conclude that, consistent with our conclusions from the selectivity analysis with administrative records (see Section 3.4.4), the selection criteria that we apply in the main analysis do not seem to lead to a highly specific sample of eventually disabled individuals with regard to socioeconomic characteristics or types of disability. However, the sample seems to be slightly more attached to the labor market than all newly disabled employees, which is unsurprising given the restriction on employment.

As PASS also contains data on health outcomes, we can investigate whether the onset of disability is associated with a deterioration in one's health. To this end, we use individual information on three outcomes: (1) self-assessed subjective health on a scale from 1 (poor) to 5 (very good), (2) individual health satisfaction on a scale from 0 to 10 and (3) the number of days or nights spent at the hospital during the last 12 months. Table 3.C.1 in the appendix shows the averages of these variables for the group of disabled individuals closest to our definition in the administrative data (being disabled and employed now and employed for the three years prior) over the five to three years before the onset of disability (the time frame prior to matching) and from the onset up to five years afterwards. All outcomes deteriorate over time: general health decreases, health satisfaction

[60] These numbers differ from the numbers reported by Federal Statistical Office (2022) in Section 3.2.1, as PASS asks about multiple limitations while the Federal Statistical Office asks about the main limitation.

decreases and days spent at the hospital increase after the onset of disability. This clearly shows that disability is associated with a severe health shock and that it is not the case that individuals who already reported lower health measures are only now claiming disability status.

Furthermore, we use the PASS data to investigate whether periods of nonemployment indeed capture sickness periods. To this end, we analyze whether an increase in time spent in nonemployment calculated from the administrative data correlates with worsening health outcomes in PASS. In this analysis, we simply generate a binary indicator variable for an increase in nonemployment days and regress it on increases in days spent in the hospital and decreases in health satisfaction and self-rated health. In Appendix Table 3.C.2, we show the results from regressions for employed disabled individuals and the full sample of working individuals. For employed individuals with a disability, a worsening in health satisfaction from t-1 to t is associated with a 3.1 percentage point increase in the probability of experiencing an increase in nonemployment duration. The same holds true for a decrease in subjective health, while an increase in hospital days makes it 21.5 percentage points more likely to observe an increase in nonemployment days. The coefficients obtained from these estimations are comparable when we use the full sample of employed individuals. Thus, our analysis provides evidence that days in nonemployment correlate with worsening health and could thus reflect health issues.

3.5 Summary and Conclusion

Demographic change and the associated decline in the working-age population represent an increasing challenge in industrialized countries. In this context, the onset of a severe disability, for instance, due to an age-related chronic illness, can accelerate an early exit from working life and thereby the shortage of skilled workers. Our study shows that the onset of a disability strongly affects labor market performance as employment and annual labor earnings decline significantly. One important mechanism is transitions to nonemployment after disability onset: the probability of becoming nonemployed increases by 10 percentage points after one year and by 15 percentage points after five years in comparison to the probabilities in the control group. This mirrors a general picture present in all of our results: the consequences of disability are long lasting and do not reverse over time. The negative labor market effects of disability onset are more pronounced for severely disabled, older and low-skilled individuals.

For those individuals who stay in employment, a significant share reduce their working time. Individuals are also more likely to switch to less demanding jobs, both in terms of physical and psychosocial dimensions and in terms of formal requirement levels, while establishment changes play only a minor role. These findings indicate that part-time models and other forms of job adjustment are used but a significant proportion of people also drop out of the workforce altogether. However, our data do not allow us to analyze the extent to which disability pensions compensate for the loss of income.

Our findings deliver important insights into the discussion of incentives to transition early to disability pensions or other forms of social benefits and to exit the labor market permanently. Once individuals receive replacement benefits and have been out of the labor force for quite a while, barriers might appear too high to return to work. In line with this, we document that disability has virtually no impact on unemployment benefit receipt and thus on contact with the Federal Employment Agency and related support systems. Moreover, a recent report of the OECD documents that the impact of employment-oriented programs is limited in OECD countries (OECD 2022). The authors conclude that employment-oriented efforts are coming too late as persons applying for disability benefits have typically been out of the labor force for a long time or have gone through repeated phases of employment interruptions. However, as compared to other developed countries, Germany has very low recipiency rates of government provided disability insurance benefits. Relatively restrictive coverage and eligibility conditions, a quota system for employing disabled workers and the large medical rehabilitation market might be reasons for that (see, e.g., Burkhauser et al. 2016, McVicar et al. 2016).[61] Nevertheless, reintegration efforts should be examined as fast as possible, for instance, during the period of sick leave. Furthermore, support services, clear responsibilities and low bureaucratic hurdles might be ways to facilitate the reintegration for individuals with disabilities. Successful reintegration also depends on the extent to which the required occupational tasks could still be performed or whether a professional reorientation is necessary. Therefore, further training measures, adult learning programs and career guidance should be designed such that they are accessible to disabled workers and specifically adapted to their needs.

[61] Further efforts in Germany to take into account the high importance of vocational integration in medical rehabilitation are, e.g., through work-related services in the form of diagnosis, therapy and training offers (the MBOR program of the German pension insurance scheme).

3.6 Appendix

3.A.1 Appendix A: AKM Fixed Effects

Table 3.A.1: Person and Establishment Fixed Effects ("AKM Effects")

AKM Effects

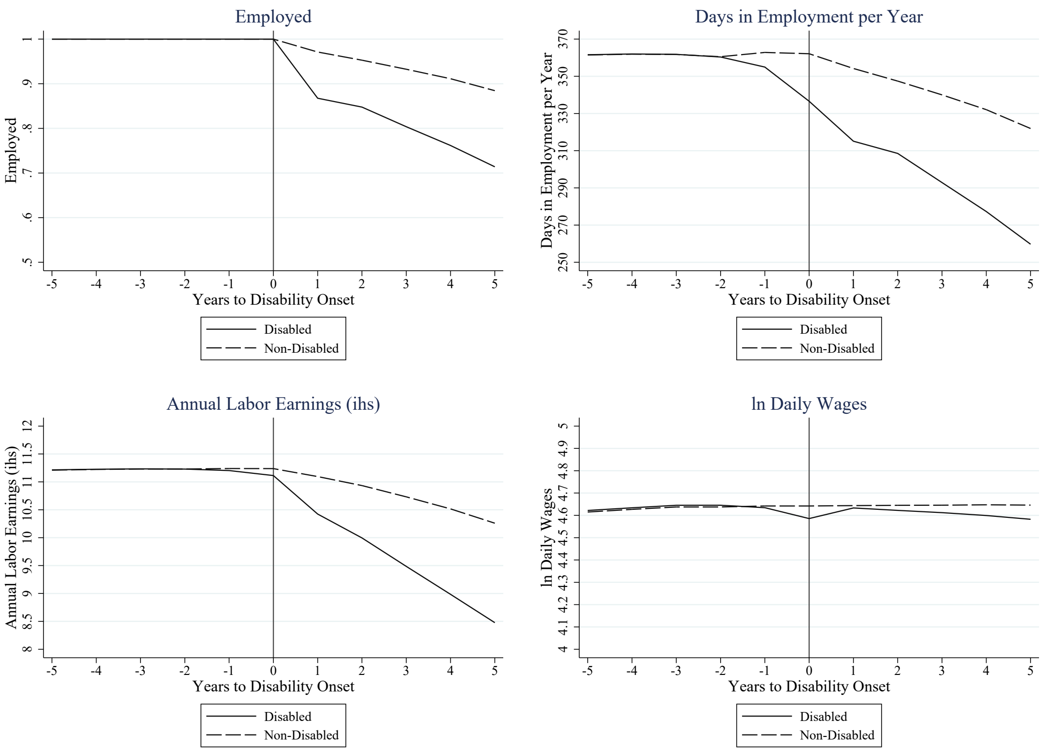
AKM individual and establishment fixed effects stem from a wage decomposition pioneered by Abowd et al. (1999), implemented for Germany by Card et al. (2013), and updated by Bellmann et al. (2020). These effects are derived from the following wage model:

$$log(wage_{it}) = \alpha_i + \Psi_{J(i,t)} + x'_{it}\beta + \epsilon_{it},$$

where the log daily wages for worker i are the sum of a time-invariant person effect α_i, a time-invariant establishment effect $\Psi_{J(i,t)}$ for the establishment at which worker i is employed at time t, and time-varying worker characteristics $x'_{it}\beta$, which affect all workers' wages equally at all establishments, and an error component ϵ_{it}, which is assumed to be independent of the right-hand-side variables. The estimates for the individual effect α_i capture time-invariant individual characteristics that are rewarded equally across employers. Likewise, the index $x'_{it}\beta$ is interpreted as measuring the time-varying worker characteristics that affect the productivity of worker i in all jobs. In x_{it}, an unrestricted set of year dummies and of quadratic and cubic terms in age fully interacted with education is included. Last, the establishment effect $\Psi_{J(i,t)}$ is interpreted as a proxy for establishment productivity, as this effect represents the proportional pay premium (or discount) paid by establishment j to all individuals (i.e., all those with $J(i,t) = j$) (Bellmann et al. 2020, p. 7).

3.B.2 Appendix B: Further Analyses

Figure 3.B.1: Descriptives: Employment and Earnings



Notes: The figure shows the trends of employment and earnings for the disabled and the nondisabled sample five years before and after disability onset. Earnings and wages are deflated to 2015 prices. Number of treated (control) observations (in t): 148,660 (624,439).
Source: BsbM and IEB, years of disability onset: 2005–2013, own calculations.

Table 3.B.1: Logit Regression: Employment Status in t+1

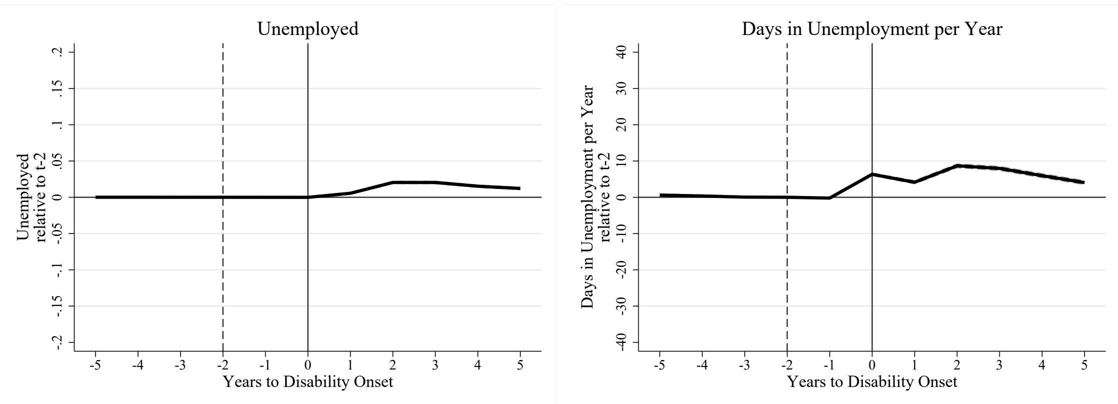
	Coefficient	S.E.
Male	0.187***	(0.021)
Age (Reference: 50–55 Years)		
30–34 Years	0.329***	(0.049)
35–39 Years	0.314***	(0.033)
40–44 Years	0.196***	(0.024)
45–49 Years	0.073***	(0.019)
Foreign	-0.002	(0.030)
Qualification (Reference: High-Skilled)		
Low-Skilled	-0.126***	(0.042)
Medium-Skilled	-0.103***	(0.031)
Occupation (Reference: Cleaning Services)		
Agriculture, Forestry, Horticulture	-0.006	(0.112)
Manufacturing	0.014	(0.061)
Production Technology	0.126**	(0.061)
Building and Interior Construction	-0.023	(0.068)
Food, Gastronomy, Tourism	-0.027	(0.070)
Medical and Nonmedical Healthcare	-0.139**	(0.060)
Social Sector and Cultural Work	-0.104	(0.066)
Commerce and Trade	-0.000	(0.068)
Business Management and Organization	0.115**	(0.058)
Business-Related Services	-0.010	(0.070)
IT Sector and Natural Sciences	0.066	(0.070)
Safety and Security	0.144*	(0.081)
Traffic and Logistics	-0.036	(0.062)
Job Requirement Level (Reference: Highly Complex)		
Unskilled/Semiskilled	-0.078	(0.052)
Specialist	-0.075**	(0.037)
Complex Specialist	-0.014	(0.044)
Industry (Reference: Public Sector (w/o PA))		
Agrarian/Fishery	-0.015	(0.147)
Energy Mining	0.054	(0.073)
Manufacturing	-0.067	(0.048)
Construction	-0.259***	(0.064)
Wholesale	-0.109*	(0.053)
Traffic/Communication	-0.048	(0.059)
Banking/Insurance	-0.113**	(0.064)
Other Services	-0.050	(0.053)
Public Administration (PA)	0.032	(0.047)
Location: East Germany	-0.038	(0.026)
Number of Employees in Firm (Reference: 1000+ Employees)		
20–49 Employees	-0.099***	(0.031)
50–99 Employees	-0.180***	(0.028)
100–199 Employees	-0.183***	(0.027)
200–499 Employees	-0.105***	(0.024)
500–999 Employees	-0.084***	(0.026)
Median Wages in Establishment	0.002***	(0.000)
Cum. Duration in Employment (Months)	0.000***	(0.000)
Cum. Duration in Nonemployment (Months)	0.001***	(0.000)
Cum. Duration in Establishment (Months)	0.000	(0.000)
Number of Nonemployment Spells	-0.018***	(0.003)
(ln) Annual Earnings in t-2	0.148***	(0.023)
Constant	0.165	(0.243)

Notes: This table displays the results of a logit regression for being employed in t+1 as outcome variable (only disabled individuals). n=148,660 disabled individuals. Standard errors in parentheses. Significance levels:* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
Source: BsbM and IEB, years of disability onset: 2005–2013, own calculations.

Figure 3.B.2: Earnings (Sample: Employed in t+1)

Notes: The figure shows the effects of disability on ln daily wages (estimates of coefficient δ_k in equation (3.1)) after propensity score matching as described in Section 3.3.3 for the sample of individuals employed in t+1 with 95 percent confidence intervals. Number of treated (matched control) observations in t-2: 128,973 (552,076).
Source: BsbM and IEB, years of disability onset: 2005–2013, own calculations.

Figure 3.B.3: Channel: Unemployment



Notes: The figure shows the effects of disability on unemployment (estimates of coefficient δ_k in equation (3.1)) with 95 percent confidence intervals after propensity score matching as described in Section 3.3.3. Number of treated (matched control) observations in t-2: 148,660 (624,439). The unemployment indicator is measured at the reference date June 30th.
Source: BsbM and IEB, years of disability onset: 2005–2013, own calculations.

Table 3.B.2: Characteristics of Establishments after Establishment Change

	Disability Estab.	New Estab.	Difference	SE
Median Daily Wages	104.545	102.578	-1.967***	0.223
AKM Establishment Fixed Effect 1998–2004	0.149	0.062	-0.087***	0.002
AKM Establishment Fixed Effect 2003–2010	-0.172	-0.264	-0.092***	0.002
Number of Observations	19,974	20,959		

Notes: This table describes characteristics of establishments in which an individual becomes disabled ("Disability Estab.") and to which a disabled individual moves after disability onset ("New Estab."). Significance level: *** $p < 0.01$. The construction of the AKM effects is explained in Table 3.A.1 in the appendix.
Source: BsbM and IEB, years of disability onset: 2005–2013, own calculations.

Table 3.B.3: Robustness Checks – Employment Outcomes

			Outcome: Employed				
Year to Disability Onset	-1	0	1	2	3	4	5
Baseline			-0.103***	-0.104***	-0.126***	-0.145***	-0.163***
			(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
R1: AKM Effects			-0.100***	-0.103***	-0.125***	-0.144***	-0.162***
			(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
R2: 1 Nearest Neighbor			-0.102***	-0.103***	-0.125***	-0.144***	-0.162***
			(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
R3: Drop Top 1%			-0.104***	-0.106***	-0.128***	-0.147***	-0.165***
Matching Weights			(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
R4: No Pre-event Wages			-0.103***	-0.105***	-0.126***	-0.145***	-0.163***
			(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
R5: Employment in t-3			-0.099***	-0.101***	-0.121***	-0.139***	-0.155***
Onward			(0.002)	(0.002)	(0.003)	(0.003)	(0.003)
R6: No Crisis Years			-0.102***	-0.101***	-0.123***	-0.141***	-0.159***
			(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
			Outcome: Days in Employment per Year				
Year to Disability Onset	-1	0	1	2	3	4	5
Baseline	-7.66***	-25.23***	-38.68***	-38.22***	-45.82***	-52.74***	-59.20***
	(0.098)	(0.156)	(0.293)	(0.331)	(0.372)	(0.402)	(0.425)
R1	-7.53***	-24.87***	-37.81***	-37.94***	-45.61***	-52.50***	-59.03***
	(0.104)	(0.167)	(0.313)	(0.355)	(0.397)	(0.431)	(0.455)
R2	-7.64***	-25.25***	-38.51***	-38.06***	-45.57***	-52.46***	-59.03***
	(0.106)	(0.164)	(0.313)	(0.361)	(0.412)	(0.450)	(0.480)
R3	-7.22***	-24.76***	-38.54***	-38.28***	-45.94***	-52.90***	-59.30***
	(0.095)	(0.154)	(0.290)	(0.326)	(0.367)	(0.396)	(0.418)
R4	-7.45***	-25.04***	-38.52***	-38.37***	-45.92***	-52.70***	-59.20***
	(0.097)	(0.156)	(0.293)	(0.330)	(0.371)	(0.402)	(0.424)
R5	-7.51***	-24.97***	-37.25***	-36.77***	-44.17***	-50.65***	-56.47***
	(0.167)	(0.254)	(0.544)	(0.659)	(0.839)	(0.946)	(1.084)
R6	-7.66***	-25.84***	-38.66***	-37.25***	-44.69***	-51.42***	-57.78***
	(0.124)	(0.199)	(0.366)	(0.410)	(0.461)	(0.498)	(0.529)

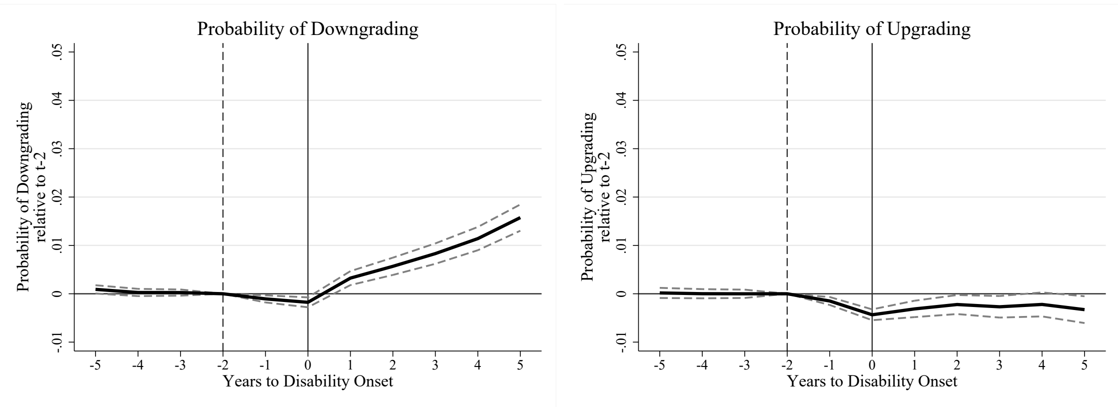
Notes: This table displays the results of the baseline model and five robustness checks. R1 uses AKM person fixed and establishment fixed effects (1998–2004) as matching variables instead of the wage variables (n=128,994 disabled individuals); R2 uses a 1-nearest-neighbor matching (without replacement) algorithm; R3 drops the top 1 percent of matching weights; R4 does not include pre-event wages in the propensity score function; R5 restricts the sample to individuals employed in t-3, t-2 and t-1 (the baseline sample is restricted to individuals employed in t-5, t-4, t-3, t-2 and t-1) (n=149,701 disabled individuals); R6 excludes disability onsets in the crisis years 2005, 2008 and 2009 (n=94,250 disabled individuals). Standard errors in parentheses (clustered at the individual level). Significance level: *** $p < 0.01$.
Source: BsbM and IEB, years of disability onset: 2005–2013, own calculations.

Table 3.B.4: Robustness Checks – Earning Outcomes

	Outcome: Annual Labor Earnings (ihs)						
Year to Disability Onset	-1	0	1	2	3	4	5
Baseline	-0.034***	-0.119***	-0.663***	-0.924***	-1.215***	-1.481***	-1.710***
	(0.001)	(0.001)	(0.007)	(0.009)	(0.011)	(0.012)	(0.013)
R1: AKM Effects	-0.034***	-0.118***	-0.655***	-0.921***	-1.215***	-1.479***	-1.710***
	(0.001)	(0.001)	(0.008)	(0.010)	(0.011)	(0.013)	(0.014)
R2: 1 Nearest Neighbor	-0.033***	-0.119***	-0.660***	-0.919***	-1.206***	-1.474***	-1.704***
	(0.001)	(0.001)	(0.008)	(0.010)	(0.012)	(0.013)	(0.014)
R3: Drop Top 1%	-0.032***	-0.118***	-0.670***	-0.936***	-1.230***	-1.496***	-1.727***
Matching Weights	(0.001)	(0.001)	(0.007)	(0.009)	(0.011)	(0.012)	(0.013)
R4: No Pre-event Wages	-0.034***	-0.120***	-0.665***	-0.931***	-1.225***	-1.489***	-1.718***
	(0.001)	(0.001)	(0.008)	(0.011)	(0.013)	(0.015)	(0.016)
R5: Employment in t-3	-0.032***	-0.117***	-0.636***	-0.888***	-1.177***	-1.439***	-1.652***
Onward	(0.001)	(0.002)	(0.013)	(0.019)	(0.025)	(0.029)	(0.033)
R6: No Crisis Years	-0.034***	-0.122***	-0.659***	-0.898***	-1.184***	-1.446***	-1.669***
	(0.001)	(0.001)	(0.009)	(0.011)	(0.013)	(0.015)	(0.016)
	Outcome: ln Daily Wages						
Year to Disability Onset	-1	0	1	2	3	4	5
Baseline	-0.015***	-0.064***	-0.020***	-0.030***	-0.041***	-0.056***	-0.072***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)
R1	-0.015***	-0.065***	-0.019***	-0.028***	-0.039***	-0.054***	-0.071***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)
R2	-0.015***	-0.064***	-0.020***	-0.029***	-0.040***	-0.054***	-0.070***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)
R3	-0.015***	-0.063***	-0.020***	-0.029***	-0.041***	-0.055***	-0.071***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)
R4	-0.014***	-0.064***	-0.021***	-0.031***	-0.043***	-0.058***	-0.076***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)
R5	-0.013***	-0.062***	-0.016***	-0.024***	-0.035***	-0.046***	-0.062***
	(0.001)	(0.001)	(0.002)	(0.002)	(0.003)	(0.003)	(0.004)
R6	-0.016***	-0.065***	-0.019***	-0.028***	-0.038***	-0.055***	-0.068***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)

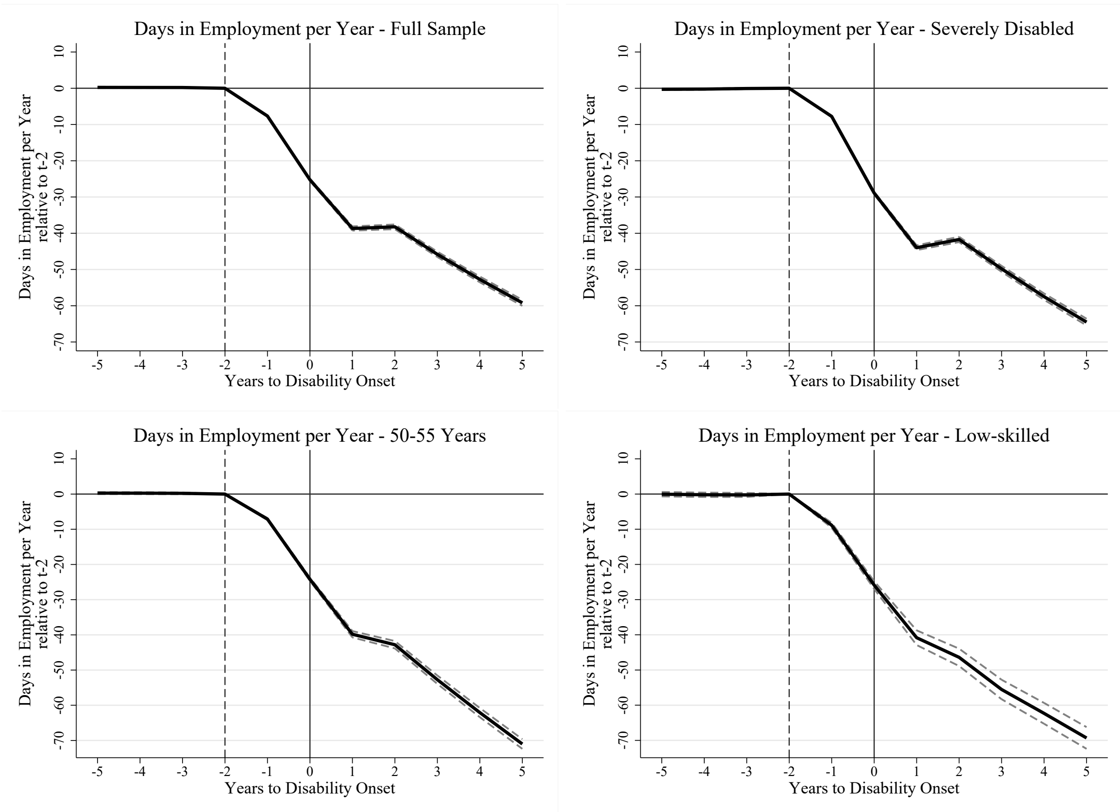
Notes: This table displays the results of the baseline model and five robustness checks. R1 uses AKM person fixed and establishment fixed effects (1998–2004) as matching variables instead of the wage variables (n=128,994 disabled individuals); R2 uses a 1-nearest-neighbor matching (without replacement) algorithm; R3 drops the top 1 percent of matching weights; R4 does not include pre-event wages in the propensity score function; R5 restricts the sample to individuals employed in t-3, t-2 and t-1 (the baseline sample is restricted to individuals employed in t-5, t-4, t-3, t-2 and t-1) (n=149,701 disabled individuals); R6 excludes disability onsets in the crisis years 2005, 2008 and 2009 (n=94,250 disabled individuals). Standard errors in parentheses (clustered at the individual level). Significance level: *** $p < 0.01$.
Source: BsbM and IEB, years of disability onset: 2005–2013, own calculations.

Figure 3.B.4: Channel: Vertical Occupational Changes



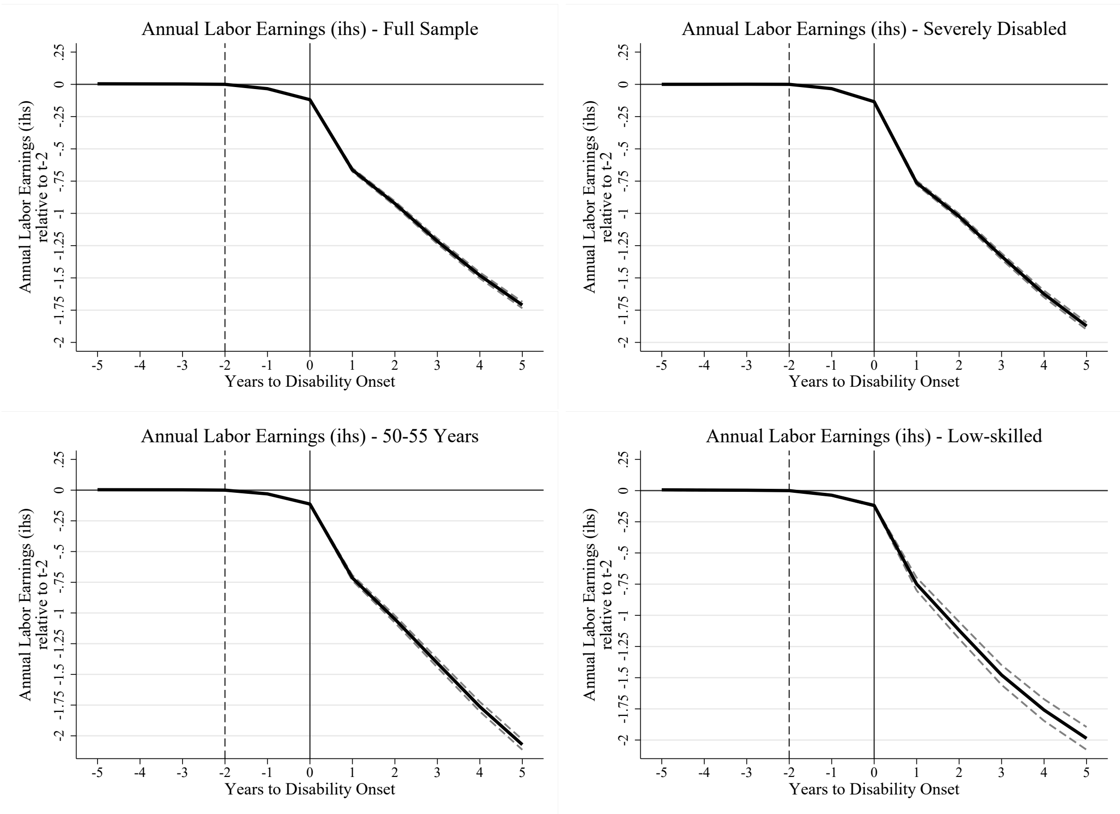
Notes: The figure shows the effects of disability on job downgrading and upgrading (estimates of coefficient δ_k in equation (3.1)) with 95 percent confidence intervals after propensity score matching as described in Section 3.3.3. We define a vertical occupational change as an upward or downward change in the 5-digit level of the German Classification of Occupations 2010. The 5-digit level describes the job requirement level of an occupation with the categories unskilled/semiskilled, specialist, complex specialist and highly complex (Paulus & Matthes 2013). Number of treated (matched control) observations in t-2: 148,660 (624,439).
Source: BsbM and IEB, years of disability onset: 2005–2013, own calculations.

Figure 3.B.5: Heterogenous Effects (Outcome: Days in Employment per Year)



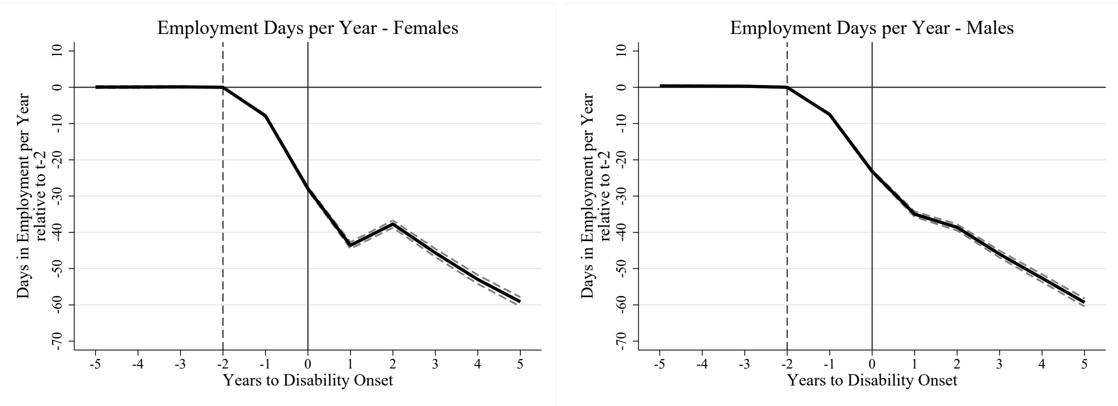
Notes: The figure shows the effects of disability on days in employment (estimates of coefficient δ_k in equation (3.1)) with 95 percent confidence intervals after propensity score matching as described in Section 3.3.3 for different subgroups. Number of disabled individuals: Baseline: 148,660, severely disabled: 121,165 (81.50%), 50–55 years old: 66,387 (44.66%), low-skilled: 12,728 (8.56%).
Source: BsbM and IEB, years of disability onset: 2005–2013, own calculations.

Figure 3.B.6: Heterogenous Effects (Outcome: Annual Earnings)



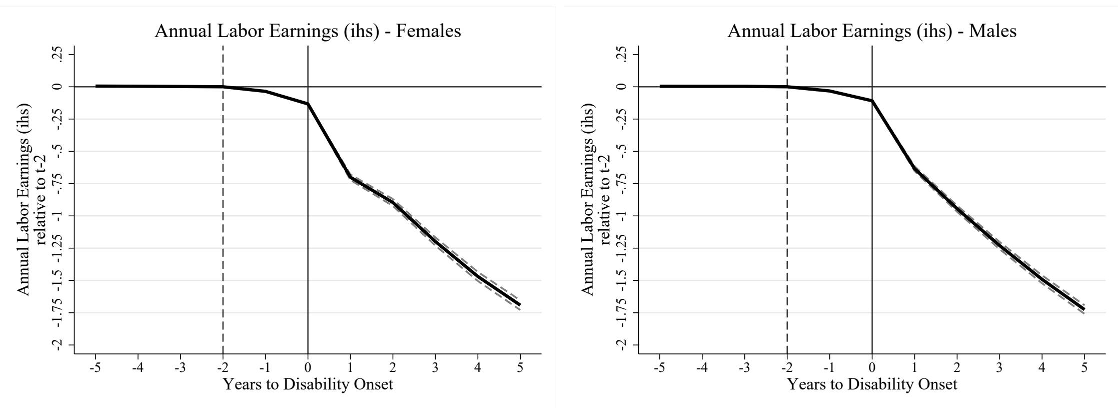
Notes: The figure shows the effects of disability on IHS-transformed annual earnings (estimates of coefficient δ_k in equation (3.1)) with 95 percent confidence intervals after propensity score matching as described in Section 3.3.3 for different subgroups. Number of disabled individuals: Baseline: 148,660, severely disabled: 121,165 (81.50%), 50–55 years old: 66,387 (44.66%), low-skilled: 12,728 (8.56%). Earnings are deflated to 2015 prices.
Source: BsbM and IEB, years of disability onset: 2005–2013, own calculations.

Figure 3.B.7: Effects on Days in Employment by Gender



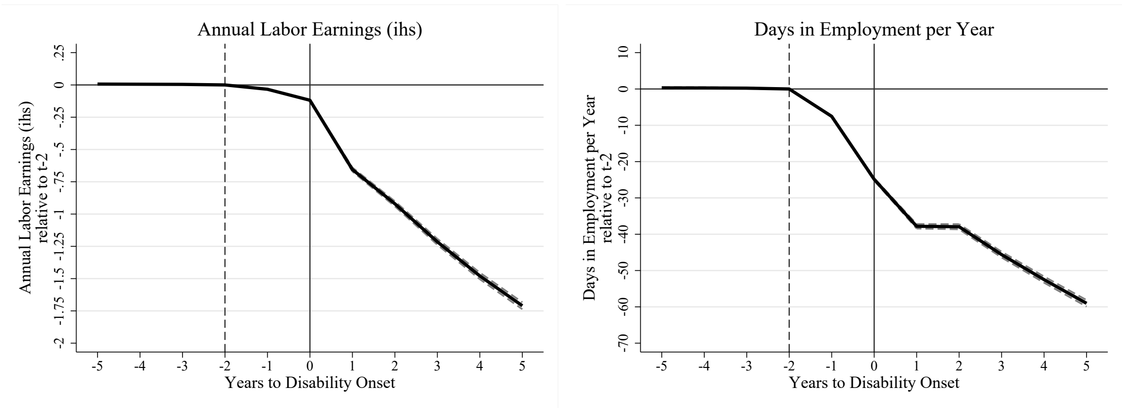
Notes: The figure shows the effects of disability on days in employment (estimates of coefficient δ_k in equation (3.1)) after propensity score matching as described in Section 3.3.3 with 95 percent confidence intervals by gender. Number of disabled females (males) in t-2: 63,878 (84,782).
Source: BsbM and IEB, years of disability onset: 2005–2013, own calculations.

Figure 3.B.8: Effects on Annual Earnings by Gender



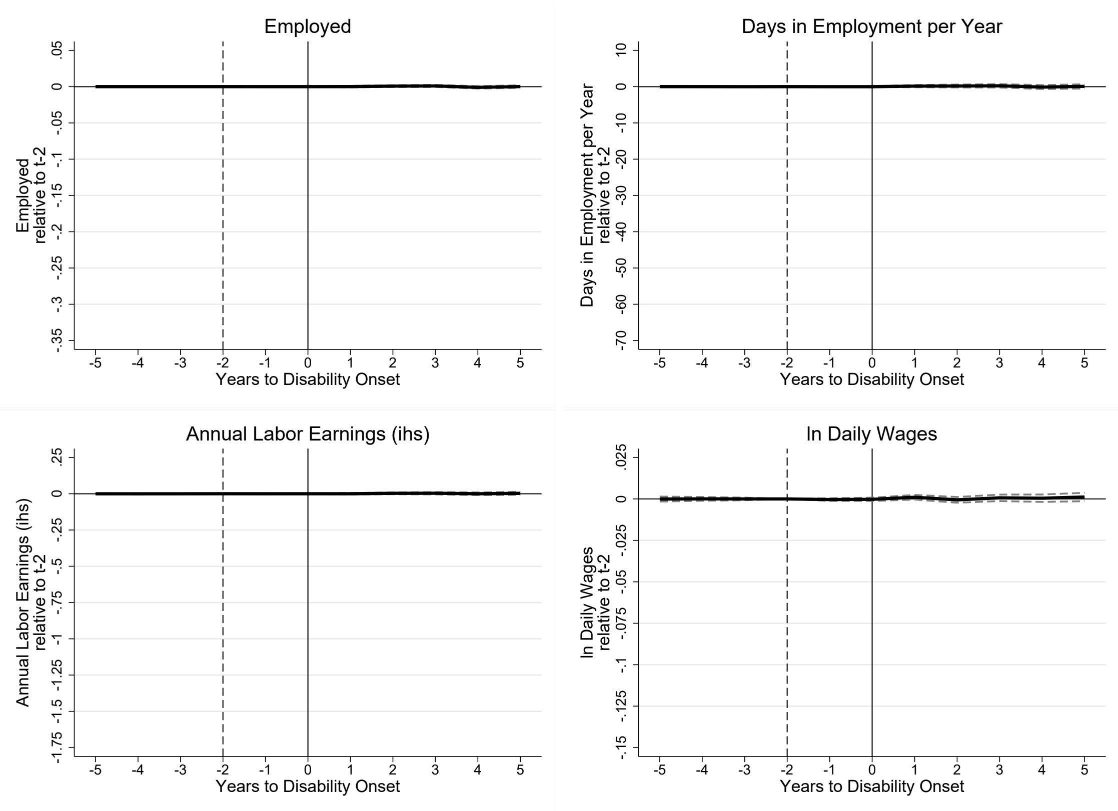
Notes: The figure shows the effects of disability on Iihs-transformed annual earnings (estimates of coefficient δ_k in equation (3.1)) with 95 percent confidence intervals after propensity score matching as described in Section 3.3.3 by gender. Number of disabled females (males) in t-2: 63,878 (84,782). Earnings are deflated to 2015 prices.
Source: BsbM and IEB, years of disability onset: 2005–2013, own calculations.

Figure 3.B.9: Robustness Checks: AKM Effects



Notes: The figure shows effects of disability on IHS-transformed annual earnings and on days in employment per year (estimates of coefficient δ_k in equation (3.1)) with 95 percent confidence intervals after propensity score matching. This specification uses AKM individual fixed and establishment fixed effects (1998–2004) as matching variables instead of the wage variables, including only the daily wages in t-2 (n=128,994 disabled individuals). The effects are also shown in the second row (R1) in Tables 3.B.3 and 3.B.4.
Source: BsbM and IEB, years of disability onset: 2005–2013, own calculations.

Figure 3.B.10: Placebo Estimations



Notes: The figure shows placebo effects of disability on employment and earning outcomes (estimates of coefficient δ_k in equation (3.1)) with 95 percent confidence intervals. We randomly assign 100,000 observations in our control group to a placebo treatment group and perform propensity score matching as described in Section 3.3.3. Number of placebo treated (controls) in t-2: 99,003 (479,620).
Source: BsbM and IEB, years of disability onset: 2005–2013, own calculations.

3.C.3 Appendix C: Further Descriptives Based on PASS

Table 3.C.1: Health Outcomes before and after the Onset of Disability

	(1) 5–2 Years before Onset	(2) Onset and up to 5 Years after
Avg. General Health (1–5)	3.13	2.69
Avg. Health Satisfaction (0–10)	6.39	5.49
Avg. Days/Nights Spent at the Hospital	0.73	2.61
Observations	594	324

Notes: All aged 30–55, for disabled individuals according to column (3) of Table 3.4.2.
Source: PASS0621v1 merged with administrative data, own calculations.

Table 3.C.2: Correlation between Nonemployment Duration and Worsening of Health Outcomes

	(1) Decrease in Health Satisfaction	(2) Increase in Hospital Days	(3) Decrease in Subjective Health
Currently Disabled & Employed (N=4,461)	0.031***	0.215***	0.036***
	(0.010)	(0.021)	(0.012)
All currently Employed (N=49,897)	0.023***	0.199***	0.017***
	(0.003)	(0.008)	(0.003)

Notes: This table displays the results of a regression of a binary indicator variable for observing an increase in nonemployment duration from t-1 to t on the worsening of the respective health indicator between t-1 and t. The sample of disabled and employed individuals contains all individuals who are disabled according to PASS and who are employed in the administrative records. The estimation controls for age, age squared, years of schooling, gender and presence of children in the household. Standard errors in parentheses. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
Source: PASS0621v1 merged with administrative data, own calculations.

Overall Conclusion and Outlook

For Germany, one of the most crucial challenges in the next years will be to overcome the shortage of skilled workers in an ageing society. One possible measure is to promote activation and retention of health-impaired individuals. Thus, understanding relevant institutions is key to balance social protection and employment promotion for health-impaired or disabled individuals.

This doctoral thesis delivers valuable insights to better understand the effects of labor market institutions as a form of working conditions. To do so, I discuss the role of dismissal protection and disability policies in Germany.

Germany is an interesting case in this context for two essential reasons. First, it is a country strongly affected by demographic change. Second, it is characterized by quite strict employment protection and regulations concerning the promotion of sick or disabled workers in comparison to other OECD countries (OECD 2010, 2020). At the same time, Germany is characterized by a fairly generous benefit system, which kicks in after a health shock.

In the following, I discuss the key insights of my dissertation, possible policy implications and starting points for future research. Specifically, I draw conclusions on the labor market institutions dismissal protection, disability policies and threshold regulations in German labor law. Finally, I discuss the relevance of administrative data in this context.

Dismissal Protection

Chapter 1 (co-authored by Nicole Gürtzgen) shows that workers' sickness behavior is influenced by changes in dismissal protection. Specifically, workers exhibit fewer long-term sickness spells during their second year after being hired when they are subject to weaker dismissal protection. In contrast, establishments seem not to react significantly to changes in employment protection with regard to changes in their workforce composition. Furthermore, the probability of involuntary unemployment after sickness seems not to increase with weaker protection. In sum, our results indicate that weaker dismissal protection affects the incidence of long-term sickness spells. The effect is particularly pronounced among medium-skilled men. With regard to the institution of sick pay, Ziebarth (2013) provides evidence for similar effects of a weaker institutional protection: For middle-aged full-time employed people and those in the bottom part of the earnings distribution cuts in long-term sick pay significantly reduces the length of long-term sickness spells. In sum, it seems that especially those who have a high dependency on full salary, e.g., because they are the main earner in the household, adapt their sickness behavior to changes in institutional protection.

To deduce policy implications, it would be essential to dig deeper into the mechanisms and to explore what type of sickness behavior caused the decline in long-term sickness after weakening dismissal protection: Do our results reflect a decline in absenteeism (staying away from home without being sick) or an increase in presenteeism (attending work despite being sick)? We tried to address this issue by a complementary analysis using survey data. However, the complementary analysis provides no clear evidence about which mechanism is more relevant and leads us to conclude that neither mechanism can be excluded as an explanation. While there is a lot of literature on absenteeism (e.g., Riphahn 1999, Riphahn & Thalmaier 2001, Chatterji & Tilley 2002, Ichino & Riphahn 2005, Frick & Malo 2008), presenteeism has only recently received increased attention and research on this phenomenon is still limited. One reason may be that presenteeism, and in particular long-term presenteeism, is difficult to measure as it is often not explicitly asked for in surveys. However, the rare existing evidence suggests that presenteeism is a relevant and widely spread issue (Hirsch et al. 2017, Dietrich & Hiesinger 2020). In light of the Covid 19-pandemic and the increased possibilities to work from home, it is likely that presenteeism has become even more relevant in the last years (Steidelmüller et al. 2020). Given that both types of sickness behavior impose high costs for workers and firms, further research is needed on absenteeism and presenteeism. Ideally, institutions such as dismissal protection or sick pay should be designed in a way that they minimize both types of sickness behavior.

Disability Policies

Corncering the German disabled worker quota, I provide evidence that the quota does promote the employment of disabled workers (Chapter 2).[62] This is relevant, in particular

[62] In general, this result is in line with results for female or racial employment quotas which also find

for German policy, as no studies explicitly addressed the intended consequences of the German disabled worker quota so far.[63] The issue is especially of high policy relevance as the coalition partners agreed on a remarkably higher noncompliance fine for firms that do not employ *any* individual with a severe disability in the last coalition agreement (2021-2025). This reform is scheduled to take effect in 2024 (Bundesministerium der Justiz 2023). As the fine promotes the employment of disabled workers, the reform will probably further increase employment prospects for individuals with disabilities. My study also shows, however, that the fine has unintended consequences, which could be harmful to overall employment, as it may incentivize firms to stay below the threshold and substitute away from regular employed workers. This firm behavior, also described as bunching, is particularly pronounced among those firms which face the highest costs at the threshold. Thus, with an increasing fine for noncomplying firms, the reform will probably also promote the firms' bunching behavior.

Furthermore, many noncomplying employers claim that they cannot fulfill the quota because there are too few (suitable) applicants (Hiesinger & Kubis 2022). In 2021, 308,441 more individuals with severe disabilities would have had to be employed for all firms to meet the quota. In contrast, only 172,000 disabled individuals were registered unemployed (Statistik der Bundesagentur für Arbeit 2021). Thus, even if it were possible to bring all unemployed disabled individuals into employment, many firms would still not be able to meet the quota. However, as my co-authors Matthias Collischon and Laura Pohlan and me show in Chapter 3, the typical adjustment path after a disability onset is not a transition into unemployment, but into nonemployment. As disability onsets virtually have no impact on unemployment benefit receipt, the impact of the contact with the Federal Employment Agency and related activation programs is limited. As a consequence, a main policy challenge is to bring *non*employed disabled individuals back into employment. Four aspects may be relevant in this context: First, in order to prevent a very long persistence in nonemployment, reintegration efforts should be examined as fast as possible, for instance already during the period of sickness absence. Clear responsibilities and support services such as work-related programs in medical rehabilitation can help to accelerate reintegration. Second, a substantial part of individuals who stay in the labor market reduces working time after disability onset. This suggests that flexi-

a positive effect of the quota on the employment of the underrepresented group of workers (McCrary 2007, Gopal 2022).

[63] The German Bundestag mentions the absence of scientific research with regard to effects of the German quota in a 2019 paper (Deutscher Bundestag 2019).

ble working arrangements, concerning both the working hours as well as the workplace, could promote the (re-)integration of disabled workers (see also OECD 2022). Third, as we find evidence for horizontal and vertical occupational switches after a disability onset, occupational reorientation seems to be an important adjustment mechanism. Thus, measures to better increase occupational flexibility such as further training measures as well as career counseling and guidance, particularly designed for workers after a severe health shock, could help to create better employment perspectives. Fourth, incentives to take up employment within the disability pension should be increased. Gaining access to reduced earning capacity pensions is often a bureaucratically complex and lengthy process. When returning to the labor market, workers with disabilities often lose their access to pensions – which constitutes a major risk if their return is not successful.

In sum, there is a notable absence of research on individuals with disabilities in nonemployment. In particular, discouragement effects of nonemployed individuals with disabilities have not yet been thoroughly analyzed. Discouragement may arise from a long absence from the labor market, from the loss of occupation- or firm-specific human capital, discrimination and stigmatization in the labor market or poor labor market conditions (e.g., during recessions). Thus, further research should focus on disabled individuals in nonemployment and how to encourage them to return to the labor market as they represent an important source for overcoming the shortage of skilled workers.

Threshold Regulations in German Labor Law

The first two chapters of this thesis provide new and possible contradicting evidence on how employers react to (changes in) threshold regulations. While Chapter 1 suggests that it is less the establishments which react to changes of the threshold for dismissal protection, Chapter 2 provides extensive evidence for firms bunching below the threshold of the disabled worker quota. This result is in line with evidence from France, where firms react to the 50-employee threshold – a threshold where many labor laws start to bind on firms (Garicano et al. 2016). The different types of threshold costs that employers face might be one possible explanation for this discrepancy: The threshold regulation in dismissal protection involves rather abstract and difficult-to-measure costs. It is, for example, not unlikely that it is difficult for firms to assess how many of the employees and which ones become long-term sick and how much it costs to keep these possibly less productive workers employed after sickness. In contrast, the threshold regulation in the disabled worker quota encompasses clearly measurable financial costs in the form of the noncompliance fine. However, there is little empirical evidence so far on which types of costs influence firm behavior at thresholds. In view of the large number of thresholds in German labor law, which are accompanied by different types of costs, further research should put emphasis on evaluating threshold regulations and employers' adaption to such regulations in other contexts.

Administrative Health Data

As a final note, this dissertation demonstrates the relevance of large administrative data sets, as all three essays were able to provide more precise evidence compared to previous research for Germany, which mainly relied on survey data. At the same time, it became evident that the administrative data sets still have limitations, in particular with regard to health characteristics such as the individual sickness history, health behavior or detailed information on health shocks. Complementary analyses with survey data can help overcome these limitations to some extent. Nevertheless, it would be desirable to have more health information in the administrative records. The exploration of and access to administrative health data sources and their linkage to labor market data should therefore be promoted to enable further research on health-related labor market institutions and to draw conclusions for efficient policy measures.

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