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February 2009

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Abstract

Small firms are seen as important drivers of dynamics and innovation. They need to be particularly flexible and be able to react quickly to new challenges. This paper uses the latest change in dismissal protection legislation in Germany as a natural experiment and tries to find causal effects on the hiring and firing behaviour of small firms. Using a difference-in-differences approach, I find only small but positive effects on the total number of hirings. However, there are substantial substitution effects from temporary contract hirings to permanent contract hirings. The results remain robust when using count data models and applying fixed effects specifications.

Keywords: Employment Protection, Small Business, Worker Flows
JEL: J38, K31, M21

1. Introduction

Policies focusing on small firms are en vogue as small firms are seen as important drivers of dynamics and innovation. Rather than inefficient replicas of large firms, small firms have become to be regarded as *agents of change*, which take on a role complementary to large firms along technological trajectories. However, large and small firms differ substantially when it comes to managing the innovative process. In contrast to the more routinized large firms, small firms need to be particularly flexible and be able to react quickly to new challenges. This is why any regulations that reduce firms' flexibility might be especially detrimental for small firms.

One of the latest reforms in Germany intended to increase small firms' flexibility was the relaxation of the Protection Against Dismissal Act (PADA) in 2004. With this reform, the dismissal protection regulation had been abandoned for workers who were hired after the 31st December 2003 by small establishments. Whereas in theory, the effects of dismissal protection legislation on overall employment stocks are ambiguous, there are rather clear cut implications for jobflow variables. Dismissal protection acts like a tax on firing and thus raises firms' adjustment costs. As a consequence, stricter dismissal protection legislations are expected to reduce both the number of hirings and firings. In contrast, relaxed dismissal protection should lead to an increase in hirings and firings due to lower adjustment costs.

This paper can be seen in the tradition of empirical analysis of dismissal protection on the firm level. Applying a classical difference-in-differences approach on a large German firm level dataset, we try to find causal effects of relaxed dismissal protection legislation on the hiring and firing behaviour of small firms. In particular, we draw upon the latest change in dismissal protection legislation in Germany, which constitutes a natural experiment. The fact that data for several time periods before as well as after the legal change are available, which are "unspoiled" by other legal changes, further allows me to directly test the underlying

assumption of any difference-in-differences estimations, namely that the time trends of treatment and control group are the same.

Results from difference-in-differences regressions suggest that the latest change of dismissal protection legislation in Germany had a small but positive effect on the total number of hirings in small firms. However, there are substantial substitution effects from temporary contract hirings to permanent contract hirings. In a multiple period specification, we explicitly show that the key identifying assumption of this approach is met. The results remain robust after using count data methods and fixed effects regressions.

The remainder of the paper is organized as follows: Section 2 gives a theoretical overview of small firms and dismissal protection. Section 3 introduces the empirical method used and section 4 describes the dataset. Descriptive statistics and estimation results are given in section 5. After presenting some robustness checks, I conclude in section 6.

2. Small Firms and Dismissal Protection

Starting in the 80s of the last century, we have seen a rise in interest in the economics of small business. In the traditional view, small firms were seen as being inefficient and costly for the whole economy due to a lack of scale economies (cf. Weiss 1964, Scherer 1973, Pratten 1971). Thus, politicians were advised to take economic activities away from small firms and give it into the hands of large corporations, which would lead to an increase in overall economic welfare (Weiss 1979, Brown et al. 1990). However, Audretsch (1995) criticized that this view was misleading since it only took a static perspective. Rather than small and inefficient replicas of large enterprises, Audretsch considers small firms to be *agents of change* in a dynamic sense. Applying new, more direct measures of innovation, Acs and Audretsch (1987, 1990, 1998) show in a series of papers that small firms account for a considerable amount of innovative activities. In another article, Audretsch (2002) gives empirical evidence that small enterprises were major sources of employment growth and

innovation in the resurgence of the US economy in the early 90s of the last century. Nooteboom (1994) suggests that small and large firms play complementary roles along the life cycle of a new product group, or a "technological trajectory" (cf. Nelson and Winter 1982; Dosi 1984, 1988). What follows is, in a sense, a synthesis of the early Schumpeter (1909), who highlights the role of entrepreneurs or small firms in the process of creative destruction, and the late Schumpeter (1942), who considers large routinized firms to be the engines of progress. Nooteboom (1994) argues that while small firms might not be responsible for major scientific and technological breakthroughs, they play an important role by implementing, applying, differentiating and adapting inventions along their "technical trajectories". Baumol (2002) states that initially, small firms or entrepreneurs might have advantages in coming up with radical innovations. But after that, technical progress is further driven by large routinized firms with specialized research facilities. In a recent article, Falck (2009) revisits this "David-Goliath symbiosis" and finds empirical evidence for Baumol's theory with German manufacturing industry data.

Large and small firms differ substantially when it comes to managing the innovative process. One might argue that large firms have often incorporated innovation processes in a routinized manner. In contrast, small firms might rather be characterized in an evolutionary sense as bounded rational agents that live under fundamental uncertainty and imperfect information and generate innovations in a process of trial and error. Thus, they need to constantly update their information on recent developments, keep their flexibility and react quickly to new challenges. This is why it might be argued that regulations that reduce firms' flexibility are particularly detrimental for small firms. These problems have been recognized by politicians. And since small firms are at the same time seen as important drivers of dynamics and innovation, policies focusing on small firms have become en vogue.

One of the latest reforms in Germany intended to increase small firms' flexibility was the relaxation of the Protection Against Dismissal Act (PADA) in 2004. This was part of a broad reform framework, called AGENDA 2010, which aimed at reducing Germany's high structural unemployment. Until then, workers in establishments with more than five full-time equivalent employees were subject to legal dismissal protection. With the reform of 2004, this establishment size threshold was moved upwards. Dismissal protection regulation had now been abandoned for workers who were hired after the 31st December 2003 by establishments (not firms) with more than five and up to ten full-time equivalent employees. During the 90s of the last century, small establishments have seen quite a few PADA changes. What seems particularly interesting with this latest PADA change in 2004 is the fact that it is possible to analyze several "unspoiled" time periods before and after the reform.

The effects of employment protection have been rigorously studied since the seminal work by Lazear (1990) who stated that if labor markets were perfect, any state-mandated severance pay could be undone by Coasian bargaining between employer and employee. However, if labor markets were imperfect, dismissal protection legislation acts as a tax on firing, which makes on the one hand incumbent workers more likely to retain their jobs, but on the other hand decreases the chance of new workers being hired. This boils down to ambiguous effects on overall employment levels which depend on the labour demand function, the size of the discount and attrition rates, and the relative sizes of hiring and dismissal costs (Bertola 1992). While there has been much research on the effects of employment protection legislation on aggregate unemployment and job turnover, much less is known about its effect at the firm level. Dismissal protection raises firms' adjustment costs and thus reduces worker flows. Theory suggests that firms' productivity declines due to distorted production choices (Blanchard and Portugal 2001). Autor et al. (2007) find tentative empirical evidence for these theoretical predictions using US firm level data. Messina and Valanti (2007) use European firm level data and show that stricter employment protection laws dampen the response of job

destruction to the cycle. For Germany, Bauer et al. (2007) find no effect of variable enforcement of dismissal protection legislation during the late 90s on firms' worker turnover. This paper is to be seen in this tradition of empirical analysis of dismissal protection on the firm level. To the best of my knowledge, the effects of the relaxed dismissal protection legislation for small German firms, which was introduced in 2004 in the broad context of AGENDA 2010, has not been investigated yet.

3. Method

During the last two decades, methods of estimating causal effects of programs and policy interventions have been widely discussed in econometrics. See Angrist and Krueger (1998) or Imbens and Wooldridge (2008) for a review of developments in this field of research. The legal change in the dismissal protection legislation in Germany can be seen as a natural experiment, which might allow us to detect real causal effects of this policy intervention. When estimating the effects of the change in the PADA regulations on small establishments' hiring and firing behaviour, the major concern is that unobserved heterogeneity might bias the results. Imagine, we use post-treatment data and run regressions where we include several control variables and a treatment group dummy as explanatory variable. If there are other unobserved characteristics of the treatment group that permanently co-determine the outcome variable, the treatment group dummy correlates with the error term and thus the coefficient is only a biased estimate of the treatment effect. Similarly, using time-series data for the treatment group might lead to biased estimates of the treatment effect due to time trends unrelated to the treatment but also not captured by the control variables. Yet, the problem of biased estimates can be tackled if (at least) repeated cross-section data are available for the treatment and an appropriate control group that cover pre- as well as post-treatment periods. Then, difference-in-differences techniques can be applied, which allow for time-invariant unobserved heterogeneity. This identification strategy yields unbiased estimates under the

assumption that the underlying trends in the outcome variable are the same for both the treatment and the control group, i.e., there are no time-variant unobserved determinants of the outcome variables that cause differential effects on the treatment group and the control group. The idea lying behind this difference-in-differences approach can be described as follows. Z_i is a binary variable which takes the value of unity if an establishment i belongs to the treatment group, and is 0 for observations belonging to the control group. D_i is a dichotomous variable which is unity for observations of an establishment i made after the implementation of the policy change. $Y_i(0)$ is the outcome variable for establishment i in the absence of the policy intervention and can be written as:

$$Y_i(0) = \alpha + \beta * Z_i + \gamma * D_i + \varepsilon_i, \qquad (1)$$

where α is the intercept, β captures time effects common to both the treatment and the control group, and γ represents time-invariant base line differences between treatment group and control group. ε_i is the error term and as such represents unobserved characteristics of establishment i that influence its outcome $Y_i(0)$. The outcome after the treatment is given by the equation $Y_i(1) = Y_i(0) + \delta$. Using a standard difference-in-differences approach, the treatment effect is then described by Equation (2).

$$\begin{aligned} \delta &= E[Y_i(1)] - E[Y_i(0)] \\ &= \{E[Y_i | Z_i = 1, D_i = 1] - E[Y_i | Z_i = 0, D_i = 1]\} \\ &\quad - \{E[Y_i | Z_i = 1, D_i = 0] - E[Y_i | Z_i = 0, D_i = 0]\} \end{aligned} \qquad (2)$$

A simple way to estimate δ, i.e., the treatment effect, in a regression framework is given by Equation (3), where the difference-in-differences parameter δ is the coefficient on the interaction term of the group and time indicators Z_i and D_i.

$$Y_i = \alpha + \beta Z_i + \gamma D_i + \delta(Z_i * D_i) + \varepsilon_i \qquad (3)$$

In general, this term can be estimated by using ordinary least square methods. However, ignoring serial correlation might bias the estimated standard errors downwards. By running simulations, Bertrand et al. (2004) find that clustering robust standard errors, which take into account that the error terms ε_i might correlate within group cells (cf. Rogers 1993), can overcome this bias as long as the number of clusters is reasonably large..

4. Data

The empirical test on the effects of relaxed dismissal protection for small establishments in Germany is based on data from the IAB establishment panel (Betriebspanel). Access to the data was granted via controlled data teleprocessing at the FDZ. The IAB establishment panel is a singular representative dataset of the demand side of the German labour market. Establishments contained in the German Social Insurance Statistics form the population of the IAB establishment file. The establishments are selected according to the principle of optimum stratification of the random sample. Since the stratification cells are defined by firm size categories and industries, these dimensions must later be included in the econometric estimations to ensure representative results.

The annual surveys of the IAB establishment panel started out in 1993 with 4,265 establishments from West Germany. In 1996, establishments from East Germany entered the dataset for the first time. Nowadays, data about nearly 16,000 establishments are collected every year. The surveys include a fixed battery of questions which are repeated every year and a variable part which comprises questions with annually changing topics. As already mentioned above, the PADA does not refer to firms but to establishments, which are the units of measurement in the IAB establishment panel. For a more detailed description of this data source see Bellmann (2002).

To analyze the effects of the latest reform of the PADA in January 2004, data from the IAB establishment panel for the years 2001-2005 are used. All observations from the public

administration and non-profit sector, as well as all private households are dropped since these sectors differ from private firms with regard to their hiring and firing behaviour. Furthermore, the highly subsidized agricultural and mining sectors are excluded. As to the dependent variables, I use the total number of hirings and dismissals[1] per establishment in the first half of a year. The respective numbers for the second half of a year are not available in the IAB establishment panel. Unfortunately, it is also impossible to say whether the newly hired or the dismissed workers are or were full-time or part-time workers.

The establishment size classes are computed as close to the PADA regulations (§23 I KSchG) as possible. I take the total staff number, as reported at the 30th June, and subtract apprentices and establishment owners. In a second step, the number of all persons hired within the first half of the year is subtracted and the number of all persons who left the establishment within the first half of the year is added. The resulting figure is weighted by a full-time equivalent factor *fef*, which is computed following Equation (4):

$$fef = \frac{e_n - (0.5 * e_{pt})}{e_n}, \qquad (4)$$

where e_n is the total number of employees and e_{pt} stands for the number of part-time employees. Since there is no information on the exact number of working hours for every part-time employee, a global weight of 0.5 is assumed, which is considered appropriate at least for Germany (cf. Troost and Wagner 2002). Thus, we arrive at the establishment size which can be regarded as being relevant according to the PADA regulations described in §23 I KSchG. Later on, those establishments with more than 20 PADA relevant workers at the beginning of the year are dropped. These establishments are neither in the treatment group nor can they be regarded as an appropriate control group. We also drop observations with more than 20 hirings or dismissals in the first half of a year since these can be classified as

[1] The dataset allows identifying the number of persons who left an establishment because they retired or died. These individuals do not enter the dismissal rate.

outliers. The remaining establishments are grouped into three size classes: establishments with up to 5 full-time equivalent employees, establishments with more than 5 and up to 10 full-time equivalent employees, and establishments with more than 10 and up to 20 full-time equivalent employees. Since a difference-in-differences approach is applied, these establishment size dummies as well as year dummies have to be included in the estimations.

Apart from size class dummies, we include further variables to control for establishment characteristics, among them a binary variable indicating whether an establishment has got a works council and dummy variables that indicate whether an establishment is bound to a union contract, where we can distinguish between firm level and industry level union contracts. Moreover, we include dummies which classify establishment according to their founding year into one of the following groups: founded before 1990, between 1990 and 1995, between 1995 and 2000, or after 2000. Another pair of dichotomous variables indicates whether an establishment was innovative between 2002 and 2004. Here, a distinction is made between radical innovations, i.e. launching completely new products and services, and incremental innovations, which comprise product/service enhancements as well as imitations. The lagged capital stock is proxied by using the perpetual inventory method as suggested by Black and Lynch (2001), Zwick (2004), and Hempell (2005). Following this method, the book value of the capital stock is calculated by dividing replacement investments by the sum of the assumed average depreciation rate (10 percent) and the assumed average growth rate of investments (5 percent). To take into account sector specific effects, industry dummies are included. Finally, federal state dummies and a dummy for West Germany are used to control for regional effects.

In extended versions of the estimations, we additionally include two year average rates of female employment, and two year average ratios of apprentices as well as of unqualified workers on the whole staff. The two year average ratio of workers that retired or died within

the first half of the respective year is also included as a control variable. Furthermore, the net number of hirings in the previous year is introduced as a control variable, i.e., we take the number of hirings and subtract the number of workers who left the establishment in the first half of the preceding year. Moreover, the lagged average wage per full-time equivalent worker and the lagged value added per capita are computed and enter the extended specification.

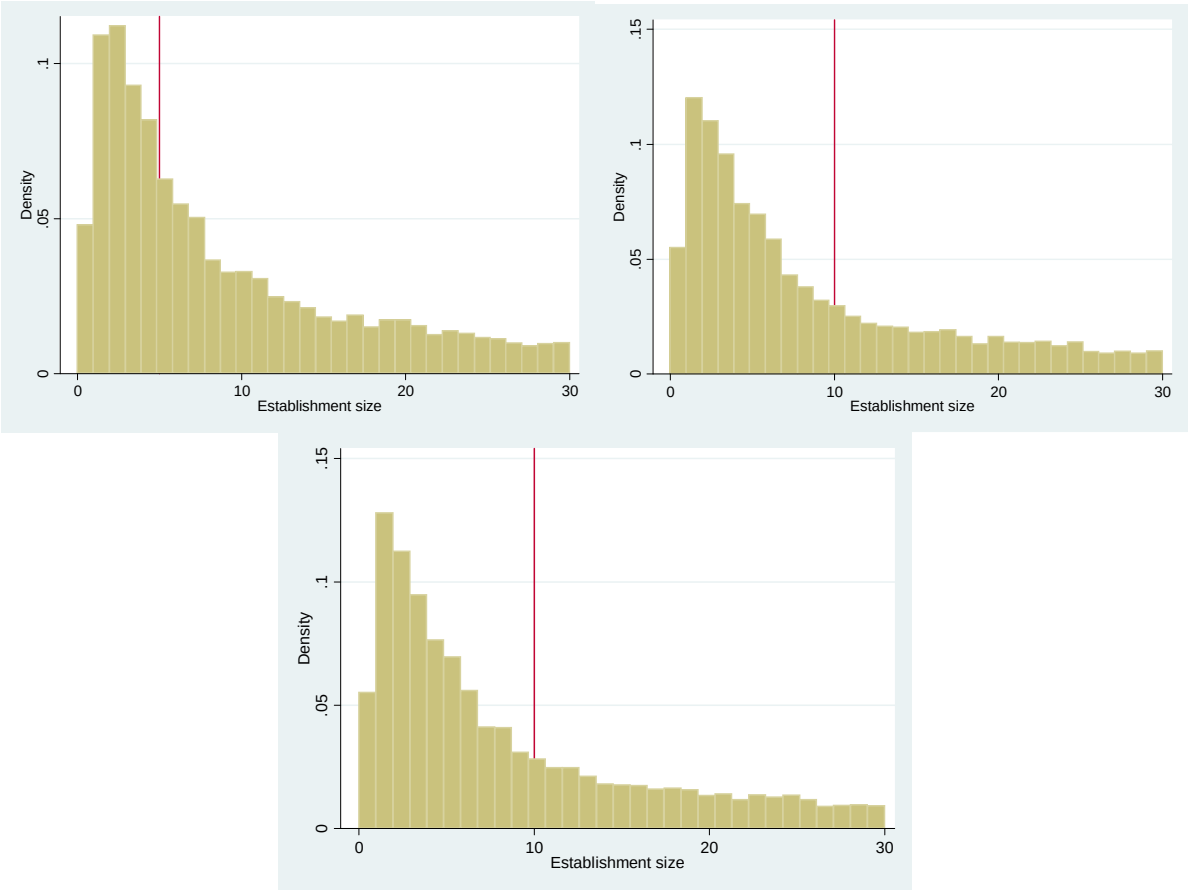
5. Results

Descriptive Statistics

Before going into details of the regression results, we dwell on some descriptive statistics that give an overview over the dataset and support our multivariate strategy. Graph 1 depicts the computed PADA relevant size distributions of establishments at the 1st January 2003, 2004, and 2005. The vertical red lines indicate the PADA thresholds for the respective years, as explained in Section 2. There are no conspicuous chunks in distribution around the thresholds. It seems that German establishments do not self-select into the treatment group, i.e., try to avoid dismissal protection legislations by consciously keeping the employment size below the legal threshold. This is in line with the findings of Bauer et al. (2007), who analyzed earlier changes in PADA regulations with German establishment data for the period from 1995 to 1999.

More detailed descriptive information about the establishments included in the dataset can be found in the Appendix, where summary statistics of all categorical and metric control variables are presented.

Graph 1: Size distribution of establishments in 2003, 2004 and 2005



Analyzing the hiring and firing behaviour of small establishments, not surprisingly, we find that the average number of hirings as well as the average number of dismissals steadily increases with establishment size class in all years observed. Concerning the development over time, it is conspicuous that the average number of hirings increases from 2003 to 2004 for establishments up to ten employees whereas it decreases for those establishments with more than ten employees. It seems that the change in dismissal protection regulations in 2004 had a small positive short run effect on the number of hirings for the treatment group. Table 1 also shows that from 2004 to 2005, the average number of hirings decreases for all establishment size classes. Yet, for establishments with 5-10 employees, the average number of hirings in 2005 lies above the respective value of the base year 2003. This suggests that the relaxed dismissal protection regulations might have had a small positive effect on the number of hirings for this establishment size class. As to the development of the average number of

dismissals over time, we see a steady decline for all size classes except for the establishments with 6-10 employees from 2004 to 2005. This could again be a hint for some positive effect of the legal change, which reduced dismissal costs for establishments in this size class. The fact that this effect cannot be found in 2004 but in 2005 is not surprising. We do not expect any effects on dismissals already in the first half of the year 2004 because the relaxed dismissal protection regulations do not apply to all employees in establishments with 6-10 workers but only to those whose employment contract was concluded after the 31st December 2003.

Table 1: Hirings and Dismissals Across Size Classes and Years

Average number of hirings		**2003**	**2004**	**2005**
Size classes	0-5	.29	.32	.26
		(.94)	*(1.09)*	*(.91)*
	6-10	.54	.57	.56
		(1.24)	*(1.25)*	*(1.42)*
	11-20	1.03	.95	.89
		(2.14)	*(1.93)*	*(2.02)*
Average number of dismissals				
Size classes	0-5	.22	.19	.18
		(.59)	*(.51)*	*(.50)*
	6-10	.57	.53	.55
		(1.16)	*(1.13)*	*(1.10)*
	11-20	.98	.93	.88
		(1.92)	*(1.80)*	*(1.86)*

Note: standard deviations in parentheses.

Alternatively, hiring and dismissal rates are constructed by dividing the number of hirings and dismissals in the first half of the respective year by the number of workers at the beginning of that year. By proceeding like this, we implicitly assume that the average fraction of part-time workers is the same among incumbent, hired and dismissed workers of a specific firm. This assumption is necessary due to data limitations. Looking at the dynamics of these hiring and dismissal rates, a picture is found which is very similar to the one presented in Table 1. This shows that the descriptive findings are not driven by variations in the establishment size composition within the specific size classes.

The PADA modification was part of the AGENDA 2010, a large reformation framework of the German government under Chancellor Gerhard Schroeder. And though the idea of AGENDA 2010 was first announced in March 2003, the details of the PADA modification could not be exactly anticipated by small establishments during the first half of the year 2003, and as a consequence probably did not have impacts on their hiring and dismissal behaviour during that time period. This is why the first half of the year 2003 is considered to be an appropriate baseline. To check if the results are robust to baseline variation, estimations with alternative pre-treatment periods are presented later on. However, for the moment, we do not have major concerns that our results are biased due to a so-called Ashenfelter dip (cf. Ashenfelter 1978). However, we might face similar problems to the ones mentioned by Ashenfelter if we focus on the outcomes during the first half of the year 2004. We cannot observe the number of hirings during the second half of 2003 but it might well be that establishments anticipated the PADA change during that time and postponed intended hirings from the second half of 2003 to the first half of 2004. Thus, if we observe an increased number of hirings for the treatment group in 2004 as compared to 2003, this might also be due to a short run hiring delay effect instead of a really sustained effect on small establishments' hiring behaviour. This is why the first half of 2005 should show more reliable results in terms of the actual effects of the modified dismissal protection legislation that we would like to investigate.

Multivariate Regressions

Although the descriptive cross-tables give first impressions of the effects of the PADA change in Germany, they should be interpreted with caution. To be able to make more rigorous statements about the impact of the relaxed dismissal protection regulations on the hiring and firing behaviour of small establishments, the effects are now analyzed in a multivariate setting.

To begin with, we apply simple linear least squares techniques and compute cluster robust standard errors where federal state and industry form one cluster. The resulting number of clusters is high enough to be confident that the standard errors are not biased downwards (cf. Bertrand et al. 2004). Table 2 presents the effects of the modified PADA regulation in the short and medium run as well as for multiple treatment groups. In the first column of Table 2, we see the short run effects on hirings for establishments with up to 5 full-time equivalent employees as the treatment group, whereas in the second column, establishments with more than 5 and up to 10 employees are defined as the treatment group. The subsequent columns show the medium run effects on hirings and dismissals for the respective treatment groups. For all regressions presented in Table 2, the control group is fixed and consists of establishments with more than 10 and up to 20 employees. The coefficients on the interactions of the treatment group and the post-treatment year dummy are interpreted as the difference-in-differences estimators and thus capture the treatment effects. As a treatment group, both establishments with less than 5 and establishments with more than 5 and up to 10 employees are analyzed. There are reasons for doing this: Establishments with more than 5 and up to 10 employees are directly affected by the modified PADA regulations, whereas the effect on the establishments with up to 5 employees is more of an indirect kind. Though on paper nothing really changes for the latter, particularly establishments in the upper part of the distribution of this size class might benefit in the way that they can further grow without becoming subject to dismissal protection legislation. Moreover, since the legally defined size class concept is rather complicated, employers might just count their workers and thus overestimate their PADA relevant establishment size. One might expect that for the establishments with more than 5 and up to 10 employees the support of the conditional distribution of the covariates given the establishments might show a nice overlap with the conditional distribution of the covariates given the control group with more than 10 and up to 20 employees. This simply means that we might benefit from better comparability of the

treated and control units if the treatment group comprises establishments with more than 5 and up to 10 employees than if the treatment group contains establishments with up to 5 employees. However, this argument will be further investigated later on in the paper.

Table 2: OLS regressions

	Short run effects		**Medium run effects**			
	HIRINGS		**HIRINGS**		**DISMISSALS**	
	Establishment size class		**Establishment size class**		**Establishment size class**	
	0-5	**6-10**	**0-5**	**6-10**	**0-5**	**6-10**
Constant	.6046407 ***	0.7305745 ***	0.752728 ***	0.7903999 ***	0.8346343 ***	1.109155 ***
	(.1060372)	*(.1337743)*	*(.1094115)*	*(.152263)*	*(.0857232)*	*(.1522157)*
Treatment group * After treatment	**0.088917**	**0.1180637**	**0.1471368 ** **	**0.1931313 ** **	**0.0124853**	**0.0487679**
	(.0694478)	***(.0804302)***	***(.0739702)***	***(.0883816)***	***(.0608675)***	***(.0719659)***
Treatment group	-0.7096913 ***	-0.5049306 ***	-0.7170927 ***	-0.4986786 ***	-0.7266913 ***	-0.3942258 ***
	(.0697605)	*(.0747104)*	*(.0710962)*	*(.0732498)*	*(.0543052)*	*(.0580875)*
After treatment	-0.099931	-0.1040351	-0.216913 ***	-0.2260491 ***	-0.0488797	-0.0557836
	(.065958)	*(.0654618)*	*(.0710398)*	*(.0704296)*	*(.0596858)*	*(.0598076)*
Incremental innovation	0.0328281	0.0863103	0.0605885 *	0.0800717 *	0.0061453	-0.0032893
	(.0392375)	*(.0533136)*	*(.0347463)*	*(.0485462)*	*(.0281324)*	*(.0431036)*
Radical innovation	0.1032664	-0.0074836	0.1369487	0.0294018	-0.0542698	-0.1526176 **
	(.0887105)	*(.087107)*	*(.0902725)*	*(.1016338)*	*(.055571)*	*(.0601497)*
Works council	-0.1371618	-0.2208659 ***	-0.2021002	-0.2007017 **	-0.2111415 ***	-0.189221 **
	(.099032)	*(.0823905)*	*(.0794312)*	*(.0865997)*	*(.0812901)*	*(.0762258)*
Branch level union contract	0.1016499	0.0301686	0.0714519 *	0.0235168	0.0288872	-0.0648845
	(.0417164)	*(.0531364)*	*(.0383189)*	*(.0596896)*	*(.0311499)*	*(.0475697)*
Firm level union contract	0.1546143	0.133314	-0.0178247	-0.0152466	-0.0030988	-0.0893776
	(.0939438)	*(.1192785)*	*(.0795811)*	*(.1045317)*	*(.0931421)*	*(.1100722)*
Log Capital stock in t-1	0.0158798 ***	0.0132018 ***	0.0092587 ***	0.0062084	-0.0013315	-0.0072641 **
	(.0033327)	*(.0037995)*	*(.0032782)*	*(.0045904)*	*(.002441)*	*(.0035402)*
Establishment age dummies	Yes	Yes	Yes	Yes	Yes	Yes
Industry dummies	Yes	Yes	Yes	Yes	Yes	Yes
Federal state dummies	Yes	Yes	Yes	Yes	Yes	Yes
West Germany dummy	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	9,812	5,481	8,977	5,066	8977	5,066
R squared	0.0903	0.0840	0.0893	0.0793	0.0916	0.0550

Note: *** 1% level of significance, ** 5% level of significance, * 10% level of significance; cluster robust standard errors in parentheses.

Table 2 presents regressions in a base specification where we do not include lagged variables. First conclusions can be drawn from these figures: It seems that the relaxed dismissal protection did not have an effect on small firms' dismissals. The results also suggest that there might have been a positive effect on the number of hirings in the medium run, i.e., when we compare the hiring behaviour in 2005 to the one in 2003. These first multivariate results on the effects of the relaxed dismissal protection will be further investigated below. However, before this is done, some short remarks on the effects of the remaining independent variables might be of interest. On average, the number of firings and hirings in small firms is smaller

than the respective numbers of the control group (establishments with more than 10 and up to 20 employees), which is exactly what one would suspect. Moreover, there is at least some tendency which indicates that innovations have rather a positive hiring effect on the firm level. However, due to potential omitted variable bias this should be interpreted cautiously. Finally, works councils lower both firms' number of hirings and firings as can be clearly seen in this table. In a second step, we include lagged variables as well as variables which are constructed as averages over years. Thus, the model fit is enhanced, albeit at the cost of losing those establishments for whom we do not have data for at least two subsequent years. The regression results with this extended specification do not substantially differ from the results presented in Table 2. There is no effect of the relaxed dismissal protection on the number of dismissals; the slightly positive effects on the number of hirings can now more readily be seen in the short run rather than in the medium run.

The key identifying assumption of any difference-in-differences approach is that the time trend for the treatment group is the same as for the control group in the absence of the treatment. Bauer et al. (2007), who investigate the effects of earlier PADA changes on the hiring and firing behaviour of small German establishments, try to verify this assumption by investigating bankruptcy rates of small establishments over time. Looking at these data, they find that at least establishments with 6 to 10 employees react similarly to the control group of 11 to 20 employees along business cycles. Therefore, they conclude that this can be regarded as being in support of the difference-in-differences assumption. In order to test the assumption in a more direct way, we use a natural extension of our dataset by including several pre-treatment periods. This imposes testable restrictions on the data in the way that in the absence of the treatment in periods t_1 and t_2 it should be true that

$$\left(\overline{Y}_{Z=1,t_1} - \overline{Y}_{Z=1,t_2}\right) - \left(\overline{Y}_{Z=0,t_1} - \overline{Y}_{Z=0,t_2}\right) = 0$$

Whether this equation holds can be tested in an econometric model by including interaction terms of treatment group and pre-treatment periods. If the coefficients on these interactions are substantially different from zero, this questions the key identifying assumption of the difference-in-differences approach. Table 3 presents the results of this multiple period specification. Various conclusions can be drawn from these figures. First, it is worth mentioning that also in this setting with 2001 as the base year, in general, the positive treatment effects on the number of hirings are found whereas there are no effects on the number of dismissals. This means that the difference-in-differences estimates are not sensitive to the choice of the base year, i.e., the results are not mainly driven by a so-called Ashenfelter dip. However, most importantly, it is remarkable that all coefficients of the interaction terms in the pre-treatment years are statistically not different from zero. This means that the treatment and control groups are subject to the same time trends in the absence of the treatment, which proves that the key identifying assumption of the difference-in-differences strategy is met. Apart from this, the multiple period specification also confirms the effects of the remaining right hand side variables that were found in Table 1.

Table 3: OLS Estimations with Multiple Periods

	HIRINGS Establishment size class		DISMISSALS Establishment size class	
	0-5	**6-10**	**0-5**	**6-10**
Constant	.8625234 ***	.9889277 ***	.9341301 ***	1.082427 ***
	(.1160791)	*(.1536984)*	*(.1259641)*	*(.1607555)*
Treatment group * 2002	**.0438784**	**.0658573**	**-.1068771**	**.0128189**
	(.0855737)	***(.1082527)***	***(.0808243***	***(.0988403)***
Treatment group * 2003	**.0896064**	**-.000828**	**.079649**	**.0823667**
	(.0834018)	***(.0969951)***	***(.0689687)***	***(.0835142)***
Treatment group * 2004	**.1810404 ******	**.1218683**	**-.0051032**	**.0224554**
	(.0816143)	***(.0967755)***	***(.069122)***	***(.0764488)***
Treatment group * 2005	**.2379135 *******	**.1939963 ***	**.0911028**	**.1312296**
	(.0793061)	***(.1025261)***	***(.0734553)***	***(.086699)***
Year 2002	-.0490082	-.0634774	.0922056	.0840529
	(.0810263)	*(.0803693)*	*(.0798496)*	*(.0797384)*
Year 2003	-.0790825	-.1076117	-.102236	-.1172456 *
	(.0814382)	*(.0818089)*	*(.0682459)*	*(.0680374)*
Year 2004	-.1794023 **	-.2120785 ***	-.0478327	-.0718402
	(.0755055)	*(.0738219)*	*(.0676274)*	*(.0685794)*
Year 2005	-.300556 ***	-.3381487 ***	-.1545837 **	-.178861 **
	(.0741191)	*(.0743198)*	*(.072803)*	*(.0732546)*
Treatment group	-.8116379 ***	-.5105549 ***	-.8064165 ***	-.4754424 ***
	(.0791336)	*(.0817927)*	*(.0585979)*	*(.0678336)*
Incremental innovation	.032464	.0641955	-.0091549	.0050508
	(.0274642)	*(.0423501)*	*(.0216073)*	*(.0364673)*
Radical innovation	.1870975 ***	.1543147 *	-.016975	-.0764543
	(.0715148)	*(.0849632)*	*(.0431313)*	*(.0588246)*
Works council	-.1572827 **	-.2092081 ***	-.1973906 ***	-.2019033 ***
	(.0655867)	*(.0655563)*	*(.0535339)*	*(.0571598)*
Branch level union contract	.0464262	-.0261196	.0236795	-.0429058
	(.0313227)	*(.0459464)*	*(.0218192)*	*(.0370868)*
Firm level union contract	-.0266179	-.0242365	-.0162749	-.0553532
	(.0545751)	*(.0774436)*	*(.0586239)*	*(.0668505)*
Log Capital stock in t-1	.0125123 ***	.0099793 ***	-.0017395	-.0071927 ***
	(.002322)	*(.0029307)*	*(.0016632)*	*(.0023977)*
Establishment age dummies	Yes	Yes	Yes	Yes
Industry dummies	Yes	Yes	Yes	Yes
Federal state dummies	Yes	Yes	Yes	Yes
West Germany dummy	Yes	Yes	Yes	Yes
Number of observations	20,405	11,405	20,405	11,405
R squared	0.0912	0.0770	0.1065	0.0542

Note: *** 1% level of significance, ** 5% level of significance, * 10% level of significance; cluster robust standard errors in parentheses.

In a next step, we analyze the distribution of the outcome variables. The total number of hirings and dismissals are strongly skewed to the right with a preponderance of zeros. This

suggests that one can improve on OLS with a specification that pays attention to these characteristics. Since the discrete outcome variables can be classified as count data, Poisson regression models could be appropriate since we know that count data often follow a Poisson distribution. This statistical distribution is characterized by the fact that the mean and the variance of the observed variable are the same. However, in a deeper analysis of the discrete outcome variables it can be seen that the distribution of hirings, as well as the distribution of dismissals, show signs of substantial overdispersion. In cases of overdispersion, negative binomial regressions have shown to be more appropriate than Poisson regressions. Table 4 presents the results of negative binomial regressions in a specification with multiple pre-treatment and post-treatment periods. Alternatively, Poisson regressions are run, which deliver results very similar to the ones of the negative binomial regressions.

Qualitatively, the results are the same as with the OLS specification. There is no effect of the relaxed dismissal protection legislation on the number of dismissals in small firms. At the same time, some small effects on the number of hirings can be found. However, these effects are now even less pronounced than in the previous estimations. Again, the key identifying assumption of difference-in-differences approaches is met, i.e. the time trends of treatment and control group do not differ from each other in the pre-treatment periods. The effects of the other right-hand-side variables are broadly confirmed. Most importantly, there are positive correlations between innovations and the number of small firms' hirings while works councils seem to decrease both the number of hirings and dismissals.

Table 4: Negative Binomial Regressions with Multiple Periods

	HIRINGS Establishment size class 0-5	HIRINGS Establishment size class 6-10	DISMISSALS Establishment size class 0-5	DISMISSALS Establishment size class 6-10
Constant	-.2208503	-.0797369	-.0386729	.0708351
	(.1815485)	*(.1677874)*	*(.1984774)*	*(.1796768)*
Treatment group * 2002	**.0019627**	**.0917873**	**-.1564101**	**.1167241**
	(.1143226)	***(.1318725)***	***(.1087925)***	***(.1139082)***
Treatment group * 2003	**.1055344**	**.0070057**	**.0302808**	**.0951566**
	(.1103775)	***(.1160858)***	***(.0997711)***	***(.1085674)***
Treatment group * 2004	**.1875409 ***	**.1540828**	**-.1448817**	**.0436438**
	(.1091582)	***(.1166216)***	***(.0964176)***	***(.0929543)***
Treatment group * 2005	**.0812775**	**.149516**	**-.1061369**	**.1406899**
	(.1176118)	***(.1379294)***	***(.1077105)***	***(.111423)***
Year 2002	-.0385838	-.051415	.0910419	.0793546
	(.075694)	*(.0748882)*	*(.075212)*	*(.075547)*
Year 2003	-.104425	-.1455307 *	-.1228504 *	-.1395418 *
	(.0771424)	*(.0746468)*	*(.0714172)*	*(.071951)*
Year 2004	-.2357336 ***	-.2693316 ***	-.0802242	-.106505
	(.0754833)	*(.0714536)*	*(.0696722)*	*(.0687249)*
Year 2005	-.3575089 ***	-.392474 ***	-.1796541 **	-.21848 ***
	(.0803607)	*(.0777279)*	*(.0794544)*	*(.0783918)*
Treatment group	-1.292055 ***	-.6447177 ***	-1.548678 ***	-.6654274 ***
	(.0951649)	*(.0943188)*	*(.0786752)*	*(.0842988)*
Incremental innovation	.0962632 **	.1010518 **	.0038784	.0116065
	(.0490201)	*(.0496724)*	*(.0432979)*	*(.044467)*
Radical innovation	.3761131 ***	.1982459 **	.0628161	-.0843544
	(.09561)	*(.0861599)*	*(.0892085)*	*(.0868111)*
Works council	-.1246747	-.2939093 ***	-.2780217 ***	-.2919217 ***
	(.1090122)	*(.0997352)*	*(.0913655)*	*(.0938041)*
Branch level union contract	.0980633 *	-.0664112	.0920603 **	-.0694906
	(.0508744)	*(.052674)*	*(.0440612)*	*(.0452944)*
Firm level union contract	-.0374146	-.0031981	-.0468955	-.0705456
	(.0997299)	*(.1031514)*	*(.1172442)*	*(.0994071)*
Log Capital stock in t-1	.0258719 ***	.0127383 ***	-.001777	-.009788 ***
	(.0034835)	*(.0034061)*	*(.0033043)*	*(.0029992)*
Establishment age dummies	Yes	Yes	Yes	Yes
Industry dummies	Yes	Yes	Yes	Yes
Federal state dummies	Yes	Yes	Yes	Yes
West Germany dummy	Yes	Yes	Yes	Yes
Number of observations	20,405	11,405	20,405	11,405

Note: *** 1% level of significance, ** 5% level of significance, * 10% level of significance; cluster robust standard errors in parentheses.

Given that the theoretical predictions of positive effects of relaxed dismissal protection on firms' hirings and firings are particularly clear cut, the empirical results presented so far seem

to be puzzling in a way, although they are generally in line with earlier studies for Germany (cf. Bauer et al. 2007). The solution to this puzzle might be found in another German labour market institution. In general, dismissal protection can be avoided if workers get only temporary and not permanent contracts. Thus, one would expect that small firms substitute temporary contract hirings with permanent contracts hirings once dismissal protection is relaxed. And as a consequence, we might find substantial positive effects on the number of newly hired with permanent contracts even if there is only a very small effect on the total number of hirings. Table 5 presents multiple period OLS and negative binomial regressions, where the dependent variable is newly hired workers with permanent contracts only.[2] The results clearly confirm the theoretical predictions. Obviously, small firms substituted temporary contract hirings with permanent contracts hirings after dismissal protection for hirings was relaxed. The effect seems to be slightly stronger for firms with more than 5 and up to 10 employees, which are directly affected by the modified law. And again, the key identifying assumption of the difference-in-differences approach is met, i.e., treatment and control group follow similar trends in the absence of the treatment.

[2] As to the independent variables, we use the same specification as above. However, the coefficients of the controls are not presented in Table 5 since they cannot explain any variation of the new dependent variable.

Table 5: Hirings with permanent contracts as the independent variable

	OLS HIRINGS (PERMANENT) Establishment size class 0-5	OLS HIRINGS (PERMANENT) Establishment size class 6-10	NEGBIN HIRINGS (PERMANENT) Establishment size class 0-5	NEGBIN HIRINGS (PERMANENT) Establishment size class 6-10
Constant	.4781108 **	.4238063 *	-1.094508 **	-.5888196
	(.2367294)	*(.2486329)*	*(.4377661)*	*(.4407733)*
Treatment group * 2002	**.0082736**	**.3204117**	**-.1213832**	**.5293135**
	(.2223674)	***(.3218258)***	***(.4133122)***	***(.472649)***
Treatment group * 2003	**.0640047**	**.1482027**	**-.0493586**	**.3633581**
	(.2470012)	***(.2651993)***	***(.3871045)***	***(.4069785)***
Treatment group * 2004	**.2240838**	**.3394247**	**.0547978**	**.7991659 ***
	(.1869989)	***(.2187583)***	***(.4015016)***	***(.4214829)***
Treatment group * 2005	**.5954904 *****	**.6040911 ** **	**1.199585 *****	**1.287027 *****
	(.1981164)	***(.2504062)***	***(.4576347)***	***(.5009519)***
Year 2002	-.0065322	-.0291992	.0681956	.02676
	(.2214629)	*(.2225451)*	*(.2379508)*	*(.2654434)*
Year 2003	-.0881298	-.1120544	-.1423268	-.1979992
	(.231609)	*(.2322091)*	*(.2622102)*	*(.254051)*
Year 2004	-.3070791 *	-.4013457 **	-.6049404 ***	-.8414011 ***
	(.1840973)	*(.18577)*	*(.2334153)*	*(.2389405)*
Year 2005	-.4840682 ***	-.5646057 ***	-1.013294 ***	-1.168703 ***
	(.1828773)	*(.1868107)*	*(.2577393)*	*(.2515155)*
Treatment group	-.539486 ***	-.4932413 **	-1.118695 ***	-1.132412 ***
	(.1864431)	*(.2056498)*	*(.3272957)*	*(.3629773)*
Number of observations	1,354	1,278	1,354	1,278
R squared	0.0909	0.0839		

Note: *** 1% level of significance, ** 5% level of significance, * 10% level of significance; cluster robust standard errors in parentheses.

Robustness checks

To further check the robustness of the results and to perform an even deeper analysis, the dataset is unpooled and separate regressions for several sectors are run. What can be seen is that the positive effects of the relaxed dismissal protection legislation on the total number of hirings of small establishments are more pronounced for the trades and the construction sector, while they are less pronounced for industry and services.

So far, we basically proceeded as if we had repeated cross-section data and ran pooled regressions. Now, we want to make use of the panel character of the dataset. Therefore, we

create a balanced panel for the period from 2001 until 2005 by only keeping establishments for which information is available in every single year during this period of observation. Then, fixed effects estimations are run on this new sample; additionally, the error terms are allowed to cluster within firm cells. In contrast to our earlier regressions, we are now able to control for time-invariant effects not just on a group level but on the level of the individual establishment. It is easy to see that loosely speaking, fixed effects estimates can be seen as a certain kind of difference-in-differences estimates. In a simplified way, this fixed effects approach can be described by the following equation:

$$Y_{it} = \alpha + \beta_i + \chi_t + \delta X_{it} + \varepsilon_{it}, \qquad (5)$$

where β_i captures establishment fixed effects and γ_t captures time fixed effects. X is a variable, which is unity in case of treatment and zero otherwise. If we have got two time periods t, where t=1 is the pre-treatment period and t=2 is the post-treatment period, X_{i1} is zero for both treatment and control units, whereas X_{i2} takes the value of unity for treatment units and remains zero for control units. Thus, δ can again be interpreted as the difference-in-differences estimator and captures the treatment effect. Covariates can easily be added to Equation (5). However, it should be noted that this fixed effects approach only draws on the within-variance and therefore, you only get coefficients on covariates that change over time within a certain establishment. Time-invariant variables have to be dropped from the estimation. Running fixed effects regressions on the newly created balanced panel, we find positive and significant short and medium run effects on the total number of hirings of establishments with up to 5 full-time equivalent employees. The positive effect on hirings of establishments with more than 5 and up to 10 employees is positive and statistically significant in the medium run. The relaxed dismissal protection legislation has no significant effects on the number of dismissals of both treatment groups. Taken together, this indicates that also when using fixed effects on a newly created balanced panel, our results are generally

confirmed: Relaxing dismissal protection legislation had a small but positive effect on the total number of hirings, yet no effect can be found on small firms' dismissal behaviour.

6. Conclusion

Using the latest change in dismissal protection legislation in Germany as a natural experiment, this paper tries to find causal effects of the legal change on the hiring and firing behaviour of small firms. Applying difference-in-differences techniques, we find that the effects of relaxed dismissal protection legislation on the total number of hirings of small firms are small, while no effects on the dismissal behaviour can be found. However, there are substantial substitution effects from temporary contract hirings to permanent contract hirings. By explicitly testing the underlying assumptions of any difference-in-differences approach, namely that the time trends for treatment and control group are the same, we can confirm these causal effects.

For future research it might be worthwhile to further investigate the effects of strengthening or relaxing dismissal protection laws on the structure of firms' staffs. Do more stringent labour market regulations favour highly qualified workers relative to less qualified workers? What about the effects on the employment of elderly people or on the number of temporary employments? These questions definitely need more research, yet they could not be tackled in this paper due to data limitations. One might also ask which alternative policy measures could substantially increase small firms' flexibility or have direct effects on small firms' innovative potential.

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Appendix

Appendix 1: Categorical control variables

	Establishment Size Class			
	0-5	**6-10**	**11-20**	**Total**
Founding year				
Before 1990	5,644	2,688	2,565	10,897
	43.87	*53.93*	*53.98*	*48.22*
1990-1995	3,863	1,293	1,309	6,465
	30.03	*25.94*	*27.55*	*28.61*
1995-2000	2,176	666	602	3,444
	16.92	*13.36*	*12.67*	*15.24*
After 2000	1,181	337	276	1,794
	9.18	*6.76*	*5.81*	*7.94*
Total	12,864	4,984	4,752	22,600
	100.00	*100.00*	*100.00*	*100.00*
Incremental innovations				
Yes	2,589	1,449	1,590	5,628
	23.27	*33.07*	*38.07*	*28.59*
No	8,539	2,933	2,587	14,059
	76.73	*66.93*	*61.93*	*71.41*
Total	11,128	4,382	4,177	19,687
	100.00	*100.00*	*100.00*	*100.00*
Radical innovations				
Yes	337	219	265	821
	3.03	*4.98*	*6.33*	*4.16*
No	10,788	4,183	3,924	18,895
	96.97	*95.02*	*93.67*	*95.84*
Total	11,125	4,402	4,189	19,716
	100.00	*100.00*	*100.00*	*100.00*
Union contract				
Branch level	3,251	1,957	1,987	7,195
	25.22	*39.23*	*41.71*	*31.78*
Firm level	288	210	244	742
	2.23	*4.21*	*5.12*	*3.28*
No	9,350	2,821	2,533	14,704
	72.54	*56.56*	*53.17*	*64.94*
Total	12,889	4,988	4,764	22,641
	100.00	*100.00*	*100.00*	*100.00*
Works Council				
Yes	265	349	641	1,255
	2.10	*7.18*	*13.73*	*5.67*
No	12,330	4,515	4,029	20,874
	97.90	*92.82*	*86.27*	*94.33*
Total	12,595	4,864	4,670	22,129
	100.00	*100.00*	*100.00*	*100.00*

Appendix 2: Metric control variables

		Observations	Mean	Std. Dev.	Min	Max
Capital stock						
Size class	0-5	12,517	48,159.41	267,541.70	0	1.07e+07
	6-10	4,770	148,212.20	788,401.50	0	2.22e+07
	11-20	4,557	337,572.10	1,644,034.00	0	3.90e+07
Value added per capita in t-1						
Size class	0-5	7,400	41,725.95	89,546.21	-85,000.00	3,000,000.00
	6-10	2,876	48,703.65	72,752.75	-2,054.31	1,461,250.00
	11-20	2,883	57,041.46	146,084.30	-41,400.00	5,833,334.00
Wage per worker in t-1						
Size class	0-5	8,811	1,237.30	1,031.69	0	40,000.00
	6-10	3,486	1,876.13	979.12	0	14,285.71
	11-20	3,402	2,053.74	963.01	127.36	8,952.38
Net hirings in t-1						
Size class	0-5	9,904	-0.03	1.09	-19	20
	6-10	3,989	-0.03	1.58	-20	20
	11-20	3,818	0.07	2.08	-18	20
Average ratio of unqualified workers						
Size class	0-5	12,917	0.15	0.22	0	1
	6-10	4,996	0.17	0.23	0	1
	11-20	4,770	0.18	0.24	0	1
Average ratio of female workers						
Size class	0-5	12,915	0.45	0.34	0	1
	6-10	4,995	0.45	0.31	0	1
	11-20	4,767	0.39	0.29	0	1
Average ratio of apprentices						
Size class	0-5	12,917	0.04	0.11	0	1
	6-10	4,996	0.06	0.10	0	1
	11-20	4,770	0.06	0.09	0	1
Average ratio of retired and dead						
Size class	0-5	12,917	0.002	0.02	0	0.50
	6-10	4,996	0.003	0.01	0	0.14
	11-20	4,770	0.003	0.01	0	0.14