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ABSTRACT

Dismissal Protection and Worker Flows in Small Establishments*

Based on a large employer-employee matched dataset, the Paper investigates the effects of variable enforcement of German dismissal protection legislation on the employment dynamics in small establishments. Specifically, using a difference-in-differences approach, we study the effect of changes in the threshold scale exempting small establishments from dismissal protection provisions on worker flows. In contrast to the predictions of the theory, our results indicate that there are no statistically significant effects of the dismissal protection legislation on worker turnover.

JEL Classification: J21, J23 and J58
Keywords: difference-in-differences, employment protection and linked-employer-employee dataset

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1 Introduction

In December 2003, a broad coalition in the German parliament approved a set of reforms aimed at reducing the country's high structural unemployment. One element of the reforms is a change in the coverage of small establishments by the dismissal protection code. Specifically, the threshold determining coverage has moved from five to ten workers starting in January 2004. The recent amendment followed two previous changes in only a decade. In 1996, the center-right government under Chancellor Kohl had already raised the exemption threshold from five to ten employees; yet the center-left government under Chancellor Schröder after winning the general elections rescinded this change in January 1999.

Similar to Germany, most European countries exempt firms operating below a certain threshold scale from employment protection legislation. So far, only few studies have empirically analyzed the impact of these thresholds in employment protection legislation on the distribution of firm sizes and the hiring and firing behavior of small firms. The lack of attention is surprising, since small-sized entities represent a substantial share of all firms and total employment. In addition, variable enforcement is a source of within-country variation helping to identify the effects of employment protection legislation on employment outcomes in general.

This paper investigates the effects of dismissal protection on small firms using a large administrative employer-employee matched data set of West German establishments with less than 30 employees. As the rules for exemption from dismissal protection legislation changed over the period covered by the data, we identify the impact of dismissal protection legislation on employment not only along the establishment size dimension. We also analyze employment dynamics of treated establishments before and after the reforms using a difference-in-differences approach.

The paper improves upon previous contributions in several ways. Most importantly, we study *gross* worker flows, i.e. the total number of hirings and separations, while the few available estimates of threshold effects based on firm level data are confined to *net* changes in employment stocks. Analysis of worker flows is clearly preferable, since it is closer to the propositions offered by economic theory. Secondly, we start from a very large sample of employers which in particular does not under-represent the smallest units. Finally, our data refer to the unit of employment for which the exemption threshold scale is actually defined, i.e. establishments rather than firms.

We present estimates based on a cross-sectional approach indicating threshold effects in line with the predictions of basic economic theory. These estimates, however, rely on rather strong identification assumptions and are not robust. Estimates based on a difference-in-differences approach, which are robust, do not confirm a significant correlation between worker flows and the stringency of dismissal protection. This result is in contrast to the previous empirical evidence which taken together suggests a small but significantly positive effect of weaker dismissal protection on employment dynamics in exempted firms.

The paper proceeds as follows. The next section shortly reviews the theoretical and empirical literature. Section 3 describes the institutions of dismissal protection legislation in Germany and discusses our data. Section 4 presents results for cross-sectional and difference-in-differences estimation strategies. Section 5 concludes by discussing possible reasons why the empirical findings do not confirm the theoretical expectations.

2 Theoretical and Empirical Literature

Like other forms of mandatory employment protection, dismissal protection legislation generates non-wage labor costs, unless it is offset by proportionate wage changes through efficient labor contracts (Lazear 1990). These costs, interpreted as a resource cost per worker, reduce the profitability of firms and therefore output and employment. In addition, there might be a substitution effect reducing labor demand depending on the degree of substitutability or complementarity to other factors of production. These negative effects aggravate if protection of insiders is a source of imposing contracting costs on employers (Lindbeck and Snower 1988). However, economic theory also provides arguments in support of employment enhancing effects of employment protection legislation. Labor productivity might increase due to improved training incentives and better job matches (Mortensen and Pissarides 1999). Mandatory dismissal protection may improve the employer-employee relationship by providing insurance against unemployment if insurance markets affected by moral hazard do not provide sufficient coverage (Pissarides 2001).

A more specific interpretation of dismissal protection is that as a tax on labor shedding. Economic theory then relates to worker flows. The basic prediction is a negative correlation between the level of adjustment costs and both hiring and separation rates. During a recession, the wedge between the cost of dismissing a worker and the marginal worker's product leads to fewer firings. Firms rather let employment decline via quits. During a boom, the wedge between the product of the marginal worker and the present value of costs incurred in case of a dismissal later on reduces hirings. If firms are flexible to accommodate shocks by adjusting working hours, these effects are intensified (Hamermesh 1988).

As hiring and firing incentives work in opposite directions, the impact of mandated adjustment costs on employment stocks is theoretically ambiguous. In a partial equilibrium framework, Bentolila and Bertola (1990) and Bertola (1992) show that the direction of the employment effect hinges on the specific functional form of labor demand and the discount rate applied to evaluate future firing events. The conclusion of ambiguous average employment effects is also prevalent in general equilibrium models (Ljungqvist 2002).

In summary, though economic theory does not provide a clear-cut hypothesis about the impact of more stringent dismissal protection on employment stocks, the predicted effect on worker flows is unambiguously negative. The theory of dismissal protection furthermore suggests several structural effects. Worker reallocation may slow down more in sectors facing relatively volatile demand than in sectors facing relatively stable demand (Alvarez and Veracierto 1998). Furthermore, non-uniform enforcement may create a dual labor market: firms expand on jobs with less stringent dismissal protection provisions like part-time or fixed-term work (Saint-Paul 1996).

More generally, firms may engage in strategic behavior to avoid institutional constraints. If exemption is possible on the basis of a threshold scale, larger firms could split into formally independent units. Smaller firms could stop growing to elude the institutional constraint (Borgarello, Garibaldi, and Pacelli 2003).

The available empirical evidence for the impact of employment protection legislation on employment stocks and flows, surveyed by Addison and Teixeira (2001), does not clearly support the theoretical predictions. A vast majority of the empirical literature follows Lazear (1990) in exploiting cross-country variation in index values for the stringency of employment protection legislation, as created for example by the OECD (1999). The only robust empirical finding in this literature is that more stringent employment protection reduces flows into and out of unemployment. The results concerning employment flows are ambiguous, while the estimated effects on aggregate employment stocks are mostly insignificant.

An alternative strand of the literature working with macroeconomic data analyzes the impact of dismissal protection legislation through estimated parameters for the speed of labor adjustment in response to output shocks. In the context of a standard dynamic labor demand framework for Germany, Kraft (1993), Abraham and Houseman (1994) and Hunt (2000) do not find any evidence that a legal amendment in 1985 aimed at reducing dismissal costs fostered employment adjustment.

The empirical results obtained from aggregated data could be criticized on several grounds. In cross-country studies, significant effects mostly arise from the international variation in regulations. Because of the limited time-series variation, it is difficult to gauge the impact of specific legislation and hardly possible to correct for the interaction of employment protection legislation with other institutional features (Bertola and Rogerson 1997). Single-country studies rely on strong identification assumptions to establish a correlation between dismissal protection legislation and labor market outcomes: it is difficult to disentangle dismissal costs from other costs of adjustment. In any case, there is a possibility of aggregation bias masking heterogeneous behavior at the micro level.

Although it seems preferable to work with appropriately disaggregated data, relatively few studies analyze the impact of employment protection legislation at the firm or worker level. This literature exploits two types of within-country variation. A first source of identification is changes in regulations that constitute quasi-natural experiments. Based on labor market reforms in South America, Kugler (2004) and Saavedra and Torero (2004) estimate that lower firing costs significantly reduce the duration of individual employment.

A second source of identification is variable enforcement of regulations. On the one hand, in many countries certain groups of workers are better or worse protected against dismissal than a reference group. Along this dimension, Acemoglu and Angrist (2001) estimate a negative impact of specific protection legislation for disabled workers in the United States on the hiring probabilities of this group. This identification strategy may yield biased results, however, if agents can switch between the flexible and the rigid segment of the workforce.

On the other hand, typically certain employers are exempted from the most stringent provisions for employment protection. Along this dimension, Borgarello, Garibaldi, and Pacelli (2003) study the impact of a 15-employee-threshold in the Italian dismissal protection legislation on the distribution of firm sizes. Their empirical results support the hypothesis that the threshold has a significant but small negative impact on employment growth in exempted firms. Additional estimates by Boeri and Jimeno-Serrano (2003) exploring the same data and institutional setting do not confirm this finding. Their results rather indicate that firms operating below the exemption threshold are more dynamic in the sense of experiencing higher job destruction. Both studies take an empirical approach based on the matrix of aggregate transition probabilities between firm sizes. Hence ignoring valuable information at the disaggregate level, they may not appropriately control for firm heterogeneity.

Verick (2004) presents some significant evidence that a tightening of the exemption threshold through a change in the German dismissal protection legislation was associated with a lower probability of employment growth in the treated establishments. However, the underlying data drawn from the *IAB-Betriebspanel* is not representative for the population of small establishments in Germany. The study furthermore does not analyze worker flows.

3 The Case of Germany

3.1 Dismissal Protection Legislation in Germany

In Germany, many legal provisions governing the relationship between employers and employees depend on firm size. Employment protection legislation in many areas exempts small units altogether, or at least allows them to operate under less stringent arrangements. An example for the latter is the rules concerning dismissal. Statutory protection against dismissal is generally provided under the provisions of the German Civil Code (*Bürgerliches Gesetzbuch*) in combination with the Protection Against Dismissal Act (*Kündigungsschutzgesetz*).[1] However, the more stringent constraints on dismissals under the Protection Against Dismissal Act (PADA) do not apply to businesses employing less than a certain minimum number of workers.

Several criteria determine if the PADA applies. First, the employer must operate below the exemption threshold permanently. Consequently, employees on fixed-term contracts or employed for less than six months per year, vocational trainees and certain 'marginal' part-timers with irregular wages do not count in calculating the number of employees in a unit. Second, legislation concerns dependent employees, which excludes owners, consultants and family members without a labor contract. Third, the threshold scale is based on the concept of full-time equivalent employees. Part-time employees are given different weights depending on their contractual working hours. Finally, the exemption criteria apply to establishments, not firms. An establishment is a production unit at a single location. It can economically or legally depend on other establishments building up to a firm.

These rules apply to all establishments in the public and private sector, with the exception of private households being employers. If an establishment operates below the exemption threshold, it has the right to dismiss any worker provided that comparatively moderate Civil Code standards are met. If it is larger, the restrictions

[1] In addition, collective bargaining agreements or individual employment contracts might include provisions for the case of dismissal. Since these are private contracts, they might serve as a means to allocate the rents from job stability, and therefore do not have an obvious effect on labor market performance.

on dismissal under the PADA apply to all workers counting for the threshold scale. According to the PADA, an employment relationship can only be terminated by way of three exceptions: (i) dismissal on grounds of personal incapability or health problems, (ii) dismissal on grounds of bad conduct, and (iii) redundancy in which case the employer is obliged to adhere to social selection criteria. If the employer could not substantiate one of these three reasons, Labor Courts would generally rectify a dismissal for the reason of *fairness*.

Some systematic guidelines for typical circumstances when dismissal is fair in accordance with the PADA have developed in case law. Employers must obey the *principle of proportionality*— dismissal generally is the last resort. This means that termination of the employment relationship is not permitted, if less severe remedies are available. In particular, training to another job within the establishment or an adjustment of working conditions including a wage change are considered reasonable alternatives to dismissal. As it is rather difficult for employers to prove that all requirements for legal dismissal are met, a high proportion of dismissal cases end in settlements, both in court and out of court. Settlements predominantly result in the employment relationship being cancelled in return for severance payments.

Even if the employee wins a dismissal case, there is statutory provision for Labor Courts to dissolve the employment relationship and rule a payment of compensation for job loss. Essentially, therefore, the German PADA does not operate as a barrier to dismissal but as a mechanism securing material compensation for the dismissed. But even in cases where the provisions for protection against unfair dismissal do not lead to a severance payment, uncertainty about legal positions imposes a latent cost upon establishments under the regulations of the PADA.

Since January 2004, the minimum establishment size for applicability of the PADA is the equivalent of ten full-time employees. Before, the exemption threshold scale changed repeatedly. First, in October 1996, the center-right government under Chancellor Kohl deregulated employment protection by lifting the traditional barrier of five employees. The amendment additionally exempted establishments operating with six to ten employees, which basically is the recently reinstated policy. The liberalization potentially affected a substantial part of the work force. About 30% of all establishments, representing about 75% of all employees paying social insurance contributions, were operating under the provisions of the PADA when the reform was introduced. The more generous exemption policy eased dismissal in about 10% of all establishments, representing about 5% of all employees (Emmerich, Walwei, and Zika 1997). The amendment in October 1996 became immediately effective, however, only for new hires. Already employed workers were guaranteed the original level of protection against dismissal for a transition period of three years. This regulation will make it difficult to exactly identify the treatment group of the reform in the empirical analysis.

Even before the provisions for a gradual transition had expired, the newly appointed center-left government under Chancellor Schröder tightened dismissal protection legislation, by returning to the original threshold value of five employees. Thus, from January 1999, establishments employing six to ten employees once more operated under more stringent regulations for firings, whereas the growth dynamics of smaller units was possibly affected by behavior to avoid becoming subject to the PADA.

3.2 Data

In the empirical analysis, we will use the German exemption policy concerning small businesses, and the repeated changes therein, to estimate the effect of dismissal protection legislation on employment dynamics of establishments in a neighborhood of the threshold. Our estimates are based on a large employer-employee data set specifically constructed for this research on the basis of the German *Employment Statistics Register*. The Employment Statistics Register is an administrative, event history panel data set of individuals based on the notifying procedure for the German health insurance, statutory pension scheme, and unemployment insurance. Employers are obliged to notify the social security agencies about the beginning and the termination of any employment relationship of workers covered by social insurance.[2] All workers employed by the same unit can be matched via an establishment identifier. Following the history of events recorded for a given establishment, it is possible to calculate the stock of employees at any given point in time. In determining establishment size, missing workers without a social insurance record could be a source of measurement error. Given that regular contracted workers, as those covered by the dismissal protection legislation, as a rule are statutory contributors to the social insurance agencies, this issue, however, does not seem to be of practical importance.

¿From this administrative data set, we took 5% random cross-sections representative of West German establishments with less than 30 employees at the beginning of March 1995, March 1997, and March 1999, respectively. The original samples exclude all establishments in the highly subsidized agricultural and mining sectors, as well as non-profit firms. We further exclude all establishments in the shipping industry and all establishments active in aircraft transportation. These sectors are exempted from applicability of the PADA by specific legislation.

We follow employment behavior of sampled establishments in the course of the following twelve months. Specifically, we observe all hirings and separations of employees, though we cannot distinguish between layoffs and quits. We exclude establishments not providing information for the full year as they exit from the panel.[3] Survival probabilities are a positive function of firm size. The sample available for the empirical analysis therefore contains relatively more larger establishments than smaller establishments. Our working sample consists of 53,041 establishments with almost 281.0 thousand employees in March 1995, 54,355 establishments with almost 289.1 thousand employees in March 1997, and 54,900 establishments with slightly more than 289.1 thousand employees in March 1999.

Figure 1 illustrates the construction of our sample together with the timing of the two policy changes under study. Timing is an important issue in the context of

[2] See Bender, Haas, and Klose (2000) for more detailed information about the Employment Statistics Register and the notifying procedure.

[3] Exiting establishments may go out of business, merge with other establishments or divide into new legal entities. Since the data does not distinguish between the different reasons, we cannot be sure that firms engage in strategic exit and entry behavior. The Constitutional Court has ruled out, however, that firms achieve exemption from the PADA by splitting into smaller units.

policy evaluation, because statistical identification of policy effects is contaminated if agents have the chance to modify their behavior in anticipation of the treatment (the so-called Ashenfelter-dip problem). Our first time frame ends seven months before the minimum firm size for exemption from the PADA was lifted in October 1996. A review of the political debate preceding this step suggests that this period is sufficient to preclude anticipation effects. The discussion about the amendment only intensified from April 1996.

The second time frame starts five months after the change in the threshold value from five to ten employees and ends ten months before the reinstatement of the original regulations. We acknowledge that the observation window might begin too early for the reform to unfold its full labor market effects, especially in view of the arrangements made for a gradual transition. A time frame closer to the implementation of the second reform, however, does not seem appropriate. This reform played an important role in the Social Democrats' campaign to win the general elections in September 1998, and the landslide win of the center-left bloc was easily foreseeable.

During the time period covered by the third observation window, the reform of January 1999 was probably generally perceived as being permanent. At least there was no indication that the government would soon once more deregulate dismissal protection. Again, the time frame is probably too close for the reform effects to fully unfold. However, we cannot work with a later sample period due to data limitations. The accessible part of the Employment Statistics Register expires at the beginning of 2001. Many notifications for the final months of 2000 are missing, probably not at random.

In summary, the construction of the repeated cross-sectional samples allows us to be confident that the empirical analysis of the reform effects will not suffer from the Ashenfelter-dip problem. Given the relatively short distance of the observation windows from the implementation of the reform, the measured outcome variables, however, might not capture the full impact of the changes in dismissal protection legislation on employment dynamics.

As explained above, the threshold scale for applicability of the PADA is not based on simple head count, but on the concept of permanent full-time equivalent employees. We therefore need to recalculate establishment size according to the provisions of the law. Specifically, we deduct workers coded as being in apprenticeship training from the total number of workers in an establishment. Employees coded as working less than full-time are given a weight smaller than unity according the specifications of the PADA. The part-time correction might be a source of measurement error, because the hours grid recorded by the Employment Statistics Register does not fully coincide with that relevant for the dismissal protection legislation.[4] Another potential source of measurement error is that we cannot observe fixed-term contracts. Non-permanent workers who are actually not protected against dismissal count as regular employees. This kind of measurement error will not lead to any bias, however, as long as it does not systematically vary with firm size or the outcome variable under study.

The calculation of establishment sizes in line with the provisions of the PADA reduces average unit sizes in our sample by about 0.7 employees in all three cross-sections. Figure 2 displays the distribution of the relevant establishment sizes. One could expect that variable enforcement of dismissal protection legislation generates a discontinuity in the neighborhood of the exemption threshold values. The unconditional employment stocks, however, do not show any obvious discontinuity at the five employees threshold valid in 1995 and 1999, respectively the ten employees threshold valid in 1997.

In line with the theory of dismissal protection, our empirical analysis focuses on worker flows. Following the literature on macroeconomic labor market turnover (Burgess, Lane and Stevens 2000; Davis and Haltiwanger 1999), we define hirings as the total inflow of employees into an establishment during the twelve-months observation windows covered by our data. Accordingly, separations are defined as the total outflow of employees from an establishment during the different one-

[4] For this reason, we cannot control for the effects of a minor change in the weighting scheme which, as part of the 1999 reform, could have brought establishments closer to the exemption threshold without actually adjusting employment.

year periods. Finally, job flows are defined as the difference between hirings and separations. Note that this concept is less informative with regard to labor dynamics. The mapping between gross worker flows and the net change in employment stocks is not unique. Hiring, separation and job flow rates are obtained by dividing the respective worker flows by the initial number of workers in the establishment.

4 Empirical Analysis

Several hypotheses how the PADA affects employment dynamics at the establishment level follow from the theoretical considerations in Section 2. Focusing on the exemption threshold, the behavior of establishments operating below is expected to be different from that of those operating above. We expect that the latter show lower hiring and separation rates compared with otherwise identical, exempted establishments. Units right above the threshold, however, may have an incentive to reduce the level of employment to get below the threshold, whereas they could be expected to behave like other firms above the threshold regarding hiring. Likewise, establishments exactly at or close to the threshold should exhibit lower hiring rates, if they are reluctant to grow to avoid passing the threshold.

Focusing on the impact of the policy changes, a hypothesis to be tested is that the liberalization from October 1996 increased hirings in newly exempted firms. The effect on separations is probably ambiguous due to the provisions made for a gradual transition. Following the return to the original legislation from January 1999, one could expect that hirings and separations decreased in establishments coming under more stringent dismissal protection.

4.1 Unconditional Means

For a basic check whether behavior of establishments is in line with these hypotheses, we analyze unconditional means. Table 1 provides descriptive statistics of worker flow rates by establishment size category and sample period. As expected, worker turnover tends to decrease with establishment size. Very small establishments, however, seem to behave differently than larger establishments with regard to separations. Separation rates in establishments with less than six employees are consistently smaller than in establishments with 6-10 employees. This suggests relatively higher job stability in the former. But this could be a statistical artefact considering that the smallest companies possibly rather go out of business than fire.

The results obtained from comparing unconditional mean worker flows by firm size in consecutive sample periods (before-after) are generally in line with changes in the macroeconomic environment. Economic growth accelerated over the covered period, triggering higher worker turnover. In particular, at the peak of the business cycle from 1999-2000, both hiring rates and separation rates are significantly higher than earlier. This is true for all establishment size categories.

Focusing on the potential effects of the more liberal dismissal protection legislation during the period 1997-1998, the descriptive statistics do not indicate that the reform positively affected the newly exempted establishments. Compared with the pre-reform period, there is no significant increase in hirings and —in contrast to the expectations— even a significant reduction in separations from establishments with 6-10 employees. Hiring and separation rates in establishments with 6-10 employees increased significantly after the employment threshold was reduced once more to five employees from January 1999. Again, this observation does not coincide with the theoretical expectations. Note, however, that a similar pattern could be observed for all other establishment size categories as well. This may indicate that the change in hiring and separation rates between 1997-1998 and 1999-2000 is a result of the business cycle.

Relying on purely descriptive statistics to conclude that there are no employment effects of changes in the thresholds of the PADA may be misleading, because there may be countervailing compositional effects that need to be controlled for. Identification of causal employment effects triggered by the PADA requires a more refined econometric analysis of the data, to which we proceed next.

4.2 Cross-Sectional Analysis

Our first empirical strategy closely follows Borgarello, Garibaldi, and Pacelli (2003) in attempting to identify the employment effects of dismissal protection legislation by utilizing variation along the establishment size dimension alone.[5] Specifically, we study the cross-sectional relationship

$$Y_{it} \quad = \quad X_{it}'\beta + E_{it}'\gamma + Z_{it}'\delta_{CS} + \varepsilon_{it}, \tag{1}$$

where Y_{it} refers to a measure of employment dynamics in establishment i during the sample period t, X_{it} is a vector of establishment characteristics, and ε_{it} is an i.i.d. normal error term with mean zero and variance σ^2. E_{it} is a vector of variables fitting a parametric relationship between the outcome measure and establishment size, whereas Z_{it} is a vector of index variables for initial establishment sizes around the threshold. Note that X_{it}, E_{it}, and Z_{it} are measured at the beginning of sample period t.

The empirical strategy behind the cross-sectional specification (1) is to identify employment effects of exemption threshold legislation via outliers in the outcome variable relative to the estimated functional establishment size relationship. The hypothesis of threshold effects can be tested via the estimated parameter vector δ_{CS}, which should be statistically different from zero around the threshold.

In estimating the cross-sectional relationship (1), we include the employment share of blue collar workers, the employment share of females, the employment share of apprentices, the log average establishment wage, the mean age of the employees in an establishment and its squared, 14 industry dummies, and six regional dummies in the vector of establishment characteristics X_{it}. Calculated employment shares are based on the total number of employees in an establishment. We use different specifications of the index vector Z_{it} to study heterogeneous treatment effects. Employment in the vectors E_{it} and Z_{it} can take non-integer values, as it is measured in terms of full-time equivalent workers according to the PADA.

[5] A similar approach for Germany investigating threshold effects arising from the Disability Protection Act is used by Kölling, Schnabel, and Wagner (2001).

In the following, we focus on a selection of results estimated on the cross-sectional data from March 1995 to March 1996.[6] Columns 1-4 of Table 2 report estimation results for a specification only considering the inverse of employment for identification of threshold effects via outliers. The baseline specification excluding specific threshold effects (column 1) indicates a statistically significant negative relationship between establishment size, i.e. the inverse of inverse employment, and job flow and hiring rates. For separation rates, however, the results indicate a significant positive relationship with the inverse of employment. An explanation for this somewhat unexpected result is that separation rates grow over the bottom of the establishment size distribution (see Table 1). This constitutes the estimated establishment size effect due to the large proportion of very small establishments in the sample.

Judged against this benchmark model, the estimated parameters for an index variable marking the establishment size category exempted from applicability of the PADA (column 2) are consistent with the theoretical expectations. Establishments not treated by the stringent provisions against dismissal show statistically significant higher hiring and separation rates if compared with the population of establishments operating above the exemption threshold value of five employees. The index variable does not show, however, a statistically significant effect on job flow rates, indicating that the PADA reduces worker flows without changing employment stocks.

Column 3 of Table 2 includes separate index variables for establishments with up to four employees and with more than four and up to exactly five employees. This extended specification permits to analyze the effects of the PADA on establishments operating exactly at the threshold. The estimation results again confirm theoretical expectations. Establishments with up to four employees show statistically significant higher hiring and separation rates if compared with establishments operating above the exemption threshold. The two effects seem to cancel out each other in terms

[6] In addition to the estimates reported here, we estimated models for the benchmark functional form on establishment size effects including a specification using level of total establishment employment and its squared, and a specification using the logarithm of total establishment employment and its squared. A complete set of results, including the parallel estimates for the two later sample periods, are available from the authors upon request.

of employment levels. At least the estimated parameter on the index variable for this establishment size category is not statistically significant different from zero in the job flow equation. Establishments operating exactly at the threshold only have statistically significant higher separation rates than establishments not exempted from the PADA. This effect further translates into statistically significant lower job flow rates. These results support the hypothesis that establishments operating exactly at the threshold are reluctant to grow in order to avoid passing the threshold.

Column 4 reports the results obtained from a less parsimonious parameterization of the treatment dummies, which could control for spill-over effects around the exemption threshold value. This specification extends the previous specification shown by including two additional index variables for establishments operating just above the threshold. These firms may have an incentive to reduce the stock of workers in order to get below the threshold, while behaving similar to other firms above the threshold with regard to their hiring decisions.

Establishments with up to four employees again show significantly higher hiring and separation rates compared with the reference group under the provisions of the PADA. The finding that establishments exactly operating at the threshold value of five employees exhibit significantly larger hiring rates than the reference group, however, seems to contradict the prediction that these establishments stop growing in order to avoid the threshold. A possible explanation for the statistically significant positive outcome is replacement hires. This interpretation is backed by the significantly higher separation rates dominating worker flows in this establishment size category. Further supportive evidence comes from the regression on job flow rates, which indicates that establishments operating exactly at the threshold have statistically significant lower employment growth than do establishments in the reference group.

The estimation results for establishments operating just above the threshold are also in line with the theoretical expectations. Whereas their hiring rates are not significantly different from those of establishments in the reference group, their separation rates are significantly higher. This result supports the hypothesis that they try to reduce the stock of workers in order to get below the threshold. However, compared with establishments in the reference group, only establishments with seven employees exhibit significantly lower employment growth rates.

The results for a more flexible parameterization of the benchmark establishment size model including the level of employment in addition to inverse employment, are shown in columns 5-8 of Table 2. Compared with the parallel estimates reported in columns 2-4, the establishment size index coefficients are less precisely estimated, leading to statistically insignificant threshold effects for the specifications reported in columns 6 and 7. The specification permitting different effects both just below and just above the threshold (column 8), however, is again in line with the theoretical expectations.

Yet these results are at best tentative evidence for a causal relationship between dismissal protection legislation and worker flows. The cross-sectional approach relies on rather strong identification assumptions. First, in order to interpret the estimated coefficients δ_{CS} as a causal effect of the treatment on the outcome variable, it has to be assumed that the treated establishments would have behaved exactly like the reference group, if they had not been treated. This assumption might not hold, for example, because of omitted variable bias, unobserved heterogeneity or self-selection into establishment sizes.

Moreover, the statistical significance of outliers around the threshold crucially depends on the specification of the functional relationship between establishment size and employment dynamics, but there is no economic model for justifying a specific parametric form. Considering that dismissal protection legislation may simultaneously impinge upon the establishment size distribution, statistical criteria to select the preferred specification of E_{it} are unreliable as well. The uncertainty about the correct specification of E_{it} renders estimation results using cross-sectional data suspicious. On the one hand, the finding of statistically significant threshold effects might be due to misspecification of E_{it}. On the other hand, by including establishment employment polynomials of sufficiently high order in the vector E_{it}, one could force any threshold effects to become statistically insignificant.

In this regard, our experience with alternative specifications for the parametric relationship between establishment size and worker flows is rather disappointing—they do not generate robust results. Furthermore, while our estimation results for the sample period 1999-2000 are similar to those reported above, no clear-cut pattern of significant threshold effects emerges for the sample period 1997-1998 when the exemption threshold value stood at ten employees.

Overall, it appears that one could generate any level of statistical support for the theoretical hypotheses, depending on the theoretically and empirically ambiguous specification of the benchmark model. A more flexible higher-dimensional polynomial in employment, capturing specific behavior at the bottom of the establishment size distribution unrelated to dismissal protection legislation, might wipe away the remaining significant behavioral outliers. In lack of a satisfying economic model for unconstrained employment dynamics as a function of establishment size, we decide not to move further in this direction. Instead, we proceed to a difference-in-differences model, which relies on weaker identification assumptions.

4.3 Difference-in-differences Results

The difference-in-differences (DiD) estimator controls for unobserved, time-invariant determinants of the outcome variable by taking first differences. Application of this empirical strategy requires the availability of both cross-sectional and time-series variation of the stimulus under study. In our context, we can use the repeated changes in the threshold scale for applicability of the PADA, to gauge the specific impact of dismissal protection legislation on worker flows. More formally, we estimate the effect unique to treated establishments after treatment, δ_{DiD},

$$\begin{aligned} \delta_{DiD} \;=\; & (E[Y_{it}|X_{it}, Z_{it}=1, D_{it}=1] - E[Y_{it}|X_{it}, Z_{it}=1, D_{it}=0]) \;- \\ & (E[Y_{it}|X_{it}, Z_{it}=0, D_{it}=1] - E[Y_{it}|X_{it}, Z_{it}=0, D_{it}=0]), \end{aligned} \tag{2}$$

where Y_{it} is an outcome variable observed in establishment i at time t, and X_{it} a vector of characteristics of establishment i at time t. Z_{it} represents a dichotomous variable taking the value of unity if an establishment belongs to the treatment group and zero otherwise. Likewise, D_{it} is a dichotomous variable taking the value of unity for observations made under the state after implementation of the policy change, and zero for observations made under the pre-reform state. The straightforward intuition for equation (2) is that the policy effect shows up as an excess change of the average outcome in the treatment group, relative to the simultaneous change in a control group. A major advantage of this approach is that it will difference away the unsettled baseline relationship between establishment size and worker flows provided it is constant over time.

Specifically, the DiD parameter δ_{DiD} can be obtained from the following basic regression model:

$$Y_{it} = X'_{it}\beta + Z'_{it}\gamma + D_{it}\rho + D_{it}X'_{it}\psi + D_{it}Z'_{it}\delta_{DiD} + \varepsilon_{it} \ , \qquad (3)$$

which can be estimated by simple OLS, though it is necessary to compute robust standard errors to account for heteroscedasticity of the random error term ε_{it} across periods and groups. In the empirical model (3), the parameter γ captures the effect of characteristics of the treatment group unrelated to the treatment. The parameter ρ shows the effect of period effects unrelated to the policy change. If there remains a group-policy effect, i.e., if δ_{DiD} is significantly different from zero, this can be attributed to the treatment.

The key identification assumption of the DiD approach, therefore, is that the parameter δ_{DiD} equals zero in the absence of the policy change. This implies that the interaction term must be uncorrelated with the random model component. Put differently, as the model only includes common time effects D_{it}, it has to be assumed that conditional on X_{it}, there are no unobserved determinants of the outcome variables changing over time which have a differential effect on the treatment and control groups. This assumption rules out, for example, that changes in the macroeconomic environment display different effects on the two groups.

Figure 3 presents insolvency frequencies by firm size together with GDP growth over the sample period. Clearly, the insolvency pattern of firms with 6-10 employees (approximately the treatment group of the policy changes) resembles that of larger firms (the control group). This observation may support the claim that the employment response to macroeconomic shocks is similar among these groups of firms. In contrast, it does not appear that the economic cycle affects firms with less than six employees in the same way as the control group. If this were true, the estimated parameter δ_{DiD} for this establishment category could capture size-specific variation in outcomes due to macroeconomic shocks, rather than the effects of changes in dismissal protection legislation. This concern seems less relevant when studying the central treatment group of establishments with 6-10 employees.

In implementing the DiD estimator, we combine the repeated cross-sections to create two unbalanced panels. Specifically, we analyze a panel including the observations for periods March 1995-March 1996 and March 1997-March 1998 (panel A), covering the first reform raising the exemption threshold scale to ten employees, and a panel including the observations for periods March 1997-March 1998 and March 1999-March 2000 (panel B), covering the second reform reinstating the original legislation. In all estimations, the vector X_{it} includes the same set of establishment characteristics as for the cross-sectional approach. We allow that the parameters on these observables vary over time.

Table 3 reports the results from estimating a baseline specification of the DiD-model using establishments with 21 to 30 employees as the control group. By specifying a vector of treatment group indicators, we allow for multiple treatment groups. While we expect that the reforms change behavior in establishments with 0-5 and 6-10 employees, they should not affect establishments with 11-20 employees. The latter are only included for a specification check.

The estimates reported in Table 3 indicate that in this establishment size category worker flows following the policy changes were indeed not statistically different from those in the control group. The same applies, however, to the establishments potentially affected by the reform. In fact, none of the estimated coefficients for the treatment groups is statistically significant at any reasonable level. Hence there is no support for the hypothesis that more stringent dismissal protection legislation has a detrimental impact on worker turnover.

To check the robustness of this unexpectedly indistinct result, we investigate alternatives to the baseline specification. The previous specification could blur effects occurring just in the neighborhood of the exemption threshold. We therefore estimate an extended specification using a more detailed classification of establishment size categories. The results, reported in Table 4, do not point at the existence of specific threshold effects, which could support the hypothesis of a negative relationship between the stringency of dismissal protection legislation and worker flows. Again, none of the estimated DiD parameters is statistically different from zero.

Given the rather complicated concept of calculating the establishment size relevant for application of the PADA, another issue to be explored is potential optimization error on the side of employers. However, estimating the same model as that on display in Table 4 but using a simple head count (including apprentices) rather than the number of full-time equivalent workers, does not generate any qualitatively different results.[7]

Our results also might be sensitive toward the choice of the control group. Table 5 reports estimation results for the extended model of multiple treatment groups, using the change in behavior of establishments with 16-20 employees as a benchmark. This alternative specification does not change the qualitative outcome. Unexpectedly, the estimation on panel B indicates a highly significant reduction in hiring and job flow rates for establishments with 11 employees. It is difficult to argue that the January 1999 reform could have changed behavior of these establishments in this direction. This result rather seems to suggest that the model specification does not manage to correct for all shocks with a differential impact on the control group and the establishments in this size category.

Finally, our indistinct results might be due to heterogenous treatment effects. Given the construction of our sample, it is clear that the estimated DiD parameters only measure the short-run effects of the changes in dismissal protection legislation. The strength of these effects, however, could vary across establishments depending on the economic value put on stability of employment relationships. Following Al-

[7] The results of this specification are available from the authors upon request.

varez and Veracierto (1998), one might hypothesize that deregulation of dismissal protection legislation has a stronger short-term influence on establishments facing comparatively more volatile demand. Arguing that demand is on average less volatile in manufacturing than in services where employment relationships are relatively shorter and firms rely on comparatively little specific human capital, one might test this hypothesis by comparing policy responses in these sectors. Table 6 reports the results for the extended treatment group specification (using establishments of size 21-30 as control group) estimated on separate panels for the manufacturing and service sector.

The results do not support the interpretation that our insignificant outcomes are due to heterogeneous treatment effects. In neither sector, worker flows significantly respond to the policy treatment. The only exception are establishments located at the exemption threshold value prior to the October 1996 reform (see panel A). The significant effect appears on separation rates, however, while the theoretical expectation for this treatment group is that deregulation facilitates hirings. Likewise, it is difficult to explain the opposite signs of the DiD parameters. In detail, the point estimates for the manufacturing and service sectors, though generally insignificant, actually differ quite substantially. For example, the consistent pattern of lower separation rates in establishments with 6-10 workers despite deregulation of the PADA (compare panel A in Table 6 and previous tables) seems mainly attributable to the service sector. Following the argument that the provisions for a gradual transition change the volume of quits, this observation is in line with the hypothesis that the economic value of staying under protection against dismissal is higher in the service sector where firings are more frequent. Further investigation into differential sector effects, therefore, might be a worthwhile undertaking, but goes beyond the scope of the present paper.

5 Conclusions

This paper investigated the effects of variable enforcement of dismissal protection legislation on worker flows in small establishments, focusing on the German institutional setting. Empirical estimates based on cross sections drawn from a large employer-employee matched panel data set provide some significant evidence that worker turnover rates are lower in establishments under the provisions for protection against dismissal than establishments exempted from the legislation by application of a threshold scale. This empirical result, however, relies on strong identification assumptions, and is not satisfactorily robust over plausible specifications of the underlying empirical model.

Alternatively, we used a difference-in-differences approach demanding weaker identification assumptions. We exploit time-series variation from two recent labor market reforms changing the minimum establishment firm size for applicability of the Protection against Dismissal Act. The estimation results do not suggest any significant relationship between the stringency of dismissal protection legislation and worker turnover. Several specification checks confirmed this finding as robust. As the sample construction makes us confident that the DiD estimates do not suffer from anticipation effects, the question is what factors explain the discrepancy between the empirical facts and economic theory, which unambiguously predicts a detrimental effect of dismissal protection legislation on hirings and firings?

An obvious explanation is that measurement of outcomes is not sufficiently distant from the implementation of the policy reforms. But even if behavioral adjustment were sluggish, one would expect to observe significant effects considering the large size of our samples. A more plausible explanation is that dismissal taxes effectively are small in spite of the seemingly high hurdles to dismissal imposed by German legislation. First, the impact of voluntary quits can substantially reduce the average dismissal cost per separation. Nickell and Layard (1999) show that if the volume of voluntary quits is sufficiently large, this effect might in theory fully offset the incentive effects of dismissal protection legislation, though in practice this extreme case is unlikely to occur.

A second factor possibly reducing dismissal taxes is exploitation of unconstrained employment buffers. Most importantly, establishments could expand or contract employment along the hours margin. In fact, variation of working hours appears to be strong in Germany compared with countries with less stringent dismissal protection legislation. Moreover, as the legal provisions only apply to workers with regular contracts, employers might respond by adjusting employment at the relatively more flexible contractual margin, i.e. through fixed-term workers or apprentices. A third example of a potential structural effect of dismissal protection which would not be detected by our empirical analysis, is changes in the quality of hirings and separations. Firms might reduce the expected value of dismissal tax payment by establishing better job matches. If this is the case, the amount of hiring, for example, might not change, whereas the composition shifts toward more productive employees earning higher wages.

Clearly, although the paper provides convincing evidence that dismissal protection legislation in the German institutional setting does not have substantial effects on worker flows and employment levels in small establishments, the results are not sufficient to claim that constraints on dismissals do not matter at all. On the one hand, we do not know how inflexibility with regard to firings affects small establishments that go out of business, or, more fundamentally, are not established because of expected firings costs. On the other hand, we do not know how larger establishments behaved in the presence of less stringent dismissal protection legislation. Therefore, qualified policy recommendations require further research with disaggregated firm level data.

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Figure 1: Sample Construction

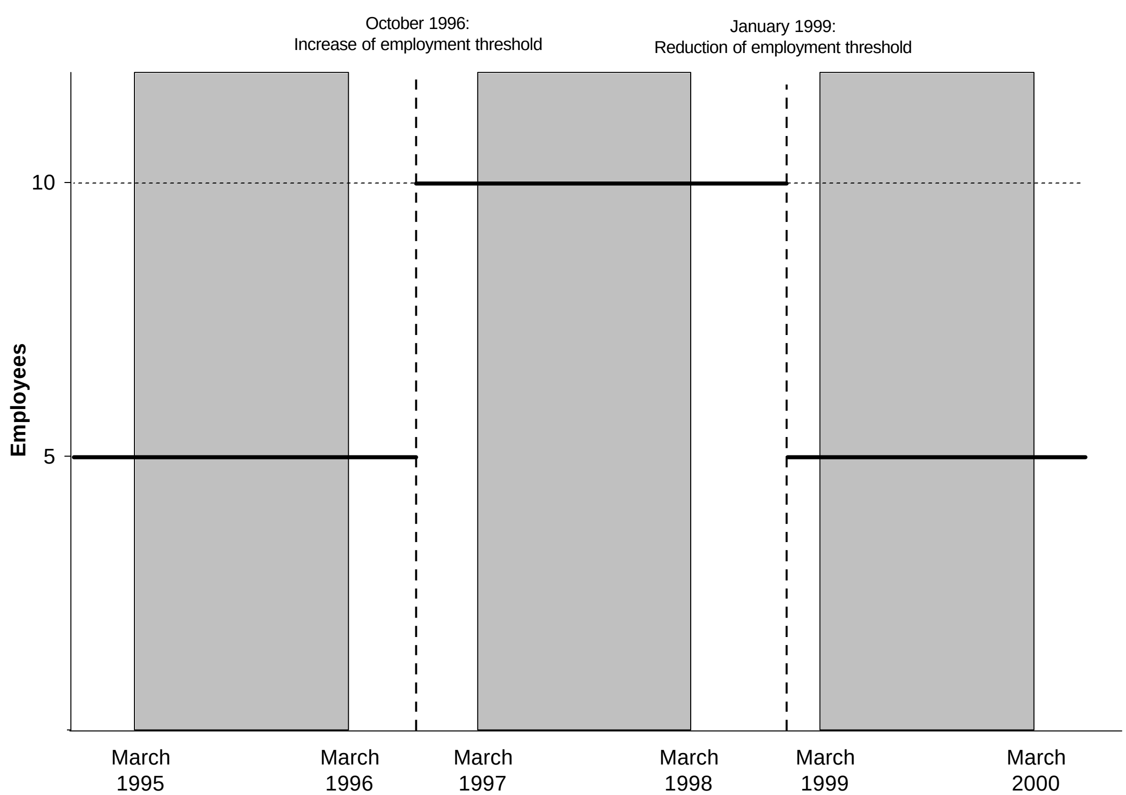
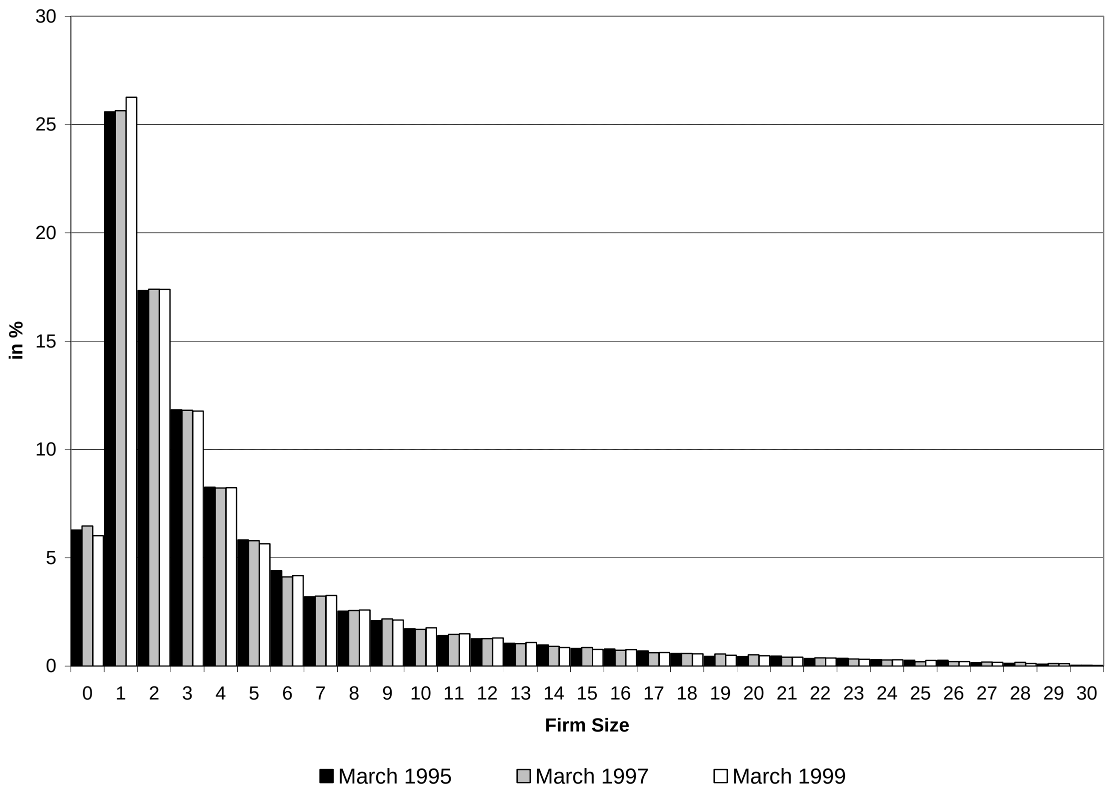
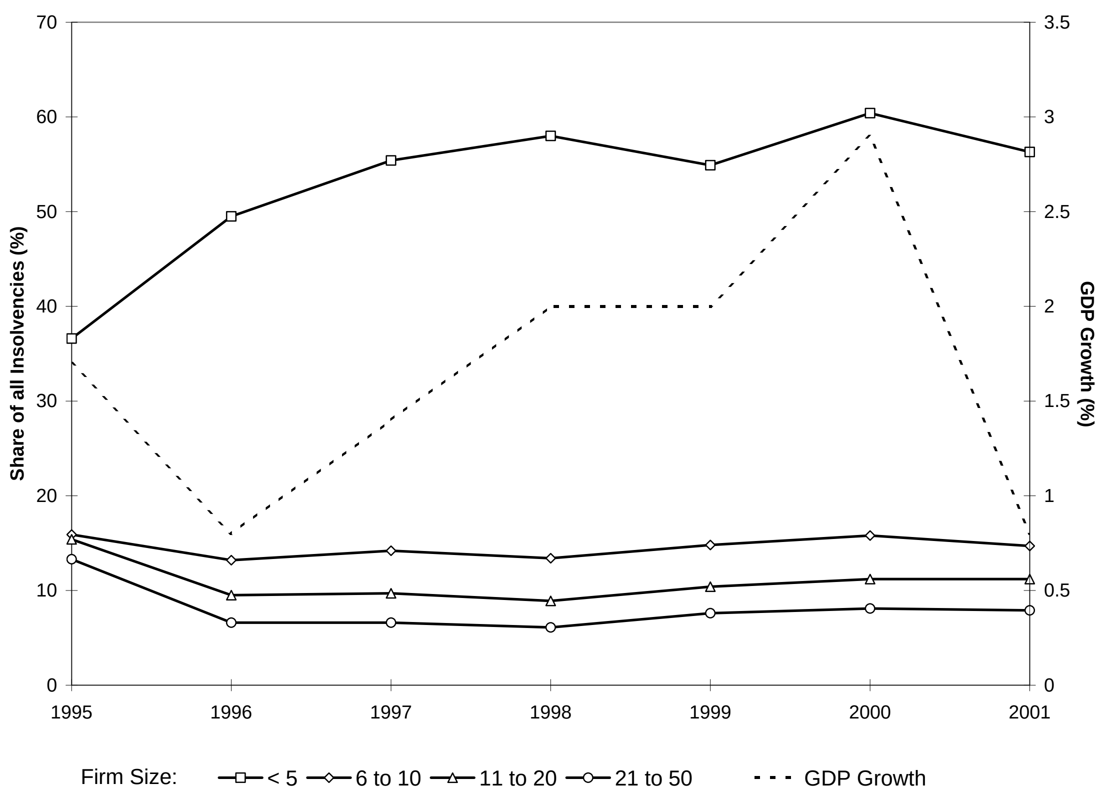


Figure 2: Frequency of Establishment Sizes



Note: Establishment sizes calculated according to provisions of German Protection against Dismissal Act.

Figure 3: GDP Growth and Bankruptcies by Firm Size



Source: Statistics Germany, Creditreform.

Table 1: Worker Flow Rates by Establishment Size

		1995-1996	*1997-1998*	*1999-2000*	*1995-1996*	*1997-1998*	*1999-2000*
			Firm Size 0-5			*Firm Size 6-10*	
HR	Mean	46.177	49.976	55.273	37.611	36.764	42.196
	S.D.	(1.016)	(1.425)	(0.815)	(1.835)	(0.587)	(0.623)
	Before-After		-3.799†	-5.297†		-0.847	-5.432†
SR	Mean	34.623	35.496	39.520	39.182	37.231	41.606
	S.D.	(0.427)	(0.435)	(0.432)	(0.478)	(0.427)	(0.461)
	Before-After		-0.872	-4.025†		1.951†	-4.375†
JFR	Mean	11.553	14.480	15.752	-1.570	-0.467	0.590
	S.D.	(0.867)	(1.174)	(0.578)	(1.777)	(0.451)	(0..067)
	Before-After		-2.926†	-1.272		-1.104	-1.057
			Firm Size 11-20			*Firm Size > 20*	
HR	Mean	32.926	35.039	40.704	33.558	32.365	38.111
	S.D.	(0.568)	(0.687)	(0.912)	(0.987)	(0.980)	(1.171)
	Before-After		-2.112†	-5.665†		1.194	-5.746†
SR	Mean	35.828	35.649	40.168	35.201	33.946	37.527
	S.D.	(0.490)	(0.540)	(0.598)	(0.898)	(0.868)	(0.859)
	Before-After		0.178	-4.520†		1.255	-3.581†
JFR	Mean	-2.902	-0.610	0.535	-1.642	-1.581	0.584
	S.D.	(0.416)	(0.440)	(0.562)	(0.721)	(0.685)	(0.842)
	Before-After		-2.291†	-1.145		-0.061	-2.165†

Notes: † indicates significance at the 95% confidence level. JFR: Job Flow Rate; HR: Hiring Rate; SR: Separation Rate. Before-After: comparison of means in sampling period with means in preceding sampling period.

Table 2: Cross-Section Results, 1995-1996

	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*	*(7)*	*(8)*
				Dependent Variable: Hiring Rates				
0-4			0.062†	0.081†			0.026	0.049†
			(0.015)	(0.013)			(0.023)	(0.014)
0-5		0.051†				0.013		
		(0.013)				(0.022)		
5			0.008	0.025†			-0.020	-0.001
			(0.013)	(0.010)			(0.020)	(0.014)
6				0.077				0.054
				(0.052)				(0.053)
7				0.018				-0.003
				(0.011)				(0.013)
Employment^{-1}	0.284†	0.257†	0.245†	0.244†	0.244†	0.243†	0.233†	0.236†
	(0.032)	(0.032)	(0.032)	(0.031)	(0.030)	(0.030)	(0.031)	(0.031)
Employment					-0.006†	-0.005†	-0.005†	-0.003†
					(0.001)	(0.002)	(0.002)	(0.001)
				Dependent Variable: Separation Rates				
0-4			0.053†	0.066†			0.031†	0.050†
			(0.007)	(0.008)			(0.008)	(0.010)
0-5		0.048†				0.026†		
		(0.006)				(0.007)		
5			0.031†	0.044†			0.014	0.031†
			(0.008)	(0.008)			(0.009)	(0.010)
6				0.039†				0.027†
				(0.008)				(0.010)
7				0.040†				0.029†
				(0.010)				(0.011)
Employment^{-1}	-0.106†	-0.131†	-0.136†	-0.137†	-0.137†	-0.140†	-0.144†	-0.141†
	(0.012)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)
Employment					-0.005†	-0.003†	-0.003†	-0.002†
					(0.001)	(0.001)	(0.001)	(0.001)
				Dependent Variable: Job Flow Rates				
0-4			0.010	0.014			-0.004	-0.001
			(0.012)	(0.009)			(0.022)	(0.010)
0-5		0.003				-0.013		
		(0.011)				(0.021)		
5			-0.023†	-0.019†			-0.034	-0.032†
			(0.011)	(0.007)			(0.019)	(0.009)
6				0.039				0.027
				(0.051)				(0.052)
7				-0.021†				-0.032†
				(0.008)				(0.009)
Employment^{-1}	0.390†	0.389†	0.381†	0.381†	0.381†	0.383†	0.377†	0.377†
	(0.028)	(0.027)	(0.027)	(0.027)	(0.026)	(0.026)	(0.026)	(0.026)
Employment					-0.001	-0.002	-0.002	-0.002
					(0.001)	(0.002)	(0.002)	(0.001)

Notes: Results from OLS. Robust standard errors in parentheses. 53,041 observations. † indicates significance at the 95% confidence level. All regressions include the following control variables: share of blue collar workers, share of females, share of apprentices, log average wage, mean age of employees, mean age of employees squared, 14 industry dummies, and six regional dummies.

Table 3: Difference-in-Differences Results: Basic Specification

Establish-ment Size	*Panel A: 1995 - 1997* *HR*	*SR*	*JFR*	*Panel B: 1997 - 1999* *HR*	*SR*	*JFR*
0-5	-1.822	-0.826	-0.996	2.107	-0.302	2.409
	(3.873)	(1.504)	(3.297)	(3.651)	(1.436)	(3.054)
6-10	-2.409	-1.623	-0.786	0.907	0.460	0.447
	(2.642)	(1.346)	(2.301)	(2.096)	(1.335)	(1.618)
11-20	1.622	0.545	1.076	1.038	1.207	-0.170
	(1.714)	(1.360)	(1.228)	(1.911)	(1.392)	(1.349)

Notes: Results from OLS. Robust standard errors in parentheses. Control Group: Establishment Size 21-30. Observations 1995-1997: 107,396. Observations 1997-1999: 109,255. † indicates significance at the 95% confidence level. All regressions include the following control variables and interactions of these variables with an index variable for year 1997 respectively 1999: share of blue collar workers, share of females, share of apprentices, log average wage, mean age of employees, mean age of employees squared, 14 industry dummies, and six regional dummies. JFR: Job Flow Rate; HR: Hiring Rate; SR: Separation Rate.

Table 4: Difference-in-Differences Results: Extended Specification

Establish-ment Size	*Panel A: 1995 - 1997* *HR*	*SR*	*JFR*	*Panel B: 1997 - 1999* *HR*	*SR*	*JFR*
0-4	-2.582	-1.090	-1.492	2.749	-0.109	2.858
	(4.104)	(1.541)	(3.523)	(3.839)	(1.461)	(3.239)
5	2.861	0.807	2.053	-2.581	-1.416	-1.165
	(4.358)	(1.976)	(3.094)	(4.413)	(1.978)	(3.126)
6	-7.104	-1.886	-5.218	2.785	1.095	1.691
	(5.715)	(1.594)	(5.489)	(2.702)	(1.662)	(2.100)
7-9	-0.626	-1.420	0.793	-0.295	-0.394	0.099
	(2.118)	(1.452)	(1.598)	(2.122)	(1.411)	(1.614)
10	1.130	-2.185	3.315	2.163	3.186	-1.023
	(3.122)	(2.007)	(2.560)	(3.334)	(2.049)	(2.610)
11	2.467	1.217	1.250	-3.550	-0.029	-3.521
	(2.839)	(2.214)	(1.938)	(2.716)	(2.100)	(1.967)
12-15	1.339	0.272	1.066	1.335	1.784	-0.449
	(1.862)	(1.493)	(1.346)	(2.066)	(1.556)	(1.455)
16-20	1.617	0.558	1.060	2.933	0.983	1.949
	(2.169)	(1.679)	(1.491)	(2.815)	(1.848)	(1.808)

Notes: See notes to Table 3.

Table 5: Difference-in-Differences Results: Alternative Control Group

Establish-ment Size	*Panel A: 1995 - 1997*			*Panel B: 1997 - 1999*		
	HR	*SR*	*JFR*	*HR*	*SR*	*JFR*
0-4	-4.252	-1.670	-2.582	-0.144	-1.071	0.927
	(3.889)	(1.571)	(3.237)	(3.988)	(1.685)	(3.096)
5	1.218	0.237	0.982	-5.506	-2.388	-3.119
	(4.311)	(1.997)	(2.974)	(4.657)	(2.144)	(3.124)
6	-8.754	-2.464	-6.290	-0.148	0.112	-0.260
	(5.655)	(1.621)	(5.392)	(3.112)	(1.858)	(2.127)
7-9	-2.250	-1.982	-0.268	-3.246	-1.383	-1.863
	(2.136)	(1.484)	(1.484)	(2.687)	(1.643)	(1.718)
10	-0.507	-2.762	2.255	-0.793	2.211	-3.004
	(3.150)	(2.033)	(2.507)	(3.735)	(2.218)	(2.693)
11	0.859	0.669	0.190	-6.496†	-1.028	-5.468†
	(2.907)	(2.237)	(1.914)	(3.223)	(2.269)	(2.096)
12-15	-0.295	-0.303	0.008	-1.620	0.799	-2.419
	(1.995)	(1.528)	(1.348)	(2.736)	(1.777)	(1.685)

Notes: Results from OLS. Robust standard errors in parentheses. Control Group: Establishment Size 16-20. Observations 1995-1997: 104,616 . Observations 1997-1999: 106,481. See also notes to Table 3.

Table 6: Difference-in-Differences Results by Industry

Establishment Size	*HR* Manuf.	*HR* Serv.	*SR* Manuf.	*SR* Serv.	*JFR* Manuf.	*JFR* Serv.
			Panel A: 1995 - 1997			
0-4	-8.787	1.134	0.250	-2.064	-9.037	3.198
	(6.178)	(5.110)	(2.638)	(1.915)	(5.559)	(4.250)
5	1.160	4.313	5.941†	-2.195	-4.781	6.508
	(3.668)	(6.343)	(2.826)	(2.631)	(2.672)	(4.459)
6	-4.804	-7.547	-0.812	-2.837	-3.993	-4.710
	(3.508)	(8.562)	(2.811)	(1.930)	(2.578)	(8.295)
7-9	-3.439	0.894	-0.658	-2.224	-2.780	3.119
	(3.469)	(2.715)	(2.468)	(1.774)	(2.668)	(2.038)
10	3.209	-0.339	0.523	-4.042	2.686	3.703
	(4.704)	(4.161)	(3.107)	(2.595)	(3.920)	(3.409)
11	-2.686	4.151	0.086	1.031	-2.773	3.121
	(4.468)	(3.735)	(3.737)	(2.637)	(2.979)	(2.643)
12-15	-0.406	1.023	0.923	-0.706	-1.330	1.729
	(3.011)	(2.484)	(2.320)	(1.928)	(2.418)	(1.719)
16-20	-0.369	1.935	1.454	-0.688	-1.823	2.623
	(2.997)	(3.075)	(2.394)	(2.293)	(2.380)	(1.954)
Obs.	31,215	76,181	31,215	76,181	31,215	76,181
			Panel B: 1997 - 1999			
0-4	5.525	1.033	2.004	-0.909	3.522	1.942
	(4.404)	(5.201)	(2.312)	(1.902)	(3.608)	(4.380)
5	1.732	-5.018	-2.841	-0.831	4.572	-4.188
	(3.860)	(6.446)	(2.637)	(2.700)	(2.894)	(4.539)
6	7.108	0.396	3.793	-0.282	3.315	0.678
	(3.839)	(3.659)	(2.729)	(2.116)	(2.790)	(2.894)
7-9	2.966	-2.454	0.722	-1.099	2.244	-1.355
	(3.052)	(2.871)	(2.199)	(1.843)	(2.341)	(2.164)
10	-0.086	2.457	2.336	2.881	-2.422	-0.424
	(4.693)	(4.515)	(2.966)	(2.724)	(3.778)	(3.514)
11	0.214	-5.302	1.548	-0.551	-1.335	-4.750
	(3.918)	(3.693)	(3.168)	(2.772)	(2.746)	(2.716)
12-15	2.284	0.839	2.114	1.550	0.170	-0.711
	(2.738)	(2.911)	(2.175)	(2.140)	(2.039)	(1.999)
16-20	9.931†	-1.585	4.698	-1.365	5.233	-0.220
	(5.059)	(3.269)	(2.986)	(2.339)	(3.135)	(2.186)
Obs.	31088	78167	31088	78167	31088	78167

Notes: See notes to Table 3.