Dismissals for cause: The difference that just eight paragraphs can make*

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Abstract

This paper provides evidence about the effects of dismissals-for-cause requirements, a specific component of employment protection legislation that has received little attention despite its potential relevance. We study a quasi-natural experiment generated by a law introduced in Portugal in 1989: out of the 12 paragraphs in the law that dictated the costly procedure required for dismissals for cause, eight did not apply to firms employing 20 or fewer workers. Using detailed matched employer-employee longitudinal data and difference-in-differences matching methods, we examine the impact of that differentiated change in firing costs upon several variables, measured from 1991 to 1999. Unlike predicted by theory, we do not find robust evidence of effects on worker flows. However, firm performance improves considerably while wages fall. Overall, the results suggest that firing costs of the type studied here decrease workers' effort and increase their bargaining power.

Keywords: Employment protection legislation, worker flows, wages, firm performance.

JEL Codes: E24, J64, J65.

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1 Introduction

Employment protection legislation (EPL) tends to be studied from the point of view of the constraints imposed upon firms that want to adjust their workforces as a response to economic shocks (Lazear 1990, Bertola 1990, Hopenhayn & Rogerson 1993, Bertola & Rogerson 1997). This approach is justified as many dimensions of EPL, such as the rules regarding the termination of permanent contracts, collective dismissals, or the regulation of temporary contracts, may create important barriers against job and worker reallocation prompted by economic fluctuations. Such barriers can therefore affect the efficiency of labour markets and have important welfare consequences.

However, firms may also need to adjust their workforces for other reasons than economic shocks. A major alternative reason - but also a much less studied one - concerns dismissals for cause, which are driven instead by worker performance or disciplinary issues. EPL can also play an important role here, given the moral hazard problems that arise when workers are protected against firings. Such problems can be considerable, particularly if severance payments are large enough (Blanchard & Tirole 2003) and/or firms face liquidity constraints.

While some countries' EPL is based upon 'employment at will',[1] in which the concept of cause in a dismissal is irrelevant, arbitrary firings tend to be forbidden or subject to heavy penalties in most economies. In the latter case, firms may need to go through potentially very costly administrative procedures when they dismiss for cause. These procedures include notice periods, notifications and/or approval of third parties (including courts and employment ministeries), law-mandated retraining or replacement prior to dismissal, culminating in some cases in compulsory worker reinstatements.[2]

The legal procedures required in firings for cause were probably originally designed to protect employees by dissuading employers from disguising layoffs as dismissals for cause - in which case severance benefits would typically not have to be paid to the dismissed worker. Perhaps legislators also regard such legal procedures as a way to erode inequality, in the

[1]The US is one of the most notable examples of 'employment at will', although with some important differences across states and over time. Autor et al. (2006) document these differences and use them to evaluate the impact of restrictions on employment at will.

[2]For instance, in Germany, employers must notify the employees they want to dismiss and their work councils in writing, after oral or written warnings to employee. If the work councils disagree with the employers' intentions, dismissal has to wait for a decision by the employment court, which can take several months. In South Korea, employers need to send advance notice to the unions 60 days prior to dismissal and consult with them over efforts to avoid dismissal. See OECD (2004) for several other examples of costly and/or time-consuming dismissal procedures. The evidence in Botero et al. (2004) also indicates that these restrictive practices surrounding dismissals for cause are particularly common in developing and transition countries.

sense that those firing constraints may strengthen the bargaining power of incumbent workers *vis-à-vis* their employers, while the latter tend to be regarded as the stronger part in the bargaining process. In any case, the legal procedures surrounding dismissals for cause can induce deadweight losses, as the costs borne by firms when carrying out such dismissals are not recouped by the workers affected. Those deadweight losses can then result in inefficiently low numbers of separations and, indirectly, also in inefficiently low numbers of hirings.

Moreover, while one can argue that employers may be able to bargain with workers over a compensation payment large enough for the latter to accept to quit, adverse selection problems may make such approach particularly costly for firms (if workers to be fired have worse outside options, they are likely to demand higher compensation payments). Furthermore, if firms circumvent the constraints imposed by EPL by making compensation payments to workers that underperform or that have disciplinary problems, that may undermine worker morale (Bewley 1999) and reduce the levels of effort of the remaining workforce.[3]

In this paper, we seek to assess the causal impact of the administrative procedures that restrict firings for cause. As far as we know, we are the first to examine empirically the role of this specific component of EPL. Our analysis is based on a quasi-natural experiment that occurred in Portugal, a country well known for its very strict EPL (Blanchard & Portugal 2001, OECD 2004, Botero et al. 2004). Specifically, the experiment results from a new law governing employee dismissals introduced in 1989, under which firing constraints were reduced for all firms. However, firms employing 20 or fewer employees (unlike larger firms) were exempted from a number of administrative restrictions regarding dismissals for cause. Moreover, since until then there was no differentiation in firing constraints across firms in terms of their size or other characteristics, one can set up a difference-in-differences analysis, by contrasting the outcomes of 'smaller' and 'larger' firms, before and after 1989.

As we can draw on particularly detailed panel data, we also conduct a difference-in-differences propensity score matching analysis, in order to minimise any bias driven by unobserved differences and by differences in the common support across the two firm types and their workers (Heckman et al. 1998). This is the first study in the EPL literature that adopts this specific empirical approach.

Finally, we provide an exhaustive analysis of the impacts of the new law, by considering

[3]One can argue that the stringency and detail of the legal procedures surrounding dismissals for cause also determine, albeit indirectly, the *minimum* level of effort that employees need to exert to keep their jobs.

many variables that typically have only been studied separately before. We examine not only employment and job and worker flows (Oyer & Schaefer (2000), Acemoglu & Angrist (2001), Blanchard & Portugal (2001), Autor (2003), Kugler & Saint-Paul (2004), Boeri & Jimeno (2005), Autor et al. (2006), Varejao & Portugal (2007), Kugler & Pica (forthcoming), Bauer et al. (forthcoming), Marinescu (2007)), but also productivity (Besley & Burgess 2004, Autor et al. 2007) and wages (Leonardi & Pica 2007).

The structure of the paper is as follows: First, Section 2 presents some of the main features of the legislative reform exploited in our analyses. We then introduce our empirical methodology in Section 3, after which Section 4 describes the data. Sections 5 present the results and Section 6 describes the robustness analyses. Finally, Section 7 concludes.

2 The 1989 employment law reform

After the 1974 *coup d'etat* that overthrew a 48-year-old conservative dictatorship, Portuguese politics became dominated by socialist ideas. Many firms were nationalised, especially those firms in sectors considered 'strategic' (utilities, banking, insurance, transports, media, etc); the control of the largest farms were transferred from owners to employees; price controls were introduced in many markets; and several new laws that regulated economic activity came into force. Amongst the several markets that became subject to tighter government intervention, the labour market was particularly hardly hit.

The foremost example of the labour market restrictions imposed at the time was the law that regulated dismissals, *Decreto-Lei 372-A/75*, introduced in 1975. As in the employment laws of other countries, the 1975 Portuguese law also indicated that permanent labour contracts could be terminated only when the worker was of retirement age or if the worker was fired for cause. However, unlike many other countries, cause was defined in a particularly restrictive way. Specifically, cause existed only when it was "absolutely and definitively impossible, in the present and in the future, for the worker to perform his/her job or for the firm to take the worker's labour" (article 8). Moreover, for a firm to fire a worker for cause, it would also need to conduct a particularly lengthy administrative procedure, which included, amongst several other procedures, writing a detailed document to be sent to the worker and to the worker's union outlining why the firm wanted to fire the worker. A firm should also collect evidence from a potentially very large number of witnesses indicated by the employee.

It is important to underline that if any formal aspect of this time-consuming administrative procedure were not pursued and if the dismissed worker subsequently challenged the legality of his/her dismissal, then the court would most likely declare the dismissal as illegal. In that case, the court would also order the firm to reinstate the worker and to pay him/her all wages corresponding to the period since the worker was unlawfully dismissed until the worker was reinstated. Even if the dismissal was deemed legal or if the legality of the dismissal was not challenged by the employee, the firm was still always obliged to pay severance benefits. These benefits were considerable, as they corresponded to one month of pay per year of tenure, with a minimum of three months of pay.

After about ten years of relative economic stagnation that followed the 1974 *coup d'etat*, Portuguese economic policy eventually became more market friendly. Under the governments of the mid- and late-1980s, several reforms envisaging more flexible product and factor markets were introduced. Moreover, in 1986, the country became a member of the European Community, after which capital inflows increased substantially. Under this positive economic context, a new employment law sought to revert or, at least, attenuate the very restrictive conditions governing the termination of permanent contracts described above. After a period of heated political debate, public demonstrations (including a general strike), and detailed scrutiny by the constitutional court - all events which generated considerable uncertainty about whether the intended reforms would indeed come through - a new law, *Decreto-Lei 64-A/89*, finally came into force at the end of May of 1989.[4]

This new law softened considerably the dismissal constraints faced by firms, namely by widening the range of circumstances in which a firm could fire a worker employed under a permanent contract. Unlike under the old law, it became possible for firms to fire a worker because of structural, technological or business-cycle reasons. However, while the benchmark administrative procedure required for dismissals for cause remained lengthy and complex, the new law allowed small firms (defined as those firms employing 20 or fewer workers) to follow a much simpler procedure. In particular, out of the 12 specific rules that larger firms needed to follow (each rule outlined in a separate paragraph of article 10 of the 1989 law), only four of those rules needed to be considered by smaller firms (article 15).[5] This differentiation

[4] Cavaco Silva (1995) provides an analysis of this and other reforms introduced in Portugal from the mid-1980s to the mid-1990s.

[5] The only exception to this streamlined procedure for smaller firms was when the worker being dismissed was a union leader. In this case, the benchmark, 12-paragraph-long procedure applied.

established an important contrast between the new and old laws, as firm size was irrelevant in the 1975 document.

The aggregate impact of the differentiation in the law in terms of the size of the firms was potentially very large, as a considerable number of persons worked in firms employing 20 or less workers. Our data, described in detail below, which covers the entire population of firms and their employees, indicates that, in 1989, there were a total of 136,558 firms in Portugal, of which 120,433 firms (88.2%) employed 20 or less workers. In terms of the total number of employees in all firms, 2,169,830, a still considerable number of workers, 620,373 (28.6%), were employed in the smaller firms.

Moreover, the eight paragraphs that did not apply to smaller firms were also particularly important in terms of their content. First, unlike larger firms, smaller firms did not have to discuss (and to be able to prove that they had discussed) the motives for the dismissal with the worker that they wanted to fire. Second, smaller firms were not required to inquire any witnesses indicated by the worker. Third, unions did not have to be involved in the dismissal process. Finally, again unlike firms with more than 20 employees, smaller firms were not required to write a document detailing the entire dismissal process. Larger firms would have to present this document in court if the employee challenged the legality of his/her dismissal, lest the firing was declared invalid.

Although most of these differences between smaller and larger firms have a strong *formal* dimension, one should underline that, according to both the pre- and post-1989 laws, courts were forced to declare a dismissal as null even if only one of these formal steps had not been undertaken.[6] Moreover, again according to both the pre- and post-1989 laws, void dismissals implied the reinstatement of the worker in the firm and the payment of all foregone wages since the time when the worker was unlawfully dismissed until the final court decision. Furthermore, it was not uncommon that employment courts took one year or longer to reach their verdict, a fact that further compounded the liabilities faced by firms.[7]

Finally, it is important to refer that other adjustments in employment law were also introduced in 1989 or soon after. The two most relevant additional reforms involved the tightening of temporary contracts (which were restricted to a narrower range of employment

[6]The emphasis upon formality is also common in the civil law legal systems of many other European and developing countries. Voiding the dismissal if at least one formal requirement was not followed by the firm may have been meant to 'protect' the workers.

[7]Galdon-Sanchez & Guell (2004) study the court outcomes of dismissal conflicts using data from four European countries and the US.

relationships than before 1989) and the easening of collective dismissals. Other new legal diplomas in employment law covered child work, health and safety practices and strikes. Unlike with dismissals for cause, the tighter temporary contracts did not change in a different way for firms of different sizes. However, it is not impossible that smaller firms had a different percentage of their workforces made up of temporary contracts and that may confound the assessment of the main reform. On the other hand, collective dismissals were easened in a slightly different way for firms of different sizes. Firms employing less than 50 employees were from 1989 allowed to conduct a collective dismissal involving only two employees, while for firms employing 50 or more employees a collective dismissal required that at least five workers were laid-off.

In order to minimise any bias in our results related to the changes in temporary contracts or collective dismissals, we focus our analysis of the impact of the costs of dismissal only on firms employing a number of workers 'sufficiently' close to the firm size threshold of interest (20). Moreover, we only consider firms which are 'relatively' far from the threshold that applied for collective dismissals (50). Specifically, in our benchmark results, we consider only firms employing between 10 and 30 workers up to May 1989 (when the new dismissals law came into force). This relatively narrow range of firm sizes also implies that any biases related to differences in the share of temporary contracts across the two types of firms are likely to be small. Moreover, our use of matching techniques - described below - also helps our identification in this regard, by restricting our comparison to firms that are effectively comparable along a large set of observable variables. Finally, the restriction upon the range of firm sizes we consider is also important in itself, even if there were no asymmetric changes in collective dismissals. In fact, the assumption of common trends for the treatment and control groups is less likely to hold for a wider range of firm sizes. This and other methodological issues are described in a more formal way in the next section.

3 Identification

Let Y_{it}^{D} be the potential outcome of interest for individual i (a firm, in our context) at time t had they been in state D, where $D = 1$ if exposed to the treatment (a firm employing less than 20 workers) and 0 otherwise. Let treatment take place at time t (from May 1989, in our case). The fundamental identification problem lies in the fact that we do not observe, at time

t, individual i in both states. Therefore, we cannot compute the individual treatment effect, $Y_{it}^1 - Y_{it}^0$. One can, however, if provided with a convenient control group, estimate the average effect of the treatment on the treated.

One approach is a difference-in-differences (D-in-D) estimator (see Meyer (1995)), in which one uses an untreated comparison group to identify temporal variation in the outcome that is not due to the treatment. However, in order to achieve identification of the general D-in-D estimator we need to assume that the average outcomes for treated and controls would have followed parallel paths over time if there had been no treatment. This is known as the time-invariance assumption,

$$E[Y_{it}^0 - Y_{it'}^0 \mid D = 1] = E[Y_{it}^0 - Y_{it'}^0 \mid D = 0], \tag{1}$$

where t' is a time period before the program implementation. The assumption states that, over time, the outcome variable of treated individuals ($D = 1$), in the event that they had not been exposed to the treatment, would have evolved in the same fashion as actually observed for the individuals not exposed to the treatment ($D = 0$).

If assumption (1) holds, the D-in-D estimate of the average treatment effect on the treated can be obtained by the sample analogs of

$$\widehat{\alpha}_{\text{D-in-D}} = \{E[Y_{it} \mid D = 1] - E[Y_{it} \mid D = 0]\} - \{E[Y_{it'} \mid D = 1] - E[Y_{it'} \mid D = 0]\}. \tag{2}$$

The expression above simply states that the impact of the program is given by the difference between participants and nonparticipants in the before-after difference in outcomes.

A potential problem with this approach is that the time-invariance assumption can be too stringent if the treated and control groups are not balanced in covariates that are believed to be associated with the outcome variable (see Ashenfelter (1978)). In this case, the D-in-D setup can be extended to accommodate a set of covariates, something which is usually done linearly, taking into account eligibility specific effects and time or aggregate effects. In the following model, based on a sample of treatment and control units:

$$Y_{it} = \lambda D + \tau_t + \theta' Z_{it} + \alpha_D D \tau_t + \varepsilon_{it}, \tag{3}$$

where D is as before and represents the eligibility-specific intercept, τ_t captures time or aggregate effects, and Z is a vector of covariates included to correct for differences in observed characteristics between individuals in treatment and control groups, $\widehat{\alpha}_D$ would correspond to the D-in-D estimate. This estimator controls for both differences in the Zs and for time-specific effects, but it does not impose common support on the distribution of the $Z's$ across the cells defined by the D-in-D approach.

In order to address this possible shortcoming of the standard D-in-D method, we complement it with a matching framework (Rosenbaum & Rubin 1983), resulting in a difference-in-differences matching (DDM) estimator (Heckman et al. 1997, 1998). DDM has been recently reviewed and compared with other methods by Smith & Todd (2005) and has been shown to have the potential benefit of eliminating some sources of bias present in non-experimental settings, improving the quality of evaluation results significantly. Moreover, DDM is particularly appropriate for our analysis as we can draw upon a rich set of covariates, all data are compiled by the same agency and we can also use data for comparison groups from the same local labor market (Heckman et al. 1997). In general, the feasibility of the matching strategy relies on a rich set of observable individual characteristics, X, so that the distribution of the individual characteristics important to the evaluation exercise is the same in the difference-in-differences cells.

The matching process then models the probability of participation and matches individuals with similar propensity scores. Moreover, the time invariance assumption for the DDM estimator is now

$$E[Y^0_{it} - Y^0_{it'} \mid p, D = 1] = E[Y^0_{it} - Y^0_{it'} \mid p, D = 0], \tag{4}$$

where $p = \Pr(D = 1|X)$ is the propensity score. When estimating the mean impact of the treatment on the treated, the matching estimator requires a conditional mean independence assumption,

$$E\left[Y^0_{it}|X, D = 1\right] = E\left[Y^0_{it}|X, D = 0\right] = E\left[Y^0_{it}|X\right], \tag{5}$$

and also requires that there is a nonparticipant analogue for each participant, implying that $\Pr(D = 1|X) < 1$.

The DDM estimator takes two forms, depending on the nature of the data, namely repeated cross-sections and panel data. In the latter case, the one employed in this paper, the estimator involves first calculating the differences over time in the dependent variable for each observation and then matching treatment and control units using propensity score estimates based on 'before'-period characteristics. Formally,

$$\widehat{\alpha}_{DDM} = E\left[(Y_t^1 - Y_{t'}^1) - \widehat{E}\left(Y_t^0 - Y_{t'}^0 | P\right)\right], \tag{6}$$

where $\widehat{E}\,(Y|P)$ represents the expected outcome of individuals in the control group matched with those in the treatment group.

4 Data

The data used in this paper are derived from 'Quadros de Pessoal' (QP), a particularly rich annual census of all firms that operate in Portugal and that employ at least one worker. Under the regulations of this census, which is administered by the Ministry of Employment, each firm is legally required to provide extensive information about itself and also about each one of its workers that are employed at the census reference month (the reference month is March up to 1993 and October from 1994 onwards). Given the extensive coverage of the data, the only groups of workers not present in the data are the self-employed and the public sector employees, besides the unemployed. Moreover, the period covered by the data is also relatively long, as the census has been ongoing since 1982.[8]

The long list of variables available in the data includes unique identifiers for each firm, for each establishment and for each employee. These identifiers allow us to follow workers over time, even if they move between firms. Other firm-level variables are the economic sector/industry (measured at the five-digit level), region (up to 400 different units), number of employees (constructed from the worker-level data), firm age, type of ownership (public, private/domestic or foreign owned), sales, and equity. At the worker-level, the data make available information about schooling, age (month and year when the worker was born), gender, tenure (month and year when the worker was hired by the firm), occupation (five-digit code), wages, hours worked, job level (a two-digit variable, comparable across firms and over time) and promotions (month and year when the worker was last promoted in the firm). Experience is constructed as $age - education - 6$.

[8]There have been two discontinuities over this period: there is only employer-level data for the year of 1990; the census data (both at the employer- and the employee-level) are not available for 2001. Overall, on average, between 1982 and 2004, there are approximately 2.5 million workers and more than 200,000 firms per year.

There are several wage variables, all of them expressed in monthly values (the most common frequency of pay in Portugal), including base wages, tenure-related payments, overtime pay, 'subsidies' and 'other payments' (including bonuses and profit- or performance-related pay). All wages have been deflated using Portugal's CPI and are expressed in 2004 euros. There is also information about normal hours and overtime hours per month. The benchmark measure of pay adopted in this study is based on the sum of all five types of pay divided by the sum of the two types of hours worked, resulting in a measure of total real hourly pay.

Based on the firm- and the worker-level data, we construct job and worker flow variables following Davis et al. (1996*a*). Each flow rate is obtained by dividing a given flow by the average employment of the firm over the two periods analysed. Specifically, the job creation rate is defined as $JC_t = \frac{L_t - L_{t-1}}{0.5(L_t + L_{t-1})}$, if $L_t >= L_{t-1}$, or $JC_t = 0$, if $L_t < L_{t-1}$, in which L_t denotes the number of workers in period t. Similarly, the job destruction rate is defined as $JD_t = \frac{L_{t-1} - L_t}{0.5(L_t + L_{t-1})}$, if $L_t <= L_{t-1}$, or $JD_t = 0$, if $L_t > L_{t-1}$. Moreover, the net job creation rate ($NJCR_t$) corresponds to $JC_t - JD_t$ and the job reallocation rate (JR_t) is equal to $JC_t + JD_t$.

In terms of worker flows, the hiring rate is $H_t = \frac{Hirings_{t,t-1}}{0.5(L_t + L_{t-1})}$, in which $Hirings_{t,t-1}$ denotes the number of workers employed by the firm in period t but not in period $t-1$, and the separation rate is $S_t = \frac{Separations_{t,t-1}}{0.5(L_t + L_{t-1})}$, in which $Separations_{t,t-1}$ denotes the number of workers employed by the firm in the period $t-1$ but nor in period t.[9] Finally, the worker reallocation rate (WR_t) is $H_t + S_t$, and the churning rate (CR_t), a measure of 'excessive turnover' (Burgess et al. 2000), is defined here as $WR_t - JR_t$.

In terms of the sample of the data used in this paper, we consider only firms with sizes ranging between 10 and 30 employees in 1989 (the 1989 data refers to March, before the new law about firings came into force, which occurred only in May). Such range of firm sizes seems appropriate as we want to use a sample including only firms that are very similar, except that their sizes are slightly different, in the spirit of a regression discontinuity approach (Hahn et al. 2001). Moreover, any results based on a wider range could also be affected by the new law about collective dismissals, which changed differently for firms larger or smaller than 50

[9]We calculate hirings by considering the information about the the year and the month in which each worker is hired and we calculate separations using the identity $L_t - L_{t-1} \equiv Hirings_{t,t-1} - Separations_{t,t-1} \Leftrightarrow Separations_{t,t-1} \equiv Hirings_{t,t-1} - (L_t - L_{t-1})$ rather than by comparing individual identifiers between periods $t-1$ and t. The reason for this choice is that we believe there is less scope for measurement error in the tenure data than in the individual identifiers (the latter have to be compared over two periods while the former need to be considered in only one period).

employees (see Section 2). We also drop firms employing exactly 20 workers, the threshold level in the new dismissals law, as it may have been unclear if such firms belonged to the treatment or the control groups (although, strictly speaking, firms with 20 employees would be in the 'treatment' group). Furthermore, firm size will also typically fluctuate over time, even if only slightly, which would possibly make it risky to assign firms with 20 employees to the control or to the treatment groups. Our definition of size is based only on paid employees, excluding other types of workers (employers, unpaid family workers and other residual categories).

One concern when selecting the sample of interest is mean reversion or the 'regression falacy' (Davis et al. 1996*a*,*b*), as selecting firms into the treatment or control groups based upon size in a single year only could bias our results. In fact, such assignment would probably imply that some firms in the small (large) size category would correspond to firms that are typically of a larger (smaller) size but that had had a relatively bad (good) year in that period. These firms would therefore be likely to switch back to their 'permanent' size after 1989, thus potentially distorting our analysis.

In order to address this problem, we construct a sample made up of firms that are likely to have reached their 'permanent' size by 1989. Specifically, we restrict our sample to firms that remain in the same size category, between 10 and 19 workers or between 21 and 30 workers, over a period of three years up to 1989. Initially, we find 16,267 firms that employ 10-19 or 21-30 workers in 1987, while in 1988 and 1989, the equivalent numbers of firms are 17,565 and 18,964, respectively. When restricting the sample to firms present in the data in all three years and that remain in the same size category over the period (i.e. that are always 'small' or 'large' from 1987 to 1989), we obtain 7,480 different firms, of which 5,863 are 'small'.[10] In terms of their observable characteristics, some noteworthy differences between the two groups of firms include worker reallocation rates (an average of 0.37 for smaller firms and of 0.33 for larger firms in the 1989 data) and hourly pay (2.76 euros per hour for smaller firms and 2.96 for larger firms, again in the 1989 data) - see Table A.1 for a list of descriptive statistics for each type of firms.

The 7,480 firms considered employed 122,062 individuals in 1989. This is also the year in which the total number of employees of those selected firms peaks, although the equivalent numbers for 1987 and 1988 are very similar (119,401 and 121,561, respectively) - see Table

[10] 44% of these firms are present in all years from 1986 to 1999, the period we cover in our data. That is also by far the most common time pattern in the data, as the second most common pattern, comprising firms that are present from 1986 to 1993, includes only 4% of all firms.

A.2. The lowest number of employees is found in the last period covered, 1999, although by then the number of firms has also fallen considerably, from 7,480 to to 4,866, due to firm exits (and no firm entry, by definition from our sample construction criteria). While firm sizes range, again by construction, between 10 and 30 workers between 1987 and 1989, the range of the size variable is larger in 1986 and in the years after 1989. For instance, in 1999, 32% of the firms remaining employ less than 10 employees (the lower threshold for the 1987-89 period), while 10% employ more than 30 employees (the upper threshold in the 1987-89 period).[11]

5 Results

In this section we present our results regarding the impact of the lower firing costs in terms of the variables measured at the firm level. We consider job and worker flows, wages and firm performance and conduct the estimations using the difference-in-differences matching approach described above.

5.1 Matching

We match firms in terms of their characteristics (including worker variables averaged to the firm level), as measured in 1989 and 1988. Specifically, the variables used in the matching between the treatment and control groups are: schooling (average level of schooling of all employees in the firm), experience, tenure, gender (percentage of women amongst all the employees in the firm), hourly pay, hours worked, job level (average job level across all workers, ranked from level 1, top managers, to level 9, apprentices), foreign ownership (a dummy variable taking value one if 50% or more of the firm is owned by foreign investors), and firm age. We also consider in the matching process the squares and the cubes of each one of these variables for the year of 1989. As mentioned above, we also include in the set of variables to be matched the lagged values of all the linear terms of the 1989 variables. Finally, we add firm type dummies (based on differences in their legal structure; 5 categories), sector dummies (28) and region dummies (also 28), as determined by the characteristics of the firms in 1989. The results are based on Epanechnikov kernel matching, using a bandwidth of 0.06, and the Leuven & Sianesi (2004) software.

[11] See Cabral & Mata (2003) for an analysis of the distribution of firm sizes also based on the 'Quadros de Pessoal' data.

Table 1 presents the results concerning the balancing of covariates across the treatment and control groups, before and after matching. We display the results for the main variables measured in 1989 and their lags, but not the values for the squared and cubic terms or for the legal type, region and sector dummy variables (results available upon request). Except for two variables (schooling and hours), all variables indicate considerable reductions in the absolute value of bias, from 50% to 90%, after matching. Moreover, most of those variables also result in the non-rejection of tests of equality of their average values, between the treated and the control groups, after matching.[12]

Furthermore, we also find that the imposition of the common support is not too restrictive, as only 22 firms are left out from the analysis as a consequence of that constraint. The distributions of the propensity scores across the control and treatment groups are also very similar, although we find the expected greater density of control observations at lower levels of the propensity score when compared to treatment observations. The pseudo-R^2's from probit estimations of the propensity score on all the variables before and after matching (Sianesi 2004) also indicate that the matched sample is considerably more homogeneous after the matching (the pseudo-R^2 falls from 0.068 to 0.019). Overall, our view is that there is strong evidence that the matching is of particularly good standards.

5.2 Job and worker flows

After estimating the propensity score and evaluating the quality of the matching, we now assess the impact of the new law in terms of different firm outcomes. In the case of job flows, theory does not offer clear predictions, as hirings may be greater or smaller than separations. However, worker flows (hirings and/or separations) are expected to increase. Given that the costs of dismissals for cause fall, some workers that exhibit poor levels of performance but that have been employed until then due to the protection offered by the law may be dismissed once the new law is in force. At the same time, employers can also be expected to hire more, as such matches will no longer be as difficult to terminate as before the 1989 law. However, it is also possible that workers that exhibit poor levels of performance change their behaviour under the new circumstances created by the new law, so that separations do not necessarily increase. This possibility could also weaken the expected increase in hirings. On the other

[12]The case of schooling is not particularly important as the test of equality of its averages is still not rejected. The performance of hours is less good, as the bias increases after matching and the equality test is rejected. However, the economic difference in the means is very small.

hand, the effects of the law may be 'too micro' to be captured at the firm level analysis if, for instance, only one or two workers per firm tend to behave in such a way that there are grounds for a dismissal for cause.

In order to test these different predictions, we consider two versions of the net job creation variable, the first including the year of 1990 (for which only firm-level data are available) and the second not including that year (i.e. computing net job creation in 1991 based on a comparison between 1989 and 1991). Hirings and separations rates also ignore 1990: since there is no worker data for that year, one cannot decompose the net job creation rate in 1990 in terms of hirings and separations. For instance, hirings in 1991 correspond to all workers hired after March 1989 that are still employed in the firm by March 1991. From 1992 onwards, the two versions of net job creation coincide (for instance, net job creation in 1992 corresponds to the difference in firm size between 1992 and 1991).

In our results, we find evidence that small firms, those subject to a greater decrease in their firing costs, experience higher net job creation growth rates in 1990, the first year after the introduction of the new law (see Table 2). The ATT statistic for this year is 0.034 (t-ratio of 4.3). However, this higher net job creation rate was partially reversed in 1991, when the ATT statistic is equal to -0.025 (t-ratio of -2.8). All remaining t-ratios tend to be less than 2 in absolute value, except for 1993 and 1997, when the net job creation rates are also positive (ATT statistics of 0.019 and 0.028, respectively).[13]

The second block of estimates in Table 2 refers to the measure of net job creation in which the comparison in 1991 takes 1989 as the base period (not 1990 as in the first block). In other words, the information available for 1990 is ignored for the sake of greater comparability with the variables for which there is no 1990 data (e.g. hirings and separations). In this case, we find that there was no significant difference between the two types of firms (ATT=-0.001; t-ratio=-0.1). This result is not surprising as in the previous set of estimates the values for 1990 and 1991 were of opposite sign and of similar magnitudes.[14] Finally, Table 2 also reports

[13]We consider in this table all firms that are present in the data for at least one year in the period 1990-1999. Those firms are also present in all years in the 'before' period, 1987-1989, by construction of our sample. The fact that some firms exit the market while, by construction, no new firms are added into our sample explains why the number of firms falls, in our case from approximately 3,900 to 2,900 (treatment group) or from 1,200 to 900 (control group). Moreover, each flow rate is constructed as a comparison between adjacent years (e.g. of 1993 with respect to 1992) in the 'after' period and 1989 with respect to 1988 (for the 'before' period), not as the flow from 1989 to each year from 1990 onwards (e.g. the sum of the flow from 1989 to 1993), while no flows are registered for firms that leave the data.

[14]The results for the remaining years (1992-1999) under the second type of net job creation variable are not reported, as they are, by construction, precisely the same as those for the first net job creation variable.

results for the hirings and separations rates. Across the nine-year period covered and for each one of the two worker flow variables, we find almost no evidence of significant differences between the two types of firms. The only significant estimate indicates lower separations rates for smaller firms in 1993 (ATT=-0.019; t-ratio=-2.2). Results for the remaining flow variables (job reallocation, worker reallocation and churning) are not reported as they are also insignificant.

Overall, our conclusion from the results displayed in Table 2 is that there is only some moderate evidence of a slight increase in job creation in smaller firms. Strikingly, there is no robust evidence of increased worker flows (hirings or separations), unlike what theory predicts. However, there are some issues and caveats that need to be taken into account when interpreting these findings, on top of the argument described above about the possible particularly micro nature of the effects. One caveat is that our data are measured annually, a frequency that may not be sufficiently high to capture the worker-flow adjustments carried out by firms (Blanchard & Portugal 2001). This problem is compounded by the fact that there is no information on hirings or separations for 1990, although only if such matches are terminated before March 1991.

Another caveat is that changes in the values in the data are driven, in part, by both voluntary and involuntary separations - and perhaps an increase in the latter, as suggested by theory, coincides with a decline of the former. This trade-off could arise if those workers that before the new law would have left now believe they will be more easily fired at the prospective new job and prefer not to move. Moreover, as in Bauer et al. (forthcoming), our study cannot control directly for differences across firms in their numbers of temporary workers. It could be that smaller firms tend to employ a greater share of temporary workers and are thus less affected by the reform.[15] However, as we control carefully for the tenure level of each firms' workforce, we believe this potential problem is not serious in our case. Furthermore, this explanation would also be at odds with the findings reported in subsection 5.4 (see below).

Finally, there is one additional effect that may influence the absence of any increases in worker reallocation. This effect concerns the nature of the reform itself: if a firm expands above the 20-employee threshold, the firm will then become subject to the more costly dismissal

[15] Moreover, some evidence based on subjective, cross-section data suggests that medium-size firms are more negatively affected by EPL than smaller firms (Pierre & Scarpetta 2006). Taking this result at face value, then any increased levels of worker reallocation in smaller firms induced by the eight-paragraph difference may have been just cancelled out by such greater sensitivity of larger firms with respect to EPL.

procedures of that size category, implying that the marginal factor cost of the 21^{st} employee will exceed considerably that worker's wage (plus any wage increases paid to infra-marginal workers). Symmetrically, larger firms that move below the 20-employee threshold will become subject to the more favourable EPL regulations.[16] Larger firms may achieve this by abstaining to replace workers that separe or even by splitting into two or more firms. If these behaviours are present in the data, then the impact of the new law upon net job creation and worker flows could be weaker than otherwise, which again can explain the difference between our results and theory.

In the next two subsections, we examine the impacts in terms of wages and firm performance. As these two variables correspond to stocks, not flows, some of the caveats described above (e.g. the frequency of the data) are of less importance.

5.3 Wages

There are two different theoretical models one can appeal to in order to think about the relationship between EPL and wages. On the one hand, in a bargaining framework (e.g. Autor (2003)), lower firings costs will transfer bargaining power from incumbent workers to their employers, as dismissal threats will carry more weight. Wages will fall, potentially by more the greater the surplus in the employer-employee relationship. On the other hand, as in the case of the framework described in Lazear (1990), the fall in firing costs will lead to an increase in wages, since competition will no longer drive employers to discount the wages paid to workers by the amount of the firing costs. However, the Lazear (1990) model is not particularly well suited in our specific context as that model focuses on the case of new hires (see also Leonardi & Pica (2007)) while we consider the entire workforce of each firm. Moreover, while Lazear (1990) assumes that firing costs are given, the specific dimension of EPL we study involves firing costs that are difficult to determine at the time the worker is hired, although firms could establish their own expectations of such costs for each worker and then adjust entry wages correspondingly.

Table 3 presents our results, in which we take as the outcome variable the logarithm of the firm-level average hourly wage (measured in 2004 prices) of all employees in each firm

[16] There is some evidence of this type of behaviour, as we find that while only approximately 10% of the smaller firms eventually grow above the 20-employee threshold, a large percentage, about 40%, of the larger firms eventually (i.e. after 1989) employ less than 20 workers. In future research, we plan to examine in more detail the impact of the law in terms of the probability that firms move across the 20-employee threshold (Schivardi & Torrini 2005).

and in each year. We find significant differences between the treated and the control groups, whereby wage growth rates in smaller firms fall systematically and significantly with respect to larger firms. For instance, in the first year in which we can measure the impact of the reform with worker-level data, 1991, we find that wage growth is 1.6 percentage points lower in smaller firms than in larger firms (t-ratio of -2.0). The magnitude of the decline varies slightly over the nine-year period considered, from -0.011 (1995; not significant t-ratio of -1.1) to -0.035 (1997; t-ratio=-2.9). In six of the nine years, there is a significant decline in the wages in the treated group when compared to the control group. Moreover, the insignificant results found for 1994 and 1995 may be related to the recession that ocurred over those years and its interaction with downward wage rigidity.

Although wages fall significantly, that would not necessarily imply differences in the reward policy of firms or in their relative bargaining power with respect to employees if, for instance, worker composition also changed. We checked for this by contrasting several different average worker characteristics (schooling, gender, job level, tenure, hours worked, etc) across the treatment and control groups, over the 1991-1999 period, employing the same methodology as for worker flows and wages. For none of those worker characteristics did we find any significant differences across the two groups of firms. This result strengthens our interpretation of the evidence in this subsection as indicating that, faced with less burdensome firing procedures, employers saw their bargaining power increase and were able to extract more surplus from their employees. However, the results could also be consistent with constant bargaining parameters but a decrease in the surplus generated by employer-employee matches due to any downturn in firm performance caused by the new law. The next subsection examines this possibility by considering the impact of the new law upon firm performance.[17]

5.4 Firm performance

Theoretically, one can again outline two opposing types of impacts of lighter firing costs upon firm performance. If firing costs are a sufficiently important incentive for workers to invest in firm-specific skills (see Autor (2003)), then firm performance may actually suffer from the reform studied here. In other words, if firms are no longer able to commit on a

[17] In current work (Martins 2007) we are examining the impact on wages in more detail, by considering possible differences across workers of different types, namely between those that are more or less likely to benefit from bargaining power. Our preliminary results reinforce the view that bargaining power differences are driving the changes in wages observed here.

long-term employment relationship, workers will tend to find it less advantageous to invest in firm-specific skills, leading to a deterioration in firm performance.

However, if workers earn rents at their current jobs, then worker effort (and therefore firm performance) may instead increase when firing costs fall. As it becomes less unlikely that workers will lose their current wage premiums, if effort is observable by employers, then effort will increase and firm performance will improve (see also the discussion in Autor et al. (2007)). Similarly, improvements in personnel management warranted by the greater flexibility of the new law can also be expected to lead to better performance. Firm performance may increase not only (or not necessarily) because workers exert more effort but also (or simply) because uncooperative or even disruptive workers are fired.

In order to shed light on these contrasting views about the role of EPL, we examine different measures of firm performance that can be constructed from our data (see the last three block of estimates in Table 3). In our first measure, we consider the logarithm of total sales (2004 prices) as the outcome variable, again in a DDM approach. We find that all estimates, across the nine-year period considered, are positive, and four of them are significant. Those significant estimates are also particularly large, ranging between 8.3pp (1995) and 12.8pp (1998), while the estimates with t-ratios below 2 range from 3.7pp and 6pp. Moreover, the pattern of results is relatively unchanged when considering instead the logarithm of sales per worker as the outcome of interest (second block of estimates). Now five out of the nine estimates have t-ratios above 2, indicating effects between 5.2pp and 10.5pp.

We consider one additional measure of performance, 'surplus per worker', which is defined as the difference between total sales and the wage bill of each firm. The wage bill is computed by summing all individual wages, and multiplying that sum by 14 (the number of months of pay due to each worker per year, according to Portuguese law) and by 1.2 (corresponding to employer payroll taxes of approximately 20%). We then take as our dependent variable the logarithm of the ratio of the surplus by the number of workers. The results (see the bottom of Table 3) are again consistent with the previous findings, indicating that treated firms underwent a positive relative increase in their performance. Four out of the nine years produce large and significant increases in surplus, while the remaining estimates are also positive and, in most cases, marginally significant. 1994 is a year which never exhibits a significant difference across the three sets of estimates but, as mentioned above, this may be related to the early/mid 1990's recession.

Moreover, while performance could be increasing in smaller firms because they were investing more in capital, we have found no evidence of capital deepening. For this specific analysis, we considered a variable available in the data set (equity) that can be used as a proxy for the amount of capital invested in the firm. Using the same DDM framework as for the results described above, we found no significant differences between treated and control group firms (results not presented but available upon request). Previously, in Section 5.3, we have also shown that there are no observable differences in worker composition. Finally, it is worthwhile to take into account that, in the case that medium and large firms tend to be those typically most (negatively) affected by EPL (Pierre & Scarpetta 2006), then our estimates of the impact of the reduction in the dismissal costs upon firm performance are only lower bounds of the true effect.

Overall, the results support decisively the view that strict constraints against dismissals for cause hurt firm performance. It is, however, less straightforward to decide if the increase in performance is due to increasing worker effort or to better personnel management. Even the fact that all measures of performance indicate that the effects increase over time, typically from about 4pp in 1991 to about 10pp in 1999, suggesting that such benefits arise gradually, can be reconciled with either view. However, as these firm performance results reinforce the view that bargaining power is transferred from employees to employers (wages fall in a context of increasing performance), it is difficult to believe that such increase in employer bargaining power had no positive impact upon workers' effort.

6 Robustness

We pursue several different types of robustness analysis in this section. In our first approach, we reestimate the benchmark firm-level results presented in subsections 5.2, 5.3 and 5.4 but considering now only continuing firms from 1987 to 1999. Previously, we had considered all firms present in the data from 1987 to 1989 and then in each year from 1990 to 1999 in which those firms are present in the data. The present robustness analysis seeks to consider the possibility that composition differences over time (namely as firms leave business or are acquired) drive, at least partially, our results. For instance, perhaps the relative increases in performance that we find for smaller firms are driven by the firms that perform poorly and then disappear from the sample (possibly because they become bankrupt), consequently being ignored in the results. By considering now only firms that are always present in our data set, we rule out such censoring bias.

We present the findings for this more restrictive sample selection in Table 4 (job and worker flows) and Table 5 (wages and firm performance). First, we observe that the number of firms declines from between 5114 in 1990 and 3816 in 1999, when considering all firm-years, to 1671, when only considering firms present in all years, from 1987 to 1999, indicating that the sample of firms used for each year before can vary reasonably. Second, and more important, the results indicate that our benchmark findings are robust. There are no qualitative differences in terms of job or worker flows (including the 1990 spike, which is still discernible) when compared to the benchmark results of Tables 2 and 3. Moreover, wages also decrease significantly and firm performance also undergoes a significant increase, at very similar magnitudes, although in the latter case the precision of the estimates falls. While the decrease in precision may in part indicate that firm selection matters, we believe a more important reason is the smaller sample size. In any case, it is important to underline that the qualitative results are unchanged and that several point estimates remain statistically significant.

In our second robustness test, we reestimate the main firm-level results but using a different matching algorithm. Instead of kernel matching, we now consider nearest neighbour matching (five nearest matches). The results (job and worker flows in Table 6 and wages and firm performance in Table 7) are again very similar to those obtained in the main estimates, both in qualitative and in quantitative terms. As before, we find insignificant differences in flows (except for the year of 1990), a significant decrease in wages and a significant increase in our different proxies of firm performance.

Our third robustness analysis is based on considering an 'artificial' firm size threshold above the one determined by the law (20 employees). The concern this strategy seeks to address is that the findings reported above may be driven by differences in firm size only, in such way that they are not related to any real impact of the law. For instance, although most research documents a very strong positive relationship between firm size and wage levels (see Oi & Idson (1999) for a survey), it is also possible that wage growth is higher at bigger firms. Such possibility could explain our findings about wages, as they are based on contrasting wage growth over time between two groups of firms wof different sizes.

We implement this falsification exercise by creating a new data set, including only firms that employ, in the period 1987-1989, either between 20-29 workers (the new 'treatment' group) or 31-40 workers (the new 'control' group), i.e. assuming that the size threshold indicated by the law was 30. In the results based on this new dataset, we find, as before, that there is a spike in job creation in 1990, although there are no significant differences when considering only the difference between 1991 and 1989 (see Table 8). The results for hirings and separations are also very similar to those obtained when considering the correct firm-size threshold (20). These results therefore support our previous conclusion that there are no significant differences induced by the law regarding employment and job and worker flows (and that the 1990 spike is spurious). However, when examining the effects in terms of wages and firm performance (see Table 9), we now fail to observe any significant differences between 'small' and 'large' firms. This contrasts dramatically with the findings based on the threshold established in the law (20 employees), from which we documented significant decreases in wages and significant increases in firm performance. The differences between the two sets of results are particularly clear in the case of firm performance, in which many point estimates are even negative when considering the artificial threshold. The contrast between the two sets of findings therefore lends additional credibility to our results in Section 5 and their interpretation as causal effects of the law reform.

Finally, we address the possibility that by March 1989 (the reference period in our data) some firms were already responding to the reform, implying that 1989 cannot be considered as the last year of the 'before' period. As mentioned before, we do not believe this to be the case, as there was still great uncertainty at the time concerning the specific content of the employment law, not to mention if any reform would go ahead at all. In any case, in order to examine this possibility, we consider 1988 as the last year of the 'before' period and keep 1990 and 1991 as the first years of the 'after' period. Again we obtain the same qualitative results as in the benchmark case, although the results about firm performance tend to be slightly less significant in the first years (results available upon request).[18]

[18] We have also replicated our main analysis but trimming the range of firm sizes covered, in the spirit of a regression-discontinuity approach (Hahn et al. 2001). When considering only firms employing between 15 and 19 workers in 1987-1989 and firms employing between 21 and 25 workers in the same period, we find the same qualitative results as those presented in Section 5. However, as the sample size becomes considerably smaller (less than 700 treated firms and less than 400 control firms) and, in relative terms, the scope for misclassification increases (e.g. firms employing 19 workers in March 1989 may be employing 21 workers in June), the estimates tend to be somewhat less significant than in the benchmark results (results available upon request).

7 Conclusions

This paper provides evidence about the effects of a specific component of employment protection legislation that has received relatively little attention but may be very important in practice - the regulations involving dismissals for cause. Indeed, while the literature has so far focused on constraints regarding dismissals driven by economic shocks, adjustment costs imposed upon dismissals related to worker performance or disciplinary reasons may also be particularly relevant.

In order to identify the impact of the regulations governing dismissals for cause, we study a quasi-natural experiment generated by a law introduced in Portugal in 1989 which cut firing costs for all firms, but particularly for smaller firms. Until then, there was no differentiation in firing costs for firms of different size, unlike in other countries. In the new law, out of the 12 paragraphs that dictated the costly procedure that firms should follow when dismissing a worker for cause, eight of those paragraphs did not apply to firms employing 20 or fewer workers. Firing costs related to dismissals for cause thus became considerably lighter for those smaller firms.

Using detailed matched employer-employee longitudinal data and difference-in-differences matching techniques, we examine the impact of this differentiated change in firing costs upon a large range of firm- and worker-level outcomes, measured over an extended period of time (1991-1999). In our firm-level results, we do not find any robust evidence of significant effects in job or worker flows, although theory predicts increased worker turnover. However, we find that smaller firms (our 'treatment' group) exhibit significant and largely permanent falls in wage growth (of about 2pp). Smaller firms also exhibit significant and largely permanent increases in different measures of firm performance (typically increasing from about 4pp to 10pp over the 1990s). Moreover, we also provide evidence that these developments cannot be explained by any significant differences in terms of observable worker composition, capital deepening or by firm heterogeneity that coincides with the size threshold defined in the law. The results are also robust to different matching algorithms, sample definitions and other robustness analyses.

Overall, the results suggest that worker effort responds to the severity of EPL constraints of the type examined here, although this increase in performance could also be partly due to improvements in personnel management. At the same time, the reduction in firing constraints is also likely to transfer bargaining power from employees to their employers, which would explain the decline in wages found in our results.

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Table 1: **Analysis of balancing properties (firm-level analysis)**

Variable	Sample	Mean Treated	Mean Control	% bias	% reduct bias	t-test t	t-test $p > [t]$
Schooling	Unmatched	5.6168	5.6442	-1.5		-0.48	0.633
	Matched	5.6162	5.6674	-2.9	-87.2	-1.3	0.193
Experience	Unmatched	23.46	24.104	-9.5		-2.9	0.004
	Matched	23.443	23.416	0.4	95.8	0.17	0.866
Tenure	Unmatched	7.5301	8.5899	-23.4		-7.29	0
	Matched	7.5424	7.3735	3.7	84.1	1.74	0.082
Female	Unmatched	0.33958	0.34491	-1.8		-0.56	0.577
	Matched	0.33959	0.33718	0.8	54.8	0.37	0.71
Pay	Unmatched	2.719	2.9566	-16.7		-5.37	0
	Matched	2.7108	2.781	-4.9	70.5	-2.48	0.013
Hours	Unmatched	168.98	168.75	0.9		0.29	0.769
	Matched	169.01	166.71	9.5	-911.1	4.13	0
Job level	Unmatched	5.7259	5.6502	11.2		3.35	0.001
	Matched	5.728	5.7405	-1.9	83.4	-0.81	0.42
Foreign	Unmatched	0.00937	0.01178	-2.4		-0.76	0.445
	Matched	0.00941	0.00847	0.9	61.1	0.46	0.645
Firm age	Unmatched	1970.5	1965.9	19.5		7.04	0
	Matched	1970.5	1970.7	-0.9	95.3	-0.58	0.56
Lag schooling	Unmatched	5.5861	5.5712	0.8		0.25	0.801
	Matched	5.5872	5.6554	-3.7	-357.8	-1.67	0.096
Lag experience	Unmatched	23.257	23.914	-9.8		-3.01	0.003
	Matched	23.234	22.848	5.8	41.2	2.6	0.009
Lag tenure	Unmatched	7.3098	8.3859	-23.7		-7.41	0
	Matched	7.3198	7.1296	4.2	82.3	1.95	0.051
Lag female	Unmatched	0.33437	0.33799	-1.2		-0.38	0.706
	Matched	0.33416	0.33328	0.3	75.7	0.14	0.893
Lag pay	Unmatched	2.6314	2.8861	-17.9		-6.04	0
	Matched	2.6323	2.7078	-5.3	70.4	-2.76	0.006
Lag hours	Unmatched	170.94	170.76	0.7		0.23	0.816
	Matched	170.95	168.87	8.4	-1027	3.62	0
Lag job level	Unmatched	5.7403	5.657	12		3.61	0
	Matched	5.7416	5.7577	-2.3	80.6	-1.02	0.308

Notes: Source: Author's calculations based on *Quadros de Pessoal*. *Treatment* and *Control* indicates the number of firms in the treatment and control groups respectively (firms with 10 to 19 employees in 1989 are in the treatment group; firms with 21 to 30 employees in 1989 are in the control group). All variables are firm-level averages based on the characteristics of firms in 1989 (lags correspond to 1988 information).

Table 2: **Effects on job and worker flows (firm-level analysis);** Matching method: kernel

Variable	*Year*	*ATT*	*t(ATT)*	*Treated*	*Control*
Net job creation rate					
	1990	0.034	4.3	3923	1191
	1991	-0.025	-2.8	3482	1088
	1992	-0.012	-1.4	3392	1066
	1993	0.019	2.0	3405	1056
	1994	0.011	0.9	3421	1061
	1995	0.001	0.1	3319	1052
	1996	0.007	0.6	3198	977
	1997	0.028	2.6	3114	954
	1998	-0.012	-1.1	2967	906
	1999	0.014	1.2	2924	892
Net job creation rate (2)					
	1991	-0.001	-0.1	3562	1117
	1992	-0.012	-1.4	3392	1066
Hirings rate					
	1991	-0.001	-0.2	3562	1117
	1992	-0.002	-0.3	3392	1066
	1993	-0.000	-0.0	3405	1056
	1994	0.007	1.0	3421	1061
	1995	0.001	0.1	3319	1052
	1996	0.005	0.7	3198	977
	1997	0.008	0.9	3114	954
	1998	-0.003	-0.3	2967	906
	1999	0.002	0.3	2924	892
Separations rate					
	1991	0.000	0.0	3562	1117
	1992	0.010	1.3	3392	1066
	1993	-0.019	-2.2	3405	1056
	1994	-0.005	-0.4	3421	1061
	1995	-0.000	-0.0	3319	1052
	1996	-0.002	-0.2	3198	977
	1997	-0.020	-1.8	3114	954
	1998	0.009	0.9	2967	906
	1999	-0.011	-1.1	2924	892

Notes: Source: Author's calculations based on *Quadros de Pessoal*. *ATT* refers to the average treatment on the treated in terms of the outcome variables considered and at the year under analysis. *t(ATT)* denotes *t*-ratios based on analytical standard errors. The outcome variable is measured by the difference between the value of the variable in the year under analysis and the base year, 1989. See main text for the formal definition of each variable. *Treatment* and *Control* indicates the number of firms in the treatment and control groups respectively (firms with 10 to 19 employees in 1989 are in the treatment group; firms with 21 to 30 employees in 1989 are in the control group). 'Net job creation rate (2)' considers the 1991 level as driven by the difference in employment between 1989 and 1991, while in 'Net job creation rate' the 1991 level is constructed from the difference in employment between 1990 and 1991.

Table 3: **Effects on wages and firm performance (firm-level analysis);** Matching method: kernel

Variable	*Year*	*ATT*	*t(ATT)*	*Treated*	*Control*
Wages					
	1991	-0.016	-2.0	3991	1211
	1992	-0.022	-2.2	4010	1198
	1993	-0.024	-2.4	3966	1199
	1994	-0.015	-1.4	3980	1207
	1995	-0.011	-1.1	3813	1152
	1996	-0.024	-2.1	3633	1080
	1997	-0.035	-2.9	3567	1079
	1998	-0.033	-2.9	3399	1022
	1999	-0.012	-1.0	3360	1001
Total sales					
	1991	0.045	1.7	3336	987
	1992	0.046	1.7	3320	981
	1993	0.090	2.8	3272	980
	1994	0.037	1.1	3136	945
	1995	0.083	2.3	2953	873
	1996	0.057	1.5	2832	839
	1997	0.060	1.4	2724	812
	1998	0.128	2.9	2673	790
	1999	0.107	2.5	2635	791
Sales per worker					
	1991	0.046	1.8	3336	987
	1992	0.052	2.0	3320	981
	1993	0.062	2.1	3272	980
	1994	0.002	0.1	3136	945
	1995	0.065	2.0	2953	873
	1996	0.043	1.4	2832	839
	1997	0.025	0.7	2724	812
	1998	0.105	2.9	2673	790
	1999	0.087	2.5	2635	791
Surplus per worker					
	1991	0.069	2.2	3222	957
	1992	0.085	2.6	3198	959
	1993	0.057	1.7	3128	933
	1994	0.013	0.3	3012	910
	1995	0.068	1.8	2850	845
	1996	0.054	1.4	2721	806
	1997	0.033	0.8	2629	782
	1998	0.167	3.8	2579	757
	1999	0.150	3.4	2546	770

Notes: Source: Author's calculations based on *Quadros de Pessoal*. *ATT* refers to the average treatment on the treated in terms of the outcome variables considered and at the year under analysis. *t(ATT)* denotes t-ratios based on analytical standard errors. The outcome variable is measured by the difference between the value of the variable in the year under analysis and the base year, 1989. See main text for the formal definition of each variable. *Treatment* and *Control* indicates the number of firms in the treatment and control groups respectively (firms with 10 to 19 employees in 1989 are in the treatment group; firms with 21 to 30 employees in 1989 are in the control group).

Table 4: **Robustness - Effects on job and worker flows (firm-level analysis; only continuing firms);** Matching method: kernel

Variable	*Year*	*ATT*	*t(ATT)*	*Treated*	*Control*
Net job creation rate					
	1990	0.026	1.9	1252	419
	1991	-0.015	-1.2	1252	419
	1992	-0.017	-1.4	1252	419
	1993	0.026	1.8	1252	419
	1994	-0.005	-0.3	1252	419
	1995	-0.007	-0.5	1252	419
	1996	-0.020	-1.4	1252	419
	1997	0.021	1.5	1252	419
	1998	-0.015	-1.1	1252	419
	1999	0.023	1.3	1252	419
Net job creation rate (2)					
	1991	0.008	0.5	1252	419
	1992	-0.017	-1.4	1252	419
Hirings rate					
	1991	0.003	0.2	1252	419
	1992	-0.006	-0.6	1252	419
	1993	0.005	0.4	1252	419
	1994	-0.004	-0.3	1252	419
	1995	-0.013	-1.0	1252	419
	1996	-0.017	-1.3	1252	419
	1997	0.001	0.1	1252	419
	1998	-0.011	-0.9	1252	419
	1999	0.007	0.5	1252	419
Separations rate					
	1991	-0.005	-0.4	1252	419
	1992	0.011	1.0	1252	419
	1993	-0.022	-1.6	1252	419
	1994	0.001	0.1	1252	419
	1995	-0.006	-0.5	1252	419
	1996	0.003	0.2	1252	419
	1997	-0.020	-1.4	1252	419
	1998	0.003	0.2	1252	419
	1999	-0.017	-1.0	1252	419

Notes: Source: Author's calculations based on *Quadros de Pessoal*. *ATT* refers to the average treatment on the treated in terms of the outcome variables considered and at the year under analysis. *t(ATT)* denotes t-ratios based on analytical standard errors. The outcome variable is measured by the difference between the value of the variable in the year under analysis and the base year, 1989. See main text for the formal definition of each variable. *Treatment* and *Control* indicates the number of firms in the treatment and control groups respectively (firms with 10 to 19 employees in 1989 are in the treatment group; firms with 21 to 30 employees in 1989 are in the control group). 'Net job creation rate (2)' considers the 1991 level as driven by the difference in employment between 1989 and 1991, while in 'Net job creation rate' the 1991 level is constructed from the difference in employment between 1990 and 1991.

Table 5: **Robustness - Effects on wages and firm performance (firm-level analysis; only continuing firms);** Matching method: kernel

Variable	*Year*	*ATT*	*t(ATT)*	*Treated*	*Control*
Wages					
	1991	-0.013	-0.9	1245	419
	1992	-0.040	-2.0	1248	419
	1993	-0.024	-1.3	1247	418
	1994	-0.023	-1.3	1245	419
	1995	-0.036	-2.0	1244	414
	1996	-0.050	-2.3	1239	413
	1997	-0.039	-1.8	1243	416
	1998	-0.073	-3.8	1241	418
	1999	-0.064	-3.5	1241	419
Total sales					
	1991	0.087	1.9	1102	368
	1992	0.067	1.5	1098	370
	1993	0.076	1.5	1108	369
	1994	0.058	1.2	1100	372
	1995	0.057	1.1	1099	372
	1996	0.057	1.1	1099	367
	1997	0.082	1.4	1094	363
	1998	0.084	1.4	1097	367
	1999	0.117	1.7	1034	355
Sales per worker					
	1991	0.079	1.8	1102	368
	1992	0.090	2.0	1098	370
	1993	0.069	1.4	1108	369
	1994	0.049	1.0	1100	372
	1995	0.049	1.0	1099	372
	1996	0.076	1.5	1099	367
	1997	0.095	1.8	1094	363
	1998	0.111	2.1	1097	367
	1999	0.113	2.0	1034	355
Surplus per worker					
	1991	0.097	1.8	1068	360
	1992	0.099	1.7	1056	363
	1993	0.073	1.3	1056	355
	1994	0.063	1.1	1054	364
	1995	0.066	1.1	1053	368
	1996	0.107	1.9	1055	356
	1997	0.100	1.5	1053	355
	1998	0.161	2.4	1053	359
	1999	0.130	2.0	996	348

Notes: Source: Author's calculations based on *Quadros de Pessoal*. *ATT* refers to the average treatment on the treated in terms of the outcome variables considered and at the year under analysis. *t(ATT)* denotes *t*-ratios based on analytical standard errors. The outcome variable is measured by the difference between the value of the variable in the year under analysis and the base year, 1989. See main text for the formal definition of each variable. *Treatment* and *Control* indicates the number of firms in the treatment and control groups respectively (firms with 10 to 19 employees in 1989 are in the treatment group; firms with 21 to 30 employees in 1989 are in the control group).

Table 6: **Robustness - Effects on job and worker flows (firm-level analysis); Matching method: five nearest neighbours**

Variable	*Year*	*ATT*	*t(ATT)*	*Treated*	*Control*
Net job creation rate					
	1990	0.040	4.6	3803	1163
	1991	-0.023	-2.3	3381	1064
	1992	-0.006	-0.6	3289	1039
	1993	0.023	2.2	3302	1028
	1994	0.002	0.1	3326	1032
	1995	-0.001	-0.1	3225	1023
	1996	0.017	1.4	3105	951
	1997	0.026	2.1	3023	929
	1998	-0.015	-1.3	2875	881
	1999	0.021	1.7	2841	867
Net job creation rate (2)					
	1991	0.001	0.1	3458	1091
	1992	-0.006	-0.6	3289	1039
Hirings rate					
	1991	0.003	0.4	3458	1091
	1992	0.004	0.6	3289	1039
	1993	0.001	0.2	3302	1028
	1994	0.003	0.4	3326	1032
	1995	-0.003	-0.3	3225	1023
	1996	0.008	1.0	3105	951
	1997	0.006	0.6	3023	929
	1998	0.000	0.0	2875	881
	1999	0.014	1.6	2841	867
Separations rate					
	1991	0.002	0.2	3458	1091
	1992	0.010	1.2	3289	1039
	1993	-0.022	-2.2	3302	1028
	1994	0.002	0.1	3326	1032
	1995	-0.002	-0.2	3225	1023
	1996	-0.009	-0.8	3105	951
	1997	-0.019	-1.5	3023	929
	1998	0.015	1.3	2875	881
	1999	-0.007	-0.6	2841	867

Notes: Source: Author's calculations based on *Quadros de Pessoal*. *ATT* refers to the average treatment on the treated in terms of the outcome variables considered and at the year under analysis. *t(ATT)* denotes *t*-ratios based on analytical standard errors. The outcome variable is measured by the difference between the value of the variable in the year under analysis and the base year, 1989. See main text for the formal definition of each variable. *Treatment* and *Control* indicates the number of firms in the treatment and control groups respectively (firms with 10 to 19 employees in 1989 are in the treatment group; firms with 21 to 30 employees in 1989 are in the control group). 'Net job creation rate (2)' considers the 1991 level as driven by the difference in employment between 1989 and 1991, while in 'Net job creation rate' the 1991 level is constructed from the difference in employment between 1990 and 1991.

Table 7: **Robustness - Effects on wages and firm performance (firm-level analysis); Matching method: five nearest neighbours**

Variable	*Year*	*ATT*	*t(ATT)*	*Treated*	*Control*
Wages					
	1991	-0.021	-2.3	3870	1183
	1992	-0.028	-2.5	3897	1170
	1993	-0.029	-2.6	3854	1170
	1994	-0.021	-1.7	3872	1178
	1995	-0.014	-1.2	3704	1124
	1996	-0.032	-2.4	3530	1054
	1997	-0.048	-3.5	3464	1052
	1998	-0.040	-3.1	3297	995
	1999	-0.018	-1.4	3261	975
Total sales					
	1991	0.054	1.7	3259	967
	1992	0.043	1.3	3242	964
	1993	0.094	2.5	3197	962
	1994	0.045	1.2	3062	928
	1995	0.085	2.0	2877	854
	1996	0.085	1.9	2761	820
	1997	0.085	1.7	2658	793
	1998	0.139	2.8	2612	774
	1999	0.124	2.6	2574	778
Sales per worker					
	1991	0.049	1.6	3259	967
	1992	0.040	1.3	3242	964
	1993	0.051	1.5	3197	962
	1994	0.005	0.1	3062	928
	1995	0.061	1.6	2877	854
	1996	0.064	1.8	2761	820
	1997	0.034	0.9	2658	793
	1998	0.115	2.8	2612	774
	1999	0.098	2.5	2574	778
Surplus per worker					
	1991	0.059	1.7	3153	937
	1992	0.072	1.9	3125	942
	1993	0.051	1.3	3058	916
	1994	0.016	0.4	2944	893
	1995	0.067	1.5	2780	828
	1996	0.079	1.8	2654	787
	1997	0.042	0.9	2570	764
	1998	0.171	3.4	2523	744
	1999	0.171	3.4	2491	757

Notes: Source: Author's calculations based on *Quadros de Pessoal*. *ATT* refers to the average treatment on the treated in terms of the outcome variables considered and at the year under analysis. *t(ATT)* denotes *t*-ratios based on analytical standard errors. The outcome variable is measured by the difference between the value of the variable in the year under analysis and the base year, 1989. See main text for the formal definition of each variable. *Treatment* and *Control* indicates the number of firms in the treatment and control groups respectively (firms with 10 to 19 employees in 1989 are in the treatment group; firms with 21 to 30 employees in 1989 are in the control group).

Table 8: **Robustness - Effects on job and worker flows - Artificial threshold (firm-level analysis, 20-40 employees in 1989);** Matching method: kernel

Variable	*Year*	*ATT*	*t(ATT)*	*Treated*	*Control*
Net job creation rate					
	1990	0.042	3.6	1195	504
	1991	-0.018	-1.4	1090	465
	1992	0.022	1.8	1071	441
	1993	-0.012	-0.9	1047	436
	1994	0.016	0.9	1057	454
	1995	0.013	0.9	1048	444
	1996	0.003	0.2	982	410
	1997	-0.023	-1.4	957	402
	1998	0.015	1.1	904	398
	1999	0.011	0.6	889	388
Net job creation rate (2)					
	1991	0.004	0.3	1112	472
	1992	0.022	1.8	1071	441
Hirings rate					
	1991	0.008	0.9	1112	472
	1992	0.004	0.5	1071	441
	1993	-0.011	-1.4	1047	436
	1994	-0.008	-0.9	1057	454
	1995	-0.015	-1.6	1048	444
	1996	-0.022	-1.9	982	410
	1997	-0.014	-1.1	957	402
	1998	-0.011	-1.0	904	398
	1999	-0.011	-1.1	889	388
Separations rate					
	1991	0.004	0.3	1112	472
	1992	-0.018	-1.7	1071	441
	1993	0.001	0.1	1047	436
	1994	-0.024	-1.5	1057	454
	1995	-0.028	-2.0	1048	444
	1996	-0.025	-1.6	982	410
	1997	0.009	0.6	957	402
	1998	-0.026	-1.8	904	398
	1999	-0.022	-1.3	889	388

Notes: Source: Author's calculations based on *Quadros de Pessoal*. *ATT* refers to the average treatment on the treated in terms of the outcome variables considered and at the year under analysis. *t(ATT)* denotes t-ratios based on analytical standard errors. The outcome variable is measured by the difference between the value of the variable in the year under analysis and the base year, 1989. See main text for the formal definition of each variable. *Treatment* and *Control* indicates the number of firms in the treatment and control groups respectively (firms with 10 to 19 employees in 1989 are in the treatment group; firms with 21 to 30 employees in 1989 are in the control group). 'Net job creation rate (2)' considers the 1991 level as driven by the difference in employment between 1989 and 1991, while in 'Net job creation rate' the 1991 level is constructed from the difference in employment between 1990 and 1991.

Table 9: **Robustness - Effects on wages and firm performance - Artificial threshold (firm-level analysis, 20-40 employees in 1989);** Matching method: kernel

Variable	*Year*	*ATT*	*t(ATT)*	*Treated*	*Control*
Wages					
	1991	-0.008	-0.7	1212	494
	1992	-0.005	-0.4	1198	489
	1993	-0.027	-1.9	1192	482
	1994	-0.048	-3.1	1213	499
	1995	-0.021	-1.5	1151	479
	1996	-0.013	-0.8	1090	449
	1997	-0.019	-1.2	1077	450
	1998	-0.018	-1.1	1019	434
	1999	-0.026	-1.5	999	422
Total sales					
	1991	0.015	0.4	1006	418
	1992	0.008	0.2	998	417
	1993	-0.090	-2.1	999	417
	1994	0.014	0.3	968	407
	1995	0.028	0.6	897	379
	1996	0.025	0.5	859	368
	1997	0.039	0.6	827	362
	1998	0.014	0.2	807	345
	1999	-0.031	-0.5	806	352
Sales per worker					
	1991	0.006	0.2	1006	418
	1992	-0.012	-0.3	998	417
	1993	-0.087	-2.2	999	417
	1994	-0.016	-0.4	968	407
	1995	-0.018	-0.4	897	379
	1996	-0.021	-0.5	859	368
	1997	0.002	0.0	827	362
	1998	-0.043	-0.8	807	345
	1999	-0.077	-1.6	806	352
Surplus per worker					
	1991	0.040	1.0	976	412
	1992	-0.020	-0.5	974	406
	1993	-0.113	-2.5	952	398
	1994	-0.059	-1.2	929	391
	1995	-0.054	-1.1	869	364
	1996	-0.031	-0.5	827	358
	1997	-0.016	-0.3	794	346
	1998	-0.018	-0.3	777	337
	1999	-0.151	-2.6	783	342

Notes: Source: Author's calculations based on *Quadros de Pessoal*. ATT refers to the average treatment on the treated in terms of the outcome variables considered and at the year under analysis. *t(ATT)* denotes t-ratios based on analytical standard errors. The outcome variable is measured by the difference between the value of the variable in the year under analysis and the base year, 1989. See main text for the formal definition of each variable. *Treatment* and *Control* indicates the number of firms in the treatment and control groups respectively (firms with 10 to 19 employees in 1989 are in the treatment group; firms with 21 to 30 employees in 1989 are in the control group).

Table A.1: **Descriptive statistics, firm characteristics in 1989**

Variable	Treatment group Mean	St.Dev.	Obs	Control group Mean	St.Dev.	Obs
Firm size	13.86	2.62	5863	25.25	2.65	1617
Sales per worker	89.67	470.99	4815	92.39	252.64	1354
Foreign firm	0.01	0.1	5863	0.01	0.11	1617
Year firm started	1970	20.72	4587	1966.12	27.96	1338
Net job creation rate	-0.05	2.07	5599	-0.04	2.46	1566
Hiring rate	0.18	0.16	5599	0.16	0.13	1566
Separation rate	0.19	0.16	5599	0.16	0.12	1566
Job reallocation rate	0.11	0.1	5599	0.07	0.06	1566
Worker reallocation rate	0.37	0.29	5599	0.33	0.23	1566
Churning rate	0.26	0.27	5599	0.25	0.23	1566
Schooling	5.62	1.86	5805	5.65	1.8	1595
Experience	23.59	7.19	5787	24.12	6.52	1594
Tenure	7.47	4.62	5821	8.45	4.54	1613
Female	0.35	0.31	5863	0.36	0.31	1617
Job level	5.72	0.73	5763	5.64	0.64	1602
Hourly pay	2.72	1.45	5732	2.96	1.53	1596

Notes: Source: Author's calculations based on *Quadros de Pessoal*. *Treatment* and *Control* refers to firms in the treatment and control groups respectively (firms with 10 to 19 employees in 1989 are in the treatment group; firms with 21 to 30 employees in 1989 are in the control group). See main text for the formal definition of each variable.

Table A.2: **Number of firms and workers, 1986-1999**

Year	Firms Treated	Firms Control	Firms Total	Workers Treated	Workers Control	Workers Total
1986	5,349	1,543	6,892	69,491	37,077	106,568
1987	5,863	1,617	7,480	79,199	40,202	119,401
1988	5,863	1,617	7,480	80,829	40,732	121,561
1989	5,863	1,617	7,480	81,238	40,824	122,062
1990	5,430	1,522	6,952			
1991	5,136	1,463	6,599	72,848	37,257	110,105
1992	5,006	1,422	6,428	71,053	36,315	107,368
1993	4,768	1,380	6,148	66,678	34,074	100,752
1994	4,475	1,301	5,776	61,429	31,057	92,486
1995	4,286	1,244	5,530	59,008	29,434	88,442
1996	4,084	1,174	5,258	55,675	27,826	83,501
1997	3,999	1,163	5,162	55,343	27,324	82,667
1998	3,825	1,110	4,935	53,716	26,342	80,058
1999	3,779	1,087	4,866	53,726	25,591	79,317
Total	67,726	19,260	86,986	860,233	434,055	1,294,288

Notes: Source: Author's calculations based on *Quadros de Pessoal*. *Treatment* and *Control* refers to firms in the treatment and control groups respectively (firms with 10 to 19 employees in 1989 are in the treatment group; firms with 21 to 30 employees in 1989 are in the control group).