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ABSTRACT

Threshold Effects of Dismissal Protection Legislation in Germany*

In this paper we use a reform in the applicability of the Protection Against Dismissal Act or Kündigungsschutzgesetz in Germany to identify employment effects of the legislation for small establishments. Using a panel of establishments for the period 1997-2001, we find some evidence that a tightening of the threshold resulted in a higher probability of persistence and a lower probability of growth for the units in the treatment group during the post-reform years. When using the establishment size as according to the legislation, the effects were the strongest for establishments which were further away from the threshold. This result could reflect the presence of other institutional barriers or adjustment costs than those imposed by the Act. Alternatively, small establishments are unlikely to have perfect information regarding the applicability of the legislation. This is somewhat supported by the results when taking all employees as the measure for the establishment size. However, there is also some evidence that these results are being driven by other factors than the reform of the legislation.

JEL Classification: J21, J23, J58

Keywords: dismissal protection legislation, difference-in-differences, labour demand

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1 Introduction

Protection of workers from dismissal are a prominent feature of labour market regulations in Germany, as it is in much of the European Union. According to the OECD, Germany has the 7th strictest employment protection legislation, following amongst others, Portugal, Spain, Italy and France (OECD 1999). With the return to persistently high levels of unemployment in recent years, there has been a discussion over the need to relax labour laws in Germany, including the *Kündigungsschutzgesetz* or Protection Against Dismissal Act (PADA). However, while there has been considerable debate amongst politicians, unions, employer groups and academics of this issue, there is surprisingly little empirical research into the impact of the PADA on small establishments in Germany, though this has not prevented any of the interested parties to make claims regarding the impact of future reforms to the legislation.

To contribute to this important debate, we use a reform in the applicability of the dismissal protection legislation to identify the employment effects for small German establishments. In October 1996, the threshold for the applicability of the PADA was raised from 5 to 10 workers by the then Chancellor Helmut Kohl. After being elected in January 1999, the new centre-left government reduced this threshold back to 5 workers. This implies that the establishments with 5 or less workers had the ability to grow above the old threshold from October 1996 to January 1999, without becoming subject to the legislation, and then subsequently faced a more stringent environment after the second reform in 1999. Establishments at the threshold and those with less workers this level are not subject to the firing regulations of the law. This quasi-natural experiment provides us with a source of identification of the effects of the threshold on small establishments.

To investigate the effects of the reform on labour adjustment, we employ a difference-in-differences estimator to look at the impact of the reform on the probability of changing employment levels. Since the legislation potentially imposes firing costs on affected establishments, we would expect that units at or near the threshold to have a higher probability of not changing employment, and similarly, a lower probability of increasing employment above the threshold level after the reform in January 1999. Secondly, identification of the effects of the changes in the legislation

depends on the appropriate allocation of establishments to the treatment and control groups. Thus, we also discuss how different classification of establishments and use of alternative sub-samples from the data affect the estimates. We focus specifically on whether or not there is evidence of threshold effects. That is, whether the effects of the reform were the strongest for the establishments at the threshold level, those units with 5 workers. Important issues such as whether establishments substitute secondary workers for regular employees will be explored in other research. Moreover, it is well documented that firms in countries with high levels of employment protection such as Germany adjust hours more frequently than employment (Abraham and Houseman 1994). Unfortunately, this important issue is difficult to identify with panel data available in Germany.

The structure of the paper is as follows: after reviewing the literature in Section 2, we summarise the role of small establishments in Germany and the institutional setting in Section 3. In Section 4 we present the econometric model and discuss identification issues. The data is described in Section 5 with results and robustness checks presented in Section 6. Finally, we conclude in Section 7.

2 Literature

In the last decade there has been a large number of theoretical and, more recently, empirical studies on the impact of employment protection legislation, of which dismissal protection is one aspect, on labour adjustment and unemployment. For an overview of the literature see OECD (1999), Bertola, Boeri, and Cazes (1999) and Addison and Teixeira (2001).

In the theoretical literature, firing costs resulting from dismissal protection legislation are typically modeled as a tax. As discussed by Lazear (1990), if the legislation only imposes costs in the form of severance pay, this ex-post transfer can be compensated by an ex-ante transfer from the worker to the employer. Given the assumption of a perfect labour market, this implies that imposing such firing costs on employers will have no employment effects since it is offset by an equivalent reduction in wages. However, as it is in practice, labour markets are imperfect, which results in

rigid wages, and dismissal protection legislation imposes a range of firing costs on employers in addition to the severance pay which cannot be written into a labour contract. These include administrative and legal costs, and disruption to production (lost revenue) resulting from dismissals. When firing regulations represent such costs, theory has a number of predictions.¹ Firstly, such costs result in lower firing rates during an economic downturn since the benefit from firing an employee has to also cover the costs of dismissal. Furthermore, firms will also take into account the probability of firing in the future when hiring a new worker during periods of expansion, and thus, firing costs will also translate to lower rates of hiring. The actual dynamics of employment will depend on the nature of the firing (and hiring) costs. Higher firing costs should also reduce the exit rate from unemployment and increase its duration. However, the net effect of dismissal costs on average employment is ambiguous, and, as presented in Bertola (1992), will depend on the labour demand function, size of the discount and attrition rates, and the relative sizes of hiring and firing costs. Given strictly positive discount and attrition rates, firing costs can even lead to an increase in average employment.

The majority of the theoretical studies look at how dismissal protection legislation hinders adjustment of labour and leads to increased unemployment duration, which, even if only implicitly, suggests that the legislation has a negative effect on labour markets. In contrast, another strand of the theoretical literature considers models where employment protection legislation has an insurance role since markets are imperfect and workers are risk averse. For example, Pissarides (2001) constructs such a model where insurance markets are imperfect as a result of moral hazard. Given the absence of other (administrative) costs arising from dismissals, this study finds that it is possible to design a “flexible” employment protection that does not reduce job creation.

Empirical work could potentially provide some evidence of the above theoretical predictions and help clarify the ambiguities. This literature has been dominated until recently by cross-country studies starting with the paper by Lazear (1990) who investigates the effects of severance pay on employment. Using a sample of

¹See for example Saint-Paul (1996), Bertola, Boeri, and Cazes (1999), and Bertola (1999)

20 countries over the period 1956-1984, the results of this study 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. Using a broader definition of the legislation, the OECD (1999) report “Employment Outlook” finds that stricter employment protection legislation reduces both inflows and outflows from unemployment, but has no significant effect on net unemployment. Furthermore, stricter legislation seems to have a more negative impact on females, younger and older workers in comparison to prime-age males. The results of this report also support the implication that jobs in the presence of stricter legislation will be more stable and that the duration of unemployment will be longer. Finally, the strongest empirical result in this study is that stricter legislation is associated with higher rates of self-employment, but it has an ambiguous effects on temporary workers. A range of other studies have tried using country-level data to improve on these rather inconclusive results; see Addison and Teixeira (2001) for more details.

More relevant to this paper is the small but increasing number of empirical studies which use firm(or establishment)-level panel data to look at the effects of changes to labour market institutions such as dismissal protection legislation. Using this type of data allows for the identification of employment effects using reforms in legislation as quasi-natural experiments. A recent discussion paper by Garibaldi, Pacelli, and Borgarello (2003) investigates the impact of a reform in the threshold for the applicability of the dismissal protection legislation in Italy in 1990. They find that firms which were near the threshold were more likely to keep their employment levels unchanged and fire rather than hire workers. Their results also suggest that a tightening of the legislation increased the level of persistence where firms do not change their employment levels.

A few studies have recently addressed the impact of the dismissal protection legislation in Germany on employment adjustment. Using the IAB Establishment Panel, Wagner, Schnabel, and Kölling (2001) investigate the impact of the threshold for the applicability of the PADA on small German establishments. This study does not find any evidence of threshold effects, however, they do not use the reform to identify these effects and restrict the sample to only the establishments around

the threshold. Boockmann and Hagen (2001) look at the use of atypical workers in West Germany using the same dataset. The results of this study suggest that the relaxation of the dismissal protection legislation in October 1996 led to a decrease in the demand for fixed-term workers in establishments with between 6 and 10 workers. However, given the three year clause for this group in the 1996 reform, it is unlikely that these units make a valid treatment group. In a recent issue of the *Wirtschaftsdienst*, Jahn and Schnabel (2003) and others comment further on this issue, but none contain a thorough econometric analysis of the legislation. Though not directly related to dismissal protection, the impact of the *Schwerbehindertengesetz* or Severe Disability Act is investigated by Kölling, Schnabel, and Wagner (2001) who find only weak evidence of lower growth for establishments at this threshold. This study, however, also does not use a reform in this legislation to identify employment effects and restricts the sample to establishments around the threshold.

3 Small Establishments and the German Institutional Setting

Before turning to the empirical analysis of the effects of the PADA on small establishments in Germany, it is useful to briefly discuss the relevant features of the economy and the institutional peculiarities of the legislation.

3.1 Small establishments in Germany

Germany's economy has been and is still dominated by small and medium-sized companies, the *Mittelstand*, which has often been proclaimed as the backbone of their economy and the *Wirtschaftswunder*. As displayed in Table 1 in the Appendix, in 2001 there were almost 1.5 million small establishments with 1 to 5 workers who were subject to social insurance, accounting for over 68% of all establishments in Germany. These units employed around 3.2 million workers in 2001 which represented over 11% of all workers. The majority of establishments with 1-5 workers are in the retail and service sectors (Günterberg and Wolter 2002). It is approximately these

establishments which are currently not subject to the constraints of the PADA.² In the next category, 6 to 9 workers, there is large decrease in the both the number of establishments and employees. This could be an indication of the effect of the threshold if small establishments with less than 6 workers were not expanding due to the costs imposed by the legislation. However, there is no indication that there was a higher number of establishments with 6 to 9 employees in the distribution for small establishments in 1998 when the threshold stood at 10 workers. In comparison to other countries in the European Union, Germany has a relative high number of small establishments with less than 10 workers, with only Portugal and Austria having more of these units as a proportion of all establishments in the population.³

As also indicated in Table 1, the number of establishments decreases with the size class, while the total number of employees per class only increases for establishments with 10-19 workers and more. This aggregate data shows the importance of small establishments in the Germany economy and the potential magnitude changes to the threshold for the applicability of the PADA has for the German economy. For example, if the threshold was raised to 20 workers, as currently being proposed by employer groups and some political parties, approximately a further 21% of establishments and 16% of workers would no longer be subject to the stringent firing regulations imposed by the legislation.⁴

In order to interpret the results, in particular in light of the identification assumptions of the difference-in-differences estimator used in this paper, it is also important to understand the state of the German economy during the period 1997 to 2001. Unification in 1990 brought major challenges and changes to not only the East German economy but to that of the West. In 1993, Germany and much of the European Union suffered a recession and conditions did not improve until the mid-1990s. As listed in Table 2, growth in the German economy, as measured by GDP (1995 prices), increased from 1997 to 2000 reaching a reasonable 2.9% before experiencing a significant decline in 2001 to 0.9%. However, this general trend dif-

²The size of establishment for these figures does not match exactly the calculation of the size as stipulated in the legislation, see below for further details.

³See Eurostat (2001) for further details

⁴Data for 2001, (Günterberg and Wolter 2002)

fers across sectors. Growth in value-added in manufacturing, as displayed in Table 2, was much more variable with high rates in 1997, 1998 and 2000 and periods of negative growth in 1999 and 2001. The good performance of manufacturing in 2000 was driven by an improvement in export competitiveness (OECD 2003). Growth in value-added in the service sector has been on average better than in manufacturing and also experienced the upswing in 2000 followed by a downturn in 2001, though not to the same extent as the manufacturing sector. The results for this period slightly improve when restricting the figures to private firms. Furthermore, as reported in OECD (2003), employment in manufacturing has decreased over the 1990s but has increased in the service sector. Employment in the manufacturing sector is in general much more volatile than the service sector which tracks more closely the overall economic conditions as represented by GDP. Finally, when excluding the public sector, employment growth was stronger in 1998-2000 than in 1996-1997. However, there is some indication that establishments decreased employment in 2001. These macroeconomic trends could influence the estimates in particular if the year dummies are not capturing all these effects or if these trends were different for the treatment and control groups. Hence, it is important to check the sensitivity of the results by restricting the sample to various groups such as the manufacturing and service sectors, and East and West Germany.

3.2 Provisions of the PADA

The *Kündigungsschutzgesetz* or Protection Against Dismissal Act (PADA) provides protection to workers in both private and public sector establishments⁵ from unfair dismissal where the unit regularly employs more workers than threshold for the applicability of the legislation. In establishments with less workers than this level, employees can be dismissed without any justification so long as other legal requirements are met.⁶ Seasonal work and private households are excluded from

⁵The legislation pertains to the establishment not the firm level. An establishment is a production unit based in a single location and is not necessarily legally and economically independent from other companies.

⁶In such establishments workers are provided protection under the provisions of the *Bürgerliches Gesetzbuch* (BGB) or the German Civil Code which regulates matters concerning the individual

application of the legislation. Dismissals are deemed as being justifiable in three cases: firstly, due to the lack of capability on the part of the worker or illness, though this is difficult in practice; secondly, as the result of misconduct; and finally, redundancies (BAS 2002). As stated in Paragraph 1 of the legislation, the establishment must take into account in the case of redundancies appropriate social considerations when deciding on the workers to be dismissed, unless there are some special technical skills that are needed by the establishment. Dismissals are considered unjustifiable or unfair if the establishment has not met the above three criteria, or if the dismissed worker could have been retained in the establishment in another position, or after further training, or under different work conditions even if this means lower pay. Work councils add a further complexity to the dismissal process since establishments are required to inform them of any redundancies, though few small establishments have such worker representation.⁷

As mentioned in the introduction, the threshold for the applicability of the PADA has changed twice, once from 5 to 10 workers in October 1996 and then back again to 5 workers in January 1999. Figure 1 in the Appendix illustrates these policy changes from 1996 to 2001. Any analysis of the threshold is complicated by the definition of which workers contribute towards the evaluation of the establishment size, which is used to determine the applicability of the PADA. Firstly, it refers to full-time workers who have been regularly employed in the previous six-months. The contribution of regular part-time workers is determined by Paragraph 23 of the legislation. After the first reform in October 1996, the contribution of part-time workers was such that employees working 10 hours or less were given a weighting of 0.25, and employees who worked 20 hours or less and between 20 and 30 hours a week were calculated as contributing 0.5 and 0.75 of a full-time worker, respectively, to the sum of employees. Those working more than 30 hours a week were given a full-time weighting. The second reform of January 2001 increased the weighting for those working 10 hours or less to 0.5. For example, if an establishment has 2 full-time workers, and 4 part-

employment contract and prohibits such acts as discrimination, etc. There may also be certain conditions in wage contracts that give protection to such workers from unfair dismissal.

⁷As stated in the *Betriebsverfassungsgesetz*, only establishments with more than 5 employees are required to have some form of worker representation.

time workers who all regularly work 25 hours per week, the establishment would be calculated as having 5 workers and thus would remain just at the current threshold. If the establishment hired another permanent full-time worker, then all workers would be afforded the protection of the PADA. Secondly, fixed-term workers are counted when they are regularly employed for more than 6 months of the year. Trainees/apprentices, freelance, marginal part-time workers⁸, consultants, family members(without contracts), and owners are not included in the final calculation of the establishment size. This suggests that establishments have a number of strategies to avoid the threshold, such as hiring apprentices. Finally, this law applies to the establishment level, which implies that firms could split operations into various units to avoid the restrictions of the PADA. However, the Federal Constitutional Court or *Bundesverfassungsgericht* ruled in 1998 that companies cannot use this route to avoid the application of the law.⁹ This is another example of how additional complications arise from decisions made by the courts. In fact, applicability of the legislation may only be clear to establishments once they are before the labour courts rather than ex-ante when they decide to hire a worker.

Establishments in Germany face many other thresholds based on size such as the Severe Disability Act (*Schwerbehindertengesetz*) which states that establishments with 16 or more workers must fill 6% of all jobs with workers with disabilities or face a monthly penalty of DM200 (Friedrich and Hägele 1997). In January 2001, this threshold was relaxed to 20 employees and the quota to 5%. The health and safety regulations in Germany also introduce many thresholds and costs for firms. For example, establishments with more than 5 workers are required to have separate toilets for women and men.¹⁰ Thus, in the attempt to identify the impact of the threshold resulting from the PADA, interaction with other regulations, in addition to macroeconomic and other factors, must be kept in mind.

Ultimately the degree to which the PADA in Germany constrains small establishments depends on the costs imposed on them by the legislation in terms of not

⁸These employees work fewer than 15 hours per week and whose income does not exceed one seventh of the monthly reference wage or, where pay is higher, one sixth of the total income, and are not subject to social security payments.

⁹See ruling of the *Bundesverfassungsgericht* on 27.01.1998.

¹⁰See Paragraph 37 of the *Arbeitsstättenverordnung* or Workplace Ordinance.

just the potential redundancy payments but also the legal and administrative costs involved in defending dismissal cases. Once fired, workers are able to sue the employer in a *Arbeitsgericht* or Labour Court for wrongful dismissal. In 2002 there were around 296,000 cases before these courts, which is the dominant type of proceedings.¹¹ Thus, the majority of dismissals are dealt with outside the courts, which is perhaps a reflection of the costs of pursuing it further. In particular, the employer is required to pay the employees wages during period of the court case. Once before the labour courts, however, proceedings are relatively fast; in 2002, only 32% of the cases took longer than 3 months and 2% more than one year. Though the handling of cases may be efficient, the labour court typically rules on the side of the employee; in 2001, 75-80% of unfair dismissal cases before the labour courts were decided in favour of the worker, which would also encourage firms to negotiate with the worker outside the court system (Jahn and Schnabel 2003). Finally, around 90% of disputes resulted in the separation of the worker from their workplace, though they have the right to return to work if the court finds that they have been unfairly dismissed. In this case, workers are awarded compensation for the job loss which can be up to 1 year of wages, and in cases where the worker has been with the same firm for more than 20 years and is older than 55, the compensation can be 18 months of wages (Boockmann and Hagen 2001), (Jahn and Schnabel 2003).

Whether these costs imposed by the legislation constrain small establishment growth and development is the central theme of this paper. Specifically, were establishments which were near or at the threshold for PADA after the reform more likely to keep employment unchanged because of the costs involved in firing workers once the establishment is above this level? In particular, were these units less likely to increase employment in the post-reform period? Since the establishments in the treatment group never faced firing costs, only the potential for future firing costs if they cross the threshold, we do not expect any effect of the reform on the probability of downsizing.

¹¹For details on these figures and those quoted below please refer to BWA (2002).

4 Estimation and Identification

The effects of the changes to the PADA are estimated using the difference-in-differences (DID) estimator¹², which identifies the excess outcome growth for the establishments in the treatment group, those which were effected by the reform, in comparison to the ones in the control group, those which were not effected. The DID estimator can be stated as the following

$$\delta_2 = (\bar{Y}_{t_1}^1 - \bar{Y}_{t_0}^1) - (\bar{Y}_{t_1}^0 - \bar{Y}_{t_0}^0),$$

where \bar{Y}_t^1 is the mean outcome for the treatment group in period t and likewise \bar{Y}_t^0 is the mean outcome for the control group in period t . The timing is as follows: $t_1 > t^* > t_0$, where t^* is the period when the reform took place. The effect of the reform as captured by δ_2 can also be estimated by OLS as the following, though the standard errors should account for the correlation of the error term within each group-year cell,

$$Y_{it}^j = \alpha + \beta' X_{it}^j + \gamma' \lambda_t + \delta_1 \pi^j + \delta_2 d_t^j + \epsilon_{it}^j, \quad (1)$$

where Y_{it}^j is the outcome variable for establishment i in period t , where the superscript j denotes the group such that $j = 1$ for establishments in the treatment group and zero otherwise. X_{it}^j is a vector of exogenous variables and includes the log of lagged investment, log of lagged average product of labour, log of the per worker wage, sector and regional dummies. The first three variables are included to control for other establishment attributes which vary over time. Investment here proxies for capital stock, while the average product of labour controls for efficiency and the per worker wages for wage costs. The sector and regional dummies control for differences in employment adjustments between sectors and regions. λ_t , π^j and d_t^j are dummy variables and are defined as

$$\lambda_t = \begin{cases} 1 & \text{iff } t = \textit{year} \\ 0 & \text{otherwise} \end{cases}$$

¹²See Meyer (1995) for a good overview of this estimator and the potential problems.

$$\pi^j = \begin{cases} 1 & \text{iff } L^* \leq \tilde{L} \\ 0 & \text{iff } L^* > \tilde{L} \end{cases}$$

$$d_t^j = \begin{cases} 1 & \text{iff } t \geq t^* \ \& \ L^* \leq \tilde{L} \\ 0 & \text{otherwise,} \end{cases}$$

where L^* is the establishment size according to the PADA¹³, \tilde{L} is the threshold employment level for the applicability of the legislation, which stands at 5, and t^* is the year in which this threshold changed (January 1999). λ_t is a vector of year dummies which captures time effects that are constant across groups, while π^j controls for time-invariant differences between the treatment and the control group. The dummy of interest is d_t^j , which is an interaction of two dummies, a dummy being in the treatment group, establishments with 5 or less workers, and one for the post-reform period (1999-2001). Given the assumptions of the DID estimator, this approach identifies the true causal effect of the second reform in January 1999 for the treated establishments. ϵ_{it}^j is a random error term and is likely to be correlated within group-year cells. Ignoring this serial correlation can bias the estimated standard errors downwards, which would increase the probability of rejecting the null hypothesis that there is no significant effect of the reform (type I error).

The main identification assumption of the difference-in-differences estimator is that the error term is not correlated with the dummy d_t^j such that $E(\epsilon_{it}^j | d_t^j) = 0$. This implies that coefficient on the reform dummy δ_2 is zero in the absence of the reform. Furthermore, the specification of Equation (1) assumes that the vector of coefficients β is equal across both the treatment and control groups. Moreover, since we only include common year dummies, we assume that any macroeconomic trends affect both the treatment and control groups equally. This can be a particular threat to the validity of this estimator. Identification of the employment effects depends on the changes to the applicability of the PADA and whether this reform can be seen as a quasi-natural experiment. As discussed above and presented in

¹³ L^* = total employees - owners - trainees - part-time workers + (part-time workers*correction), see Section 3.2 for further details.

Figure 1 in the Appendix, for the period 1996-1998 the threshold for the legislation stood at 10 workers though establishments with between 6 and 10 workers before October 1996 remained subject to the law for another three years. In January 1999, before this three year period had expired, the threshold was dropped again to 5 by the newly elected SPD government. Thus our potential treatment group is the establishments with 5 or less workers since they experienced a tightening of the law from 1999 onwards, if we assume that the law was perceived as being permanent. The potential for expectations to change within the pre- and post-reform period may contaminate our estimates. For this reason, we only use data from June 1997 to June 1998 and compare it to the period from June 1999 to June 2001. In the first period, there was no indication that the SPD would win the next election as the then Chancellor candidate Gerhard Schroeder did not start his political campaign until August 1998. Secondly, during the second period (June 1999 - June 2001) there was no suggestion in the polls that the CDU could win the next election or of any major discussion by the government regarding further reforms to the PADA.¹⁴

Another important issue is the composition of the treatment and control groups. The difference-in-differences estimator requires that the two groups are comparable, which implies that they are similar in all respects besides the applicability of the PADA and the reform of this legislation. Different treatment groups are used in the empirical analysis to compare whether there is evidence of true threshold effects, where the treatment group consist of only the establishments at the threshold, to the other extreme, where this group includes all units with 5 or less workers. The potential control group for the analysis in this paper are establishments with 6 or more workers. Alternatively, establishments with between 6 and 10 workers could have been affected by the reform, but as a result of the clause in the 1996 reform, would have most likely reacted differently than the establishments with 5 or less workers. If this is the case, then the estimates are a lower bound of the effects of the reform as the control group consists of units that were effected by the changes, though not to the same degree as the ones in the “true” treatment group. Only establishments with up to 13 workers are included in the control group since at 15 workers establish-

¹⁴See ZDF Politbarometer, www.zdf.de.

ments face another threshold, namely that of the *Schwerbehindertengesetz* or Severe Disability Act, which we don't want to interact with the effects of the PADA. The sensitivity of this assumption is discussed in Section 6.3. Furthermore, as discussed in Section 3, the calculation of the threshold is complicated by different provisions for various worker types. Due to lack of data, not all of these types are consistently observable and thus it is essential that the results are tested for different treatment and control groups. If the control group contains establishments which were also affected by the reform of the legislation in 1999, then the estimates of the average effect of the reform on the treated will be contaminated.

To implement the DID estimator, we estimate the effects of the reform on the probability of keeping employment unchanged, growing and downsizing from period t to $t+1$ using a linear probability model. The results from the linear probability model are compared to that of the probit model to check the sensitivity of the results to the specification of the binary choice model. The variance of ϵ_{it} in the linear probability model is heteroskedastic as it will depend on the vector of parameters.¹⁵ Moreover, as a consequence of using establishment-level panel data, we would expect that ϵ_{it} is correlated within establishments, in addition to being correlated within group-year cells. These issues have to be taken into account when making inferences regarding the statistical significance of the parameter estimates.

5 Data and Summary Statistics

To investigate the effects of the PADA on establishments, we use the IAB Establishment Panel, a establishment-level panel dataset administered by the Institute for Employment Research, in Nürnberg, Germany.¹⁶ The panel started in 1993 with around 4000 establishments, expanding to almost 14,000 in 2000. The sample for the dataset is drawn from the employment statistics register of the Federal Labour Office (*Bundesanstalt für Arbeit*) which covers all establishments with at least one employee (or trainee) that is subject to social security provisions and does not in-

¹⁵If the model is given by $y = \beta'X + \epsilon$, then the $Var(\epsilon|x) = \beta'X(1 - \beta'X)$.

¹⁶See the *Institut für Arbeitsmarkt- und Berufsforschung* (IAB), www.iab.de, and in particular, betriebspanel.iab.de for further details, available only in German.

clude the self-employed. The dataset has very rich information on employment, broken down by different categories of workers. One problem with this panel is that it is only possible to identify net changes in employment. Though the survey does collect information on hirings and firings, this is restricted to the first six months of the survey year and it is not possible to infer the gross adjustments for the whole period. In order to accurately construct the establishment size it is necessary to weight part-time workers as according to the legislation. However, the IAB Establishment Panel does not provide these details in accordance to the PADA and thus approximations for the part-time weightings have to be used. Moreover, these weightings are missing for 2000, which requires imputing the values for this year using the data provided in 1999 and 2001. Due to data constraints, we focus on the impact of the second reform in January 1999, but check the results for a larger sample covering both reforms, though the latter sample of data contains even less information regarding part-time workers.

Establishments in the non-profit, public, agriculture, forestry, mining, construction sectors and private households are dropped from the sample used in estimation. Thus we focus on private establishments in the manufacturing and the service sectors in both West and East Germany. We keep only establishments with 13 or less workers, which appear in the panel for the period 1997 to 2001. Net employment is defined as the total number of full-time workers less apprentices and owners, plus part-time workers weighted according to the number of hours worked. Fixed-term workers are included in the calculation of the establishment size. Since we look at net changes in employment from the 30th of June of one year to the 30th of June of the following year, we are unable to use the data in 1998 since this period crosses the reform in January 1999. Thus, our pre-reform period consists of one year, June 1997 - June 1998, while two years, June 1999 - June 2000 and June 2000 - June 2001, make up the post-reform period.

To show how the exemptions affect the ultimate establishment size used in determining the applicability of the PADA, we compare in Table 3, establishment size using the total number of workers and the one using the net employment as defined above. As seen by the off-diagonal cell, around 43% of establishments, which have

a gross employment level of 6 to 13 workers, have in fact only 0 to 5 employees according to the PADA, and thus were technically not constrained by the legislation. This indicates that ignoring the exemptions could potentially lead to false allocation of establishments to the treatment and control groups. However, the assumption here is that establishments have all information regarding the legislation and its applicability. If this is not the case, then establishments may behave differently than expected based on the calculated establishment size. We return to this issue later when considering the robustness and interpretation of the results in Section 6.

In the sample, around 78% of establishments have 0-5 net employees as defined by the legislation, while the rest of the units have 6-13 workers.¹⁷ Here the treatment group is assumed to consist of establishments with 5 or less workers as calculated according to the legislation. In terms of location, around 45% of establishments with 5 or less workers are located in West Germany, with the majority of these being situated in North Rhine-Westphalia, followed by Bavaria and Baden-Württemberg. This proportion remains roughly the same for the establishments which have 6 or more workers. Small establishments with less than 6 workers are more concentrated in the service sector, in particular, in retail, restaurants, catering and other services. Larger establishments are more represented by the manufacturing sector. Over 89% of establishments used for estimation are stand-alone firms, while only 8% of the units are the head-quarters for a larger firm and 3% a branch or department of a bigger company.

Figure 2 shows the distribution of the establishments based upon the size classification as stipulated in the PADA for 1997, when the threshold stood at 10, and for 2001, when it was at 5. The distribution for 1997, as pictured here by the black boxes, is concentrated in the lower end which is consistent with the figures presented in Section 3. The distribution of establishments by net employment size for 2001, here denoted by the grey shaded boxes, has shifted to the left, and if anything, there is a kink at establishment size 7, which is not in line with the threshold level for that year. This aspect of the distribution may also be due to the use of approximations in determining the establishment size as according to the legislation. In general, it

¹⁷Establishments with 0 workers are ones which only employ non-applicable workers who are still subject to social security payments such as trainees.

is difficult to identify in this graphical comparison a strong correlation between the distribution of the establishments over time and the impact of the threshold for the applicability of the PADA.

Turning to the outcome variables, we can see in Table 4 that the mean rate of keeping employment unadjusted for the treatment group from period t to $t + 1$ is increasing from 1997 to 1999 and then decreasing over the last year. In contrast, the mean of this variable for the control group is decreasing over the whole period. A different trend is also perceptible in the mean rate of increasing employment from period t to $t + 1$. The mean rate of growth for the treatment group slightly decreases from 1997 to 1999 and then stays at the same level for the last period. In comparison, the mean of this variable for the control group increases from 1997 to 1999 and then experiences a slight decline in the following year. The change in the mean rate of downsizing in the treatment and control groups is similar, but the change between 1997 and 1999 is larger in the control group. We can also compare the treatment and control groups based on observable variables such as the monthly per worker wage, average product of labour and the level of investment. The treatment group has a lower per worker monthly wage than the control group with DM 1946 compared to DM 2935. Furthermore, the average product of labour is also lower in the treatment group with the average worker producing DM 257,685 worth of output in comparison to DM 490,438 for the establishments in the control group. This suggests that the larger establishments use labour more productively. Finally, the larger establishments, those in the control group, invest more than the treated units; DM 121,764 compared to DM 36,536 per year. Though the two groups differ in terms of these factors, their influence on employment adjustment are controlled for in the regression setting of Equation (1).

Another way to check whether the treatment and control groups are comparable, is to look at the outcome variables in the period 1993-1995, which is prior to the first reform in October 1996. Using the gross employment measure for calculating the establish size¹⁸, we see in Table 5 that the mean rate of growth for the treatment and control group move in opposite directions over 1993-1995. This suggests that there

¹⁸Data on part-time workers is missing for this period.

are changes in the outcome variable that are not related to changes in the PADA, which implies that large changes in these variables across groups is not uncommon. Hence, it is possible that the coefficient on the reform dummy δ_2 picks up other changes in macroeconomic conditions or institutions that are not observed. There is also evidence that there are differences between the mean rate of growth for the treatment and control groups for 1993-1995, though the mean rate of downsizing for the two groups tends to move together for this period.

It is also important to look at whether establishments of different size classes vary in their use of secondary workers such as part-time, fixed-term workers and trainees. Around 50% of establishments with 5 or less workers use part-time workers in comparison to 62% for the larger establishments in the control group. Considering the use of an approximation for the part-time adjustment to the establishment size, this would suggest that on average the calculated size is subject to more measurement error for units in the control group than in the treatment group. However, it is not clear which direction this would bias results.¹⁹ In general, very few establishments in the sample use fixed-term workers; only around 5% of units with 5 or less workers use employees on fixed-term contracts, while approximately 13% of the larger units use such workers. Furthermore, only 1% of the establishments have fixed-term workers in more than 3 of the 5 years. This implies that it is unlikely that the assumption regarding the contribution of fixed-term workers to the establishment size will make a large difference to the results. Finally, around 21% of the establishments with 5 or less workers have trainees or apprentices, which is considerably lower than that for larger units, of which 31% have such workers. These figures indicate that there are perhaps some systematic differences between the establishments in the treatment and control groups in terms of employment of secondary workers, which could be influenced by such institutions as the dismissal protection legislation (PADA). These issues are not discussed further in this paper but will be addressed in future research.

¹⁹The variable measured with error is binary and is likely to be correlated with the true measure and hence the standard result for classical measurement error is not likely to hold.

6 Results

To separate the different effects of the tightening of the legislation in January 1999 on the treated establishments, we estimate a linear probability model for employment persistence, growth and downsizing. As discussed in Section 3, we expect that the reform results in a higher probability of persistence and lower probability of growth for the treatment group in the post-reform period, while there should be no effect on downsizing.

Before turning to the results, we first check some of the specification and identification assumptions of the model. Firstly, using the Breusch-Pagan Test, the null hypothesis of a constant variance is clearly rejected. Thus all standard errors need to be robust to heteroskedasticity. Secondly, there is evidence that the error term has negative serial correlation, which may result from the use of a short time period for the analysis or model misspecification.²⁰ However, there is not a large change in standard errors when going from no correction, to allowing for heteroskedasticity, and then finally allowing for both heteroskedasticity and correlation of the error terms within establishments.²¹ As discussed in Section 4, the error term will also be correlated within group-year cells as a result of the difference-in-differences specification. Contrary to expectations, controlling for this correlation leads to a decrease in standard errors, which results from the negative correlation of the error term within the group-year cells. The source of this correlation requires further investigation. Finally, we check for the equality of the coefficients between the treatment and control groups. Using a Chow test, the null hypothesis of no difference is not rejected for a joint test of equality of all coefficients. There is, however, some (weak) statistical evidence that the coefficients on the year dummies are not equal across the treatment and control groups, which is a problem given the assumption of common macroeconomic effects.

²⁰This was done by regressing the residual from the estimation of one of the linear probability specifications on itself lagged by one and two periods.

²¹This was done by using the *cluster* option in Stata.

6.1 Probability of employment persistence, growth and downsizing

In order to identify the effect of changes to the PADA on the probability of persistence, we estimate Equation (1) as a linear probability model where the outcome variable Y_{it} is binary and equals one when the establishment has not changed employment from period t to $t + 1$, and zero otherwise. As discussed above, it is expected that establishments at or near the threshold of 5 workers have a higher probability of persistence in the post-reform period, since if they cross this level, all workers can become subject to the firing regulations of the PADA. We first construct the establishment size as according to the legislation and as discussed above in Section 3.2.

We begin with the assumption that the treatment group consists only of establishments at the threshold, those with 5 workers, and which were not able to increase total employment without becoming subject to the legislation or by hiring non-applicable workers. That is, whether or not it is possible to identify true threshold effects. This implies that the control group consists of establishments with 4 or less workers and those with 6-13 workers. As presented in Column (1) of Table 6 in the Appendix the coefficient on the dummy d_t^j is negative, which is contrary to what is expected, but the coefficient estimate is not statistically significant. This result is not surprising as it is likely that establishments, which were near the threshold, were also affected by the reform. To test the other extreme, we expand the treatment group to include all establishment with 5 or less workers. As listed in Column (2) of Table 6, the coefficient estimate on the dummy d_t^j for this specification has the expected sign (positive) and is statistically significant at the 5% level. This implies that establishments with 5 or less workers were more likely to keep unemployment unchanged in the post-reform period, which is consistent with theoretical predictions.

To further investigate the sensitivity of this result, we also consider multiple treatment groups, which allows for different effects of the reduction in the threshold level in 1999 on establishments based on their size. This could arise from interactions with other institutions or barriers to growth that are not observable but vary across

the treatment groups. Specifically, we allow for 5 different treatment groups which requires the following dummies in Equation (1) to capture the effect of the reform

$$\begin{aligned}
 d_t^1 &= \begin{cases} 1 & \text{iff } t \geq t^* \text{ \& } L^* \in [0, 1] \\ 0 & \text{otherwise} \end{cases} \\
 d_t^2 &= \begin{cases} 1 & \text{iff } t \geq t^* \text{ \& } L^* \in (1, 2] \\ 0 & \text{otherwise} \end{cases} \\
 d_t^3 &= \begin{cases} 1 & \text{iff } t \geq t^* \text{ \& } L^* \in (2, 3] \\ 0 & \text{otherwise} \end{cases} \\
 d_t^4 &= \begin{cases} 1 & \text{iff } t \geq t^* \text{ \& } L^* \in (3, 4] \\ 0 & \text{otherwise} \end{cases} \\
 d_t^5 &= \begin{cases} 1 & \text{iff } t \geq t^* \text{ \& } L^* \in (4, 5] \\ 0 & \text{otherwise.} \end{cases}
 \end{aligned}$$

These multiple treatment groups include establishments that have “fractions” of workers which result from the part-time weightings. As displayed in Column (3) in Table 6, doing this separation of the treatment group indicates that the sign for the coefficients on reform dummy is positive for all five groups. However, the estimates are only statistically significant for the first three treatment groups. This indicates that the reform had the strongest and most significant effect on establishments further away from the threshold, which is not entirely consistent with the theoretical effects of the threshold in the legislation. The coefficient on the year dummy for 2000 is negative and statistically significant which is in line with the generally good economic performance in this year. Finally, the coefficient on the log of the per worker wage is positive and significant at the 1% level and implies that the higher the per worker wage, the higher the probability of not adjusting employment. Note that the results for the reform dummies do not change when this and the other time-varying variables, log of lagged investment and log of lagged average product of labour, are excluded.

The same analysis is repeated for the probability of increasing net employment, where the dependent variable is binary and equals one if the establishment increased employment from period t to $t + 1$. Again, we consider first whether or not there are true threshold effects, that is, just allocating establishments with 5 workers to the treatment group. As presented in Column (1) of Table 7, the coefficient on the reform dummy δ_2 is positive, which is contrary to theoretical predictions, but it is not statistically different from zero. When the treatment group is expanded to include all establishments with 5 or less workers, as listed in Column (2) of Table 7, the coefficient δ_2 becomes negative and significant at the 1% level. This implies that these affected establishments had a lower probability of expanding employment after the threshold for the legislation was brought back to 5 workers. Multiple treatment groups are used as in the case of persistence, and as displayed in Column (3), the effect of the reform is negative but only statistically significant for the establishments with 1 or less workers and units with between 1 and 2 workers. As with the specification for persistence, this indicates that the reform had the strongest and most significant effect on establishments further away from the threshold. Furthermore, the coefficients on both year dummies for 1999 and 2000 are positive and significant. The coefficient for the log of the lagged average product of labour is positive and significant at the 5% level. This suggests that more efficient establishments had a higher probability of expanding employment. The coefficients for the log of the per worker wage and log of lagged investment are both positive but statistically insignificant.

Finally, this exercise is carried out for the probability of downsizing, where the dependent variable is binary and equals one when the establishment has decreased employment from period t to $t + 1$. As mentioned above, the reform should have no effect on this aspect of establishments behaviour as the actually firing costs faced by units never changed, it was only the potential for future ones that posed a threat. The results for the estimation of this model are contained in Table 8 and illustrate that there is no evidence of an effect of the reform on the probability of downsizing. The coefficient on the log of investment is negative and statistically significant at the 10% level indicating that the probability of downsizing is lower the higher the

investment level in the previous year. Finally, the coefficient on the log of the per worker wage is also negative and significant at the 1% level.

6.2 Why are there no true threshold effects?

The results presented so far are in some respects consistent with theory and what is expected to occur when the dismissal protection legislation becomes binding for establishments. However, the result that establishments further away from the threshold were the most affected by the reform needs further investigation. Firstly, it is possible that establishments face other sorts of thresholds that are unobservable. For example, there may be some sort of fixed costs of adjustment that establishments face when they are smaller than the threshold level. This could be due to the need to find new office premises or make investments in equipment that entail fixed costs when expanding. To test this explanation we estimate the effect of the reduction in the threshold on the probability to expand employment by 2 or more workers. Establishments would make such discrete jumps in their employment adjustment in the presence of fixed or other non-convex costs. The effect of the reform should be to reduce the probability of making such a larger adjustment for these establishments further away from the threshold. As reported in Column (3) of Table 9, the coefficient on the reform dummy is negative and significant at the 5% level for establishments with 1 or less workers and for those with between 1 and 2 workers. This implies that such units had a lower probability of expanding employment by 2 or more workers in the post-reform period, which is consistent with the presence of some sort of fixed costs of adjustment.

Secondly, these results could alternatively be explained by the affected establishment having imperfect information regarding the applicability of the PADA. In such a situation, it is possible that establishments below the threshold actually perceive themselves to be above it, and vice versa for the larger units. For example, they may not accurately calculate the part-time weightings or whether fixed-term workers are applicable. The results of a survey recently carried out by the Institute of Economic and Social Research of the Hans Böckler Foundation (WSI)²² indicate

²²See www.boeckler.de

that two-thirds of small companies, those with 1-5 workers, falsely believe that the legislation applies to their businesses. Furthermore, 14% of establishments with 6-9 workers believe that the PADA is not applicable. In order to check this explanation, we estimate the specifications for the probability of persistence, growth and downsizing, using the gross measure for the establishment size rather than that calculated according to the legislation. That is, the establishment size includes all permanent workers excluding trainees and there is no adjustment for part-time employees. As reported in Column (1) of Table 10, the effects of the reform on the probability of keeping employment unadjusted are now strongest for the middle treatment group, those with three workers. However, the coefficients on the dummies for the two larger treatment groups are now negative but are not statistically significant. As displayed in Column (2), the results are also not clear for the probability of growth with only the coefficient on the smallest treatment group being significant. Moreover, the effects are now weaker than before when the establishment size was based on the provisions of the legislation. As seen in Column (3), there remains no evidence of an effect of the reform on the probability of downsizing.

The sensitivity of the results are further tested using different measures for establishment size. When using all permanent workers and trainees, there is slightly more evidence of threshold effects, with the size and significance of the coefficients on the reform dummies for the treatment groups near the threshold increasing.²³ Finally, the establishment size is calculated using gross employment which includes the owner and in this case, the coefficient on the dummy for the treatment groups at the threshold in the model for probability of growth, those with between 4 and 5 workers, is now negative and statistically significant. This result is more consistent with true threshold effects than that with the size as calculated according to the legislation. However, it is not clear why true threshold effects emerge only when using the measure for establishment size that includes the owner. It is likely that small establishments do not have full information regarding the legislation, but this does not imply that these companies do not react at all to changes in legislation, rather, their response may not be entirely consistent with the theoretical predictions

²³Results are not reported here but are available on request from the author

based on the provisions of the legislation.

Finally, the above effects identified with the difference-in-differences estimator may be picking up macroeconomic trends or other changes in institutions that affected the treatment and control groups differently over time. It is important to undertake further robustness checks in light of these potential explanations.

6.3 Robustness checks²⁴

As a check of the linear probability model, we also estimate the binary models, probability of persistence, growth and downsizing, using a probit specification. The signs for the coefficients on the reform dummies and their statistical significance remain unchanged. To investigate how the results depend on the sampling of the small establishments in the IAB Establishment Panel, we look at the effect of using population weightings. However, the use of these weights in estimation do not change the estimates by a significant amount. The above the linear probability models are also repeated for sub-samples based on sector and regions. The results from this exercise indicate that above effects are present in both the manufacturing and service sectors. This is reassuring in the sense that both sectors had experienced different trends in the late 1990s and thus gives an indication that the results are not being driven by such differences in macroeconomic trends. Furthermore, the effects are evident in both sub-samples for West and East Germany. In the case of the West German sub-sample, the coefficient on the reform dummy for the treatment group with between 3 and 4 workers is positive and significant in the estimation of the probability of persistence model.

As discussed above, identification of the effects of the change in the applicability of the PADA depends on the accuracy of the separation of establishments to the treatment and control groups. In order to check the sensitivity of the results, we look at the impact of changing the definition of the control group. Firstly, the results remain unchanged when the control group is increased to include establishments with up to 30 workers as defined by the legislation. It is also important to test

²⁴Results are not reported here but are available on request from the author unless stated otherwise.

whether establishments which had above 5 and less than or equal to 10 workers also reacted to the reform in January 1999. As mentioned already, it was these establishments that had to wait 3 years for the effect of the first reform in October 1996 to come into effect, which was interrupted by the second reform in 1999. Thus, the assumption is that either they were not affected by this second reform or that they responded to less of a degree than the “true” treated establishments, those with 5 or less workers. The coefficient on the reform dummy for the establishments with between 5 and 10 workers, $d_t^6 = 1$ if $L^* \in (5, 10]$ and $t \geq t^*$, and 0 otherwise, is not significant in all three specifications; probability of persistence, growth and downsizing.

We also compare the above estimates to those when using a longer panel. However, when using earlier periods in the IAB Establishment Panel, it is not possible to identify the establishment size in accordance with the legislation. A balanced panel is taken for the period 1993-2001 which allows for the identification of the effects of the first reform. That is, the changes in the probability of persistence, growth and downsizing in 1993-1995 (pre-Reform 1) and in 1999-2001 (post-reform 2) are compared to 1997 when the threshold was at 10. As illustrated in Figure 1 in the Appendix, the threshold stood at 5 in both the period before the first reform (1993-1995) and after the second (1999-2001). Now the reform dummy d_t^j is equal to one if the establishment is in the treatment group and if the observation is for 1997 and zero otherwise. By taking a balanced sample, the number of observations decreases to around 1200 establishment-year observations and are restricted to West German establishments. Using the specification with multiple treatment groups, there is no evidence of an effect of the first reform in 1996 on the probability of persistence, growth or downsizing, which may be due to the small sample size.

Using the entire length of the dataset also allows to check for spurious effects of the reform. Firstly, the above specifications are redone using another dummy to capture the effects of the second-reform. Given that the threshold stood at 5 in both 1993-1995 and 1999-2001, this dummy should not pickup any effects from the reduction in the threshold in the latter period. However, in the case of the specifications for the probability of persistence and growth, coefficient on the dummies which

capture the effect of the second reform are statistically significant. Secondly, this is repeated using just the observations for 1993-1995 and 1999-2001, thus excluding the ones for 1997 when the effect of the first reform should be evident. The coefficient on the dummy to capture the effects of the second reform is again statistically significant in specifications for both persistence and growth. This indicates that the above results are potentially being driven by other macroeconomic trends or changes to legislation than that of the PADA and which affected the treatment and control groups differently in the period after the second reform in January 1999.

Another potential problem is sample selection bias since we have only used a balanced sub-sample of establishments which remained in the panel for the whole period of 1997-2001 or 1993-2001. When taking the unbalanced sample of establishments for 1997-2001, there are some changes to the estimates of the impact of the reform and a Hausman-type test leads to rejection of the null hypothesis that the coefficients are equal in both the balanced and unbalanced sample. The causes of this requires further investigation and whether this poses a problem of sample selection bias. This would be a particular problem if entry and exit from the panel was correlated with the impact of the legislation.

7 Conclusion

In this paper we use a reform in the applicability of the Protection Against Dismissal Act or *Kündigungsschutzgesetz* in Germany to identify employment effects of the legislation for small establishments. In October 1996, the threshold for the applicability of the PADA was raised from 5 to 10 workers by the then Chancellor Helmut Kohl. After being elected, the new centre-left government reduced this threshold back to 5 workers in January 1999. This implies that the establishments with 5 or less workers had the ability to grow above the old threshold from October 1996 to January 1999, without becoming subject to the legislation, and then faced unexpectedly a more stringent environment after the second reform in 1999. Though the time frame is short, if there is any evidence of employment effects resulting from changes to the PADA, they need to be explained.

Using a balanced panel of establishments for 1997-2001 and the establishment size as according to the legislation, we find some evidence that a tightening of the threshold resulted in a higher probability of persistence and a lower probability of growth for the units in the treatment group during the post-reform years. However, the effects are the strongest for establishments which were further away from the threshold. This could reflect the presence of other institutional barriers or adjustment costs than those imposed by the PADA. When using total employment as the measure for establishment size, there is slightly clearer evidence of threshold effects which are consistent with theoretical predictions. This seems to indicate that small establishments do not have perfect information regarding the applicability of the legislation, which is also supported by recent survey evidence. However, there is also evidence that these results are being driven by other underlying differences in macroeconomic trends or factors rather than the change in the PADA in 1999.

Ultimately we need to improve our understanding of how small establishments evolve and make decisions regarding adjustment of employment. Their understanding of the impact of such legislation as the PADA is likely to differ than that of our theoretical and empirical models. As Truman Bewley states "...it would be presumptuous to ignore the testimony of people who make economic decisions and observe and participate in economic life. To do so would be to make economics a religion rather than a responsible analysis of experience. Good instincts about a subject can be developed only by contact with the phenomena studied." (Bewley 1999, p.14).

A final point is necessary regarding the policy interpretation of the results presented in this paper. While there is some evidence that changes to the applicability of the *Kündigungsschutzgesetz* or Protection Against Dismissal Act in Germany had an effect on the labour adjustment of small establishments, it is not clear what the overall effects of this particular legislation were in terms of unemployment and the economy as whole. Moreover, there are too many interactions with other institutions and potential secondary effects to make any clear predictions about the outcome of a further relaxation of the Act. Thus any type of policy recommendations from such an analysis must be done with the utmost caution.

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8 Appendix

Table 1: Establishment Size and Number of Employees in Germany - 2001

Size	No. of establishments	%	No. of employees	%
1-5	1,457,809	68.4	3,167,851	11.4
6-9	256,498	12	1,849,960	6.7
10-19	202,871	9.5	2,718,272	9.8
20-49	128,738	6	3,899,082	14
50-99	46,531	2.2	3,212,303	11.5
100-499	35,318	1.7	6,919,619	24.9
500+	5,046	0.2	6,050,027	21.7
Total	2,132,811	100	27,817,114	100

Source: Unternehmensgrößenstatistik 2001/2002 - All sectors,
(Günterberg and Wolter 2002)

Table 2: Macroeconomic Indicators for Germany - 1997-2001

Economic Indicator	1997	1998	1999	2000	2001
	%	%	%	%	%
Growth in GDP (1995 prices)	1.3	1.9	2.1	2.9	0.9
Growth in VA (Manufacturing)	3.4	2.1	-1.9	4.4	-2.2
Growth in VA (Services)	2.1	2.9	2.2	3.5	1.3
Growth in VA (Private firms)	2.2	2.7	2.6	3.9	1.5

Source: Time-series data, Federal Statistical Office, Germany

Table 3: Employment Versus Net Employment

Employment No. of employees	Net Employment		
	1-5	6-13	Total
1-5	2338	0	2338
6-13	644	850	1494
Total	2982	850	3832

Source: Balanced panel from the IAB Establishment Panel 1997-2001. Employment is the total number of employees and net employment is the number of employees as according to the legislation.

Table 4: Mean Rates of Persistence, Growth and Downsizing

Year	Treatment Group	No. of Obs.	Control Group	No. of Obs.
$\mathcal{I}(\Delta L_{t+1} = L_{t+1} - L_t = 0)$				
1997	0.37	742	0.27	216
1999	0.43	740	0.24	218
2000	0.41	742	0.19	216
$\mathcal{I}(\Delta L_{t+1} = L_{t+1} - L_t > 0)$				
1997	0.32	742	0.24	216
1999	0.29	740	0.34	218
2000	0.29	742	0.30	216
$\mathcal{I}(\Delta L_{t+1} = L_{t+1} - L_t < 0)$				
1997	0.30	742	0.50	216
1999	0.28	740	0.42	218
2000	0.30	742	0.51	216

Source: Balanced panel from the IAB Establishment Panel 1997-2001. Means are based on net employment as specified in the legislation. \mathcal{I} is an indicator variable and equals 1 if the condition inside the parentheses is fulfilled.

Table 5: Mean Rates of Persistence, Growth and Downsizing - 1993-1995

Year	Treatment Group	No. of Obs.	Control Group	No. of Obs.
$\mathcal{I}(\Delta L_{t+1} = L_{t+1} - L_t = 0)$				
1993	0.59	101	0.52	99
1994	0.66	102	0.43	98
1995	0.61	105	0.49	95
$\mathcal{I}(\Delta L_{t+1} = L_{t+1} - L_t > 0)$				
1993	0.21	101	0.16	99
1994	0.24	102	0.30	98
1995	0.25	105	0.21	95
$\mathcal{I}(\Delta L_{t+1} = L_{t+1} - L_t < 0)$				
1993	0.20	101	0.32	99
1994	0.11	102	0.28	98
1995	0.14	105	0.29	95

Source: Balanced panel from the IAB Establishment Panel 1993-1995. Means are based on gross employment only. \mathcal{I} is an indicator variable and equals 1 if the condition inside the parentheses is fulfilled.

Table 6: Linear Probability Model - Persistence in Employment

Dependent Variable: $\mathcal{I}(\Delta L_{i,t+1} = 0)$			
Variable	(1)	(2)	(3)
$\pi^1 = \mathcal{I}(L_t^* = 5)$	0.016 (0.067)		
$d_t^1 = \mathcal{I}(L_t^* = 5 \ \& \ t > t^*)$	-0.067 (0.085)		
$\pi^1 = \mathcal{I}(L_t^* \in [0, 5])$		0.115*** (0.040)	
$d_t^1 = \mathcal{I}(L_t^* \in [0, 5] \ \& \ t > t^*)$		0.103** (0.047)	
$\pi^1 = \mathcal{I}(L_t^* \in [0, 1])$			0.317*** (0.055)
$\pi^2 = \mathcal{I}(L_t^* \in (1, 2])$			0.161*** (0.051)
$\pi^3 = \mathcal{I}(L_t^* \in (2, 3])$			0.015 (0.050)
$\pi^4 = \mathcal{I}(L_t^* \in (3, 4])$			0.046 (0.061)
$\pi^5 = \mathcal{I}(L_t^* \in (4, 5])$			0.050 (0.063)
$d_t^1 = \mathcal{I}(L_t^* \in [0, 1] \ \& \ t > t^*)$			0.146** (0.064)
$d_t^2 = \mathcal{I}(L_t^* \in (1, 2] \ \& \ t > t^*)$			0.110* (0.062)
$d_t^3 = \mathcal{I}(L_t^* \in (2, 3] \ \& \ t > t^*)$			0.113* (0.062)
$d_t^4 = \mathcal{I}(L_t^* \in (3, 4] \ \& \ t > t^*)$			0.056 (0.072)
$d_t^5 = \mathcal{I}(L_t^* \in (4, 5] \ \& \ t > t^*)$			0.030 (0.076)

Source: Balanced panel from the IAB Establishment Panel 1997-2001. Number of observations = 2432. Robust standard errors corrected for clustering at establishment level are reported. *** - 1% significance level, ** - 5% significance level, * - 10% significance level. Independent variables also include year, sector and regional dummies, log of lagged investment, log of lagged average product of labour and log of per worker monthly wage. \mathcal{I} is an indicator variable and equals 1 if the condition inside the parentheses is fulfilled.

Table 7: Linear Probability Model - Growth in Employment

Dependent Variable: $\mathcal{I}(\Delta L_{i,t+1} > 0)$			
Variable	OLS (1)	OLS (2)	OLS (3)
$\pi^1 = \mathcal{I}(L_t^* = 5)$	-0.106*		
	(0.059)		
$d_t^1 = \mathcal{I}(L_t^* = 5 \ \& \ t > t^*)$	0.056		
	(0.073)		
$\pi^1 = \mathcal{I}(L_t^* \in [0, 5])$		0.117***	
		(0.037)	
$d_t^1 = \mathcal{I}(L_t^* \in [0, 5] \ \& \ t > t^*)$		-0.126***	
		(0.046)	
$\pi^1 = \mathcal{I}(L_t^* \in [0, 1])$			0.183***
			(0.052)
$\pi^2 = \mathcal{I}(L_t^* \in (1, 2])$			0.139***
			(0.049)
$\pi^3 = \mathcal{I}(L_t^* \in (2, 3])$			0.132***
			(0.050)
$\pi^4 = \mathcal{I}(L_t^* \in (3, 4])$			0.013
			(0.056)
$\pi^5 = \mathcal{I}(L_t^* \in (4, 5])$			0.039
			(0.059)
$d_t^1 = \mathcal{I}(L_t^* \in [0, 1] \ \& \ t > t^*)$			-0.219***
			(0.060)
$d_t^2 = \mathcal{I}(L_t^* \in (1, 2] \ \& \ t > t^*)$			-0.154***
			(0.059)
$d_t^3 = \mathcal{I}(L_t^* \in (2, 3] \ \& \ t > t^*)$			-0.085
			(0.064)
$d_t^4 = \mathcal{I}(L_t^* \in (3, 4] \ \& \ t > t^*)$			-0.011
			(0.069)
$d_t^5 = \mathcal{I}(L_t^* \in (4, 5] \ \& \ t > t^*)$			-0.064
			(0.073)

Source: Balanced panel from the IAB Establishment Panel 1997-2001. Number of observations = 2432. Robust standard errors corrected for clustering at establishment level are reported. *** - 1% significance level, ** - 5% significance level, * - 10% significance level. Independent variables also include year, sector and regional dummies, log of lagged investment, log of lagged average product of labour and log of per worker monthly wage. \mathcal{I} is an indicator variable and equals 1 if the condition inside the parentheses is fulfilled.

Table 8: Linear Probability Model - Downsizing in Employment

Dependent Variable: $\mathcal{I}(\Delta L_{i,t+1} < 0)$			
Variable	OLS (1)	OLS (2)	OLS (3)
$\pi^1 = \mathcal{I}(L_t^* = 5)$	0.091 (0.069)		
$d_t^1 = \mathcal{I}(L_t^* = 5 \ \& \ t > t^*)$	0.011 (0.080)		
$\pi^1 = \mathcal{I}(L_t^* \in [0, 5])$		-0.232*** (0.042)	
$d_t^1 = \mathcal{I}(L_t^* \in [0, 5] \ \& \ t > t^*)$		0.023 (0.049)	
$\pi^1 = \mathcal{I}(L_t^* \in [0, 1])$			-0.501*** (0.046)
$\pi^2 = \mathcal{I}(L_t^* \in (1, 2])$			-0.300*** (0.050)
$\pi^3 = \mathcal{I}(L_t^* \in (2, 3])$			-0.147*** (0.055)
$\pi^4 = \mathcal{I}(L_t^* \in (3, 4])$			-0.059 (0.067)
$\pi^5 = \mathcal{I}(L_t^* \in (4, 5])$			-0.089 (0.065)
$d_t^1 = \mathcal{I}(L_t^* \in [0, 1] \ \& \ t > t^*)$			0.073 (0.052)
$d_t^2 = \mathcal{I}(L_t^* \in (1, 2] \ \& \ t > t^*)$			0.044 (0.059)
$d_t^3 = \mathcal{I}(L_t^* \in (2, 3] \ \& \ t > t^*)$			-0.028 (0.065)
$d_t^4 = \mathcal{I}(L_t^* \in (3, 4] \ \& \ t > t^*)$			-0.045 (0.079)
$d_t^5 = \mathcal{I}(L_t^* \in (4, 5] \ \& \ t > t^*)$			0.034 (0.080)

Source: Balanced panel from the IAB Establishment Panel 1997-2001. Number of observations = 2432. Robust standard errors corrected for clustering at establishment level are reported. *** - 1% significance level, ** - 5% significance level, * - 10% significance level. Independent variables also include year, sector and regional dummies, log of lagged investment, log of lagged average product of labour and log of per worker monthly wage. \mathcal{I} is an indicator variable and equals 1 if the condition inside the parentheses is fulfilled.

Table 9: Linear Probability Model - Larger Employment Adjustments

Dependent Variable: $\mathcal{I}(\Delta L_{i,t+1} \geq 2)$	
Variable	OLS (1)
$\pi^1 = \mathcal{I}(L_t^* \in [0, 1])$	0.051* (0.028)
$\pi^2 = \mathcal{I}(L_t^* \in (1, 2])$	0.058** (0.027)
$\pi^3 = \mathcal{I}(L_t^* \in (2, 3])$	0.064** (0.030)
$\pi^4 = \mathcal{I}(L_t^* \in (3, 4])$	0.030 (0.030)
$\pi^5 = \mathcal{I}(L_t^* \in (4, 5])$	0.009 (0.030)
$d_t^1 = \mathcal{I}(L_t^* \in [0, 1] \ \& \ t > t^*)$	-0.064** (0.029)
$d_t^2 = \mathcal{I}(L_t^* \in (1, 2] \ \& \ t > t^*)$	-0.068** (0.030)
$d_t^3 = \mathcal{I}(L_t^* \in (2, 3] \ \& \ t > t^*)$	-0.036 (0.033)
$d_t^4 = \mathcal{I}(L_t^* \in (3, 4] \ \& \ t > t^*)$	-0.025 (0.035)
$d_t^5 = \mathcal{I}(L_t^* \in (4, 5] \ \& \ t > t^*)$	0.009 (0.037)

Source: Balanced panel from the IAB Establishment Panel 1997-2001. Number of observations = 2432. Robust standard errors corrected for clustering at establishment level are reported. *** - 1% significance level, ** - 5% significance level, * - 10% significance level. Independent variables also include year, sector and regional dummies, log of lagged investment, log of lagged average product of labour and log of per worker monthly wage. \mathcal{I} is an indicator variable and equals 1 if the condition inside the parentheses is fulfilled.

Table 10: Probability of Persistence, Growth and Downsizing (Gross Employment)

Variable	$\mathcal{I}(\Delta L_{i,t+1} = 0)$	$\mathcal{I}(\Delta L_{i,t+1} > 0)$	$\mathcal{I}(\Delta L_{i,t+1} < 0)$
	OLS	OLS	OLS
	(1)	(2)	(3)
$\pi^1 = \mathcal{I}(L_t^* \in [0, 1])$	0.295*** (0.053)	0.091** (0.045)	-0.386*** (0.043)
$\pi^2 = \mathcal{I}(L_t^* \in (1, 2])$	0.203*** (0.054)	0.068 (0.045)	-0.271*** (0.047)
$\pi^3 = \mathcal{I}(L_t^* \in (2, 3])$	-0.005 (0.064)	0.094* (0.054)	-0.089 (0.060)
$\pi^4 = \mathcal{I}(L_t^* \in (3, 4])$	0.077 (0.074)	0.001 (0.057)	-0.078 (0.070)
$\pi^5 = \mathcal{I}(L_t^* \in (4, 5])$	0.061 (0.070)	0.011 (0.056)	-0.072 (0.066)
$d_t^1 = \mathcal{I}(L_t^* \in [0, 1] \ \& \ t > t^*)$	0.046 (0.060)	-0.097* (0.050)	0.052 (0.049)
$d_t^2 = \mathcal{I}(L_t^* \in (1, 2] \ \& \ t > t^*)$	0.015 (0.063)	-0.086 (0.053)	0.071 (0.056)
$d_t^3 = \mathcal{I}(L_t^* \in (2, 3] \ \& \ t > t^*)$	0.141* (0.076)	-0.096 (0.063)	-0.045 (0.070)
$d_t^4 = \mathcal{I}(L_t^* \in (3, 4] \ \& \ t > t^*)$	-0.034 (0.089)	0.028 (0.070)	0.006 (0.085)
$d_t^5 = \mathcal{I}(L_t^* \in (4, 5] \ \& \ t > t^*)$	-0.028 (0.090)	-0.013 (0.067)	0.041 (0.084)

Source: Balanced panel from the IAB Establishment Panel 1997-2001. Number of observations = 2494. Robust standard errors corrected for clustering at establishment level are reported. *** - 1% significance level, ** - 5% significance level, * - 10% significance level. Independent variables also include year, sector and regional dummies, log of lagged investment, log of lagged average product of labour and log of per worker monthly wage. \mathcal{I} is an indicator variable and equals 1 if the condition inside the parentheses is fulfilled.

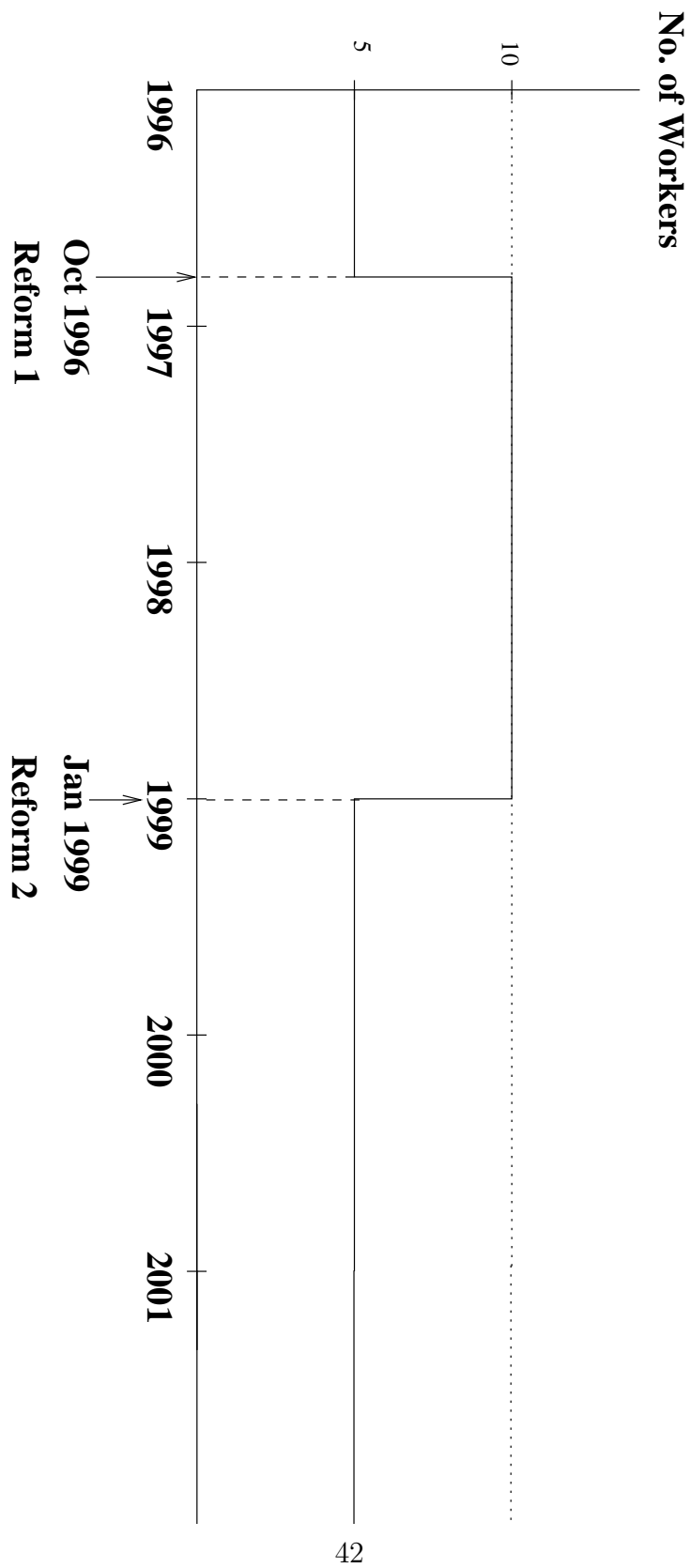


Figure 1: Threshold Level for Applicability of PADA - 1996-2001

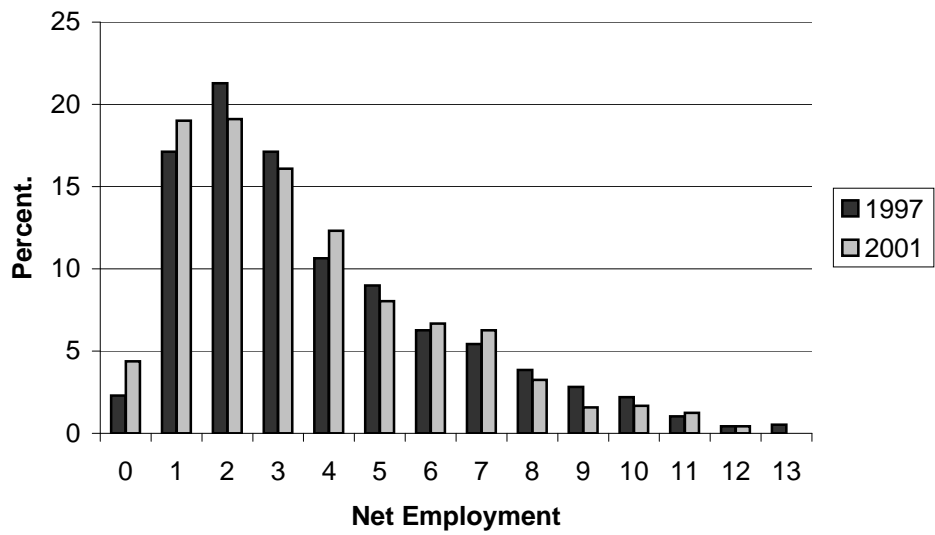


Figure 2: Distribution of Establishments by Number of Net Employees

Source: Balanced panel from the IAB Establishment Panel 1997-2001.

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