Dismissal Protection and Small Firms' Hirings: Evidence from a Policy Reform

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Abstract

Small firms are important drivers of employment creation and innovation. Dismissal protection raises firms' adjustment costs and thus reduces worker flows. In many countries, small firms are exempted from dismissal protection regulations in order to increase their flexibility. This paper exploits a shift in the firm-size threshold of the German dismissal protection law in 2004 to analyze the causal effects of relaxed dismissal protection on the hiring behavior of small firms. Using difference-in-differences techniques, we find positive effects on hirings. Placebo treatment tests based on pre-treatment periods and a fake treatment group confirm the validity of our empirical strategy.

Keywords: dismissal protection, small firms, difference-in-differences

JEL code: J38, K31, M21

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1. Introduction

Small firms take on a role complementary to the role of large, incumbent firms in a market economy. In particular, it is generally accepted that small firms play a major role in employment creation, as well as in the production and commercialization of innovations. Moreover, they produce important spillovers affecting employment growth rates of all firms in a region in the long run (cf. Van Praag and Versloot 2007). Governments have introduced policies that are meant to relieve administrative burdens especially for small firms in order to increase their flexibility. In general, dismissal protection raises firms' adjustment costs and thus reduces worker flows. However, we see small firms being exempted from strict dismissal protection regulations in many European countries.[1] This is also true for Germany, a country generally known for employment protection laws which are rather fierce by international standards (OECD 2004). Still, empirical studies for Germany show mixed results as to whether relaxing dismissal protection regulations for small firms has indeed any positive effects on their flexibility.

This paper adds establishment level evidence to this open debate by exploiting the latest reform of the Protection Against Dismissal Act (PADA) in 2004, which can be considered as a natural experiment. The PADA reform was part of a large reform package called AGENDA 2010, which aimed at reducing Germany's high structural unemployment. With the PADA reform dismissal protection regulation was abandoned for workers who were hired after December 31, 2003 by small establishments with more than five and up to ten full-time equivalent workers. Establishments with more than ten full-time equivalent workers were not affected by this policy change. Firms could not anticipate the details of this PADA reform long before its actual implementation. In November 2003, when Germany's Chancellor Schroeder published a detailed leaflet about AGENDA 2010, the threshold of ten full-time equivalent workers was not mentioned at all. Moreover, the final dismissal protection reform was not approved before December 23, 2003, that is, just about a week before its implementation.

Applying difference-in-differences techniques to a large establishment level dataset, we find positive causal effects of relaxed dismissal protection on small firms' hirings. Since data are available for several years before this legal change, which are "unspoiled" by other such

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[1] Martins (2009), for example, studies the effects of relaxed dismissal protection for small firms in Portugal,

changes, we can perform placebo treatment tests. In other words, we can show that the key identifying assumption of any difference-in-differences approach, namely that the time trends of treatment group (establishments with more than five and up to ten employees) and control group (establishments with more than ten and up to 20 employees) are the same in absence of the treatment, holds at least in the periods preceding the treatment. Furthermore, a second placebo treatment test is applied using a fake treatment group consisting of establishments with more than ten and up to 15 employees, which is then compared to the group of establishments with more than 15 and up to 20 employees. Both groups are not affected by the treatment. The results of this exercise again support the validity of our empirical strategy.

Still, we should be cautious and not over-interpret our results. After all, our findings are based on a representative but restricted sample of German establishments, where data are available only for the first halves of the respective years. Additionally, if firms adjust slowly to the policy change, one might wish to take a more long-term perspective in order to evaluate the general effects of relaxed dismissal protection, instead of focusing on the first one and a half years after the policy change.

The remainder of the paper is organized as follows: Section 2 gives an overview of the relevance of small firms for an economy and the empirical and theoretical literature on dismissal protection. Moreover, the German institutional setting is described in detail. Section 3 introduces our empirical strategy to identify causal effects of relaxed dismissal protection, while Section 4 describes our data. Descriptive statistics and estimation results are given in section 5, including some placebo treatment tests to check the validity of our empirical strategy. Section 6 concludes.

2. Small firms and dismissal protection

2.1 The relevance of small firms for an economy

Starting in the 1980s, we have witnessed a rise in interest in the economics of small firms. In the traditional view, small firms were considered inefficient and costly for the whole economy due to a lack of scale economies (cf. Pratten 1971; Scherer 1973, Weiss 1964). Thus, politicians were advised to take economic activities away from small firms and place them in

whereas Boeri and Jimeno (2005) or Kugler and Pica (2008) analyze these effects for Italy.

the hands of large corporations, which, it was hoped, would lead to an increase in overall economic welfare (Brown et al. 1990, Weiss 1979). However, Audretsch (1995) criticizes this view as misleading since it takes a static perspective only. Rather than small and inefficient replicas of large enterprises, Audretsch considers small firms to be agents of change in a dynamic sense. Applying new, more direct measures of innovation, Acs and Audretsch (1987; 1988; 1990) show that small firms account for a considerable amount of innovative activities. Audretsch (2002) provides empirical evidence that small enterprises were major sources of employment growth and innovation in the resurgence of the U.S. economy in the early 1990s.

Van Praag and Versloot (2007) give an extensive review of empirical research on the value of small firms and entrepreneurs in an economy. They conclude that entrepreneurs account for a disproportionally high share of employment creation although the process of job creation is more volatile than with incumbent firms and wages are somewhat lower. In terms of innovative activity, entrepreneurs are more efficient and produce higher quality innovations than other firms. Baumol (2002) states that, initially, small firms or entrepreneurs might have advantages in coming up with radical innovations. But then, adoption and technical progress is further driven by large routinized firms with specialized research facilities. Falck (2009) revisits this "David-Goliath symbiosis" and finds empirical evidence for Baumol's theory using German manufacturing industry data. When it comes to productivity, Van Praag and Versloot (2007) state that although entrepreneurs' contribution to the productivity level is low, their contribution to value added growth and productivity growth are relatively high. At the same time, small firms produce important spillovers affecting productivity and employment growth rates of all firms in a region (Robbins et al. 2000; Scott 2006).

Thus, one can conclude that small firms take on a role which is complementary to the role of older, bigger, and incumbent firms in an economy. In order to increase small firms' flexibility, governments have introduced policies that are meant to relieve small firms from administrative burdens. One prominent example is dismissal protection, where we have seen small firms being exempted from fierce regulations throughout many European countries.

2.2 The effects of dismissal protection

The effects of employment protection have been rigorously studied since the seminal work by Lazear (1990) who states that under three rather extreme conditions (risk neutral workers, flexible wages, employment protection contains transfer component between employer and employee but no tax component in the favour of a third party), any state-mandated severance pay could be undone by Coasian bargaining between employer and employee. However, if any of these assumptions is relaxed, dismissal protection legislation should have clear effects on labor allocation. In particular, dismissal protection can act like a tax on firing, on the one hand making incumbent workers more likely to retain their jobs, but on the other hand decreasing the chance of new workers being hired. This boils down to ambiguous effects on overall employment levels, depending on the labor demand function, the size of the discount and attrition rates, and the relative sizes of hiring and dismissal costs (Bertola 1992). However, in the long run, dismissal protection might hamper structural change and therefore have rather negative effects on employment (Hopenhayn and Rogerson 1993). Indeed, on a macro level, Lazear (1990), Nickel (1997), and Nickel and Layard (1999) find negative correlations between employment rates and dismissal protection, mainly driven by the elderly, the young, and women. Similarly, Scarpetta (1996) finds that strong dismissal protection increases structural unemployment with particular problems for the young and the permanently unemployed. On the other hand, an OECD study (1999) reports no effects of dismissal protection on unemployment, while positive effects on self-employment are detected.

Whereas we have seen numerous studies on the effects of dismissal protection legislation on aggregate unemployment and employment rates, research on the firm level effects of dismissal protection has emerged only recently. Dismissal protection raises firms' adjustment costs and thus reduces worker flows. Theory suggests that firms' productivity declines due to distorted production choices (Blanchard and Portugal 2001). Autor et al. (2007) find tentative empirical evidence for these theoretical predictions using U.S. firm-level data. Messina and Vallanti (2007) use European firm-level data and show that stricter employment protection laws dampen the response of job destruction to the cycle. Martins (2009) shows that relaxed dismissal protection in Portugal had little effect on hirings but positive effects on firm performance, probably via an increase in workers' efforts. For Italy, Boeri and Jimeno (2005) find positive effects of relaxed employment protection on dismissals whereas no effects are detected for firm growth. Kugler and Pica (2008) exploit both the cross-sectional and temporal variation in the Italian employment protection law and report negative effects of employment protection on accessions and separations for workers in small firms, while they do not find any effects on net employment.

Using German data, Wagner et al. (2001) do not find any effects of the establishment size threshold that determines whether an establishment is bound by dismissal protection regulations. However, they do not exploit any policy changes. Verick (2004) suggests that a tightening of the dismissal protection threshold in Germany in the late 1990s resulted in a lower probability of growth for the treated establishments. This result is gained from the German Establishment Panel which is the same data set also used in our study. Bauer et al. (2007) use a large employer-employee data set, which was especially constructed for their study, and find no effect of variable enforcement of dismissal protection legislation on firms' worker turnover in 1996 and 1999. This paper follows in this tradition of empirical analysis of dismissal protection on the firm level by exploiting the latest relaxation of the dismissal protection law for small German firms in 2004. To the best of our knowledge, the effects of this policy reform have not yet been investigated.

2.3 The German institutional setting

The OECD Employment Outlook report (OECD 2004) lists Germany amongst the countries with the strictest employment protection regulations. In particular, the German Protection Against Dismissal Act (PADA) states that workers cannot be dismissed just to be replaced by another worker. Rather, there are only three possible ways for establishments to justify dismissals: Firstly, dismissals are on a just basis if they are made on grounds of conduct; however, in this case, the employer has to admonish the worker at least once. Secondly, dismissals can be justified on grounds of personal capability, in particular sickness. Here, the employer has to prove substantial times of absence of the employee due to the same kind of sickness. Moreover, he has to reasonably predict a negative future state of health. Thirdly, employers can dismiss workers on operational grounds. However, in this case, the employer has to respect certain social criteria, e.g., tenancy, age, obligations to pay maintenance, or handicaps. If a worker feels he is unjustly dismissed, he can go to court and sue his (former) employer. Indeed, Jahn and Schnabel (2003) estimate that in 2001, 27 percent of all dismissals were appealed against in court with three out of four appeals being successful. If the court decides that the dismissal was unjust, the employer has to reinstate the worker in the firm or pay monetary compensations.[2] Overall, we can observe substantial dismissal costs for

[2] Starting in 2004, in case of dismissals on operational grounds, dismissed workers can choose between appeal to court and a legal compensation payment of half a month's earnings per year employed, but only if this is explicitly pointed out in the letter of notice.

employers in terms of a restricted choice of whom to dismiss, in terms of insecurity about which dismissals are considered “just”, as well as in terms of monetary costs like compensation payments.

However, note that in Germany not all firms are subject to these rules. As in many other European countries, e.g., Portugal, Spain, or Italy, small establishments are exempted from strict dismissal protection regulation. From 1999 until the end of 2003, employees in establishments with up to five full-time equivalent workers were not protected by the German dismissal protection law. In 2004, this threshold was shifted up to ten full-time equivalent workers. While incumbent workers did not lose their prior protection, dismissal protection regulation was now abandoned for workers hired after December 31, 2003 by establishments with more than five and up to ten full-time equivalent employees. This institutional reform was part of AGENDA 2010, a large reform package which aimed at reducing Germany’s high structural unemployment. Although this package included a substantial number of labor market and social policy reforms, there was just one single element in the AGENDA 2010, referring to the establishment size threshold of ten full-time equivalent employees, which we will exploit. Consequently, our analysis of the causal effects of relaxed dismissal protection should not be confounded by other elements of the reform package AGENDA 2010.[3]

The central elements of AGENDA 2010 were first publicly announced in a government policy declaration by former Chancellor Gerhard Schroeder on March 14, 2003. This declaration also contained some passages concerning dismissal protection. In particular, Schroeder mentioned that dismissal protection regulations should be relaxed for small firms; he referred to two alternative models of how this could be reached. However, most importantly, the government declaration of March 14, 2003, did not contain any hint about raising the relevant establishment size threshold from five to ten full-time equivalent employees. Even in November 2003, when Schroeder published another detailed leaflet about AGENDA 2010, this threshold of ten full-time equivalent workers was not mentioned at all. The final dismissal protection reform was not approved until December 23, 2003, that is, just about a week before its implementation. These facts strongly suggest that firms could not anticipate the exact

[3] Some of the reforms affected marginal employment contracts (so-called “mini” or “midi jobs”) or the financial support of hiring unemployed and handicapped persons. However, none of these reforms interferes with the establishment size threshold we will exploit in this paper. Although we do not see any obvious problems here, we should note that these reforms would blur a clean identification of the causal effects of relaxed dismissal protection if establishments with more than five and up to ten employees reacted differently to these policy changes than establishments with more than ten and up to 20 employees.

details of the PADA reform long before its actual implementation. Thus, our estimates should not suffer from a so-called Ashenfelter dip (1978).

3. Identification strategy

When estimating the effects of dismissal protection regulations on small establishments' hiring behavior, the major concern is that unobserved heterogeneity might bias the results. Imagine we use cross-sectional data in a given institutional setting, where one group of establishments is exempted from dismissal protection rules whereas another group is not. Running regressions with a group dummy as explanatory variable will not give us unbiased estimates of the causal effect of dismissal protection as long as unobserved characteristics are correlated with the group variable and at the same time with the outcome variable. To overcome this omitted variable bias, one might think of including a variety of control variables. However, due to data constraints and lack of theory it is highly unlikely that empirical researchers can actually address these endogeneity concerns by simply adding further covariates. There will probably still be unobserved heterogeneity between the groups, making the estimated group coefficient only a biased estimate of the true causal effect of dismissal protection.

In this paper, we exploit the discontinuous increase in the legal establishment size threshold below which establishments are exempted from dismissal protection laws. The shift of this threshold from five to ten full-time equivalent workers can be considered as a natural experiment in order to estimate the causal treatment effect of relaxed dismissal protection for firms that employ more than five but less than ten full-time equivalent workers. We define this group of establishments as our treatment group and compare them to a control group comprised of establishments slightly above the legal threshold, that is, with more than ten but less than twenty full-time equivalent workers.

We follow treatment and control group over time and apply difference-in-differences techniques, allowing for time-invariant unobserved heterogeneity between treatment and control group. In this difference-in-differences framework, the first differences are the within-group differences in the outcome variable over time, whereas the second difference is the across-groups difference of the first differences. This identification strategy yields unbiased estimates of the treatment effect under the assumption that the underlying trends in the outcome variable are the same for both the treatment and the control group in the absence of the treatment (see, e.g. Angrist and Pischke 2009). In other words, there are no time-variant unobserved determinants of the outcome variables that cause differential effects on the treatment group and the control group. In mathematical terms, the treatment effect δ is given by the following equation:

$$\begin{aligned} \delta &= E[Y_i(1)] - E[Y_i(0)] = \\ &\{E[Y_i \mid Z_i = 1, D_i = 1] - E[Y_i \mid Z_i = 0, D_i = 1]\} - \\ &\{E[Y_i \mid Z_i = 1, D_i = 0] - E[Y_i \mid Z_i = 0, D_i = 0]\}, \end{aligned} \tag{1}$$

where $E[\cdot]$ is the expectation operator, and $Y_i(1)$ is the outcome variable for establishment i under relaxed dismissal protection, while $Y_i(0)$ is the outcome variable for establishment i under conventional dismissal protection. D_i is a binary variable which takes the value of unity if an establishment i belongs to the treatment group, and is 0 for observations belonging to the control group. Z_i is a dichotomous variable which is unity for any observations of an establishment i after dismissal protection was relaxed in 2004, and which is zero for observations made before the implementation of the modified dismissal protection law.

A simple way to estimate the treatment effect δ in a regression framework is given by Equation 2, where the difference-in-differences parameter δ is the coefficient on the interaction term of the group and time dummies D_i and Z_t.

$$Y_{it} = \alpha + \beta Z_t + \gamma D_i + \delta(Z_t D_i) + \varepsilon_{it} \tag{2}$$

α is the intercept, β captures common time effects, and γ accounts for level differences in the outcome variable between treatment and control group; ε_{it} is the error term. In general, this equation can be estimated by using ordinary least square methods. However, ignoring serial correlation might bias the estimated standard errors downward, in particular when using long time series data. Since our difference-in-differences approach uses data from three years only, namely one pre-treatment year 2003 as the baseline and the two post-treatment years 2004 and 2005, serial correlation might not be a major problem. Still, we are conservative and cautious in our estimations and try to take account of serial correlation problems. By running simulations, Bertrand et al. (2004) find that clustering robust standard errors, which take into account that the error terms ε_{it} might correlate within group cells (cf. Rogers 1993), can overcome this bias as long as the number of clusters is reasonably large. We take this finding into account and run all our regressions with standard errors clustered at the individual establishment level; this means we allow for correlated standard errors within establishments over time.

Restricting the sample to those observations right below and those observations right above the threshold value is a first important step to make sure the key identifying assumption of our empirical approach holds, namely that the treatment and control group follow the same time trend in absence of the treatment. Of course, we face a trade-off: On the one hand, we would like to have observations as close to the threshold as possible; on the other hand, we would like to have a large number of observations. To take this trade-off into account, we restrict our control group to establishments with no more than 20 full-time equivalent employees; the treatment group consists of establishments with more than five and up to ten full-time equivalent employees. Since these bins are still rather wide, we apply several strategies to ensure the parallel time trend assumption is met and our findings are robust. First, we additionally include a set of control variables in our difference-in-differences framework. In a further robustness check, we restrict the control group to include establishments with less than fifteen full-time equivalent workers only. Third, we explicitly investigate whether treatment and control group follow the same time trend in the years preceding the policy change. In particular, we use the pre-treatment years from 2001 until 2003, which are "unspoiled" by other legal changes, and test whether the time trends of treatment and control group are different from each other. Let us now define $Z_i = -1$ as an observation of establishment *i* in 2001, while $Z_i = 0$ denotes an observation of establishment *i* in 2002 or 2003. If treatment and control group indeed follow the same time trend in the years preceding the treatment, it should be true that

$$\begin{aligned}&\{E[Y_i|Z_i=-1,D_i=1]-E[Y_i|Z_i=0,D_i=1]\}-\\&\{E[Y_i|Z_i=-1,D_i=0]-E[Y_i|Z_i=0,D_i=0]\}=0.\end{aligned} \tag{3}$$

Whether this equation holds can be econometrically tested in a difference-in-differences framework where we use the pre-treatment years from 2001 until 2003 and introduce a placebo treatment in 2002. If the difference-in-difference coefficients for 2002 and 2003 are significantly different from zero, this brings into question the key identifying assumption of our empirical approach, namely that the time trends of treatment and control group are the same in absence of the treatment.

4. Data

The empirical test on the effects of relaxed dismissal protection on small establishments in Germany is based on data from the IAB Establishment Panel Survey.[4] The IAB establishment panel is a dataset of the demand side of the German labor market. Establishments listed in the German Social Insurance Statistics[5] represent the population of the IAB establishment file. The establishments are selected according to the principle of optimum stratification of the random sample, where the stratification cells are defined by firm size categories and industries.

The annual surveys of the IAB establishment panel started in 1993 with 4,265 establishments from former Western Germany. In 1996, establishments from former Eastern Germany entered the survey for the first time. Nowadays, data on nearly 16,000 establishments are collected every year. The surveys include a fixed battery of questions, which are repeated every year, and a variable part, which comprises questions with annually changing topics. As already mentioned, the PADA does not refer to firms but to establishments, which are the units of measurement in the IAB establishment panel. "Establishments" in that sense can be either firm headquarters or subsidiaries. For a more detailed description of this data source, see Bellmann (2002).

To analyze the effects of the latest PADA reform in January 2004, data from the IAB establishment panel for the years 2001 to 2005 are used. All observations of the public administration and non-profit sectors as well as all private households are dropped since these sectors differ from private firms with regard to their hiring behavior. Furthermore, the highly subsidized agricultural and mining sectors are excluded. As to the dependent variable, we use hiring rates as well as the total numbers of hirings per establishment in the first half of a year. The respective figures for the second half of a year are not available in the IAB establishment

[4] Access to the data was granted via controlled data teleprocessing at the Research Data Centre of the German Federal Employment Agency (BA) at the Institute for Employment Research (IAB).

[5] Every employer in Germany has to report information about every employee subject to obligatory social insurance to the German Social Insurance Statistics.

panel.[6] Unfortunately, we also do not have any information whether the newly hired workers are employed on a full-time or part-time basis.

The establishment size classes are computed as close to the PADA regulations (§23 I KSchG) as possible. We take the total number of staff, as reported, on June 30, and subtract apprentices and establishment owners because these should not be included in the establishment size figure according to §23 I KSchG. In a second step, the number of staff hired within the first half of the year is subtracted while the number of staff who left the establishment within the first half of the year is added. Since the PADA regulations refer to full-time equivalent workers, the resulting figure is weighted by a full-time equivalent factor *f* which is computed by

$$f = \frac{e_n - \left(0.5 * e_p\right)}{e_n}, \tag{5}$$

where e_n is the total number of employees and e_p stands for the number of part-time employees. Since there is no information on the exact number of working hours for every part-time employee, a global weight of 0.5 is assumed, which is considered appropriate at least for Germany (cf. Troost and Wagner 2002).[7] Thus, we arrive at the establishment size that can be regarded as being relevant according to the PADA regulations described in §23 I KSchG. We drop establishments with less than five and more than 20 PADA relevant employees at the beginning of the year. Furthermore, establishments with more than 20 hirings or dismissals and/or hiring rates greater than two in the first half of a year are dropped to make sure our results are not driven by outliers. Then, we create a balanced panel for the period from 2001 to 2005.

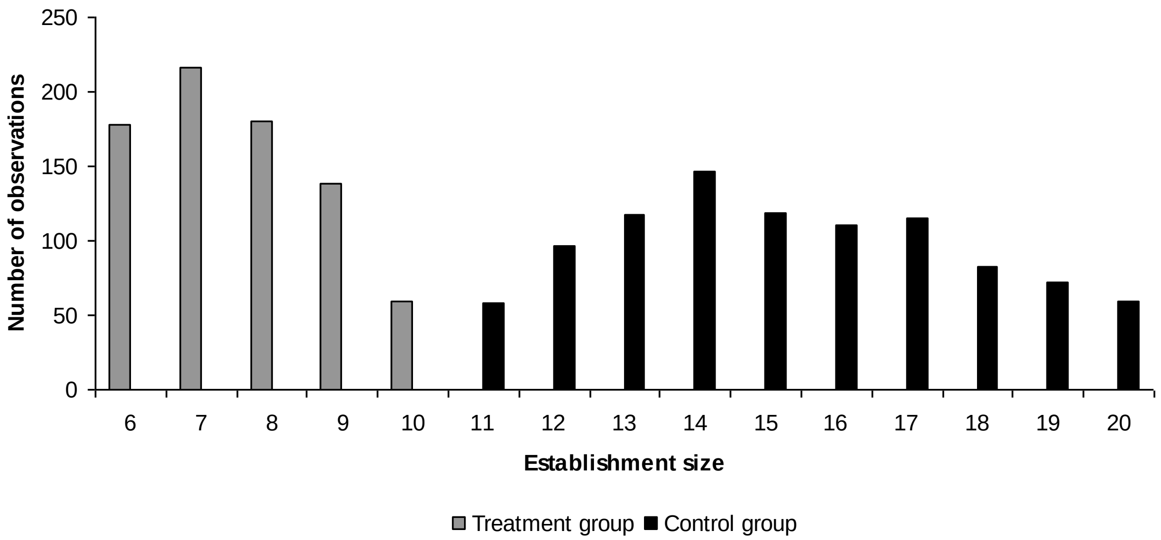
Our treatment group comprises establishments that consistently employ between five and ten full-time equivalent workers in the years preceding the treatment. Similarly, in the control group we include establishments that consistently employ between ten and 20 full-time equivalent workers in this period. Thus, we abstract from establishments that frequently change size classes in order to reach a clear definition of treatment and control group. Last but

[6] As long as any seasonal patterns of hirings are similar across treatment and control group, this does not hamper the identification of our treatment effects. However, even if they were different across these groups, as long as the difference would not vary over time, this would pose no identification problems due to our double differences approach.

[7] According to §23 I KSchG, employees working not more than 20 hours a week are weighted by a factor 0.5, whereas employees working more than 20 hours but not more than 30 hours are weighted by 0.75. Slight

not least, we drop observations that are right at the threshold in the treatment years (cf. Martins 2009). First of all, this should help to exclude firms that strategically adjust their size in order to benefit from the treatment. Note that if this strategic behavior is present in the market at all, it is firms right at the threshold which should be most likely to display this kind of behavior. Additionally, dropping observations right at the threshold minimizes measurement error, given that the calculation of the PADA relevant firm size is somewhat complicated due to the legislation and the data at hand. The final size distribution of establishments in the pre-treatment years is depicted in Figure 1. Our outcome variable of main interest is small firms' hiring behavior. In the years from 2003 to 2005, which is the observation period of main interest, the average number of hirings was 0.527 with a standard deviation of 1.280. We also construct hiring rates by dividing the number of hirings in the first half of the respective year by the number of employees at the beginning of that year. By proceeding in this way, we implicitly assume that the average fraction of part-time employees is the same among incumbent and newly-hired employees of a specific establishment. This assumption is necessary due to data limitations. The average hiring rate from 2003 to 2005 was 0.035 with a standard deviation of 0.081. More detailed descriptive statistics on our sample can be found in Tables A.1, A.2, and A.3 of the Appendix.

Figure 1: Establishment size distribution



Note: The figure shows the distribution of establishment size measured by the number of full-time-equivalent workers. The data are pooled over the pre-treatment years 2001 until 2003. The lower numbers of observations around the threshold are due to our sample restrictions explained in the text. Data: IAB Establishment Panel.

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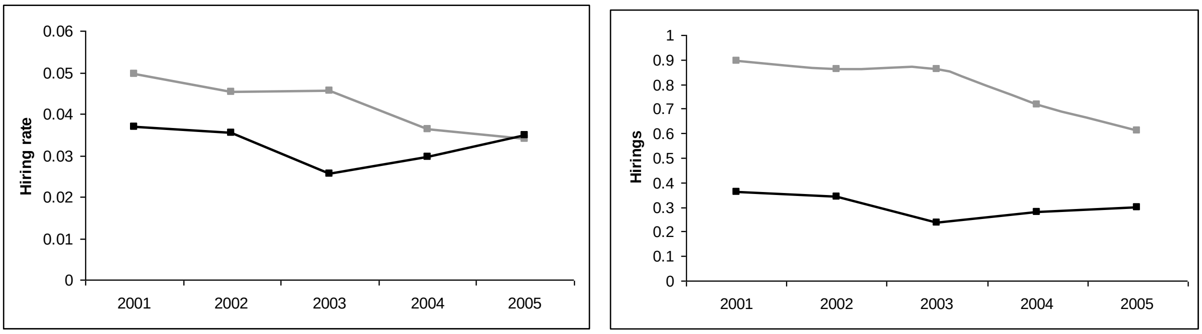
modifications of our global weight do not affect the results.

5. Empirical results

Before going into the details of the regression results, we dwell on some descriptive statistics to give a first impression of the data. Figure 2 depicts the dynamics of hiring rates (left figure) as well as the absolute numbers of hirings (right figure) for treatment group (black line: establishments with more than five and up to ten workers) and control group (grey line: establishments with more than ten and up to 20 workers). As our outcome variable, we prefer hiring rates to absolute numbers of hirings since hiring rates abstract from any potential establishment size effects.

Figure 2: Dynamics of hirings in treatment and control group



Note: The left figure shows average hiring rates for treatment (black line) and control group (grey line) over time. The right figure shows average absolute hirings for treatment (black line) and control group (grey line) over time. The treatment group comprises establishments with more than five and up to ten full-time equivalent workers while control group consists of establishments with more than ten and up to 20 full-time equivalent employees. Data: IAB Establishment Panel.

Looking at the left figure, we observe that hiring rates are somewhat smaller in the treatment group in the pre-treatment years. It is nice to see that hiring rates of treatment and control group follow similar time trends in the years preceding the treatment. Then, right after dismissal protection was relaxed in 2004, hiring rates of the treatment group start converging to the level of the control group. The right figure depicts the respective time trends for the average absolute numbers of hirings. For all years, the average absolute numbers of hirings are larger for the control group than for the treatment group. But also in this figure, time trends of treatment and control group look very similar in the pre-treatment years, whereas we see some convergence in trends after dismissal protection was relaxed for the treatment group in 2004. Thus, both figures provide first evidence that relaxing dismissal protection could have had positive effects on the number of hirings in small firms. The fact that hirings in the treatment group steadily converge after the treatment could be a sign of information lags which might lead to a retarded reaction of treated firms to the new legislation.

The picture drawn in Figure 2 is mirrored in Table 1, where we present difference-in-differences results for hiring rates.[8] In Column (1), we start with plain difference-in-differences estimations where we do not include further control variables. This boils down to a regression framework representation of the left graph of Figure 2. Establishments with more than five and up to ten full-time equivalent workers (treatment group) show on average lower hiring rates than establishments with more than ten and up to 20 full-time equivalent workers (control group). Moreover, we observe a common negative time trend. Most importantly, the difference-in-differences coefficients suggest a positive and significant effect of relaxed dismissal protection on hiring rates in 2004 (DiD 2004) as well as in 2005 (DiD 2005) and thus confirm the first impressions gained from Figure 2.

Table 1: DiD estimates on hirings

	Hiring rate (1)		Hiring rate (2)		Hiring rate (3)	
	coeff.	std. err.	Coeff.	std. err.	coeff.	std. err.
DiD 2004	.013 **	.007	.016 **	.007	.020 **	.009
DiD 2005	.021 **	.009	.021 **	.009	.020 **	.009
Treatment group	-.020 ***	.006	-.019 ***	.007	-.024 ***	.006
Year 2004	-.009 *	.004	-.013 ***	.004	-.014 ***	.006
Year 2005	-.012 **	.005	-.013 ***	.005	-.013 *	.006
Control set 1	No		Yes		Yes	
Control set 2	No		No		Yes	
N	1,749		1,658		1,285	
R^2	0.0059		0.0725		0.1197	

Note: The table reports the results of OLS difference-in-differences regressions with hiring rates as the dependent variable. The treatment group comprises establishments with more than five and up to ten full-time equivalent workers while control group consists of establishments with more than ten and up to 20 full-time equivalent employees. The baseline year is 2003. Specification (1) includes no further controls. In specifications (2), we additionally control for capital stock, works council, collective labor agreement, age, and industry (Control set 1). In specifications (3), we add as further controls the ratio of female workers, ratio of unqualified workers, ratio of apprentices, wage per worker in the previous year, value added per worker in the previous year as well as net hirings in the previous year (Control set 2). Standard errors are clustered at the establishment level. ***, **, * denotes significance at the 1%, 5%, and 10% level. Data: IAB Establishment Panel.

Although these figures give a first impression of the effects of the PADA change in Germany, they should be interpreted with caution. To be able to make more rigorous statements about the impact of relaxed dismissal protection regulations on the hiring behavior of small establishments, we now turn to extended difference-in-differences regressions where we control for several firm level characteristics. The positive treatment effects are not affected by the inclusion of further controls. This can be observed in Column (II), where we control for

[8] The results for the absolute number of hirings are not qualitatively different and are available from the authors upon request.

several establishment characteristics, such as an establishment's capital stock, the existence of a works council, the founding year of an establishment, information on whether the establishment is bound by a collective labor agreement as well as an extensive set of industry dummies. In Column (III), we additionally control for the ratio of female workers, the ratio of unqualified workers, and the ratio of apprentices, as well as the wage per worker in the previous year, the value added per worker in the previous year and net hirings in the previous year; all these variables are measured at the establishment level. Although the number of observations decreases with the inclusion of this wide range of controls due to random missings, we clearly see that the effect of relaxed dismissal protection on the hiring behavior of small establishments remains positive and significant. The difference-in-differences coefficient for 2004 becomes even larger, while for the year 2005 the point estimate of the treatment effect is hardly affected at all by the inclusion of all these controls.[9] This means that although these control variables increase the R^2, they are not confounding factors in a way that would conflict with our key identifying assumption and bias our difference-in-differences estimates.

Taking the most conservative estimates out of Table 1, the coefficients suggest that relaxed dismissal protection resulted in an increase in hirings that corresponds to 1.3 percent (2004), respectively 2 percent (2005), of all workers employed by establishments of the treatment group. Back of the envelope calculations using aggregate statistics of the establishment file of the Federal Employment Agency suggest that relaxing dismissal protection led to an overall increase in hirings by roughly 30,000 workers in 2004, and 45,000 workers in 2005.

So far, our control group consists of establishments with more than ten and up to 20 full-time equivalent workers. Since this bin might seem too large for our purpose, namely comparing this control group to the treatment group of establishments with more than five and up to ten full-time equivalent workers, we now restrict the control group to include establishments with less than fifteen full-time equivalent workers only. Even if this reduces our sample size, it is still interesting to investigate whether our findings also hold in this more conservative specification. Table 2 depicts the results of this exercise. The positive treatment effect on small establishments' hirings is fully confirmed already in Column (1) where no further control variables are included. Again, adding the two sets of control variables used in the earlier regressions does not affect our main findings as can be observed in Columns (2) and

[9] The detailed results of this regression can be found in Table A.4 of the Appendix.

(3) of Table 2. Comparing the difference-in-differences coefficients to the ones in Table 1, we see that, in particular for the year 2004, the estimated treatment effects are even larger with this restricted sample. Thus, we can provide first evidence for the robustness of our findings.

Table 2: DiD estimates using a more restricted control group

	Hiring rate (1)		Hiring rate (2)		Hiring rate (3)	
	coeff.	Std. err.	coeff.	std. err.	coeff.	std. err.
DiD 2004	.023 ***	.009	.024 **	.010	.031 ***	.011
DiD 2005	.026 **	.011	.028 **	.012	.024 **	.012
Treatment group	-.017 **	.008	-.014	.010	-.024 ***	.009
Year 2004	-.019 ***	.007	-.022 ***	.007	-.026 ***	.008
Year 2005	-.017 **	.008	-.020 **	.009	-.018 *	.010
Control set 1	No		Yes		Yes	
Control set 2	No		No		Yes	
N	1,080		1,023		777	
R^2	0.0052		0.0613		0.1008	

Note: The table reports the results of OLS difference-in-differences regressions using a more restricted control group that consists of establishments with more than ten and up to 15 full-time equivalent employees. The treatment group comprises establishments with more than five and up to ten full-time equivalent workers. The baseline year is 2003. Standard errors are clustered at the establishment level. ***, **, * denotes significance at the 1%, 5%, and 10% level. Data: IAB Establishment Panel.

The key identifying assumption of any difference-in-differences approach is that the time trend for the treatment and control group are the same in the absence of the treatment. This necessitates amongst other things that establishments in both groups react similarly to the business cycle. Bauer et al. (2007), who investigate the effects of earlier PADA changes on the hiring and firing behavior of small German establishments, try to verify this assumption by investigating bankruptcy rates of small establishments over time. Looking at these data, they find that establishments with more than five and up to ten employees react to the business cycle in a very similar way to the control group of ten to 20 employees. Therefore, they conclude that this is supportive of the difference-in-differences assumption of same time trends in absence of the treatment. Of course, the same time trend assumption is not rigorously testable. However, the finding of Bauer et al. (2007) is at least encouraging.

We go one step further and use several pre-treatment periods that are “unspoiled” by other dismissal protection reforms in order to directly test whether the trends for treatment and control group are similar to each other, at least in the years preceding the treatment. Figure 2 has already given a graphical intuition. Now, we analyze the pre-treatment periods in a difference-in-differences regression framework. In particular, we introduce a placebo treatment in 2002 to analyze placebo treatment effects in the years 2002 and 2003.

Technically, this means that we introduce interaction terms of treatment group and the years 2002 and 2003 as well as the respective treatment group and year dummies, where 2001 is the year preceding the placebo treatment and as such is the omitted category. If the coefficients on the interactions are significantly different from zero, this questions the key identifying assumption of the difference-in-differences approach since it would suggest our placebo treatment had effects. In other words, the same time trend assumption would be violated already in the periods prior to the actual policy reform in 2004. The left panel of Table 3 presents the results of this placebo treatment specification. It is encouraging that the coefficients of the interaction terms are statistically not different from zero; even further, they are very close to zero. This shows that the treatment and control groups are indeed subject to the same time trends in the years preceding the treatment, which makes us confident that the key identifying assumption of the difference-in-differences strategy is met.

Table 3: DiD estimates using placebo treatment tests

	Hiring rate (1)			Hiring rate (2)	
	coeff.	std. err.		coeff.	std. err.
DiD 2002	.003	.008	DiD 2004	.008	.011
DiD 2003	-.007	.008	DiD 2005	.000	.014
Treatment group	-.013 **	.006	Treatment group	.007	.012
Year 2002	-.005	.006	Year 2004	-.008	.007
Year 2003	-.004	.005	Year 2005	-.006	.009
N	1,749		N	378	
R^2	0.0114		R^2	0.0105	

Note: The left panel reports the results of OLS difference-in-differences regressions with placebo treatments in the pre-treatment years 2002 and 2003. The baseline year is 2001. The treatment group comprises establishments with more than five and up to ten full-time equivalent workers while control group consists of establishments with more than ten and up to 20 full-time equivalent employees. The right panel reports the results of OLS difference-in-differences regressions with a placebo treatment group that comprises all establishments with more than ten and up to 15 full-time equivalent employees. The control group consists of establishments with more than 15 and up to 20 full-time equivalent employees. The baseline year is 2003. Standard errors are clustered at the establishment level. ***, **, * denotes significance at the 1%, 5%, and 10% level. Data: IAB Establishment Panel.

In a further robustness check, we conduct yet another placebo treatment test by analyzing the actual post-treatment years but introduce a placebo treatment group. To this end, we take all establishments with more than ten but less than 15 full-time equivalent workers and compare them to establishments with more than 15 and up to 20 full-time equivalent workers.[10] Both

[10] We also ran placebo treatment tests taking all establishments with more than 20 but less than 30 full-time equivalent workers and compare them to our control group consisting of establishments with more than ten and up to 20 full-time equivalent workers. The number of observations increases to 1,308; yet, the results are not qualitatively different.

groups were not affected by the relaxation of dismissal protection in 2004. If we argue that these groups follow similar time trends, we should not find any significant difference-in-differences coefficients because there is no actual treatment. And indeed, the right panel of Table 3 shows that there are no treatment effects for the placebo treatment group. In the years after the policy change, the difference-in-differences coefficients are not statistically significant and close to zero. This is another piece of evidence for the validity of our difference-in-differences estimation strategy.[11]

Finally, we apply nearest neighbour propensity score matching techniques in order to further enhance the similarity of treatment and control group. Although not presented here, the difference-in-differences estimations on the matched sample do not yield results different from the ones obtained so far. These estimation results are available from the author upon request.

Additional Results

Dismissal protection is relaxed only for workers hired after 31st December 2003. Generally, in Germany there is no dismissal protection during the probation period of the first six months. Consequently, if we look at dismissals as the outcome variable, the first half of 2004 is not a treatment year. Given that 2004 is a non-treatment year, the period of observation from 2001 to 2005, which is used in this paper, only allows us to look at a single post-treatment year, namely 2005. This time frame is too narrow to draw any strict conclusions on the effects of relaxed dismissal protection on dismissals. Still, we would like to give at least some impression of short-run effects of relaxed dismissal protection on dismissals. To this end, dismissal rates are constructed analogously to hiring rates.[12] Applying difference-in-differences techniques, we observe no significant short-run effects of the policy reform on the dismissal behaviour of small firms in 2005. This finding does not change with the inclusion of further controls and also holds when using a more restricted control group consisting of establishments with more than ten and up to 15 full-time equivalent employees. Furthermore, placebo treatment tests suggest that the time trends in dismissal behaviour of treatment and control group are not significantly different in the pre-treatment years. We cautiously

[11] At the same time, this result suggests that we can rely on our main difference-in-differences findings from Table 1 and do not necessarily need to restrict our control group as in Table 2. This is because the entire control group shows homogenous time trends.
[12] The dataset allows us to identify the number of workers who left an establishment because they retired or died.

conclude that the relaxation of the dismissal protection law did not lead to more dismissals in the very short run. Still, the time span is too short to draw any final conclusions on the effects of relaxed dismissal protection on the dismissal behaviour of small firms.

In a final note, we should mention that we also tried to assess the causal impacts of relaxed dismissal protection on further outcome variables such as value added per worker, wage per worker, the ratio of female workers or the ratio of unqualified workers. However, our results do not paint a clear picture here. This does not mean that the policy change did not have any effects on these variables. Rather, it might be that firms adjust step by step and the full adjustment process was not finished by mid 2005, just one and a half years after the PADA relaxation. In order to make more rigorous statements about general effects, we should probably allow for a larger time span after the PADA modification.

6. Conclusions

Small firms account for a substantial share of employment creation and innovative activities. In order to enhance the flexibility of small firms, many countries have introduced laws that exempt small firms from strict dismissal protection regulations. Using the latest shift in the firm size threshold of the German dismissal protection law in 2004, this paper has tried to find causal effects of the policy change on the hiring behavior of small firms. Applying difference-in-differences techniques, we find positive effects of relaxed dismissal protection legislation on hirings of small firms. In order to test the validity of our empirical approach, we have applied placebo treatment tests based on pre-treatment periods and a fake treatment group. Both tests have provided additional support for our findings. We could not find any robust evidence for an impact of relaxed dismissal protection on other outcome variables such as dismissals, the ratio of female workers, the ratio of unqualified workers, value added per worker, or wage per worker.

It is important to point out that all these results should be viewed with caution. First, we would like to stress that our results only hold for the policy reform in 2004 which is analysed in this paper. Similar earlier reforms in Germany do not seem to have had any effects (Bauer et al. 2007). If firms adjust slowly to the policy change, we should probably allow for a longer time span after the policy change in order to make more rigorous statements about the general

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These individuals are not included in our dismissal variables.

effects of relaxed dismissal protection, which is particularly true for dismissals. Therefore assessing longer-term effects seems interesting but also comes at a cost, namely that the key assumption of the difference-in-differences approach that the time trend is the same for treatment and control group in absence of the treatment might become more fragile the further we move away from the point in time when the policy change took place; additionally, serial correlation becomes a more severe problem. Furthermore, we should be aware that our analysis is based on a restricted sample of establishments and that our data only include information for the first half of every respective year. Moreover, our analysis does not allow for any statements about the quality of the jobs that relaxed dismissal protection created. It would be particularly interesting to see whether these jobs are merely short-term or whether they are really stable so that the newly hired workers can indeed profit from long-term employment relationships. What is more, one might wonder whether workers who (in contrast to their colleagues in the same establishment) do not benefit from dismissal protection are given any compensatory rewards by the employers. Finally, we should take into consideration that our analysis does not tell us anything about how larger firms would react to relaxed dismissal protection. These questions certainly require more research but could not be tackled in this paper.

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Appendix

Table A.1: Summary descriptives

		N	Percent
Group			
	Treatment group	771	44.08
	Control group	978	55.92
		1,749	100.00
Founding year			
	Before 1990	1,062	60.89
	1990 – 1995	517	29.64
	1996 – 2000	14*	/
	After 2000	/	/
		1,744	100.00
Collective labor agreement			
	None	925	53.01
	Branch level	728	41.72
	Firm level	92	5.27
		1,745	100.00
Establishment council			
	No	1,523	89.33
	Yes	182	10.67
		1,705	100.00

Note: Pooled data from 2003 to 2005. / and * signify anonymized data. Data: IAB Establishment Panel.

Table A.2: Distribution of establishments across federal states

Federal state	N	Percent
Berlin West	51	2.92
Schleswig-Holstein	/	/
Hamburg	2*	/
Lower Saxony	153	8.75
Bremen	30	1.72
Northrhine-Westfalia	162	9.26
Hesse	120	6.86
Rhineland-Palatinate	108	6.17
Baden-Wuerttemberg	141	8.06
Bavaria	111	6.35
Saarland	48	2.74
Berlin East	54	3.09
Brandenburg	102	5.83
Mecklenburg-Western Pomerania	99	5.66
Saxony	183	10.46
Saxony-Anhalt	141	8.06
Thuringia	204	11.66
Total	1,749	100.00

Note: Pooled data from 2003 to 2005. / and * signify anonymized data. Data: IAB Establishment Panel.

Table A.3: Distribution of establishments across industries

Industry	N	Percent
Food, beverages and tobacco	60	3.43
Textile	/	/
Paper and printing	44	2.52
Wood working	41	2.34
Chemical industry	30	1.72
Plastics and rubber	21	1.20
Earths, stones and fine ceramics	24	1.37
Metal production	29	1.66
Recycling	/	/
Light metal construction	71	4.06
Engineering	69	3.95
Road vehicle construction	/	/
Other vehicle construction	/	/
Electrical engineering	34	1.94
Fine mechanics and optics	61	3.49
Furniture, jewellery and toys	29	1.66
Building	90	5.15
Finishing trade	184	10.52
Vehicle trade and garage	125	7.15
Wholesale trade	102	5.83
Retail	144	8.23
Transport	40	2.29
Telecommunication	/	/
Finance	30	1.72
Insurance	/	/
Data processing	/	/
Research and development	/	/
Legal advice and advertising	72	4.12
Real estate services	26	1.49
Leasing and renting	57	3.26
Restaurants and accomodation	51	2.92
Education	87	4.97
Health and veterinary	111	6.35
Sanitation	/	/
Culture, sports and entertainment	/	/
Other services	/	/
Total	1,749	100.00

Note: Pooled data from 2003 to 2005. / signifies anonymized data. Data: IAB Establishment Panel.

Table A.4: DiD estimates on hirings – full specification

		Hiring rate	
		coeff.	std. err.
DiD 2004		.020 **	.009
DiD 2005		.020 **	.009
Treatment group		-.024 ***	.006
Year 2004		-.014 ***	.006
Year 2005		-.013 *	.006
Constant		.111 ***	.036
Works council		-.013 **	.006
Collective labor agreement			
(omitted category: none)			
	Branch level	-.005	.004
	Firm level	-.009	.008
Founding year			
(omitted category: before 1990)			
	1990 - 1995	.010 **	.005
	1996 - 2000	.015	.011
	After 2000	-.007	.010
Log capital stock(*100)		.077 **	.035
Avg ratio of female workers		-.011	.012
Avg ratio of unqualified workers		.014	.010
Avg ratio of apprentices		-.027	.029
Log wage per worker (in t-1)		-.002	.005
Log value added per capita (in t-1)		-.004	.003
Net hirings (in t-1)		.008	.005
Industry dummies		Yes	
N		1,285	
R^2		0.1197	

Note: The table reports the results of OLS difference-in-differences regressions with hiring rates as the dependent variable. The treatment group comprises establishments with more than five and up to ten full-time equivalent workers while control group consists of establishments with more than ten and up to 20 full-time equivalent employees. The baseline year is 2003. Standard errors are clustered at the establishment level. ***, **, * denotes significance at the 1%, 5%, and 10% level. Data: IAB Establishment Panel.