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Zusammenfassung

Eines der am häufigsten genannten Vorschläge, um den deutschen Arbeitsmarkt zu flexibilisieren, ist die Lockerung des Kündigungsschutzgesetzes. Es findet Anwendung für Betriebe, die einen Schwellenwert an Mitarbeitern überschreiten. Die These, die in dieser Arbeit untersucht werden soll, ist die, dass diese Schwelle im Gesetz Kleinbetriebe daran hindere, Einstellungen vorzunehmen. Änderungen im Schwellenwert in den späten 1990er Jahren bilden die Basis für die Schätzung dieses Effektes. Der Auswertungsansatz ist ein Regression Discontinuity Design, für das diese Änderungen als natürliche Experimente verwendet werden. Lokale Treatmenteffekte können nicht-parametrisch mittels lokaler linearer Regression geschätzt werden.

Dieses Papier ist das erste, das die oben genannten Gesetzesänderungen benutzt, um für Selbstselektion zu kontrollieren, und sich dabei auf minimale Annahmen bezüglich der Modellspezifikation stützt. Die Ergebnisse stimmen mit früheren Untersuchungen überein, die keine solchen beschäftigungshemmenden Effekte für Kleinbetriebe nachweisen konnten.

JEL: J23, K31, M51

Schlagwörter: *Kündigungsschutz, Schwellenwerte-Effektes, RDD, lokale lineare Regression, Deutschland*

Summary

One proposal frequently raised to increase flexibility of the German labour market is the liberalization of the job protection law. It applies to those establishments with more than a cut-off number of employees. The argument examined in this paper is that this step in legal regulation hinders small enterprises from job creation.

Changes in the cut-off number in the late 1990's provide the basis for estimating this effect. The evaluation approach is a Regression Discontinuity Design using these changes as natural experiments. Local treatment effects can be estimated non-parametrically by local linear regression. The data base used is the IAB establishment panel.

The paper is the first one to exploit the policy changes named above and controlling for self-selection into the treatment job protection using minimal assumptions concerning model specification. The results are in line with earlier studies finding no evidence for hindering effects on job growth in small establishments.

JEL: J23, K31, M51

Keywords*: employment protection, threshold effects, RDD, local linear regression, Germany*

The Impact of German Job Protection Legislation on Job Creation in Small Establishments
An Application of the Regression Discontinuity Design

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1 Introduction

During the last couple of years the deregulation of European labour markets has been high on the political agenda, and this is still the case today. One of the proposals most frequently raised in this context in Germany is the liberalization of the job protection laws (Kündigungsschutzgesetz, KschG). Just some months ago, in January 2004, the regulatory framework has faced another change, and additional deregulation continues to be heavily discussed.

As in many other European countries, not all establishments are regulated in the same way. Those employing more than a set number of employees face considerably higher adjustment costs in times of lower demand for labour, as they are subject to the KschG. There are two arguments often raised: first, if additional non-wage costs on labour were removed then relative prices would change c.p. in favour of labour, thereby raising employment. Second, employment protection as it is implemented in Germany restricts small establishments at the regulatory threshold from hiring further employees as they would be threatened with becoming liable to the demands of the law.

It is the aim of this paper to evaluate whether this second hypothesis concerning the threshold in job protection laws and its effect on job creation in small enterprises can be confirmed by empirical analysis thus justifying the curtailing of the rights of many employees.

The evaluation method is a Regression Discontinuity Design (RDD) making use of the changes in legislation in recent years as unintended experiments. Using data at establishment level one can rely on a rich panel data set with a sample size of several thousand units.

Up to now several approaches have been taken to analyze employment effects of German job protection laws in general; however, very few of them deal with its thresholds in particular. As far as I can ascertain, only the papers by Boockmann and Hagen (2001), Verick (2004) and Bauer et al. (2004) use the natural experiments of the policy changes mentioned above to control for self-selection into the treatment. While the first paper analyzes the use of flexible working contracts, the latter apply diff-in-diff-estimators to evaluate the threshold effects on establishments' employment behaviour. In contrast to the studies mentioned the method employed in this paper does not rely on any functional form or model specification assumptions.

The paper is organized as follows: Chapter 2 describes the German job protection legislation and gives a quick overview of the theoretical discussion and empirical studies concerning this type of regulation. In the subsequent chapter the data used for the analysis will be presented. Chapter 4 explains the RDD evaluation approach, how it applies to the evaluation problem at hand and shows a way to estimate treatment effects. In chapter 5 results are presented and discussed. Chapter 6 concludes.

2 Job Protection in Germany – The Program Design

The process of dismissing employees is heavily regulated in Germany. Apart from general laws specifying periods of notice before an employee can be dismissed and the technical circumstances of this dismissal, there is specific job protection legislation. By this law – once introduced to protect workers from unjustified and short-term dismissal – a company is obliged to name reasons when making an employee redundant. These reasons might address the company's situation, characteristics of the person to be dismissed, or his/her behaviour ("betriebsbedingte, personenbedingte oder verhaltensbedingte Gründe").

The vast majority of dismissals are justified by the company's economic situation. In these cases the employer faces a second obligation: he or she is obliged to take account of social factors ("Sozialauswahl") in choosing the persons to be made redundant. Older employees, those with family, and disabled workers have to be the last ones to be dismissed.

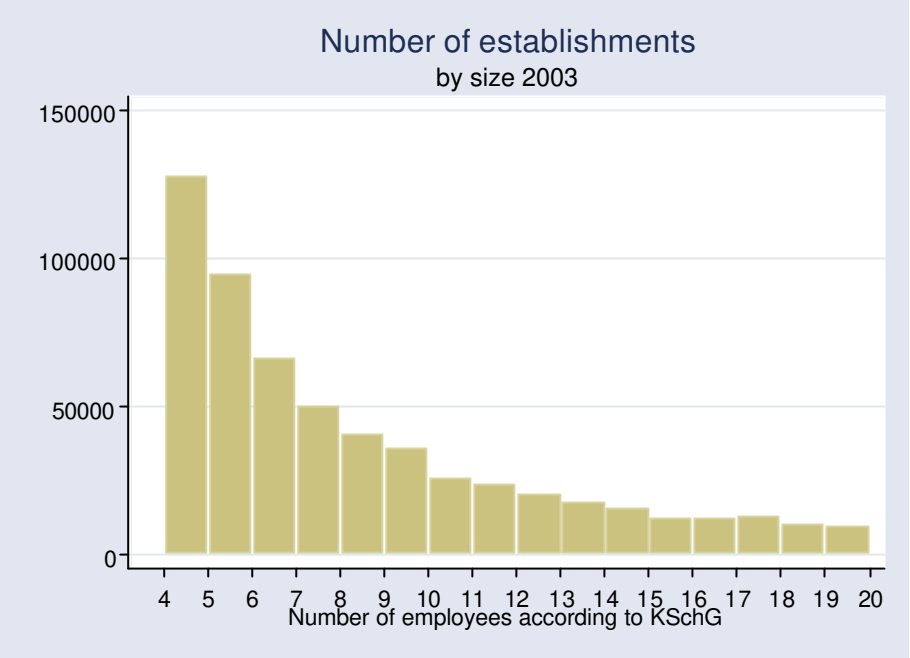
These provisions not only increase the costs of labour adjustment. Expected future costs are nearly impossible to anticipate for the employer. Jahn and Schnabel (2003) calculated in 2001 that some 27 percent of all dismissals initiated by a company are referred to the courts. At the same time about 75 to 80 percent of all cases are decided in favour of the former employee. In this case a combination of continued wages until the judges have finally decided and severance payments can easily amount to a year's wages to the illegally dismissed person. On top of that many cases can be assumed to be settled without the intervention of a court leading to similar payments to the employee.[1]

But the special employment protection law does not apply to all companies: those employing less than ten employees are excluded from these provisions. As pointed out before, the cut-off point has been changed in recent years. After many years the threshold being set at five the number was raised from to ten employees, leaving small enterprises out of scope of the regulation. In May 1996 the federal coalition government agreed upon details of this first law change examined: on September 16th 1996 the new law passed the parliament and on October 1st 1996 it became effective. The intention of the change was to promote employment dynamics in a time of mass unemployment ("Beschäftigungsförderungsgesetz"). After the subsequent election in 1998, one of the first things the new government did was to reset the cut-off number to five in order to strengthen the rights of employees. So, by January 1999 companies of between five and ten employees were covered by the KSchG again. In late 2003 it has been agreed upon to lift the number to ten for a second time starting from January 1st 2004. In a couple of years a similar analysis will be possible with regard to this year's policy change.

To highlight the importance of the law changes in January 2004 figure 1 gives an impression of how many establishments were relieved from the covering of the law. In late June 2003 some 330,000 establishments were relieved from the law.

[1] Bothfeld and Ullmann (2003) present the results of a survey on the conclusions of employment contracts. According to their paper 32% of these conclusions were initiated by the company. Out of these some 11% to 15% resulted in a procedure. For small establishments they report only a share of 2% resp. 4% of the conclusions triggered severance payments to the former employee.

Figure 1: Number of establishments by size according to employment protection law



Source: IAB establishment panel, own calculations

The number of employees as defined by the law is not equal to the number of persons working in a company. It includes neither apprentices nor the employer herself nor helping family members. It does account for part time work. Workers with regular hours of up to 20 per week only count as half an employee. Correspondingly, employees working up to ten (thirty) hours count as 0.25 (0.75). The establishment is thereby defined as a place of production and not as a legal entity. Temporary employees are included only if they regularly work at least six months per year at the company. Thus the number of employees according to the law can differ substantially from the number of persons working in an establishment.

When taking a closer look at the policy change in 1996 and 1999 there are two points that might dilute the results of the analysis: first, the reestablishment of the old threshold level of five employees in late 1998 had already been anticipated by many employers as it was a central message of the opposition politicians – and later members of the government – in the election campaign. Second, the deregulation only applied to workers employed after the change of legislation. Workers employed before the law change in 1996 were granted the same protection level for a transition period of three years. Still, one can expect that establishments finding themselves below the threshold as a result of the changes had a strong incentive to abstain from further job growth.

From a theoretical point of view the question seems clear: Hiring a sixth (eleventh) employee raises marginal costs of employment as expected future severance payments will be included in the firm's decision to invest into manpower.[2] Especially at the threshold level decision makers will be discouraged from hiring additional personnel. Once the number five (ten) is exceeded the law applies to all employees. And the decision can not be reversed easily. Depending on whether the employee takes court action and how busy the judges in charge are, the process of firing can easily take twelve months. In the meantime, the law applies to all employees.

The only model known to me including threshold values in the application of employment protection laws is the one of Garibaldi et al. (2003). It is shown that some establishments choose to maximize their expected average profits by not jumping the threshold, choosing rather a lower level of

[2] For an overview of recent research cf. Addison and Texeira (2001).

employment instead. There are several ways to circumvent to be covered by the law: increased overtime work, investment into capital goods, an altered make- or- buy- decision etc.

Empirical studies on job protection legislation come up with slightly differing results. While most of them share a negative view of the laws, a minority can not find any hindering impacts on employment dynamics (cf. Addison and Teixeira 2003, p. 57ff). Most of the studies are based on international comparisons calculating some kind of an index of the protection. The most prominent examples are the studies conducted by Lazear (1990) and OECD (1999).

Only a few exploit micro data using individual micro observations. For Italy, Garibaldi et al. (2003) find suggestions that a similar threshold at 15 employees has a muting effect on the development of employment growth in the establishment. For the German case Friedrich and Hägele (1997) come up with descriptive results comparing the share of companies above and below the threshold hiring new personnel. They find no support for the hypothesis that thresholds like the one in the job protection law hinder employment dynamics. A study by Wagner et al. (2001) covers Germany using econometric methods on micro data level. They find no hindering effects of the threshold on small companies' hiring behaviour. Wagner et al. (2001) propose to study the effects of the policy changes described above. However, none of the papers named above regards coverage by the employment protection law as the result of a self-selection process and, thus, none controls for endogeneity of the treatment.

A somewhat different goal is followed by Brookmann and Hagen (2001): they try to find determinants for hiring fixed-term and freelance workers. To this end, they compare influences of firm size on a binary outcome variable indicating whether a company used such a flexible form of employment. For a regression on a fixed term contract dummy they find that the coefficient of a six to ten employees dummy variable is significantly lower after the law change than before and interpret this as a result of the law change. Recently, Verick (2004) has applied a diff-in-diff-estimator to analyze effects of the threshold in German employment protection law. He finds evidence that establishments do not perceive the thresholds as stated in the details of the law.[3] He finds no strong support for the hypotheses stated in public debate. Bauer et al. (2004) examine the effect of the law on worker turnover. They do not find any significant influences.

It will be the aim of this paper to find out whether and to what extent employment dynamics is slowed down among companies below the cut-off level. They will serve as the control group while establishments that happened to employ more than five (ten) people provide the treatment group.

3 The IAB Panel Data Set

Wagner et al. (2001) point out that a data set appropriate for this kind of study is the German establishment panel collected by the Institut für Arbeitsmarkt- und Berufsforschung (IAB) in Nuremberg. In order to follow company behaviour after the policy change for longer than the year reported in one wave one needs longitudinal data. As the analysis will be restricted to the establishments close to the cut-off number of five (ten) employees it is necessary to use a sufficiently large data set. Another important condition is that the observation unit has to be the establishment as a local unit not as a legal one. On top of that, information not simply about the number of employees is needed. One has to be able to distinguish owners, helping family members, apprentices and regular employees.

The IAB establishment data set is the only one available for me that fulfils these conditions. It contains some 4,000 observations in its first wave in 1993. This number has increased to some 16,000 establishments in the currently available 2003 wave. The sample used is restricted to those establishments that reported positive demand expectations for at least one year and no negative

[3] These findings can be supported by the study of Pfarr *et al.* (2003) which presented results of a survey among persons responsible for HR. It is shown that in 2003 12% of the respondents of establishments from six to nine employees were in the false belief that their establishment was not covered by the employment protection law. At establishments between one and five employees even a share of 64% of the responsible persons misjudged their legal status.

expectations for any of the years surveyed. Thus the data set shrinks to a sample size of 1883 for the year 1997 and 697 for the year 2003. It contains only establishments employing at least one person subject to mandatory social security. One-person-establishments, private households and those with only public employees are excluded from the survey. Each panel wave is collected in late June every year.

What seems problematic with the data set is the way part-time work is observed. The law differentiates between full- and part-time work as described above. The panel waves used only encompass information on which share of the employees regularly works less than 15, 24 or the ordinary number of hours a week. On the basis of these data, ad-hoc assumptions had to be used to calculate the number of employees according to the employment protection law. As there are no other data sources, this has been the only option. Thus, some companies with part-time workers will be assigned incorrectly to the treatment or non-treatment group.

Temporary employment is only sparsely observed in the data set. Unfortunately there is no information as the design of the law addresses it. This leads to an error-in-variables problem as there will be establishments incorrectly observed as below the legal limit.

Another data problem which is not specific to this analysis is a potential sample selection problem due to panel mortality. Especially a correlation between exits from the panel and the status variable determining group membership would distort the results.

The data set contains rich information on company behaviour. On top of the original outcome variable – growth of number of employees – one can look at the means of substitution which might be addressed in future research.

4 Application of the Regression Discontinuity Design

An appropriate estimation framework seems to be the RDD in its sharp form.[4] It applies to situations where assignment to a treatment is deterministically explained by a covariate z_i: once the selection variable z_i exceeds a threshold value z_0 an indicator variable x_i turns from 0 to 1:

$$x_i = \begin{cases} 0 & \text{if } z_i < z_0 \\ 1 & \text{if } z_i \geq z_0 \end{cases} \tag{1}$$

The observed outcome yi can then be described as

$$y_i = y_{0i} + x_i(z_i)\, ß_i \tag{2}$$

where y_i represents the observed outcome variable and y0i the outcome variable in the possibly hypothetical non-treatment case. $ß_i$ stands for the treatment effect of the program, in this case not being at the threshold of law.

The main advantage of using RDD is that one does not need to assume a model to hold neither in its specification of variables included nor a parametric form. As Hahn et al. (2001, p.16) put it, "this is because the effects of all these other unobserved determinants are not discontinuous at" the cut-off point [D.B.] "and so enter through y_{0i} in (1). As long as their effect is continuous at" the cut-off point [D.B.] "their effect differences out in estimation."

As Battistin and Rettore (2003) point out, the sharp RDD features two reasonable drawbacks: "firstly, its feasibility is by definition confined to those instances in which selection takes place only on observable pre-intervention variables". This seems to be the case for German job protection legislation: Given the value of z, number of workers as the law defines it, assignment to the treatment is determined. Using the employment data before shifts of the threshold level from ten to five

[4] Although the data at hand possibly would support the assumptions required for a matching estimator, the common support problem prohibits the application of such an estimator; cf. Lechner (2001). Recent applications of the RDD are Hahn *et al.* (1999), Carling and Larsson (2002) or Buddelmeyer and Skoufias (2003).

employees (and vice versa), the covariate z can be interpreted as a pre-program variable. Battistin and Rettore go on: "Secondly, […] it only permits to identify the mean impact of the intervention at the threshold for selection." One will have to be aware of this fact when interpreting the figures. An interesting point would be to assess the effects of a change of the cut-off level. Such a statement can not be made using RDD without assuming some continuity in the treatment effect (cf. Heckman et al. 1999, 1970f.). On the other hand, the effect of employment protection on small enterprises' employment behaviour is just the purpose of this study. It turns out that both of these disadvantages do not question the RDD estimation method in this application.

4.1 Identification of Treatment Effects

What assumptions have to be made in order to identify treatment effects? Hahn et al. (2002) show that under sparse assumptions treatment effects can be identified and non-parametrically estimated.

Apart from the RDD setting of the index variable x_i being a discontinuous function of z_i all one needs to assume is continuity of y_{0i} in z at z_0. Interpreting $E(y_{0i}|z_i=z)$ as a function of z, for the identification of treatment effects continuity of this function at z_0 is needed.

That means that – given they are not subject to job protection laws – establishments close to the cut-off point will show the same employment behaviour independent of from which side they are approaching the cut-off level. This crucial identifying assumption seems plausible as the cut-off point z_0 has been picked arbitrarily. As the case considered here is a type of a natural experiment, one is in a very comfortable situation to handle the problem of self-selection into the treatment. RDD just assumes that there was no such behaviour before the treatment comes into being. On top of this, it is guaranteed that there is no drop out of the program. No company exceeding the threshold of the number of employees can escape the threat of being put on trial after dismissing a person. So, neither entrance to nor exit from the treatment is assumed to be endogenously determined by the firm.

Still, there are problematic points in the estimation: First, one has to mention a general problem with estimation of effects with regard to the job protection provisions: the stable unit treatment value assumption (SUTVA) might be violated as a result of the KschG being a large scale program applying to all companies in Germany exceeding the threshold number of employees. The treatment might influence employment behaviour of control group establishments near the threshold as well: as a result, big establishments might find it easier to hire personnel, since smaller companies presumably tend to hire fewer workers. So, a violation of SUTVA spoils the estimated effect that then might be measurement of a mix: the effect on both groups, the treated one and the untreated one. One will have to keep this in mind when interpreting the results.

Another point is the problem that the existence of a threshold might influence the behaviour of both groups: the establishments currently covered by the law and the ones below the cut-off point. To circumvent the problem, the assumption is made that during times of an expected positive demand shock establishments will not be able to reduce employment in order to slip away regulation of employment protection. As pointed out above, the sample used was reduced to those cases where companies reported at least one year of positive expectations.

Another source of possible self-selection can not be neglected: as the data are collected in late June every year. For both law changes decision might have anticipated the new threshold. In 1996 the conservative coalition agreed upon the new law in late May, in summer 1998 the opposition parties made the reintroduction of employment protection for small establishments an important message of their election campaign. But as the time span to react was quite short in both cases it is unlikely that these processes have a larger impact on the results.

Last, one might object that there are further thresholds in German labour law contains further threshold values determining a company's employment behaviour: by January 2001 in establishments with five or more workers, the employees had the right to set up a works council, from the size of six on separate toilets for male and female employees have to be provided. Further provisions come into force at the cut-off points of ten (obligatory provision of both a room for breaks and a documentation of work-related risks), fifteen and twenty employees (cf. Wagner et al. 2001, p.179).

The RDD estimator can be regarded as a special case of selection on observables as it follows from the selection rule and the former assumption that

(3) $$(y_1, y_0) \perp x \mid z = z_0.$$

Locally, for $z = z_0$, the conditionality on observables is assumed to hold.

Hahn et al. (2002) show how to identify treatment effects both in a common effects as well as in a variable effects setting. First, consider the case that treatment effects are identical over the individuals, so ßi = ß. Then the local average treatment effect (LATE) of the program ß at point z_0 is identified as:

(4) $$ß = y^+ - y^-$$

with $y^+ \equiv \lim_{z \to z_0^+} E[y_i | z_i = z]$ and $y^- \equiv \lim_{z \to z_0^-} E[y_i | z_i = z]$

For identification of local treatment effects when they are regarded as heterogeneous over firms further assumptions have to be made. In this case $ß_i$ can be different from establishment to establishment. Hahn *et al.* (2002) show that

(5) $$E(ß_i | z_i = z_0) = y^+ - y^-$$

First a local continuity assumption of $E(ß_i | z_i = z)$ at $z = z_0$ is needed. Apart from that a conditional independence assumption concerning x_i and β_i has to be made. Conditional on z being close to z_0 the index variable x_i and the individual treatment effect β_i are assumed to be independent. This independence assumption would be spoiled if one could expect treated establishments to be more likely to have larger prospected gains from the treatment. Since one can rely on the quasi-experimental character of the setting, such a stochastic dependence of the two variables can be ruled out by assumption.

So, in any case, a constant and a variable effects setting, local treatment effects can be identified by a couple of innocent assumptions.

4.2 Estimation of Treatment Effects

For both cases – the constant and the variable effect one – the treatment effect ß can consistently be estimated in the sharp RDD case by

(6) $$\hat{ß} = \hat{y}^+ - \hat{y}^-$$

with $\hat{y}^+$ and $\hat{y}^-$ as consistent estimators for the limits y^+ and y^-. For estimation of the LATE several methods have been proposed. Due to the poor small sample behaviour of (e.g. one-sided uniform) kernel estimators at boundary points Hahn *et al.* (1999 and 2002) propose to use a local linear regression: the limit y^+ should then be estimated by $\hat{a}$, where $\hat{a}$ is computed as follows:

(8) $$(\hat{a}, \hat{b}) = \underset{a,b}{\operatorname{argmin}} \sum_i (y_i - a - b(z_i - z_0))^2 K\left(\frac{z_i - z_0}{h}\right) 1(z_i > z_0)$$

with K as a kernel function, h its bandwidth and $1(\cdot)$ as an index function. The asymptotic properties of the estimator $\hat{ß}_{LLR}$ are derived at Hahn *et al.* (1999, p. 11ff).

It has been argued that the choice of the kernel has a minor effect on the estimation results. The one applied here is default Stata setting of an Epanechnikov kernel estimator. In order not to interfere with other effects of thresholds in labour legislation calculations have been conducted for bandwidths one to five. E.g. from fifteen employees on, a company becomes subject to special provisions to hire disabled workers.

5 Estimation Results

To estimate the treatment effects Stata 8.2 and its newly implemented command for local linear regression was used. The estimation of its variance has been bootstrapped using 200 samplings of the original data.

The split table 1 shows the estimated effects of the local treatment effects on employment growth. The first part presents the estimations for the effect on growth from June 1996 to June 1997, the second one depicts growth to June 1998.

Tab.1: LATE estimations of employment growth; outcome variable: employment growth as defined in the employment protection law from 6/96 to 6/97 and from 6/96 to 6/98 (estimated std. error in brackets)

	Employment growth according to job protection law from 6/96 to 6/97				Employment growth according to job protection law from 6/96 to 6/98			
Bandwidth	$\hat{y}^-$	$\hat{y}^+$	LATE	n	$\hat{y}^-$	$\hat{y}^+$	LATE	n
1	1.063	1.067	.004 (1.548)	42	2.074	2.675	.601 (2.832)	35
2	.944	.611	-.333 (1.393)	85	1.775	1.753	-.022 (1.957)	71
3	.966	1.132	.166 (1.060)	132	1.113	.663	-.450 (1.406)	114
4	.927	1.356	.429 (.993)	165	1.195	.363	-.832 (1.297)	151
5	.885	1.391	.506 (.953)	204	1.323	.503	-.820 (1.230)	192

Source: IAB establishment panel, own calculations, *** 1% , ** 5%, * 10% significance level

There is no evidence that the threshold at ten employees has a hindering effect on employment growth in small establishments. Although most of the results for the one and two year interval show the expected positive sign none of them is significant. Regardless of the size of bandwidth one chooses the estimated treatment effect is insignificant.

The results for employment growth between 1996 and 1997 are visualized in the appendix. Estimations of expected employment growth are plotted against the number of employees in 1996. The step at the threshold value of five different bandwidths constitutes the estimation of the local average treatment effect of the job protection intervention.

This result is in line with the findings of Friedrich and Hägele (1997), Wagner *et al.* (2001) Verick (2004) and Bauer *et al.* (2004). None of them finds strong empirical evidence for the arguments predicted in theory and brought forward in public debates.

A problem of the evaluation is the fact that the threshold would have become fully effective first in autumn 1999 when the transition period had ended. Up to that time exceeding the number of ten employees does not imply that the whole personnel turns from an unprotected state to a protected one. Strictly speaking, the threshold at ten is mostly an expected threshold for the time from late 1999 on. Unfortunately - from the researchers' point of view - the threshold had been removed back to ten employees before it really became effective. Nonetheless, one could expect some adjustment behaviour of the employer prior to 1999 because of the time-consuming process to dismiss employees. Second, one can argue that the threshold existed already for those establishments that had hired at least one person after October 1996 as the transition period would not apply to these newly employed workers.

Tables 2 and 3 depict the estimated LATEs for the year 1999 to 2003 at the threshold of five employees. In 2000 questions on part-time employment had not been asked. The year had to be skipped from the analysis for this reason.

In general, the results remain insignificant even for this second policy. For the periods to 2002 and 2003, all results are insignificant. But there are exceptions for small bandwidths and the years 1999 and 2001. For a bandwidth of 1 the growth in employment between mid 1998 and mid 1999 one finds weak significance for the difference indicating a higher job growth for the treatment group establishments with more than five employees. In contrast to this, using a bandwidth of 1 or 2 one can observe a weakly negative significant parameter for the period to 2001.

Tab.2: LATE estimations of employment growth; outcome variable: employment growth as defined in the employment protection law from 6/98 to 6/99 and from 6/98 to 6/01 (estimated std. error in brackets)

	Employment growth according to job protection law from 6/98 to 6/99				Employment growth according to job protection law from 6/98 to 6/01			
Bandwidth	$\hat{y}^-$	$\hat{y}^+$	LATE	N	$\hat{y}^-$	$\hat{y}^+$	LATE	N
1	.632	1.80	1.169* (.605)	58	1.361	.052	-1.309* (.773)	58
2	.710	.775	.0654 (.552)	143	1.282	-.056	-1.34** (.679)	123
3	.721	.715	-.006 (.437)	232	1.211	.389	-.821 (.596)	192
4	.719	.619	-.100 (.364)	322	1.195	.610	-.584 (.612)	273
5	.718	.541	-.178 (.352)	422	1.188	.364	-.824 (.577)	356

Source: IAB establishment panel, own calculations, *** 1% , ** 5%, * 10% significance level

Tab.3: LATE estimations of employment growth; outcome variable: employment growth as defined in the employment protection law from 6/98 to 6/02 and from 6/98 to 6/03 (estimated std. error in brackets)

	Employment growth according to job protection law from 6/98 to 6/02				Employment growth according to job protection law from 6/98 to 6/03			
Bandwidth	$\hat{y}^-$	$\hat{y}^+$	LATE	n	$\hat{y}^-$	$\hat{y}^+$	LATE	N
1	1.105	1.650	.545 (1.196)	40	1.757	2.916	1.160 (2.042)	32
2	1.121	1.048	-.0730 (1.072)	85	1.895	.937	-.958 (1.578)	63
3	1.101	2.404	1.303 (.915)	131	1.805	.632	-1.174 (1.380)	98
4	1.093	2.395	1.302 (.843)	187	-.0776	.306	.383 (.837)	137
5	1.090	1.613	.523 (.863)	252	-.0975	.0111	.109 (.620)	177

Source: IAB establishment panel, own calculations, *** 1% , ** 5%, * 10% significance level

This indicates an even smaller growth in the number of employees for establishments above the threshold than for those possibly discouraged to grow into coverage of the law. These findings contradict all theoretical considerations. It is questionable whether the relatively small sample sizes justify a direct interpretation since the results might be driven by outliers.

In general, the results are quite robust against the choice of bandwidth and the duration of the period analyzed. The hypothesis of a retarding effect on small establishments can not be supported by the results reported above.

However, it is important to keep in mind the following caveats: First, the large scale program employment protection might tend to influence even the outcome variable employment growth for

both the control and the treatment group. The reluctance to hire of the smaller establishments might imply more favourable conditions for the larger companies to hire. In this case the control group would not represent the contrafactual case of a comparable unit in a world without the program. As the threshold treatment applies to a limited number of establishments and due to a regulated labour market in Germany this drawback can be considered as minor.

The same applies if establishments with more than five (ten) employees were influenced in the sense that they decided to reduce the number of employees in spite of high adjustment costs and although they faced a positive demand shock. Still the results for small bandwidths show an unexpected and significantly negative sign which might be interpreted as a hint that this assumption is violated.

A third point is the fact mentioned by Verick (2004) that many employers seem not to be aware of the details of the employment protection provisions. In this case an establishment at the threshold would not behave the expected way, owing to the false belief it had already jumped the threshold.

At last one has to warn that the results do show comparisons between establishments possibly threatened by the threshold of the law and those that are subject to the law. One can not infer that employment protection in itself does not have an employment deterring effect, just the threshold could not be confirmed to be retarding employment.

6 Conclusion

The paper at hand examined the hypothesis that employment protection as it is implemented in the German labour legislation deters small establishments from employment growth in times of positive demand prospects. It is argued that a threshold value that determines whether an establishment is covered by the law does provide such a threat. The results support earlier findings that found no evidence for the assumed hindering effect of the threshold.

To identify treatment effects of the threshold in employment protection changes in the cut-off number in the years 1996 and 1998 were used as unintended experiments. This justifies the use of a Regression Discontinuity Design as an evaluation approach which allows to estimate local treatment effects bypassing all questions of model specification. Both the question of inclusion of regressors, as well as functional form assumptions can be neglected in the RDD setting used. But as the RDD suffers a lack of generality, it only allows identification of local treatment effects for establishments at the threshold value.

The data base used were the waves 1996 to 2003 of the IAB establishment panel surveying a large sample of German establishments on a yearly basis. Despite some problems in calculation of the establishment size according to the details of the employment protection law the data set seems appropriate for this analysis providing a sufficiently large sample of the establishments at the threshold.

The results are in line with earlier studies finding no evidence for hindering effects on job growth in small establishments. Still, one should still be cautious about these findings: One can not be sure whether all assumptions were met: Other thresholds in labour legislation could thus have led to some self-selection into the treatment and control groups. Establishments above the legal cut-off number might have been influenced even during times of strong demand expectations. Data problems as non-random panel mortality and incorrect allocation to control and treatment groups limit the straight-forward and possibly premature interpretation of the results.

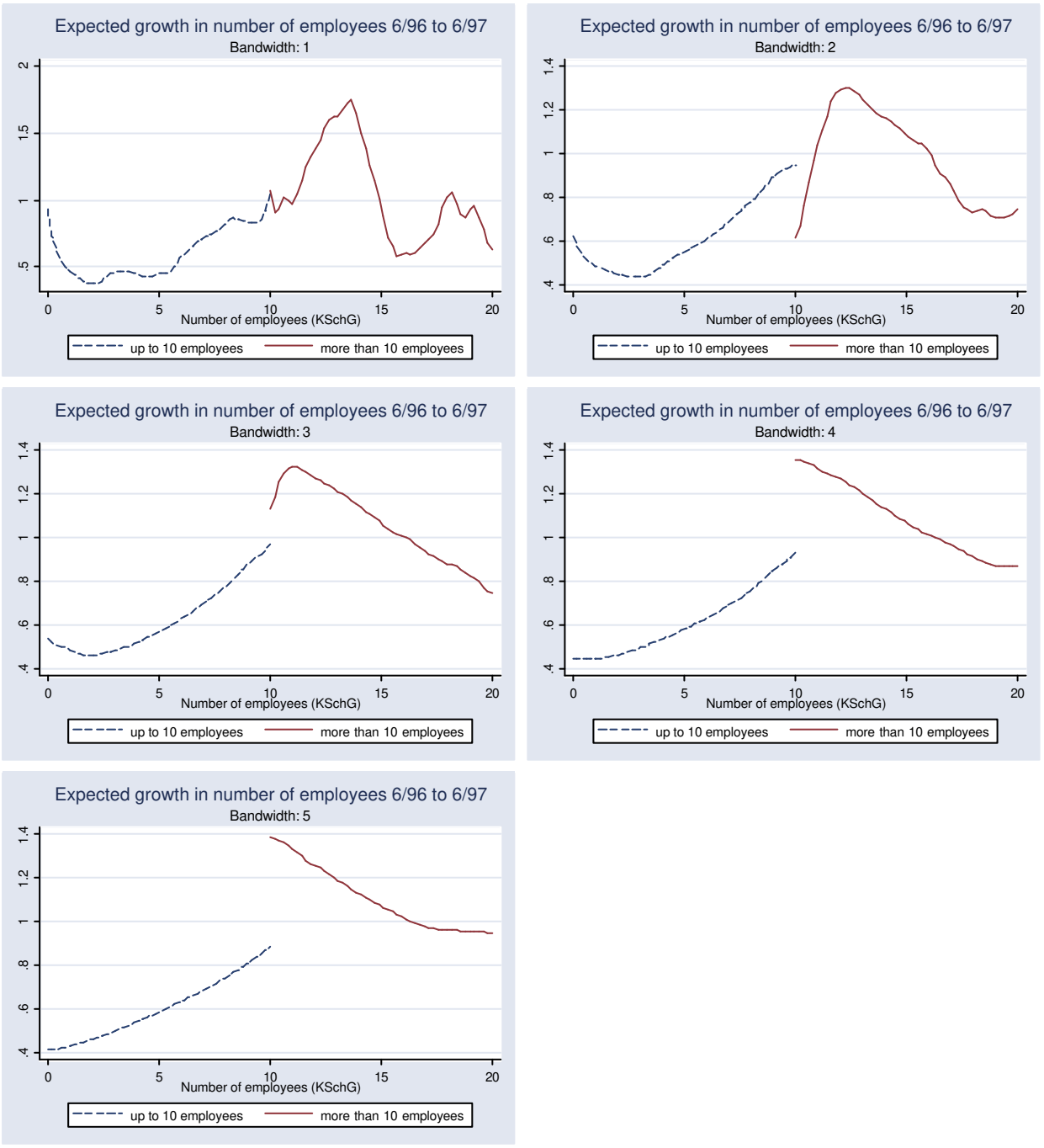
Verick (2004) presents hints that the threshold number of employees might not be fully perceived by decision makers within the establishments. So the interpretation has to be confined to the threshold as defined in the law not as they might be understood. It would be interesting to find out how decision makers really perceived the law and its applicability to their firm. To include this information problem into the analysis one could think of applying a fuzzy RD design to the problem using the perceived membership of the control and treatment group depending on establishment size.

Since there are hints pointing at a reluctant hiring behaviour of establishments at their perceived threshold, it would then be interesting to find out how establishments substitute the factor labour. Do they switch to capital-intensive production? Do they change their make-or-buy decision? Do they

rather produce somewhere else than in Germany or just increase their overtime work? These questions could be addressed in further research.

Appendix

Fig.2: Estimation of LATE by local linear regression by bandwidth



Source: IAB establishment panel, own calculations

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