Effects of Dismissal Protection Legislation on Individual Employment Stability in Germany

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Abstract: Changes in Dismissal Protection Legislation in Germany have been a subject to ongoing research in the past decade. The majority of these studies, however, has not been able to determine significant effects on job and worker flows in firms affected by the reforms. We estimate the impact of dismissal protection on individual employment stability using the 1999 reform as a "natural experiment". We provide insights into the effects of legislation on the firms' matching behaviour. Our results hint at increased job security after the reform for those spells *affected* by it. This rise in stability was, however, accompanied by modest instability at start suggesting that firms facing additional firing costs tend to cease probation to shed unproductive job-worker matches. The latter effect, however, was not found to be significant.

JEL-Codes: J32, J65, C41

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1 Introduction

According to the OECD (1999), Germany ranks among the countries with the most highly regulated labour markets in the Western hemisphere. A topic subject to ongoing debate in this context is the question about the effects of dismissal protection legislation on the hiring and separation behaviour of firms. This paper investigates the role of the German Protection Against Dismissal Act (PADA) on the stability of covered employment spells.

We provide new insights into these effects as previous studies have focused almost exclusively on aggregate (firm-level) outcomes. We track employment spells individually to determine whether a particular employment spell is or is not affected by legislation. Similar to previous research, we exploit changing provisions for a particular firm size category as a natural experiment to identify the protective effect of the law. Unlike previous studies, however, we do not compare aggregate worker and job flows, but the evolvement of individual job stability over the duration of the job.

This approach has several advantages compared to former research. First, our setup allows us to concentrate specifically on short-term spells. Due to transition periods granted to existing employment relationships, it is typically the case that legislative changes affect only newly begun employment spells. While aggregate worker and job flows cannot account for this fact, we are able to examine short-term spells exclusively. Second, we are able to account for changes in applicability of the law affecting the firm due to employment thresholds. In order to compute aggregate worker and job flows, previous studies had to categorise each firm statically at the beginning of each time frame before and after the reform, thereby ignoring switches between different firm size categories. We argue, however, that a continuous determination of the firm size may be crucial as a substantial number of firms in our sample alters its workforce due to (i) strategic action or (ii) seasonal fluctuations. These switches between treatment and control group may lead to misclassifications of firms and cause attenuation bias. Third, our data allow to control for the use of fixed-term contracts at the company (albeit not at the individual) level. If employment protection is introduced, firms could substitute regular employment by fixed-term contracts. This behaviour may neutralize the estimated impact of employment protection on gross mobility rates and lead to the false assertion that there is no causal effect of dismissal protection on protected employment spells.

According to German legal practice, employers' separation costs strongly depend on workers' job tenure since compensation payments enforced by courts in case of unfair dismissal increase with employment duration. By focussing on short-term spells, we, therefore, investigate the minimum impact of German employment protection on job separations.

The purpose of this paper is to investigate whether newly begun spells have experienced

a change in stability after becoming subject to the provisions of the PADA. Focusing on short-term spells, we provide insights into changes of the matching process once legislation becomes applicable. According to basic job matching theory, additional firing costs implied by the law lead to an increase in overall job security for those spells affected by it. This effect, however, should come at the cost of initial instability during a probation or waiting period in which the law does not yet apply. In this period, firms facing additional firing expenses for long-term spells tend to shed unproductive job-worker matches with higher intensity (Boockmann and Hagen, 2007).

We present some first results on these issues. They tend to be supportive to the theory, although their robustness does not appear to be very strong. In particular, there is little evidence of more mobility within the probationary period. By focussing attentions on separations and not on hirings, we disregard possible effects of employment protection on hirings in some of our interpretations. However, the notion that the effects on hirings are minor is given some support in the recent empirical literature on Germany (Bauer, Bender, and Bonin, 2004).

The remainder of this paper is structured as follows. In section 2, we give a brief overview of the main characteristics of German Dismissal Protection Legislation. We then turn to previous research in that field of literature (section 3). In section 4, we introduce the dataset used in our analysis. We present the methodology in section 5, followed by the discussion of our main findings (section 6). Finally, a conclusion is provided in section 7.

2   Legislative Framework

The Protection Against Dismissal Act (PADA − "Kündigungsschutzgesetz - KSchG") is the main source of legal employment protection in Germany[1].

According to the law, employers may only terminate employment spells if they are able to justify this by one of the three following principal reasons (Article 1 KSchG):

1. dismissals on grounds of personal incapability or health problems,

2. dismissals as a consequence of bad conduct, and

3. redundancies, i.e. separations due to operational reasons.

Regarding the fact that the employer has the burden of proof, it is not surprising that dismissals due to bad conduct or personal incapability are relatively rare. A common opinion is that employers tend to appeal to the third reason, as this may be the most convenient way to

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[1]Some statutory protection against dismissal is also provided by the German Civil Code ("Bürgerliches Gesetzbuch") in Articles 611 - 630, which defines the general terms of dismissal (e.g. the period of notice the employer is required to adhere to). In addition to the general employment protection provided by the PADA, there are also specific mandates for groups such as women before childbirth or disabled persons.

comply with the law. According to Article 102 of the Works Constitution Act, he also has to give additional notice to the works council (in case such an institution is installed), which has to agree upon the proposed dismissal plan.

If the dismissed worker believes that the employer cannot justify the dismissal by one of the above criteria, he or she may take legal action. Labour Courts then have to judge on the adequacy of the dismissal and may, in the extreme case, rescind dismissals for fairness reasons. In case of redundancies, "social criteria" (Article 1 para. 3 of the PADA) such as age, employment duration and maintenance obligations have to be met. The concept of *fair dismissal* has been predominantly shaped by case law, which is of importance insofar as it is up to the employer to prove that all legal duties regarding fairness have been met[2]. As this may be a difficult task, legal cases often end in court settlements such that the employee receives a compensation payment from the employer in return for the termination of the employment relationship. Compensation payments generally increase with the number of years the individual has been employed at the firm. According to Hümmerich (1999), a rule of thumb followed by Labour Courts when settling legal cases is to grant half the monthly wage for each year of seniority. Therefore, exceeding the threshold value does not prohibit a company from dismissing workers, but rather raises its costs for lay-offs significantly. In particular, short-term spells are protected to a much smaller degree than long-standing employment relationships. Moreover, there is a waiting period of six months of tenure before dismissal protection can be claimed.

According to Koller (2005), German legislation counts more than 160 firm size threshold values governing the relationship between employers and employees. In the case of the PADA, a large part of its provisions applies only beyond a firm size threshold, while only some provisions such as dismissal of works council members affects all firms. The threshold for applicability has changed back and forth several times in recent history. In an attempt to tackle high German structural unemployment, the government led by Chancellor Kohl (centre-right) raised the threshold to ten full-time equivalent employees on 1st October 1996 (Article 23 para. 1 sentence 2 of the PADA). The legislator did, however, grant a preliminary protection of the status quo ("Vertrauensschutz") passing a decree that rendered spells that had begun before October 1996 subject to prior legislation (where the threshold value stood at five employees). This exception was due to expire on 30th September 1999.

A political swing to centre-left under Chancellor Schröder did, however, render this

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[2]According to Jahn and Schnabel (2003), 27% of all dismissals filed by employers in 2001 ended up before court. At the same time, 75 to 80% of all lawsuits concerning unjustified dismissals were ruled in favour of the employee. Regarding the fact that costs involved in lawsuits are relatively low compared to international standards (see Jahn, 2002), it becomes apparent that the PADA inherits substantial incentive to take legal action. If the employee is represented by a trade union or if he disposes of a legal expenses insurance, then no monetary costs arise from the trial.

exemption rule obsolete. The threshold value was again set down to five employees, this time allowing for no transition period. In addition, the newly elected government changed the weighting scheme in a manner that implicitly raised the number of workers employed at a firm. The revised act became effective on 1st January 1999 (see Table 1).

A difficult subject is the determination of the headcount used in deciding whether the threshold is exceeded. The threshold refers to establishments, i.e. a production unit at a single location which may economically and legally depend on other units[3]. The PADA thereby refers explicitly to all employees who work regularly in the company (*"In Betrieben und Verwaltungen, in denen* **in der Regel** *zehn oder mehr Arbeitnehmer ... beschätigt sind..."*; Article 23 of the PADA). Thus, emphasis on the *long-term* employment level comprises dismissed workers and requires knowledge about past and future evolvement. According to the Federal Labour Court, this knowledge should reach beyond the pure computation of annual averages, but should also comprise future trends in employment (Bundesarbeitsgericht (31/01/1991); Az: 2 AZR 356/90).

Individuals are considered based on a full-time equivalent calculation. Thus, workers with an hours margin less than full-time are weighted according to an explicit allocation in the act (see Table 1). It is irrelevant for the headcount whether the employee is hired on a permanent or a fixed-term contract since this feature does not affect the juridical existence of an agreement between the employer and the employee. The act does, however, explicitly exclude employees on vocational training as the legislator does intend to promote this form of worker formation. Moreover, the PADA explicitly excludes non-dependent employees. This comprises owners, consultants and family members without a labour contract. By contrast, executives and managers authorized to hire and dismiss employees are explicitly *included* in the computation as they remain in a dependent position within the firm.

3   Previous Research

Over the last decade, Employment Protection Legislation (EPL) has attracted the interest of labour economists and policy makers alike, in particular concerning the effects on aggregate labour markets. Literature thereby has progressed from a cross-country and macro to a single-country and micro perspective.

Some important cross-country studies emerged at the beginning of the 1990s. Bertola (1990), for example, found a negative correlation between the variance of employment growth and job security rankings using data which ranged from the 1960s to the mid 1980s. Yet, his

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[3]Exemptions are made for firms of the navigation or the aviation sector and for private households operating as employers.

findings can only be considered consistent with theoretical predictions if countries are similar in all respects other than the stringency of EPL[4]. Using a sample of 20 countries over the period of 1956 to 1984, Lazear (1990) studies the effect of severance pay requirements on employment. The results suggest that the level of severance pay is negatively correlated with the employment-population ratio and labour force participation rate, and positively correlated with unemployment. In contrast, Addison and Grosso (1996) find no significant evidence between EPL and unemployment using the same data but corrected for a number of deficiencies. Both results could be in line with theory: while theory predicts that a given set of legal provisions should affect movements in employment, there is no general result for the level of employment or unemployment.

The evidence uncovered by this early empirical work, while not as univocal as theoretical models would predict, offers much useful information as to the implications of EPL for employment dynamics and its interaction with other institutional and economic features of industrialised economies. Yet, despite the undoubted achievements, researchers highlighted several critical issues regarding these first studies such as the availability of comparable cross-country data, the difficulty to capture legislative complexity in aggregate measures, or the issue of differences in the enforcement of legislation among countries (e.g. Bertola, Boeri, and Cazes, 1999; Boeri, 1996). Therefore, Addison and Teixeira (2001) came to the *"inescapable conclusion... that there is a pressing need to supplement the aggregate studies with industry and especially firm data."* (p. 38)

Shifting the focus to a firm-level perspective, researchers paid particular attention to the cases of Italy and Spain, where dismissal protection regulations are known to be particularly tight. Borgarello, Garibaldi, and Pacelli (2003), for instance, investigate threshold effects in the Italian case. The authors employ a dataset based on Italian Social Security Records (INPS) to study a reform in Italian Dismissal Protection Legislation in 1990, which tightened regulations for small firms below the threshold[5]. Using a two-step approach, Borgarello et al. (2003) find that firms affected by the policy change were significantly more reluctant to increase employment as compared to corresponding firms above the threshold. This persistence of small firms was determined to be more likely after the reform in 1990.

Using a more comprehensive dataset, Schivardi and Torrini (2004) also research the Italian case running a probit regression (dependent variable: positive employment changes) to measure the effects of the threshold on growth propensity. Repeating the same exercise in steady state using a stochastic transition matrix to assess the long-run effects of EPL, results

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[4]In particular, if the dynamic volatility of labour demand and wages is similar in all countries.

[5]Unlike German studies, it was not possible for them to treat the policy change as a "natural experiment" as this reform was accompanied by an additional reform in 1991.

do confirm threshold dynamics, but effects are found to be very modest.

Boeri and Jimeno (2004) examine the relationship between strictness of EPL and job loss probabilities. Using a dataset for the Italian case similar to Borgarello et al.'s (2003) on the one hand, and the Spanish Labour Force Survey (household panel survey with a rotation scheme) on the other, the authors find their results to be in line with the predictions of their theoretical model. In Italy, workers in firms exempted from EPL are more likely to be laid-off. In Spain, firm size also matters both for lay-off probabilities and reasons the employer alleges at dismissal. Robustness checks are found to confirm the results. However, findings do not hold for the hiring side, where Boeri and Jimeno (2004) were not able to reveal any discrete jumps at the threshold.

Kugler and Pica (2005) use a differences-in-differences approach to exploit the increase in costs of unfair dismissals in small relative to large firms in the 1990 reform in Italy. The authors compare worker and job flows in small and large firms before and after the reform. That is, the authors examine individual job matches and their dissolutions on the one hand, and job flows *"on the internal and external margin"* (p. 2) on the other, referring to overall employment changes as well as to market exits and entries of firms. Unlike the studies mentioned before, they find quite robust and affirming evidence for threshold effects (relative decrease of accessions and separations decreased after the reform). Moreover, the reform is found to have exerted a larger impact in industries that were found to be volatile before the reform. Also in line with theoretical predictions, employment changes fell relatively in small firms, and entry rates for these companies decreased. Surprisingly though, the authors do not mention the issue of a second reform in 1991 that may violate the parallel trend assumption and that was put forward by Borgarello et al. (2003) as a reason why they did not exploit the time dimension.

Whereas international studies have found (albeit modest) evidence for the impact of dismissal protection on employment composition and levels, single-country studies regarding Germany have so far not confirmed a similar clear-cut pattern. Several recent surveys have investigated the effects of the legislative changes in 1996 and 1999 (see previous section) descriptively. A survey by the *Deutsche Industrie- und Handelstag* (DIHT, 1998), for instance, found employment enhancing effects regarding the first reform in 1996. In line with this result, another survey conducted by *FORSA* suggested a reduction in hirings in those firms affected by the second legislative change (IW, 2003, p. 2). Unlike these studies, however, Bielenski, Hartmann, Pfarr, and Seifert (2003) did not find any supporting evidence for threshold effects from a survey about modes and reasons for separations among employees. Obviously, these surveys may only be regarded as a first, tentative glance at the topic due to their descriptive character and the possibility of strategic answering present in this form of research.

Boockmann and Hagen (2001) suggest that flexible working forms such as freelance work,

fixed-term contracts (FTCs) and employees hired from Temporary Working Agencies serve as means to adjust for fluctuations in demand. In particular, the authors determine evidence for a decreased propensity to hire "atypical" labour for those firms affected by the reform in 1996 as this form of employment allowed to "evade" the provisions when the law applied. One drawback of their analysis certainly is the limited observation period of FTCs *before* the legislation change, which restricts variation in the sample. Verick (2004), in addition, expresses doubts about the size of the treatment group (as existing spells were granted protection for a transition period). This argument, however, does not seem to be entirely convincing. Although the transition period regarding dismissals of existing spells may have affected new hirings to some extent (as employers might have acted more cautiously), it is unlikely that this regulation has completely offset effects in the firms' employment behaviour since this transition period did not concern hirings. In a more recent study, Fritsch and Schank (2005) contest the findings of Boockmann and Hagen (2001). Extending the observation period, they are not able to confirm the results of reduced hiring probabilities for FTCs in firms with six to ten employees during the period from 1996 to 1998.

Another example for a recent empirical study evaluating threshold effects of German employment legislation are Kölling, Schnabel, and Wagner (2001). The authors not only examine the effects of changes in the threshold of the PADA, but also in the legislation of the Severe Disability Act (SDA)[6]. Unlike Boockmann and Hagen (2001), they do not determine threshold effects with respect to the PADA. By contrast, Kölling et al. (2001) do find weak evidence regarding decreased employment behaviour at the threshold of the SDA. Despite general issues concerning studies researching job and worker flows (see above), the study of Kölling et al. (2001) may be criticised on a specific ground. That is to say, the narrow definition of firm groups appears to be highly critical regarding the approximations that are needed to categorise firms. Even though Kölling et al. (2001) run several robustness checks with more distant groups, the issue of narrow firm classes cannot be entirely resolved as firms may not use different forms of work uniformly[7].

Using the IAB establishment panel for the years 1997 to 2001, Verick (2004) examines the impact of the legislation change in 1999 choosing a difference-in-differences approach. He determines slight evidence for threshold effects affecting firms just below the exemption level. Robustness checks (using different subsamples such as East and West Germany, services and manufacturing, and differing time frames) corroborate these results. Still, Verick (2004) emphasises that findings have to be taken with care as extending the observation period hints at

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[6]The latter act was modified in a rather complicated way lifting the threshold to 20 employees in 2000.

[7]Approximations regarding the number of part-time employees may affect firms differently and lead to a "widespread dispersion" of firms that lie just at the threshold in reality.

other macroeconomic factors than the PADA affecting treatment and control group differently. Verick (2004) motivates this lack of evidence with potential information deficits on behalf of the employer (with respect to exact legislation). One limitation, however, is the fact that Verick (2004) restricts his analysis to the examination of threshold effects, but does not examine the hiring and firing behaviour of firms affected directly by the PADA.

Bauer et al. (2004) evaluate both reforms in a two-step analysis with respect to the effects on job and worker flows of firms around the threshold. The data for this study is drawn from the German *Statistics Employment Register*("Beschäftigtenstatistik"). Based on this source, Bauer et al. (2004) draw a 5% random sample of West German establishments only. The authors observe employment behaviour of firms in a twelve months observation window before and after the legislation changes in 1996 and 1999 accounting thereby for potential strategic behaviour. Bauer et al. (2004) then compute average worker and job flows for each estimation window aggregating the individual-level data.

In the first step, Bauer et al. (2004) examine threshold effects along the establishment size, i.e. along the cross-sectional dimension. Estimating a model (dependent variable: hiring, separation, and job flow rates, respectively) that includes establishment characteristics, variables fitting a parametric relationship between the outcome measure and establishment size, and dummy variables for different establishment sizes around the threshold, the authors determine contradictive evidence[8]. On the one hand, Bauer et al. (2004) find slight evidence that firms operating below the threshold value have significantly larger hiring and separation rates compared to other firm size groups (even though no effect is found on the overall job flow). On the other hand, firms at the threshold value also exhibit excessive (and significant) hiring rates, which contradicts the hypothesis of firms trying to avoid the provisions of the PADA. Bauer et al. (2004) interpret the latter finding as potential replacement hires since the overall effect on job flows at the threshold is negative and significant.

In a second step, Bauer et al. (2004) also exploit time-series variation using a difference-in-differences approach: they obtain an additional estimator δ_{DiD} that captures the unique effect of establishments subject to treatment (i.e. affected by the policy change). Surprisingly, though, they do not find evidence for any significant threshold effects, neither in the first nor in the second reform. This result does not change as Bauer et al. (2004) run several robustness checks varying (i) firm size control groups to test for missed out shocks and (ii) stratifying according to industries to adjust for heterogeneous treatment effects.

A rather comprehensive approach to the effects of the PADA is taken by Bothfeld, Bradtke, Kimmich, Schneider, Ullmann, and Pfarr (2005). The authors conduct a multistage

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[8]All variables were allowed to float in the observation window.

analysis in an attempt to investigate the effects of the PADA. Running several probit regressions, they do not determine any particularly negative "psychological effects" of dismissal protection on the hiring behaviour of firms. Evaluating dismissal costs to be modest, Bothfeld et al. (2005) therefore conclude that the PADA itself and the threshold value in particular seemingly had no negative effect on firms' employment behaviour. There are, however, several critical aspects with respect to this study. As above, one may criticise the methodological approach itself, which builds on survey data susceptible to strategic answering and measurement errors. This argument applies in particular as the reference point dates back by almost five years. Secondly, the headcount computation is fairly approximative as the authors are not able to differentiate between full-time and part-time employees. Even though Bothfeld et al. (2005) acknowledge this fact, they do not explicitly account for it in the final interpretation of their results. Moreover, computing the hirings rate, Bothfeld et al. (2005) compare the employment level of each firm at the time of the interview (2003) to the average employment level of the past five years (1998-2003). Besides the issue of fluctuations in this vast time span, the frame also comprises the legislation change in 1999, which is likely to have altered the employment behaviour. Another critical aspect concerns the evaluation of costs regarding dismissals. Bothfeld et al. (2005) base their argumentation on severance pay and the length of trials exclusively, neglecting thereby important aspects, which are not reflected by these costs (e.g. expenses regarding legal counselling, reduced productivity of the claimant).

Burgert (2006) disentangles effects of the PADA regulations with respect to the elderly workforce as these employees are granted specific provisions (explicitly and implicitly). The results that are derived, however, do not display any significant evidence towards a more cautious employment behaviour regarding the elderly workforce (50 and above). The author interprets this finding as evidence against the hypothesis of the PADA as a barrier to greater employment dynamics. Besides the issue of misclassifications due to the narrow definition of firm classes mentioned[9], however, there is an issue of self-selection in his study: Burgert (2006) explicitly restricts his sample to firms that expected profits in at least one year, but never reported negative revenue expectations arguing that these firms were not supposed to be guided by any threshold values (and thus satisfied the stable unit assumption). This, obviously, limits the validity of his results since those firms that are expanding will presumably bother least about the threshold.

Summarising the body of empirical literature regarding the "German case", there is only modest evidence for effects of the PADA on outcomes such as employment mobility or firm growth. We argue, however, that this lack of evidence can, at least partly, be attributed

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[9]This issue applies only marginally since Burgert (2006) runs several robustness checks widening the firm groups without revealing any substantial changes in his results.

to the fact that the aggregate approach of previous studies is unable to capture the complexity of legislation as well as the heterogeneity of economic effects:

- First, aggregate measures cannot differentiate between newly begun and ongoing employment spells. While provisions actually *did* change for short-term spells, the latter were granted a transition period and thus remained unaffected by the reforms (see Table 1 for an illustration).

- Moreover, the setup of these studies implied a categorisation of firm size at the beginning of the observation period not accounting for switches between treatment and control group during observation.

- Finally, previous research was unable to capture the contradictive effects implied by economic theory: dismissal protection may lead to a duality in the labour market (Saint-Paul, 1996) with a share of employees hired on "atypical" contracts facing high job turnover, but a core workforce on permanent contracts benefitting from increased job security. Similarly, employers may alter their overall matching behaviour dismissing unproductive job-worker matches at the end of the waiting period before employment protection applies. This may offset the protective effect of employment protection on aggregate hiring and separation rates.

4 The Data

The dataset used in this study is the German LIAB provided by the Institute of Employment Research (IAB) of the Federal Employment Agency. It is generated linking administrative data of the IAB containing individual-level information such as age, sex, employment characteristics etc. with the IAB establishment panel containing annual information about establishment characteristics and firm decisions in the period ranging from 1993 up to 2001 (Alda, Bender, and Gartner, 2005). The individual-level data stems from the *"Beschäftigten-Leistungsempfänger-Historik-Datei"*, which draws information from the mandatory notification procedure for the health, pension and unemployment insurances. By contrast, the IAB establishment panel is a representative annual survey of establishments (Kölling, 2000).

Regarding the specific version used in this study, the LIAB contains interviews for roughly 4,200 Eastern (2,100) and Western (2,100) German establishments. The observation period ranges from 1991 (West Germany) and 1992 (East Germany), respectively, until 2001. A person is included in the dataset if his employment lasts at least one day in one of the longitudinal establishments in the period 1996 to 2001. Firms in the *longitudinal* version of the LIAB are defined as firms that had regular (annual) interviews during the period 1999 to

2001. For the purpose of our analysis, however, we required firms to stay in the sample for the entire observation period ranging from 1996 to 2001, which applied to 2,356 firms. The reason is that we could be sure to observe all employment relationships only for these firms. However, this was crucial to our research design. This reduced the overall number of observations to roughly 90,000, which then further shrunk in the selection process described below. Note that newly founded establishments and establishments that dropped out of the market during the observation period are not included in our data.

Concerning person- and firm-specific selection, we only considered individuals that had the official status of an employee. This excludes self-employed, non-employed owners, home-workers etc., who are explicitly exempted from the computation (Article 17 para. 5 KSchG). We also neglect sideline employments as these spells often occur within the same firm or correspond to marginal employment relationships ("geringfügige Beschäftigungsverhältnisse"). Similarly, individuals that had started their employment on vocational training were not of particular interest from a matching point of view and therefore dropped. Other subgroups excluded from the analysis comprised interns, disabled employees, and individuals currently serving civil or military duties.

We consider employees aged between 15 and 65 years[10]. Employees working for the government (regional or federal level) have been removed from the sample as workers from these institutions are often subject to different treatment compared to their counterparts from the private sector. Regarding the selection of specific industries, we decided to exclude firms operating in the aviation or the navy sector due special regulations that apply in these industries (Article 23 para. 1 KSchG). Furthermore, we removed firms from the agricultural or mining industry as these sectors are highly subsidised and prone to employ a large fraction of FTC-workers (agriculture).

Since we are interested in the *potential effect* of treatment (i.e. the hazard change for an individual spell), we did not only consider the number of full-time equivalent employees at the time of dismissal, but each time potential treatment changed for the firm. This also implied splitting episodes into subspells if new workers were appointed to the firm. Aggregating all spells of a firm at each splitting point provided us with the actual firm size at these points in time[11].

Another issue to be addressed was the actual computation of the headcount itself: the LIAB does provide detailed information on the job position only for those employees with a

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[10]Potential problems arising from e.g. early retirement schemes should become obsolete as long as these specific age groups were distributed evenly across the sample.

[11]Note that our methodology even allowed to account for replacement hires appropriately since terminated spells are, in line with legislation, included in the computation. By such action, we hold the level of employment constant even if workers are substituted only after a certain period of time.

full-time position. Part-time employees are gathered qualitatively, i.e. whether they have been working less or more than a regular full-time employee. Unfortunately, the hours grid utilised in the LIAB does not fully match the weighting scheme in the PADA. Weighting therefore had to be adapted approximatively.

As in Boockmann and Steffes (2007), we follow the notifications for the failure event provided by the LIAB (see Table 4). Due to its mandatory character, this information is deemed reliable. A crucial point, however, concerned uncoded breaks in the individuals' employment history. Rarely, the exact reasons for employment gaps were coded, which could relate to pure inconsistencies in the dataset, but also mark the end of a particular employment relationship. Again adhering to Boockmann and Steffes (2007), we approximatively defined employment spells to be uninterrupted if the employee did return to his or her old firm within 90 days after the break (without being on benefits). Spells that did not suffice these conditions were marked as censored (Table 4). The exception we allowed for consisted of interruptions due to parental leave or compulsory duties (e.g. compulsory official duty), which were also treated as uninterrupted employment relationships. In the former as well as in the latter case, the spell continues to contribute to the regular number of staff and thus still enters the computation of headcount. Therefore, we decided to close employment gaps for these individuals recognising that this may lead to a potential measurement error[12].

5 Estimation Approach

The question to be resolved empirically is the effect of employment protection on newly begun employment relationships. Additional firing costs implied by the law are likely to reduce the number of separations. However, since there is a waiting period of 6 months during which employment protection does not apply, the protective effect of the law should unfold only after that date. From job matching theory, there is an argument why one could observe even more separations before. At six months of duration, the employer has to consider either firing the worker at relatively low costs or retaining the worker and face higher firing costs if it wishes to fire the worker at some later period. At this moment, we should observe a larger number of dismissals with employment protection than without[13]. Since the effect of dismissal protection may, therefore, be reversed over the employment spell, it needs to be estimated at different job durations.

As an estimator, we use a difference-in-differences (DiD) model. This setup has been widely used in empirical research over the last decade (Abadie, 2005). Comparing changes

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[12]Permanent employees on parental leave could be replaced by temporary workers on fixed-term contracts.
[13]This argument has been formalised in a job matching model by (Boockmann and Hagen, 2007)

in job stability in the "treated" fraction of spells exposed to employment protection to an "untreated" control group can be used to identify the "true" effect of the PADA on employment stability.

As displayed in Table 3, we sampled spells beginning after 1st October 1996 but before 1st October 1997 for the first interval and, respectively, after 1st March 1999 and before 1st March 2000 for the second period allowing thereby for a minimum follow-up time of 365 days. Firms with 11 to 14 employees formed our initial control group. To check robustness, we use firms with 1 to 5 employees as a second control group. In order to be classified as treatment or control group, spells had to be in either of the specified groups at the *spell begin*. Thus, we allowed for a change of treatment in the aftermath.

For the construction of the intervals before and after the policy change, a flow sampling approach was chosen. This avoids the issue of length-bias. That is to say, we sample individuals who enter the state of employment (the "starting time") at some point during the interval $(0; b]$, and then measure elapsed time for a certain follow-up period. The sampling period and the follow-up time had to be defined according to the constraints imposed by the interim period between the two legislation changes: for the start of the first interval, we did not consider any "buffer" to account for adjustment effects immediately as legislation granted a transition period for existing spells that was expected to expire only on September 1999. Hence, we set the starting date of the first sampling interval to 1st October 1996 (i.e. the day the modified bill came into effect).

Defining the end of the frame was more difficult. To avoid the problem of Ashenfelter's Dip (Ashenfelter, 1978), we had to restrict the sampling period. Even though legislation only changed on 1st January 1999, the general elections for the Bundestag took already place on 27th September 1998[14]. Still, we consider a buffer period of three months to be sufficient to account for Ashenfelter's Dip since this date only marks the utmost observation point of a spell. We have therefore set the maximum observation date to be 1st October 1998 (see Table 3).

Another critical point subject to specification changes was the censoring time. We decided to determine fixed censoring dates to allow for the longest tracking time possible. Obviously, times at risk could potentially differ across groups as we sample spells within the intervals. We therefore proceeded as follows: first, we defined a minimum time for spells to evolve without entering the period immediately before the legislation change. In order to balance the trade-off between a wider sampling window, on the one hand, and a longer tracking time, on the other, we varied these minimum follow-up times to last 274 and 365 days,

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[14]According to contemporary reports, the landslide victory of the social democrats and thereby the change of the PADA (which was a key feature of their campaign) were easily foreseeable.

respectively (Table 3)[15]. Since spells that started before the final sampling point potentially exceeded this minimum duration, we then compared the maximum follow-up times of all groups and defined the shortest to be the maximum tracking time. This method allowed to compare the actual survivor functions at each point in time, which, given equation (1), was crucial for the computation of the DiD estimator. Clearly, with spells failing over time, the DiD estimator becomes unstable and imprecise. However, bootstrapped confidence intervals should account for this effect.

Since the key purpose is to investigate the effects of PADA on individual employment stability at different durations, we use survival analysis as the most flexible approach. In particular, we use the time varying unconditional effect obtained from Kaplan-Meier Survivor functions. The Kaplan-Meier survivor function indicates the probability of remaining in employment after τ days of employment. The DiD effect is thus given by

$$DiD(\tau) = [\hat{S}_{i=1,t=1}(\tau) - \hat{S}_{i=1,t=1}(\tau)] - [\hat{S}_{i=0,t=1}(\tau) - \hat{S}_{i=0,t=0}(\tau)] \tag{1}$$

where $\hat{S}_{i,t}(\tau)$ denotes the empirical Survivor Function, i denotes the treatment effect ($i = 1$ indicates treatment), and t refers to the period before and after the legislation change ($t = 1$ corresponds to the post legislation change period). Equation (1) implies that the causal effect is estimated for every point in time τ. This is convenient as it allows the legislation change to have a potentially differing impact over time. In order to judge the significance of our estimator, we use an ordinary nonparametric bootstrap with 200 resamples (Efron and Tibsharani, 1993).

The identifying assumption in this context requires that, in absence of treatment, the outcomes for both groups follow equal paths over time (*parallel trends*). This assumption requires careful justification. In our case, there are the following reasons why the parallel trends assumption could be violated:

- Most obviously, changes to other regulations could affect treatment and control group differently.
- Economic conditions such as the business cycle could have a differential impact.
- The assignment to treatment and control could be endogenous if, for instance, firms shrink deliberately in order to be exempted from employment protection.

Concerning the first point, there were no other major exogenous changes in labour market regulations during the observation period that potentially affected groups differently. For instance, after October 1996, no major modifications to the legislation of fixed-term contracts became

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[15]Thereby, we were able to check the robustness of our results as amplifying the sampling window increased the number of spells included in the analysis, but, at the same time, diminished the tracking period.

effective until January 2001 ("Gesetz über Teilzeitarbeit und befristete Arbeitsverträge"). Similarly, the Works Constitution Act was altered at the end of 2001, leaving its threshold values, however, unchanged[16]. The cut-off level of 14 employees was chosen as other threshold values become applicable once firm size exceeds the 15 employees[17].

A differential effect of other variables on firms in the treatment and control group is the more likely, the more dissimilar these groups are. We have restricted this study to small-sized businesses, i.e. firms with an initial number of 14 full-time equivalent employees at most. These firms are likely to be driven by similar economic forces as our treatment group. To verify this, Bauer et al. (2004) examine establishments of different firm classes with respect to the overall macroeconomic environment during the time of the policy change. The authors compare insolvency frequencies by firm classes (<5; $6-10$; $11-20$; $21-50$) together with GDP, but do not find substantial differences among firm categories (except for firms with less than five employees). Since we use a balanced sample from the same source of data for our approach, their findings should also apply to our analysis.

Concerning the third point, problems may arise due to strategic behaviour of firms from the control group, which may violate the parallel trend assumption. That is, in order to avoid the provisions of the PADA for new hirings, firms above the threshold of ten full-time equivalents could have deliberately reduced employment below this value before the reform in 1999. Once the threshold was changed, firms should have ceased such behaviour. Judging from the summary statistics in Table 5, however, does not really support this argument.

Potential information deficits as outlined by Verick (2004) could mark another source of measurement error. Since firms in Germany are a mandatory member of the chamber of crafts or the chamber of industry and commerce, which offer legal advice free of charge, we consider this only a minor issue[18].

Another problem not related to the difference-in-differences procedure may be that legislation affects both hirings and separations. For instance, employers may hire more restrictively after the introduction of employment protection. In a job matching model, the reservation match quality, below which an employer does not hire a worker, may increase. As the number of hirings decreases, so does the number of subsequent separations.

While this is a plausible mechanism[19], we note that this behaviour is unlikely due to the

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[16]One should keep in mind, however, that we cannot entirely rule out interactions with existing threshold values (see Table 2).

[17]For instance, until September 2000, firms with more than 16 employees were required to allocate 6% of all job positions to disabled employees (Severe Disability Act). If the employer failed to comply with this regulation, he was fined a monthly penalty of 200 DM per vacancy.

[18]Indeed, one may assume that a majority of firms will consult these institutions before appointing new employees.

[19]However, a number of studies suggest no effects on hirings, see e.g. (Bauer et al., 2004).

fact that there remains a 6-months waiting period before employment protection takes effect. Therefore, the employer will assess match quality not only at the start of the employment spell, but also at 6 months after the start, at which time unproductive workers are shed. We can, therefore, use the 6 months waiting period to identify empirically the effects of separations vis-à-vis hirings.

Finally, a problem that may bias our results and that needs to be considered when interpreting the findings is *non-random panel mortality* (Burgert, 2006). As we restricted our analysis to firms that stayed in the sample over the entire observation period ranging from 1996 to 2001, we do neglect those firms that entered into or dropped out of the market during this period. If exit from the sample is correlated with the treatment status, i.e. applicability of the PADA and the additional dismissal costs involved in the legislation, then this may represent a serious issue, which needs to be kept in mind when interpreting our results.

6 The Results

Turning to the discussion of the results, we start with specification (2) from Table 3 in the appendix. This specification will form the basis of our analysis. We will, however, also draw on results from other specifications to contrast or support certain issues. We start with the unconditional Kaplan-Meier curves in Figure 2. Median duration is at 396 and 390 days regarding specifications (2) and (4), respectively (without Table). This is somewhat lower than in the results found in other studies (e.g. Boockmann and Steffes, 2005). The difference may be due to the fact that we concentrate on small establishments in this paper. It may also reflect the inclusion of young workers below the age of 25.

Evidently, after an initial period of almost synchronised sloping, the curves diverge substantially. This gap even widens as time passes and remains at a level of roughly 10% after 400 days. Strikingly, the treatment & before[20] line constantly runs below the other curves indicating increased drop-outs after the first 200 days of duration[21]. This suggests that job exit occurs earliest in small companies exempted from employment protection.

Associating this observation with the context of theory, we obtain the following picture: for the initial 180 days of tenure, all four subgroups are exempted from the PADA since either legislation was not applicable at all (treatment & before) or it only becomes applicable after a waiting period. Thus, firms in all groups have equal legal rights to dismiss workers, which is reflected in similar survivor functions. After the end of the waiting period, however, provisions

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[20]To facilitate the comparison with the figures in the appendix, we use identical labels to those depicted in the graphs.

[21]Formal log-rank tests clearly reject the equality of survivor curves yielding χ^2-statistics of 18.24.

of the PADA become applicable to all groups except for firms of treatment & before. This is in line with the observation of increased stability for spells in the groups covered by the PADA (as employers had to dismiss bad matches during probation), unlike in treatment & before.

This interpretation is supported by the second graph in Figure 2 displaying specification (4) with 1 to 5 employees as control group[22]. Again, we observe a fairly synchronised trend during the first 180 to 200 days and diversion thereafter[23]. Although the effect is less pronounced, the interpretation is similar as in the first case. Only treatment & after was affected by the provisions of the PADA, while the other groups remained below the threshold of applicability. This translates into stability the same way as before, this time, however, with treatment & after lying above the other curves indicating increased stability for those spells covered by the PADA.

Even though the graphed survivor curves give detailed insight into the particular effects on each group, they are not able to reveal the overall impact of the reform and its significance. Therefore, we construct the DiD estimator as outlined in section 5.

Examining both graphs from Figure 3 reveals a relatively clear-cut picture, which fits well with our expectations. Regarding the evolution of the survivor curve in Figure 2a, we observe a relatively stable period at the beginning lasting roughly 180 days. Thereafter, we notice a steep increase in the estimated effect, which amounts to a rise of about 10% in the survival probability from approximately one year of job duration. Moreover, whereas the initial phase does not turn out to be significant at any reasonable level, we find the latter effect to be significant at the 5% level. This emphasises the positive effect of the PADA on job stability after the waiting period, which does not fade out but remains at a constant level.

Regarding the second graph, applicability of the PADA seems to be associated with a "shedding period" indicated by the negative effect at start. This is in line with the idea that employers dismiss more workers within the waiting period if they are subject to employment after this period. The negative effect on survival is followed by a positive shift of the survival probability after 180 days, amounting to greater stability of roughly 4 to 10 percentage points. The latter effect again corresponds to the assumption of bad matches being already dissolved during probation. Both "segments" in this graph are, however, not significant at the 5% level.

Taken together, we do find significant evidence for an increase in overall stability of spells once the PADA becomes applicable. Regarding specification (4), we also find that this

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[22]Note that survivor functions of the treatment group in both specifications do not coincide as we allow for switches between treatment and control group. That is, spells that started initially in the control group may have changed into the firm class with 6 to 10 employees attributing thereby to the treatment group.

[23]Log-rank tests reject equality with χ^2-statistics equal to 14.46.

positive impact is "preluded" by a slightly negative impact at start (although not significant), hinting at excessive shedding. This pattern *could* imply a quicker dissolution of bad matches, while productive job-worker relationships actually face a higher persistence in the long run. The fact that we observe a similar pattern in both specifications affirms our conjecture that these effects are a result of changing effects of the PADA on stability.

In order to further test the robustness of our results with respect to the different shares of firms employing fixed-term contract workers (Table 5 and 6), which may potentially violate the parallel trend assumption, we delete all firms using this form of atypical labour from the sample. We acknowledge that excluding these firms may raise a selection issue if establishments with FTCs were to display a different overall separation or employment behaviour (compared to firms without these workers). Since this procedure only serves to countercheck the robustness of our results, however, we consider the approach to be appropriate in this context.

Dropping firms employing FTC workers, we observe a slight downward shift (which even crosses the zero line) during probation in Figure 2c. Remarkably, the steep increase after the expiry of probation has now become more pronounced. After these effects, however, we find the trend almost unchanged, but again significant at a 5% level. Changing to firms with 1 to 5 employees as control group (Figure 2d) does reveal a decrease at start that becomes particularly visible at roughly 180 days[24]. This effect is followed by a downward shift that is most pronounced at medium duration. Both effects are insignificant, though.

From this sensitivity check, we do not find that results are substantially different after excluding firms with FTC spells. This hints to the presumption that a potential bias due to differing shares of establishments employing FTCs may only play a minor role in our context. While the more pronounced initial shedding phase appears to be fairly in line with evidence provided by Boockmann and Hagen (2007), the differences are by no means significant.

7 Conclusion

In this study, we have examined the effects of changes in German dismissal protection legislation on individual employment stability exploiting a legislative reform in January 1999 as "natural experiment". We have restricted ourselves to the evaluation of short-term spells since only these employment relationships were affected by the policy change. Tracking spells individually over time, we have also been able to capture differences in the application of legislation with respect to job duration due to the existence of a waiting period.

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[24]Note that extra care has to be taken when interpreting the graphs of specification (4) in the appendix as the scaling is not identical. Examining Figure 2d without confidence bands reveals that the effect at 180 days is smaller by roughly 2 percentage points.

Using the LIAB, a linked employer-employee dataset for Germany, we have found a significant increase in job stability for employment relationships covered by the PADA after the waiting period of 180 days. Using a different control group of establishments, however, indicates a certain lack of robustness for this effect. Before the waiting period, job exits occur with equal frequency regardless of whether an establishment is exempt from employment protection. In line with the idea of increased labour shedding during the waiting period, there is a slightly negative effect on stability at start. However, the effect is never significant.

Our study has provided only first insights into the effects of dismissal protection legislation on the effects of employment protection on the evolvement of job stability over tenure. Avoiding methodological shortcomings of previous research, a more detailed glance at job durations reveals some evidence for effects of the German PADA. However, the issue requires further investigation in the future. In particular, one may adjust for covariates in estimating survival probabilities to check for the influence of selection on observable variables. One might also use spells with long durations as a further control group, resulting in a difference-in-differences-in-differences design. Finally, it is also desirable to take a second look at changes in hirings behaviour as a consequence of employment protection in further research.

8   Appendix

Table 1: Key Changes in the Provisions of the PADA *before* and *after* 1999

Before	After
Applicability:	
>10 employees	>5 employees
Weighting Scheme:	
<10 hours − 0.25	
<20 hours − 0.5	<20 hours − 0.5
<30 hours − 0.75	<30 hours − 0.75
>30 hours − 1	>30 hours − 1
Transition Period:	
Old provisions to ongoing	None
spells until 31th June 1999	

Figure 1: Continuous Firm Size
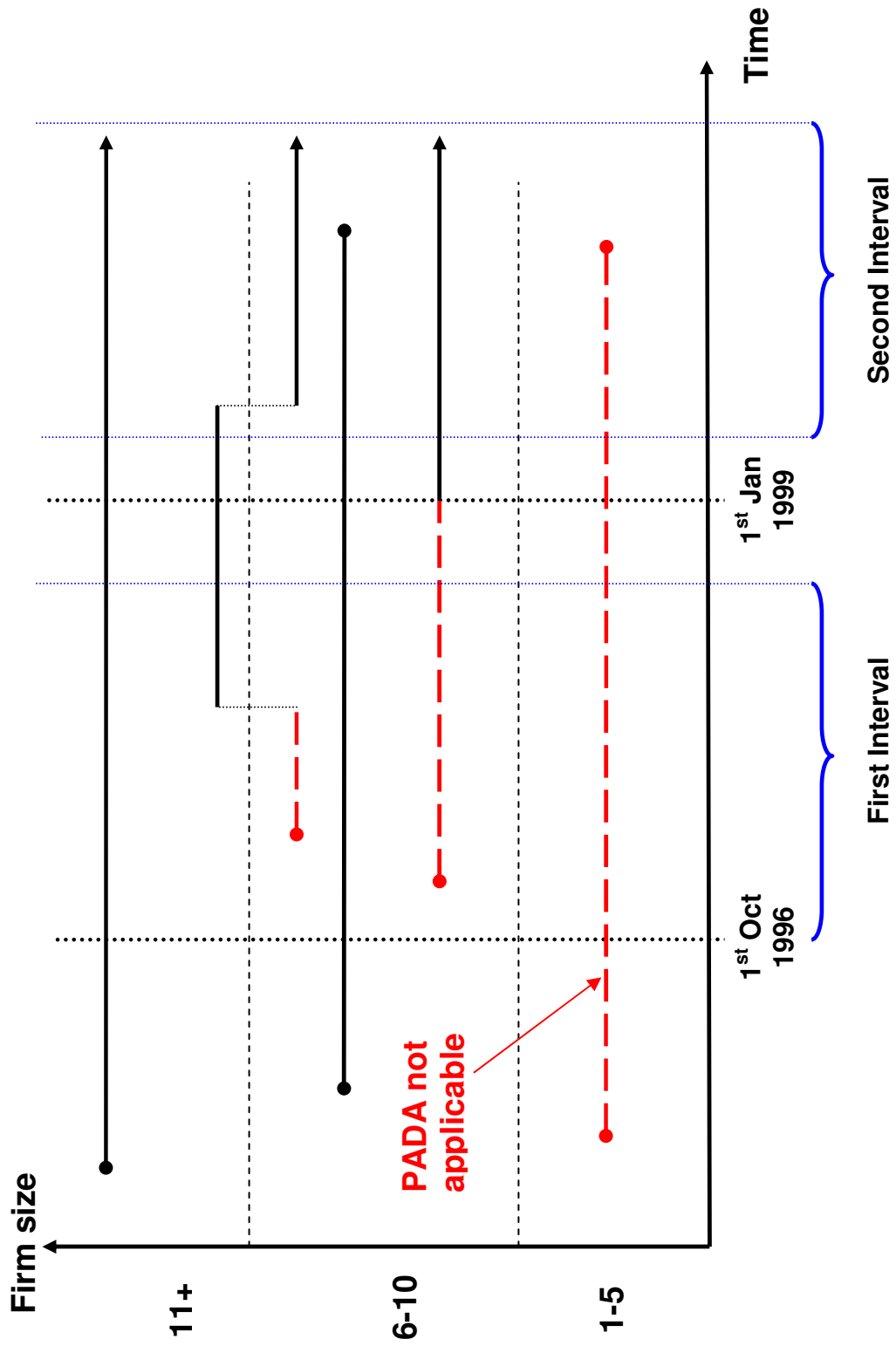
Table 2: Selective Overview of Threshold Values in German Legislation (1996-2000)

Threshold Value (no. of employees)	Provision	Legal Foundation
5	Installation of a works council	§ 1 BetrVG
more than 5	applicability of the PADA (Jan. 1999)[1]	§ 23 KSchG
more than 5	separate rest rooms for male and female employees	§ 37 ArbStättV
more than 10	applicability of the PADA (Oct. 1996)[1]	§ 23 KSchG
more than 10	provision of a recreation room	§ 29 ArbStättV
more than 15	obligation to provide positions for disabled empl. (Sept. 2000)[2]	§ 5,7 SchwbG

Note: Depending on the specific legislation, employees are weighted differently (Alewell and Schlachter, 2000). For instance, whereas the PADA attaches half weight to part-time employees working less than 20 hours, SDA completely neglects workers with less than 18 weekly hours.

[1] Month and year the regulation became effective.
[2] Provision was replaced by more complicated regulations at that date.

Table 3: Sample Specifications

	1st sampling period	2nd sampling period	min. follow-up time	latest obs. date (1st/ 2nd)[1]	Control[2]
(1)	01Oct1996 - 01Jan1998	01Mar1999 - 01June2000	274 days	01Oct1998/ 01Mar2001	11-15
(2)	01Oct1996 - 01Oct1997	01Mar1999 - 01Mar2000	365 days	01Oct1998/ 01Mar2001	11-15
(3)	01Oct1996 - 01Jan1998	01Mar1999 - 01June2000	274 days	01Oct1998/ 01Mar2001	1-5
(4)	01Oct1996 - 01Oct1997	01Mar1999 - 01Mar2000	365 days	01Oct1998/ 01Mar2001	1-5

[1] Last potential date of observation.
[2] Control Group.

Table 4: Definition of Censorings, Recalls and Failures

A spell was considered to fail if...

- notice about the end of employment was filed to the Federal Employment Agency for one of the following reasons:
 	
   - termination of the the employment spell.
   - termination after interruption exceeding one month of time.
   - contemporary registration and deregistration of the employment relationship.

A spell was marked as censored if...
- the the max. follow-up time was reached. - the firm identifier changed without notice. - the employment record was followed by a benefit spell without notice. - a spell was interrupted for more than 90 days without notice.
A spell was considered to be interrupted only, if...
- the break lasted less than 90 days and the firm identifier did not change (without benefit). - the intermission was due to parental leave or other purposes (compulsory official duty) and the firm identifier did not change.

Table 5: Sample Specifications (2)

	1st Interval	2nd Interval
No. of Spells		
fs1-5	-	-
fs6-10	383	590
fs11-14	264	592
Total	647	1182
No. of Failures[1]		
fs1-5	14	35
fs6-10	199	285
fs11-14	125	270
Total	360	637
No. of Censorings[1]		
fs1-5	19	54
fs6-10	131	226
fs11-14	74	219
Total	287	545
No. of Firms		
fs1-5	-	-
fs6-10	139	178
fs11-14	63	86
Total	202	264
No. of Firms with FTCs		
fs1-5	-	-
fs6-10	32 (.23)[2]	46 (.26)
fs11-14	17 (.27)	31 (.36)
Total	49 (.24)	77 (.29)
No. of Firms changing		
1-5 → 6-10	12	22
6-10 → 1-5	33	56
6-10 → 11-14	45 (28)[3]	69 (45)
11-14 → 6-10	49 (32)	71 (39)
11-14 ↔ 6-10	33	56

[1] Single categories may not add up to the total as some censorings and failures occurred in firms with more than 14 employees.

[2] As share of the total number of firms in that category.

[3] No. of firms changing that originated in that particular firm class.

Table 6: Sample Specifications (4)

	1st Interval	2nd Interval
No. of Spells		
fs1-5	272	388
fs6-10	383	590
fs11-14	-	-
Total	655	978
No. of Failures[1]		
fs1-5	143	221
fs6-10	190	263
fs11-14	35	44
Total	371	535
No. of Censorings[1]		
fs1-5	111	198
fs6-10	138	195
fs11-14	17	47
Total	284	443
No. of Firms		
fs1-5	148	189
fs6-10	118	159
fs11-14	-	-
Total	266	348
No. of Firms with FTCs		
fs1-5	29 (.20)[2]	26 (.14)
fs6-10	26 (.22)	41 (.26)
fs11-14	-	-
Total	55 (.21)	67 (.19)
No. of Firms changing		
1-5 → 6-10	37 (28)[3]	51 (29)
6-10 → 1-5	44 (27)	65 (44)
6-10 → 11-14	33	54
11-14 → 6-10	23	39
11-14 ↔ 6-10	26	42

[1] Single categories may not add up to the total as some censorings and failures occurred in firms with more than 14 employees.

[2] As share of the total number of firms in that category.

[3] No. of firms changing that originated in that particular firm class.
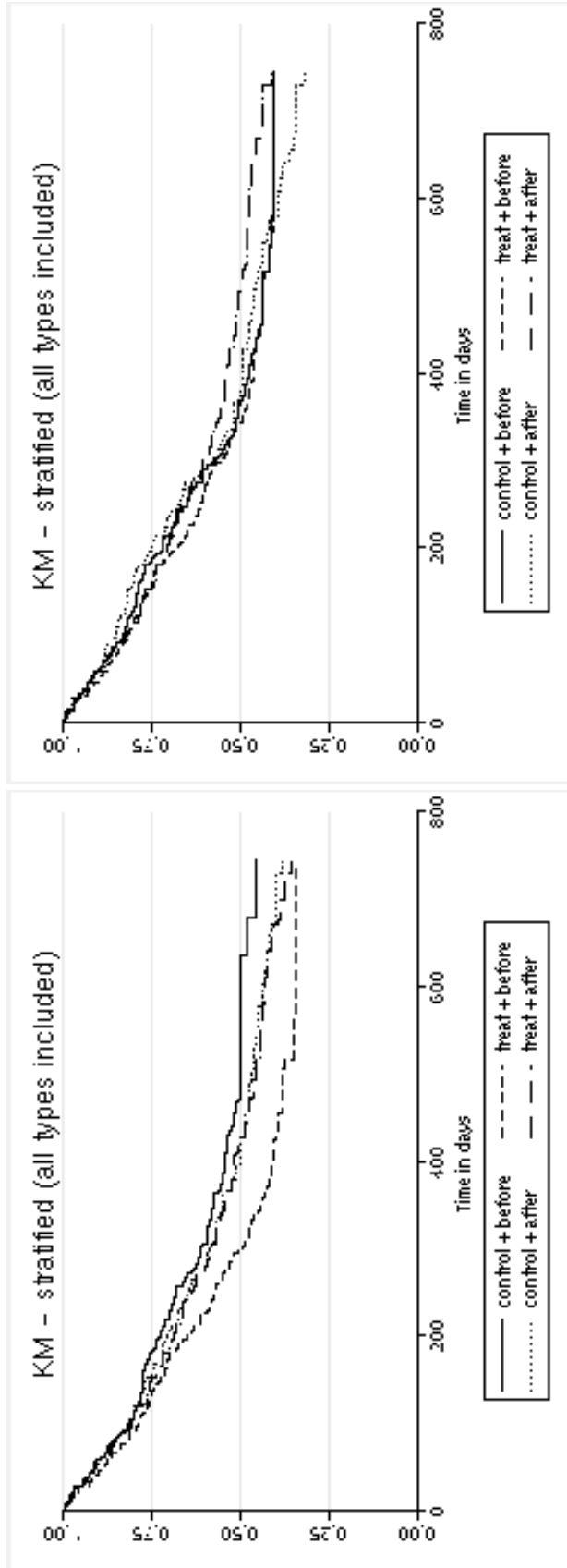
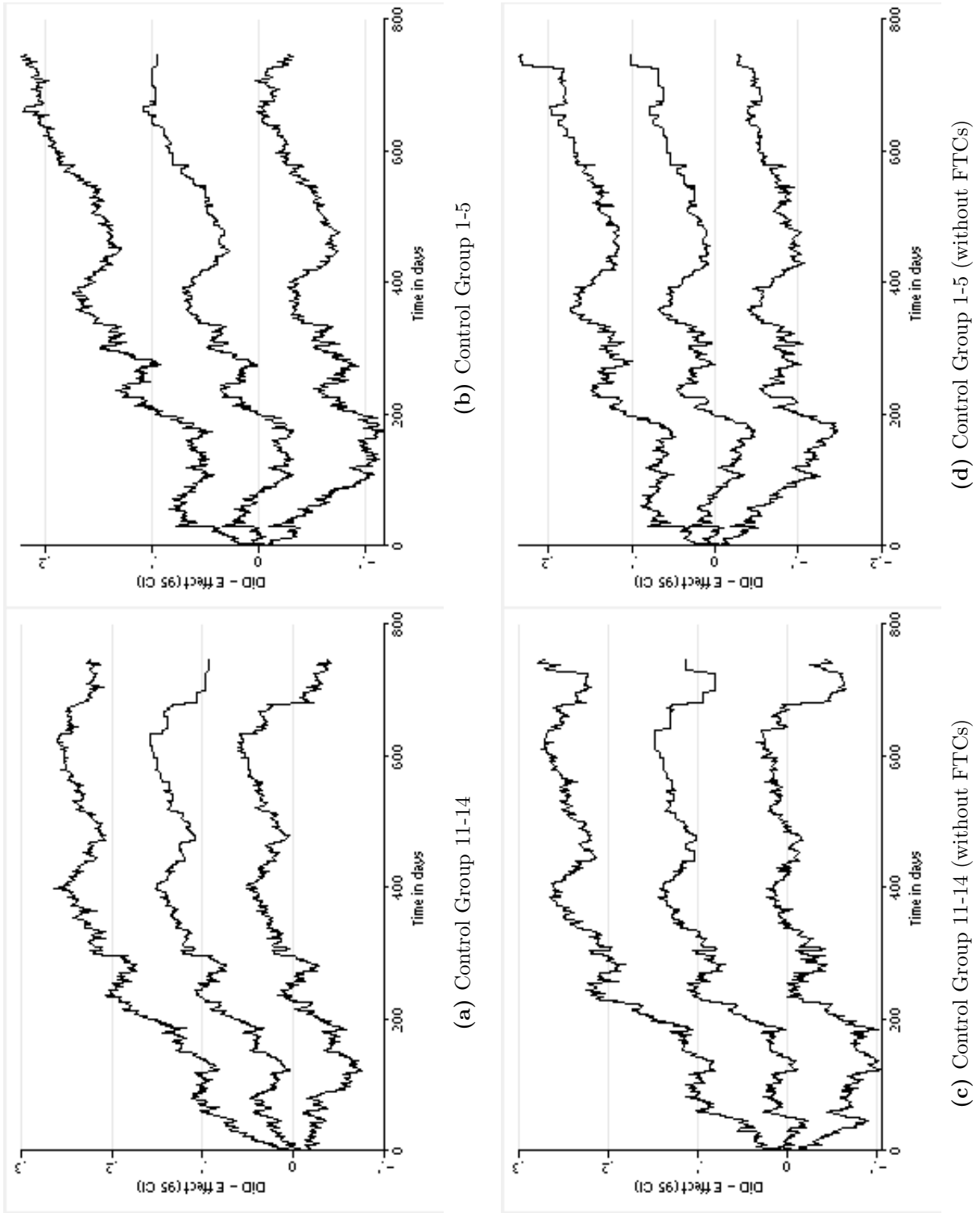**Figure 2:** Kaplan-Meier Curves - spec. (2) and (4)

(a) Control Group 11-14

(b) Control Group 1-5

Figure 3: Difference-in-Differences Effect

(a) Control Group 11-14

(b) Control Group 1-5

(c) Control Group 11-14 (without FTCs)

(d) Control Group 1-5 (without FTCs)

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