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CGR Working Paper 24

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JEL Classification: J53, J63, J31

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September 23, 2008

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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.[1]

In this paper, we examine 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 draw on particularly detailed matched employer-employee panel data, we are able

[1] In many countries, firms need to go through costly administrative procedures when they dismiss for cause. These procedures include notice periods, notifications and/or approval of third parties, law-mandated retraining or replacement prior to dismissal, culminating in some cases in compulsory worker reinstatements. 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) and Botero et al. (2004) for more examples.

to conduct an exhaustive analysis of the impact of the new law, by considering 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), Bauer et al. (2007), Kugler & Pica (2008), Marinescu (2008)), but also productivity (Besley & Burgess 2004, Autor et al. 2007) and wages (Leonardi & Pica 2007). These variables are particularly interesting, not only from the point of view of policy but also that of theory as, at least in the cases of wages and productivity, the theoretical impacts of firing costs have proved ambiguous. Furthermore, we examine the effects of lower dismissal costs on each variable over a particularly long period of time, in order to shed light on possible differences between the short- and the long-run.

We also complement our initial difference-in-differences results with a propensity score matching analysis. Although we already control for firm time-invariant heterogeneity by drawing on the longitudinal dimension of the data, the propensity score matching approach minimises any remaining bias driven by unobserved differences and by differences in the common support across the two firm types and their workers (Heckman et al. 1998), particularly in the case of the type of data used here (Heckman et al. 1997).

Finally, we pay particular attention to the robustness of our results. First, we consider different samples, either a subset of firms that are likely to have reached a 'stable' size over the years preceding the reform; or a more comprehensive set of firms, assigned into the treatment or control groups depending solely on their size in the last year before the reform was introduced. Second, we consider artificial size thresholds, in which we reestimate all results assuming the law had dictated a different size threshold. This analysis seeks to assess the possibility that our results are picking up phenomena related to firm size that just happen to coincide with the specific law reform studied in the paper. Third, we consider different subsets of the data and different matching methods.

Our results prove to be resilient to these different robustness tests, particularly when considering the difference-in-differences matching approach. While we do not find robust evidence of effects on job or worker flows - although some estimates suggest an increase in hirings -, our results indicate that the performance of firms that gain flexibility in terms of dismissals improves considerably. Moreover, we also find that the wages paid by those firms grow more slowly than the wages of firms that have not obtained greater flexibility in dismissing workers. According to our theoretical discussion, these results are consistent with models involving moral hazard and wage bargaining models.

The structure of the paper is as follows: First, Section 2 presents some of the main features of the legislative reform examined here and discusses its likely impact according to economic theory. We then introduce our data set in Section 3. Section 4 presents the main results and Section 5 describes the robustness analyses. Finally, Section 6 concludes.

2 The law reform and its theoretical impact

2.1 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.[2]

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).[3] This differentiation established an important contrast between the new and old laws, as firm size was irrelevant in the 1975 document.

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

[3]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.

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 cover the entire population of firms and their employees, indicate 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.[4]

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.[5] 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.[6]

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

[4]In order to obtain additional evidence that firms have paid attention to these differences in the law, we contacted representatives of employer organisations of two selected industries: building and retail. These industries were selected as they include large numbers of firms of small size. Both representatives have confirmed that firms in their industries were aware of the new law and its specific element studied in this paper (the 20-employee threshold) at the time the law was introduced.

[5]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 is probably meant to offer additional protection to employees.

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

tightening of temporary contracts (which were restricted to a narrower range of employment 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 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 appendix.

2.2 Theoretical predictions

It is well known that the legal procedures surrounding dismissals for cause can induce deadweight losses, as some of 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 hirings. Therefore, theory does not offer clear predictions about the impact of firing costs upon job flows, as both hirings and separations will be below their optimal levels. However, worker flows (hirings and/or separations) are unambiguously expected to fall with dismissal costs.

In the specific case of the law reform studied here, in which the costs of dismissals for cause fall, some workers that exhibit poor levels of performance but that have been protected 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 new matches will no longer be as costly 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 law, so that separations do not necessarily increase. This possibility could also weaken the expected increase in hirings, to the extent that some of those hirings are carried out to replace dismissed workers.

Moreover, the threshold introduced by the law may attenuate the expected increases in hirings by smaller firms: if the increase in hirings is large enough, then dismissal rules would change considerably at the 20-employee threshold - not only for the marginal workers but also for all the infra-marginal ones. On the other hand, firms above the threshold may be willing to pay the higher firing costs for a few marginal workers in order to benefit from lower firing costs for all remaining workers when those firms move below the threshold. Consequently, there is also scope for heterogeneity in terms of costs of adjustment within each side of the size threshold: firms 'considerably' below the threshold would be more likely to expand than firm 'just' below the threshold, while firms 'just' above the threshold would be more likely to dismiss that firms employing 'considerably' more than 20 employees.[7]

We present here preliminary evidence about some of these 'threshold effects' by examining the transition of firms across two main size categories before and after the law reform. Figure 1 describes the percentage of firms of size 10-19 in 1987, 1989 and 1991 that either stay in the same size category or move to the 21-30 category in 1989, 1991 and 1993, respectively.[8] The figure also presents the percentage of firms of size 21-30 in 1987, 1989 and 1991 that either stay in the same size category or move to the 10-19 category after a period of two years. The

[7] Moreover, larger firms will also have incentives to split into smaller firms if economies of scale are not important; or to resort to outsourcing. See also Schivardi & Torrini (2008).

[8] The figure draws on data for the entire population of firms (with at least one employee) in Portugal in each year. The specific details of the longitudinal data set used are presented in the next section.

evidence in the figure supports our prediction above about fewer transitions from below to above the threshold and more transitions in the opposite direction, as the percentage of 10-19 firms moving to the 21-30 category declines over time, while the percentage of 21-30 firms that move to the 10-19 category increases over time. Moreover, we have also found that these results also hold when examining size transitions over periods longer than two years.

We conclude from this preliminary analysis that at least some of these threshold effects may indeed be important. However, we defer to Section 4.1 for a more detailed analysis of the effects of the reform upon employment-related variables, considering also issues of differences in firm characteristics and statistical significance. Moreover, we also present some evidence about heterogeneity in these threshold effects in Section 5.3, when we examine a wide range of firm sizes. At this stage, we can already establish that the variety of theoretical aspects at play - involving varying worker effort and 'threshold effects' on top of the more standard considerations regarding hirings and separations - makes it difficult to predict the net effect of the new dismissals law upon job and worker flows. The variety of theoretical aspects also makes the empirical analysis of their effects particularly valuable.

With respect to the theoretical effect of the new law in terms of wages, there are two different theoretical models one can appeal to. On the one hand, in a bargaining framework (Lindbeck & Snower 2001, 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, or at least grow more slowly, potentially by more the greater the surplus in the employer-employee relationship.

On the other hand, as in the case of the Lazear (1990) framework, 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 instead the entire workforce of each firm. This is relevant as firm-specific skills gained by workers with some tenure will introduce a wedge between their pay and their outside option in the external market whose size is determined by bargaining. Moreover, while Lazear (1990) assumes that firing costs are given, the specific dimension of EPL we study here involves firing costs that are difficult to estimate 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.

Finally, when considering the effects of firing costs upon firm performance, we find again two opposing theoretical views. 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 to a long-term employment relationship (for instance if firm-level agreements are difficult to enforce and therefore lack credibility), then workers will generally find it less advantageous to invest in firm-specific skills, which may lead to a deterioration of 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 wage premiums, then, to the extent that effort is observable by employers, 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 because uncooperative or disruptive workers are fired.[9]

3 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

[9] Moreover, while one can argue that employers can always 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. One can argue that the stringency 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.

relatively long, as the census has been ongoing since 1982.[10]

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 $age - education - 6$.

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

[10]However, only employer-level data is available for the year of 1990. Overall, on average, between 1982 and 2004, there are approximately 2.5 million workers and more than 200,000 firms per year.

workers employed by the firm in the period $t-1$ but nor in period t.[11] 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 $WR_t - JR_t$.

As to the sample used in the paper, our benchmark results are based on 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. 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 employees (see Section 2.1). 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 belong 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).[12]

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 imply that some firms in the small (large) size category 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 tend 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,

[11] 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).

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

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'.[13] 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 (2.72 euros per hour for smaller firms and 2.96 for larger firms, again in the 1989 data) - see Table 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 2.[14]

For the benefit of robustness, we also present evidence of the transitions between size categories for the benchmark sample used in the paper, following our analysis in Section 2.2. Similarly to the case of Figure 1, Figure 2 indicates that the percentage of firms of size 10-19 (21-30) in 1989 and 1991 that either stay in the same size category or move to the 21-30 (10-19) category after a period of two years. The evidence there is again consistent with our theoretical predictions, as 10-19 firms are less likely to move into the 21-30 category while 21-30 firms are more likely to move to the 10-19 category. (By construction, there is no mobility across size categories in 1987.)

4 Results

In this section, we present our results regarding the impact of the lower firing costs in terms of job and worker flows, wages and firm performance. Our estimations are based on difference-in-

[13] 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 ininterrumptly from 1986 to 1993, includes only 4% of all firms.

[14] 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).

differences regressions on longitudinal data, following the discussion on identification presented in the Appendix (Section A). In particular, we estimate the following equation:

$$\Delta Y_{it} = \lambda D_i + \theta' Z_i + \varepsilon_{it}, \tag{1}$$

in which Y is the dependent variable of interest (job and worker flows, wages or firm performance), $\Delta Y_{it} = Y_{it} - Y_{i,89}$ ($t = 1991, 92, ..., 99$), D_i is a dummy equal to 1 if the firm employs less than 20 workers in 1989 (and 0 otherwise) and Z_i is a vector of control variables. These control variables, all measured in 1989, 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 the squares and the cubes of each one of these variables for the year of 1989 and the first and second lags of all the linear terms of the 1989 variables, firm type dummies (based on differences in their legal structure; 5 categories), sector dummies (28) and region dummies (also 28).

The tables and figures presented below report the estimates and the standard errors of λ (robust standard errors, allowing for clustering at the firm level). The samples used for each estimation are either a single year in the 'after' period (1991-1999) or the pooled analysis of periods 1991-1995, 1996-1999 or 1991-1999.

4.1 Job and worker flows

The main version of the net job creation variable used measures net job creation in each year from 1991 to 1999.[15] We present our results in Table 3 and Figure 3 (dotted line).

We find evidence that small firms, those subject to a greater decrease in their firing costs, exhibit a moderate increase in net job creation, particularly in the second part of the 1990s (coefficient of 0.011, t-ratio of 1.89). However, when considering the individual results on a

[15] For robustness, we also consider a measure of net job creation that considers 1990 data. The results, available upon request, are robust to the inclusion of that year. In any case, from 1992 onwards, the two versions of net job creation coincide. On the other hand, hirings and separations rates ignore 1990: since worker-level data is not available 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.

year-by-year basis, the significant estimate for the 1996-99 period seems to be driven almost entirely by the 1997 result (coefficient of 0.025, t-ratio of 2.6).

Moreover, when decomposing the job creation effects into its two worker flow components - hirings and separations - we find that most of the effect comes from the former. In fact, the results indicate significant increases in hirings, of between 0.008 and 0.01. (As the average hiring rate in treated firms is 0.18 (see Table 1), this increase corresponds to approximately 5%.)

Overall, our conclusion from the results displayed in Table 3 is that there is evidence of a slight increase in job creation in smaller firms brought about by a moderate increase in hirings. Although these results are consistent with our theoretical predictions, there are some issues that need to be taken into account when interpreting the findings. One is that our data are measured annually, a frequency that may not be sufficiently high to fully capture the worker-flow adjustments carried out by firms (Blanchard & Portugal 2001). Moreover, there is no information on employment spells that start after March 1989 and end before March 1991 (i.e. after the last snapshot in the 'before' period and before the first snapshot in the 'after' period).

Another caveat is that changes in the levels of employment are affected 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 workers that before the new law would have left voluntarily change their behaviour as they realise they can be more easily fired at the prospective new job. Moreover, as in Bauer et al. (2007), 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.[16] However, as we control carefully for the tenure level of each firms' workforce, we believe this potential problem is not serious in our case.

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 issues described above (e.g. the frequency of the data) are not relevant.

[16]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 at least partly cancelled out by such greater sensitivity of larger firms with respect to EPL.

4.2 Wages

Table 4 and Figure 3 (straight line) presents our results, in which we take as the outcome variable the logarithm of the firm-level real average hourly wage of all employees in each firm and in each year. We find significant differences between the treated and the control groups, as wage growth rates in smaller firms are systematically and significantly lower than in larger firms. For instance, in the pooled results referring to 1991-99, the difference in wage growth is -0.028 (t-ratio of 5.1). Similar results are found when breaking up the entire 1990s in two halves or individual years.

However, although wages fall significantly, that would not necessarily imply differences in the reward policy of firms, following differences in their relative bargaining power with respect to employees, as discussed in Section 2.2. This would also be the case if, for instance, worker composition changed. We assessed this alternative explanation by contrasting several different worker characteristics (schooling, gender, job level, tenure, hours worked, etc), in terms of both their means and standard deviations, 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 (results available upon request). This result strengthens a bargaining interpretation of the evidence, in which employers gain bargaining power when they are faced with less burdensome firing procedures and are 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 reform upon firm performance.[17]

4.3 Firm performance

In order to shed light on the contrasting theoretical views about the role of EPL on firm performance discussed in Section 2.2, we examine different measures of firm performance that can be constructed from our data (see the two bottom blocks of estimates in Table 4 and

[17] In current work (Martins 2008) we are extending the analysis of the impact on wages by considering possible differences across workers of different types, namely between those that are more or less likely to benefit from bargaining power (high- vs low-tenure workers, high- vs low-experience workers, men vs women), in the two types of firms. Our preliminary results reinforce the view that bargaining power differences are driving the changes in wages observed here as the wages of workers that tend to have stronger bargaining power are also those that grow more slowly as a consequence of the new law.

Figure 3 - dashed line). In our first measure, we consider the logarithm of total sales (2004 prices) per worker as the outcome variable. We find that the pooled result for the entire 1991-99 period is 0.05 (t-ratio of 2.4), indicating significantly higher performance growth in treated firms. Moreover, when decomposing this average effect over the decade, we find evidence of an increasing trend, suggesting that the performance effects are cumulative.

We also consider 'surplus per worker' as an additional measure of performance. This is defined as the difference between total sales and the wage bill of each firm. The wage bill is computed by summing all individual monthly 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 that surplus by the number of workers. The results (see the bottom of Table 4) are again consistent with the previous findings, indicating that treated firms underwent a positive relative increase in their performance. Furthermore, the effects for this variable appear stronger than in the case of sales per worker. This is not surprising as the difference between the two variables - wages - is found to be negatively affected by the reform (Section 4.2). Specifically, the overall effect over the 1990s is now of 0.063 (t-ratio of 2.67). As in the case of sales per worker, the effects appear to be cumulative over time.[18]

However, performance could be increasing in smaller firms because they were investing more in capital or other inputs. We therefore search for evidence of capital deepening, by considering 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 framework as for the results described above, we found no significant differences between treated and control group firms (results available upon request). Moreover, as mentioned in Section 4.2, we have also not found any observable differences in worker composition between treated and control group firms which, of course, could also explain differences in performance. Finally, it is worthwhile to take into account that, if medium and large firms are the ones that tend to be most (negatively) affected by EPL (Pierre & Scarpetta 2006), then our estimates of the impact of the reduction in dismissal costs upon firm performance are lower bounds of the 'average' effect of such costs across the entire firm size distribution.

[18]We also consider a third measure of firm performance - sales (not divided by the size of the workforce). As the results are qualitatively the same as for the two main variables, they are not presented here.

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 increased worker effort or to better personnel management (our two theoretical explanations). Even the fact that all measures of performance indicate that the effects increase over time, typically from about 5pp in the first half of the 1990s to about 6pp in the second half, 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 (as wages grow slower in a context of relative performance gains) and we do not find any significant effects in terms of separations, it is difficult to believe that such shift in bargaining power had no impact upon workers' effort.

5 Robustness

5.1 Difference-in-differences matching

As discussed before, complementing a difference-in-differences approach with matching may be particularly insightful. We therefore start out robustness analysis with this method, matching firms in terms of the very long set of variables considered in our results from Section 4. Specifically, the estimates presented here are based on Epanechnikov kernel matching, using a bandwidth of 0.06, and the Leuven & Sianesi (2004) software. As mentioned before (and in the Appendix), each estimate is obtained from the difference between the value of the dependent variable in a year in the 'after' period and the value of the same variable for the same treatment group firm in 1989. We then subtract from that difference the average difference for the matched control group firms.

Table 5 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 some of their lags and squares, but not the values for the cubic terms or for most dummy variables (full results available upon request). For almost all variables, we observe a considerable reduction in the absolute value of bias after matching. Moreover, again in almost all cases, one cannot reject the tests of equality of their average values between the treated and the control groups, but only after matching. Furthermore, we have also found from the pseudo-R^2's of probit estimations of the propensity score on all the variables before and after matching (Sianesi 2004) that the matched sample is considerably more homogeneous after the matching, as the pseudo-R^2 falls from 0.068 to 0.017.

On the other hand, we find that the imposition of the common support is not too restrictive, as only 6 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 reasonably similar, although we find the expected greater density of control observations at lower levels of the propensity score when compared to treatment observations. Overall, our view is that there is strong evidence that the matching is of particularly good standards. However, the reduction of bias achieved when balancing covariates suggests there is scope for differences in our findings in this section with respect to the results from the difference-in-differences analysis without matching.

Table 6 presents annual results concerning the effects of the law reform upon net job creation, hirings and separations rates. Unlike in the case when we did not match, here we find no significant differences between the treatment and control groups in any of the three dependent variables examined. This is particularly true in the case of hirings, which drove most of the significant findings in the analysis without matching (Table 5). In the present case, the t-ratios of the hirings variable never exceed 1.3 and in some years are even negative.

However, when considering wages and firm performance - see Table 7 -, we find that the significantly negative or positive results for those two variables found before still hold when matching. In the case of wages, their growth rates vary between 1.7pp and 3.9pp. In the case of firm performance, sales per worker increase by between about 3pp and 11pp. Both ranges of effects are very similar with respect to the results without matching and the same applies to the precision of the estimates.

5.2 Artificial threshold

Our second type of robustness analysis involves 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 in Sections 4 and 5.1 may be driven by intrinsic differences between firms of different sizes and not related to any real impact of the law. For instance, although most research focuses on the strong positive relationship between firm size and wage levels (see Oi & Idson (1999) for a survey), it is possible that wage growth is also generally 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 of different sizes. Perhaps similar explanations could also be argued for the case of firm performance.

We implement our falsification exercise by creating a new data set, including now 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. As before, we consider estimates from both difference-in-differences and difference-in-differences matching.

In the difference-in-differences results based on this new dataset - Tables 8 and 9 -, we find that there are no significant differences in the net job creation rate when considering either different groups of years or only individual years. However, the results for hirings now indicate negative estimates, although generally insignificant. Moreover, we find evidence of a relative decline in the separations rate, although only in the second part of the 1990s.

With the possible exception of the case of hirings, these results support our previous result from Section 5.1 that there are no significant differences induced by the law regarding job and worker flows. When examining the effects in terms of wages (see Table 9), we still find significant negative differences between 'small' and 'large' firms. However, the decomposition by years indicates that the difference is significant only in four or five years whereas in our benchmark results all yearly estimates are negative. Moreover, we do not find any significant differences in terms of our measures of firm performance. On top of that, most firm performance coefficients are now negative in both measures considered. This latter result about firm performance contrasts dramatically with the findings based on the threshold established in the law (20 employees), from which we documented significant increases in firm performance.

In order to shed additional light on this question, and given the previous evidence of differences in the distributions of covariates in the unmatched sample (Sections 4 and 5.1), we conduct a similar falsification analysis but now complemented with matching. In other words, we again assume the false 30-employee threshold but now obtain our estimates from a difference-in-differences matching analysis. We report the results for job and worker flows in Table 10 and the results for wages and firm performance in Table 11. Here we find that almost none of the estimates for job or worker flows is significant at conventional levels. (The single exception, hirings in 1996, turns out to be a negative estimate.)[19] Moreover, in the

[19] We also find, as before, evidence of considerable differences in the distributions of covariates in the unmatched sample. In this case, the pseudo-R^2's of probit estimations of the propensity score on all the variables

case of wages, only three out of nine estimates have t-ratios above 1.8. Finally, in the case of firm performance, all estimates are either insignificant or, if significant, negative, again in both measures considered.[20]

The comparison of Figure 4, where we present the main results of this analysis based on artificial threshold, over the entire decade, with Figure 3, which is based on a difference-in-differences analysis considering the threshold indicated in the law, is striking. In Figure 4, not only are the confidence intervals much wider, as the point estimates for each one of the three variables depicted tends to vary from positive to negative values.

Overall, we interpret the results from this analysis based on 'artificial' thresholds as evidence that our main results are not picking up effects that emanate from differences in firm size and that happen to coincide with the thresholds defined by the new law. In other words, we regard these 'falsification' results as important evidence in favour of the causal interpretation of our main findings.

5.3 Wider range of firms

As argued in Section 3, we are concerned that a 'regression to the mean' effect could bias any results based on assigning firms into the treatment or control groups solely in terms of firm size in a single year (the last year of the 'before' period, for instance). We therefore adopted an assignment procedure in our benchmark approach that considers firms' size over the last three years of the 'before' period and requires such size to be in the same category (10-19 or 21-30) over that period. In this subsection, we consider the robustness of our findings to a looser assignment procedure, based only on firm size in 1989.

Tables 12 and 13 present the results (see also Figure 5). We find that the number of firms under analysis more than doubles in the case of the control group and increases by more than 50% in the case of the treatment group. Moreover, we also obtain evidence of the 'regression to the mean' process mentioned above as, unlike in the case of the benchmark sample, we now obtain evidence of significantly higher net job creation rates within the treated group. Four out of the nine point estimates are significant at conventional levels, ranging between

before and after matching (Sianesi 2004) falls from 0.049 (p-value of 0.001) to 0.003 (p-value of 1). This result is further evidence of the usefulness of combining a difference-in-differences approach with matching.

[20] Such significantly negative results in the case of firm performance may actually be considered as evidence in favour of our main findings, as they may indicate that, although the performance of larger firms tends to grow faster than that of smaller firms, that pattern is reversed in the specific case of firms that benefit from lower firing costs.

about 2 and 3pp (top section of Table 12). When decomposing this effect in terms of hirings and separations, we find that each component is important, as the former increase while the latter fall, although fewer estimates are significant.

Moreover, we find, as before, that wages grow more slowly in firms below the 20-employee threshold, even when not requiring greater stability in terms of firm size over the 'before' period. Similarly, all point estimates regarding the effects of the new law upon firm performance remain positive, although in this case several estimates are not significant at conventional levels. As mentioned above, we explain the drop in significance of these estimates to the looser assignment of firms to either the control or treatment groups. In any case, we regard these results as supporting our main findings and also the option to obtain the benchmark results from a tighter sample of firms.

We conduct an additional robustness analysis related to the issue of size requirements for assignment to the treatment or control groups. As the treatment group is assumed to have the same *absolute* size as the control group (21 to 30 workers or 10 to 19 workers), we may be ruling out by definition high *percentage* changes in the variables of interest (in particular, net job creation). Such restriction may then have affected our main results.

Moreover, there is also an additional related point, discussed earlier in the paper, concerning the heterogeneity of the effects of the new law depending on the type of firm and its distance, in terms of firm size, to the threshold set by the new law. For instance, large firms 'considerably' away from the threshold may be less likely to engage in worker dismissals in order to benefit from the lower firing costs once they eventually cross the size threshold. To that extent, if one were to widen the range of the control group, then the results concerning job and worker flows effects may turn out to be stronger than for the current firm-size range. On the other hand, widening either the control or treatment groups may lead to bias or less precision in the estimates, to the extent that such new groups become more heterogeneous in other aspects that are not controlled for in the analysis.

Bearing in mind the trade-off described above, we investigate the robustness of our results to the consideration of a wider range of control-group firm sizes. Specifically, we follow the same approach based on a difference-in-differences matching analysis of firms that are in the same size category over the 1987-1989 period. However, we now extend the control group to include firms sized 21 to 40 up to 1989. The results, presented in Tables 14 and 15, are again supportive of our main findings, namely in terms of wages and firm performance. However, as predicted above, we find evidence of moderate increases in net job creation, mostly due to increases in hirings. On the other hand, we find strong evidence of slower wage growth and higher performance growth in smaller firms. In the former case, point estimates range between 2pp and 3pp and are always precisely estimated. In the case of firm performance, the estimates are more dispersed and less precise - which is consistent with the greater heterogeneity of the control group - but are still very much consistent with the evidence from our benchmark findings.

5.4 Other tests

In this last subsection, we describe summarily the remaining robustness analysis conducted. These results are not reported but are available by request. In our first approach, we reestimate the benchmark firm-level results (difference-in-differences propensity score matching of 'permanent' firms) 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 1991 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 disappearance from the sample of firms that perform poorly (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 find that the number of firms declines from between about 5000 in 1990 and about 4000 in 1999, when considering all firm-years, to about 1700, when only considering firms present in all years, from 1987 to 1999. This indicates that the sample of firms used in each year in previous analysis can vary reasonably. However, more importantly, our results based only on continuing firms indicate that our benchmark findings are robust in this respect. There are no qualitative differences in terms of job or worker flows when compared to the benchmark results of Tables 6 and 7. 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, the smaller sample size will also play some role in the result. 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 in this subsection, 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 or one nearest match). The results 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, a significant decrease in wages and a significant increase in different proxies of firm performance. Similar results are again obtained if we extend the set of matching variables to include measures of the dispersion of worker-level characteristics within firms (tenure, experience and schooling) or match on median worker-level characteristics rather than mean worker-level variables.

Our third analysis involves 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 explained in Section 2.1, 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 the 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 some years.

Finally, we also replicate our main analysis but trimming the range of firm sizes covered. 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 again the same qualitative results. 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. Moreover, the heterogeneity of threshold effects will also be more important in this smaller sample.

6 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 methods, we examine the impact of this differentiated change in firing costs upon a large range of outcomes, measured over an extended period of time (1991-1999). In our results, while theory predicts increased worker turnover, we do not find robust evidence of significant effects in job or worker flows, although some results do indicate a slight increase in job creation derived from higher hirings. On the other hand, we find that smaller firms exhibit significant and stable slower wage growth (of about 2pp to 3pp). Smaller firms also exhibit significant and largely permanent increases in different measures of firm performance (typically increasing from about 3pp 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 or capital deepening. Importantly, the results are also not driven by firm heterogeneity that coincides with the size threshold defined in the law. The results are also robust to alternative sample definitions, different matching algorithms, and several other robustness analysis.

Overall, our findings suggest that worker effort responds to the severity of EPL constraints of the type examined here. At the same time, the reduction in firing constraints is also likely to transfer bargaining power from employees to their employers, which can explain the slower wage growth found in our results.

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A Appendix - Identification

Let Y_{it}^{D} be the potential outcome of interest for firm i at time t had they been in state D_i, where $D_i = 1$ if exposed to the treatment (a firm employing less than 20 workers in the 'before' period) 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, firm i in both states. Therefore, we cannot compute the 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 (DID) 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 DID 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:

$$E[Y_{it}^{0} - Y_{it'}^{0} \mid D_i = 1] = E[Y_{it}^{0} - Y_{it'}^{0} \mid D_i = 0], \tag{2}$$

where t' is a time period before the program implementation. The assumption states that, over time, the outcome variable of treated individuals ($D_i = 1$), 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 ($D_i = 0$).

If assumption (2) holds, the DID estimate of the average treatment effect on the treated can be obtained by the sample analogs of

$$\widehat{\alpha}_{\text{DID}} = \{E[Y_{it} \mid D_i = 1] - E[Y_{it} \mid D_i = 0]\} - \{E[Y_{it'} \mid D_i = 1] - E[Y_{it'} \mid D_i = 0]\}. \tag{3}$$

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

A 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. In this case, the DID 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:

$$Y_{it} = \lambda D_i + \tau_t + \theta' Z_{it} + \alpha_{D_i} D_i \tau_t + \varepsilon_{it}, \tag{4}$$

where D_i 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, $\widehat{\alpha}_{D_i}$ would correspond to the DID 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 DID approach.

In order to address this possible shortcoming of the standard DID 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 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

$$E[Y_{it}^{0}-Y_{it'}^{0}\mid p,D_i=1]=E[Y_{it}^{0}-Y_{it'}^{0}\mid p,D_i=0], \tag{5}$$

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,

$$E\left[Y_{it}^{0}|X,D_i=1\right]=E\left[Y_{it}^{0}|X,D_i=0\right]=E\left[Y_{it}^{0}|X\right], \tag{6}$$

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

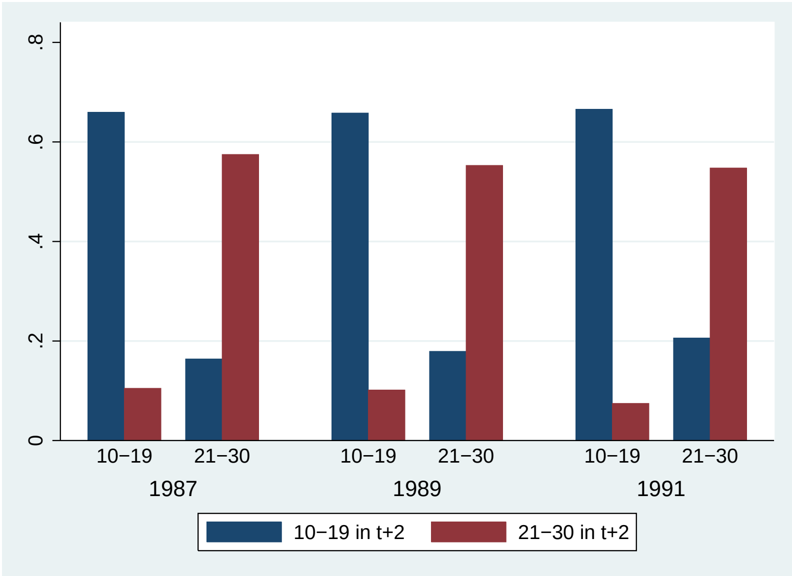
The DDM estimator takes two forms, depending on the nature of the data, namely repeated cross-sections and panel data. In the latter case, the one employed in this paper, the estimator involves first calculating the differences over time in the dependent variable for each observation and then matching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

$$\widehat{\alpha}_{DDM}=E\left[(Y_t^1-Y_{t'}^1)-\widehat{E}\left(Y_t^0-Y_{t'}^0|P\right)\right], \tag{7}$$

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

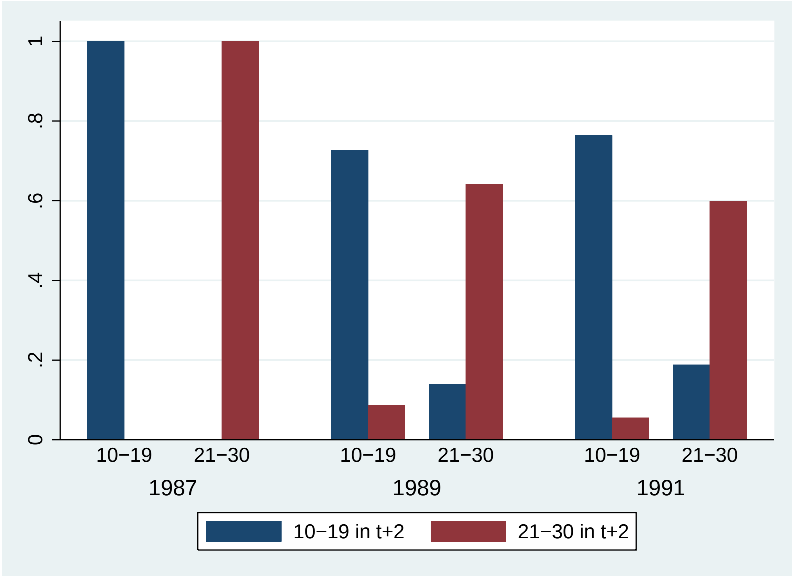
Figures

Figure 1: Firm size categories in year *t+2* given size in year *t*, all firms



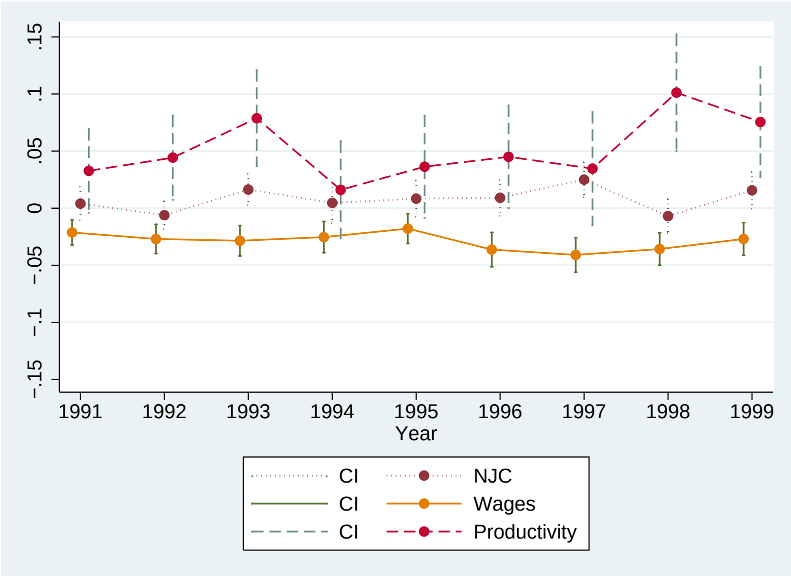
Note: Samples are all firms present in years t and t+2 ($t = 1987, 1989$ *or* 1991). Height of the bars indicate percentage of firms in either 10-19 or 21-30 size categories in $t+2$ that are of the size indicated on the x-axis in year t. Bars do not sum to one because other firm sizes besides 10-19 and 21-30 are omitted.

Figure 2: Firm size categories in year *t+2* given size in year *t*, sampled firms



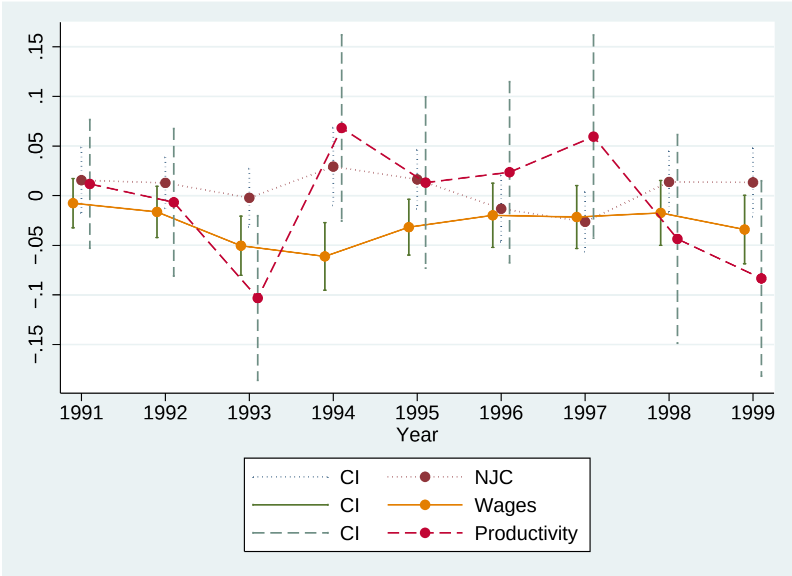
Note: Samples are firms present in the same size category over period 1987-1989. Height of the bars indicate percentage of firms in either 10-19 or 21-30 size categories in $t+2$ that are of the size indicated on the x-axis in year t. Bars do not sum to one because other firm sizes besides 10-19 and 21-30 are omitted.

Figure 3: Effects on Net Job Creation, Wages and Firm Performance (Differences in Differences, 'Permanent' Firms)



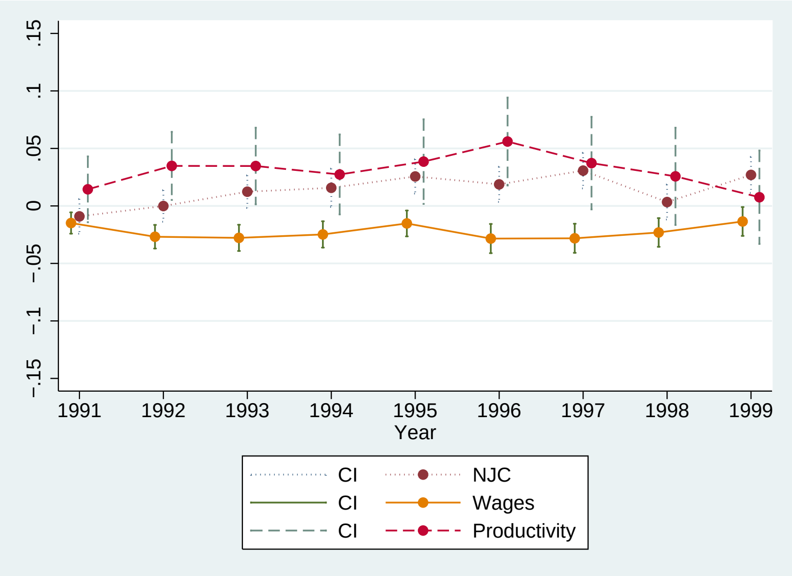
Note: Each point refers to a given variable in a given year and corresponds to an estimate from a separate difference-in-differences regression. Confidence intervals of ±1.6 standard errors around each estimate. Robust standard errors allowing for clustering at the firm level. 'Permanent firms' indicates that the assignment to the control or treatment groups requires that firms remain in the same size category from 1987 to 1989. 'Net job creation' is measured as a rate, 'Wages' correspond to the log of the real hourly wage, and 'Firm performance' correspond to the log of real total sales divided by the number of employees.

Figure 4: Artificial threshold - 'Effects' on Net Job Creation, Wages and Firm Performance (Difference-in-Differences Matching, 'Permanent' Firms)



Note: 'Artificial threshold' means that, unlike indicated by the law reform, firms sized 21-30 are assigned to the treatment group and firms sized 31-40 are assigned to the control group. Each point refers to a given variable in a given year and corresponds to an estimate from a separate difference-in-differences matching estimation. Confidence intervals of ±1.6 standard errors around each estimate. 'Permanent firms' indicates that the assignment to the control or treatment groups requires that firms remain in the same size category from 1987 to 1989. 'Net job creation' is measured as a rate, 'Wages' correspond to the log of the real hourly wage, and 'Firm performance' correspond to the log of real total sales divided by the number of employees.

Figure 5: Effects on Net Job Creation, Wages and Firm Performance (Difference-in-Differences Matching, 'All' Firms)



Note: Each point refers to a given variable in a given year and corresponds to an estimate from a separate difference-in-differences matching estimation. Confidence intervals of ±1.6 standard errors around each estimate. 'All firms' indicates that the assignment to the control or treatment groups is based only on firm size in 1989. 'Net job creation' is measured as a rate, 'Wages' correspond to the log of the real hourly wage, and 'Firm performance' correspond to the log of real total sales divided by the number of employees.

Tables

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

	Treatment group			Control group		
Variable	Mean	Std. Dev.	Obs	Mean	Std. Dev.	Obs
Firm size	13.86	2.62	5863	25.25	2.65	1617
Sales per worker	101.52	533.84	4815	104.75	286.42	1354
Foreign firm	0.01	0.10	5863	0.01	0.11	1617
Year firm started	1970.00	20.72	4587	1966.12	27.96	1338
Net job creation rate	-0.01	-0.15	5599	0.00	0.10	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	0.10	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	1.80	1595
Experience	23.59	7.19	5787	24.12	6.52	1594
Tenure	7.04	4.59	5821	7.99	4.52	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 2: **Number of firms and workers, 1986-1999**

	Firms			Workers		
Year	Treated	Control	Total	Treated	Control	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).

Table 3: **Effects on job and worker flows** (Differences in Differences, 'Permanent' Firms)

Variable	*Year*	*Coeff.*	*t*	*Treated*	*Control*
Net job creation rate					
	1991-99	0.007	1.57	28717	8841
	1991-95	0.004	0.79	13520	4190
	1996-99	0.011	1.89	15197	4651
	1991	0.004	0.44	3502	1091
	1992	-0.006	-0.80	3327	1039
	1993	0.016	1.88	3337	1028
	1994	0.005	0.42	3354	1032
	1995	0.008	0.85	3254	1023
	1996	0.009	0.93	3133	951
	1997	0.025	2.60	3048	929
	1998	-0.007	-0.74	2899	881
	1999	0.016	1.56	2863	867
Hirings rate					
	1991-99	0.009	2.17	28717	8841
	1991-95	0.008	1.96	13520	4190
	1996-99	0.010	1.97	15197	4651
	1991	0.006	1.01	3502	1091
	1992	0.003	0.57	3327	1039
	1993	0.010	1.92	3337	1028
	1994	0.014	2.48	3354	1032
	1995	0.010	1.58	3254	1023
	1996	0.012	1.80	3133	951
	1997	0.015	1.93	3048	929
	1998	-0.000	-0.01	2899	881
	1999	0.011	1.62	2863	867
Separations rate					
	1991-99	0.001	0.26	28717	8841
	1991-95	0.003	0.67	13520	4190
	1996-99	-0.001	-0.13	15197	4651
	1991	0.002	0.24	3502	1091
	1992	0.009	1.35	3327	1039
	1993	-0.007	-0.83	3337	1028
	1994	0.010	0.98	3354	1032
	1995	0.002	0.19	3254	1023
	1996	0.003	0.33	3133	951
	1997	-0.010	-1.01	3048	929
	1998	0.007	0.77	2899	881
	1999	-0.005	-0.51	2863	867

Notes: Source: Author's calculations based on *Quadros de Pessoal*. *Coeff* refers to the coefficient of the treatment dummy in terms of the outcome variables considered and at the year under analysis. t denotes the t-ratio, based on robust standard errors, allowing for clustering by firm. 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. *N. Obs* indicates the number of firms in the treatment and control groups.

Table 4: **Effects on wages and firm performance** (Differences in Differences, 'Permanent' Firms)

Variable	*Year*	*Coeff*	t	*Treated*	*Control*
Wages					
	1991-99	-0.028	-5.11	33037	9901
	1991-95	-0.025	-4.56	15644	4701
	1996-99	-0.031	-4.52	17393	5200
	1991	-0.021	-3.10	3915	1183
	1992	-0.027	-3.37	3940	1170
	1993	-0.029	-3.43	3885	1170
	1994	-0.025	-2.96	3904	1178
	1995	-0.018	-2.18	3734	1124
	1996	-0.036	-3.86	3558	1054
	1997	-0.041	-4.32	3494	1052
	1998	-0.036	-4.04	3322	995
	1999	-0.027	-3.01	3285	975
Sales per worker					
	1991-99	0.050	2.41	26430	7840
	1991-95	0.044	2.18	12853	3821
	1996-99	0.056	2.26	13577	4019
	1991	0.033	1.43	3281	967
	1992	0.044	1.92	3265	964
	1993	0.079	2.99	3221	962
	1994	0.016	0.60	3086	928
	1995	0.036	1.30	2898	854
	1996	0.045	1.60	2783	820
	1997	0.035	1.12	2677	793
	1998	0.101	3.18	2629	774
	1999	0.076	2.51	2590	778
Surplus per worker					
	1991-99	0.063	2.67	25453	7563
	1991-95	0.055	2.38	12356	3689
	1996-99	0.070	2.39	13097	3874
	1991	0.035	1.23	3173	938
	1992	0.066	2.33	3146	941
	1993	0.084	2.72	3077	916
	1994	0.031	0.91	2960	894
	1995	0.055	1.47	2800	829
	1996	0.043	1.17	2671	787
	1997	0.044	1.20	2584	763
	1998	0.117	3.10	2538	741
	1999	0.102	2.71	2504	754

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 denotes the t-ratio, based on robust standard errors, allowing for clustering by firm. The outcome variable is measured by the difference between the value of the variable in the year under analysis and the base year, 1989. *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 5: **Analysis of balancing properties**

		Mean			% reduct	t-test	
Variable	Sample	Treated	Control	% bias	bias	t	$p > [t]$
Schooling	Unmatched	5.607	5.6191	-0.7		-0.21	0.834
	Matched	5.6037	5.61	-0.4	47.6	-0.16	0.872
Experience	Unmatched	23.475	24.152	-10		-3.02	0.003
	Matched	23.479	23.663	-2.7	72.8	-1.19	0.233
Tenure	Unmatched	7.1285	8.1585	-22.7		-7.01	0
	Matched	7.1356	7.2081	-1.6	93	-0.73	0.467
Female	Unmatched	0.33483	0.3384	-1.2		-0.37	0.709
	Matched	0.33491	0.3338	0.4	68.9	0.17	0.864
Hourly Pay	Unmatched	2.7174	2.9382	-15.9		-4.99	0
	Matched	2.714	2.7783	-4.6	70.8	-2.13	0.033
Hours	Unmatched	169.38	168.92	1.9		0.59	0.554
	Matched	169.39	168.93	1.9	-0.3	0.86	0.391
Job Level	Unmatched	5.7263	5.6525	11.1		3.28	0.001
	Matched	5.7284	5.706	3.4	69.7	1.46	0.145
Foreign	Unmatched	0.00964	0.01208	-2.3		-0.75	0.454
	Matched	0.00966	0.00907	0.6	75.8	0.28	0.781
Year Birth	Unmatched	1970.6	1966.3	20		6.9	0
	Matched	1970.6	1970.2	2	89.9	1.19	0.234
Lag Schooling	Unmatched	5.5717	5.5482	1.3		0.39	0.693
	Matched	5.5681	5.5699	-0.1	92.4	-0.04	0.965
Lag Experience	Unmatched	23.268	23.95	-10.2		-3.09	0.002
	Matched	23.271	23.452	-2.7	73.4	-1.2	0.23
Lag Tenure	Unmatched	6.9071	7.9483	-23		-7.09	0
	Matched	6.9135	6.964	-1.1	95.2	-0.51	0.612
Lag Female	Unmatched	0.32984	0.3313	-0.5		-0.15	0.879
	Matched	0.3298	0.32709	0.9	-85.8	0.42	0.677
Lag Hourly Pay	Unmatched	2.6271	2.8769	-17.6		-5.87	0
	Matched	2.624	2.6888	-4.6	74.1	-2.29	0.022
Lag Hours	Unmatched	171.29	170.98	1.3		0.4	0.691
	Matched	171.31	170.58	3	-134.5	1.35	0.176
Lag Job Level	Unmatched	5.7404	5.6592	11.8		3.5	0
	Matched	5.7426	5.7231	2.8	76	1.25	0.211
$Schooling^2$	Unmatched	3.4681	3.4577	0.4		0.13	0.899
	Matched	3.4618	3.4664	-0.2	56.1	-0.08	0.935
$Experience^2$	Unmatched	60.113	62.478	-6.8		-2.06	0.039
	Matched	60.117	60.858	-2.1	68.7	-0.94	0.349
$Tenure^2$	Unmatched	7.1589	8.6803	-16.5		-5.12	0
	Matched	7.168	7.2183	-0.5	96.7	-0.25	0.801

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 6: **Robustness - Effects on job and worker flows** (Differences-in-Differences Matching, 'Permanent' Firms)

Variable	*Year*	*ATT*	*t(ATT)*	*Treated*	*Control*
Net job creation rate					
	1991	-0.005	-0.44	3480	1091
	1992	-0.012	-1.41	3309	1039
	1993	0.016	1.62	3319	1028
	1994	0.004	0.34	3335	1032
	1995	0.009	0.84	3234	1023
	1996	0.009	0.78	3114	951
	1997	0.025	2.34	3030	929
	1998	-0.012	-1.20	2882	881
	1999	0.018	1.58	2846	867
Hirings rate					
	1991	-0.001	-0.19	3480	1091
	1992	-0.001	-0.11	3309	1039
	1993	0.002	0.28	3319	1028
	1994	0.007	1.05	3335	1032
	1995	0.006	0.89	3234	1023
	1996	0.008	1.11	3114	951
	1997	0.005	0.61	3030	929
	1998	-0.002	-0.27	2882	881
	1999	0.010	1.29	2846	867
Separations rate					
	1991	0.003	0.39	3480	1091
	1992	0.011	1.51	3309	1039
	1993	-0.014	-1.56	3319	1028
	1994	0.003	0.26	3335	1032
	1995	-0.003	-0.26	3234	1023
	1996	-0.000	-0.02	3114	951
	1997	-0.020	-1.79	3030	929
	1998	0.010	1.00	2882	881
	1999	-0.007	-0.70	2846	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 the *t*-ratio. The outcome variable is measured by the difference between the value of the variable in the year under analysis and the base year, 1989. *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 7: **Robustness - Effects on wages and firm performance** (Differences-in-Differences Matching, 'Permanent' Firms)

Variable	*Year*	*ATT*	*t(ATT)*	*Treated*	*Control*
Wages					
	1991	-0.018	-2.18	3892	1183
	1992	-0.025	-2.56	3919	1170
	1993	-0.028	-2.75	3864	1170
	1994	-0.019	-1.87	3882	1178
	1995	-0.017	-1.66	3715	1124
	1996	-0.032	-2.75	3539	1054
	1997	-0.039	-3.24	3474	1052
	1998	-0.038	-3.32	3306	995
	1999	-0.018	-1.61	3269	975
Sales per worker					
	1991	0.041	1.59	3274	967
	1992	0.046	1.76	3258	964
	1993	0.074	2.51	3213	962
	1994	0.028	0.92	3079	928
	1995	0.058	1.84	2890	854
	1996	0.049	1.55	2775	820
	1997	0.031	0.89	2672	793
	1998	0.109	2.98	2624	774
	1999	0.073	2.18	2582	778
Surplus per worker					
	1991	0.057	1.79	3167	938
	1992	0.075	2.33	3141	941
	1993	0.075	2.21	3071	916
	1994	0.046	1.18	2957	894
	1995	0.073	1.74	2794	829
	1996	0.054	1.29	2665	787
	1997	0.046	1.09	2580	763
	1998	0.158	3.71	2534	741
	1999	0.110	2.56	2499	754

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 the *t*-ratio. The outcome variable is measured by the difference between the value of the variable in the year under analysis and the base year, 1989. *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** (20-40 employees in 1989, Differences-in-Differences, 'Permanent' Firms)

Variable	*Year*	*Coeff*	t	*Treated*	*Control*
Net job creation rate					
	1991-99	0.010	1.46	9394	3862
	1991-95	0.010	1.24	4444	1811
	1996-99	0.009	1.18	4950	2051
	1991	0.008	0.53	1151	474
	1992	0.023	1.98	1110	443
	1993	-0.004	-0.35	1085	438
	1994	0.017	0.92	1098	456
	1995	0.011	0.77	1086	446
	1996	0.017	1.04	1015	411
	1997	-0.017	-1.14	992	404
	1998	0.019	1.36	938	400
	1999	0.017	1.04	919	390
Hirings rate					
	1991-99	-0.007	-1.34	9394	3862
	1991-95	-0.001	-0.14	4444	1811
	1996-99	-0.013	-1.85	4950	2051
	1991	0.008	0.97	1151	474
	1992	0.003	0.39	1110	443
	1993	-0.009	-1.30	1085	438
	1994	-0.004	-0.53	1098	456
	1995	-0.009	-1.13	1086	446
	1996	-0.021	-2.02	1015	411
	1997	-0.016	-1.35	992	404
	1998	-0.011	-1.07	938	400
	1999	-0.008	-0.92	919	390
Separations rate					
	1991-99	-0.017	-2.51	9394	3862
	1991-95	-0.011	-1.50	4444	1811
	1996-99	-0.023	-2.60	4950	2051
	1991	0.000	0.01	1151	474
	1992	-0.020	-1.97	1110	443
	1993	-0.005	-0.44	1085	438
	1994	-0.021	-1.28	1098	456
	1995	-0.020	-1.50	1086	446
	1996	-0.038	-2.67	1015	411
	1997	0.002	0.10	992	404
	1998	-0.030	-2.13	938	400
	1999	-0.025	-1.54	919	390

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 denotes the t-ratio, based on robust standard errors, allowing for clustering by firm. The outcome variable is measured by the difference between the value of the variable in the year under analysis and the base year, 1989. *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 9: **Robustness - Effects on wages and firm performance - Artificial threshold** (20-40 employees in 1989, Differences-in-Differences, 'Permanent' Firms)

Variable	*Year*	*Coeff*	*t*	*Treated*	*Control*
Wages					
	1991-99	-0.027	-3.43	10501	4216
	1991-95	-0.024	-3.10	4985	1972
	1996-99	-0.027	-2.81	5516	2244
	1991	-0.011	-1.13	1255	496
	1992	-0.012	-1.18	1240	491
	1993	-0.028	-2.29	1235	484
	1994	-0.047	-3.42	1255	501
	1995	-0.026	-2.27	1189	481
	1996	-0.022	-1.59	1125	451
	1997	-0.023	-1.71	1115	452
	1998	-0.030	-2.31	1054	436
	1999	-0.036	-2.75	1033	424
Sales per worker					
	1991-99	-0.040	-1.35	8369	3465
	1991-95	-0.030	-1.05	4072	1659
	1996-99	-0.049	-1.37	4297	1806
	1991	0.007	0.21	1032	418
	1992	-0.004	-0.11	1024	417
	1993	-0.103	-2.65	1024	417
	1994	-0.024	-0.55	992	407
	1995	-0.016	-0.40	917	379
	1996	-0.044	-1.06	879	368
	1997	-0.011	-0.24	849	362
	1998	-0.076	-1.57	827	345
	1999	-0.098	-2.16	825	352
Surplus per worker					
	1991-99	-0.047	-1.39	8076	3348
	1991-95	-0.030	-0.91	3931	1606
	1996-99	-0.063	-1.49	4145	1742
	1991	0.048	1.28	1003	412
	1992	-0.007	-0.15	999	406
	1993	-0.120	-2.75	976	398
	1994	-0.049	-0.95	953	390
	1995	-0.046	-0.88	890	364
	1996	-0.030	-0.54	847	357
	1997	-0.015	-0.27	815	345
	1998	-0.088	-1.50	794	334
	1999	-0.129	-2.40	799	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 denotes the t-ratio, based on robust standard errors, allowing for clustering by firm. The outcome variable is measured by the difference between the value of the variable in the year under analysis and the base year, 1989. *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 10: **Robustness - Effects on job and worker flows - Artificial threshold** (20-40 employees in 1989, Differences-in-Differences Matching, 'Permanent' Firms)

Variable	*Year*	*ATT*	*t(ATT)*	*Treated*	*Control*
Net job creation rate					
	1991	0.016	0.76	1155	477
	1992	0.013	0.80	1103	448
	1993	-0.002	-0.12	1092	441
	1994	0.029	1.20	1106	460
	1995	0.016	0.88	1094	452
	1996	-0.013	-0.63	1027	417
	1997	-0.026	-1.40	990	409
	1998	0.014	0.71	945	403
	1999	0.013	0.62	928	393
Hirings rate					
	1991	0.012	1.01	1155	477
	1992	0.001	0.10	1103	448
	1993	-0.012	-1.13	1092	441
	1994	0.001	0.09	1106	460
	1995	-0.007	-0.61	1094	452
	1996	-0.030	-1.98	1027	417
	1997	-0.019	-1.10	990	409
	1998	-0.007	-0.50	945	403
	1999	-0.006	-0.52	928	393
Separations rate					
	1991	-0.004	-0.25	1155	477
	1992	-0.012	-0.86	1103	448
	1993	-0.010	-0.59	1092	441
	1994	-0.028	-1.32	1106	460
	1995	-0.023	-1.27	1094	452
	1996	-0.016	-0.90	1027	417
	1997	0.007	0.38	990	409
	1998	-0.021	-1.05	945	403
	1999	-0.020	-0.92	928	393

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 the *t*-ratio. The outcome variable is measured by the difference between the value of the variable in the year under analysis and the base year, 1989. *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 11: **Robustness - Effects on wages and firm performance - Artificial threshold** (20-40 employees in 1989, Differences-in-Differences Matching, 'Permanent' Firms)

Variable	*Year*	*ATT*	*t(ATT)*	*Treated*	*Control*
Wages					
	1991	-0.008	-0.49	1259	500
	1992	-0.016	-1.01	1246	496
	1993	-0.050	-2.71	1241	489
	1994	-0.061	-2.88	1262	507
	1995	-0.032	-1.81	1196	487
	1996	-0.020	-0.98	1137	457
	1997	-0.022	-1.09	1120	457
	1998	-0.017	-0.85	1062	439
	1999	-0.034	-1.58	1039	429
Sales per worker					
	1991	0.012	0.29	1034	419
	1992	-0.007	-0.14	1025	419
	1993	-0.103	-1.99	1026	419
	1994	0.068	1.16	994	409
	1995	0.013	0.24	923	381
	1996	0.024	0.41	887	370
	1997	0.059	0.93	855	363
	1998	-0.044	-0.66	828	345
	1999	-0.083	-1.36	823	353
Surplus per worker					
	1991	0.044	0.90	1005	413
	1992	0.009	0.17	1000	408
	1993	-0.147	-2.54	978	400
	1994	-0.002	-0.02	956	392
	1995	0.029	0.44	895	366
	1996	0.088	1.13	855	359
	1997	0.011	0.14	820	346
	1998	-0.066	-0.83	792	334
	1999	-0.106	-1.51	798	343

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 the *t*-ratio. The outcome variable is measured by the difference between the value of the variable in the year under analysis and the base year, 1989. *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 12: **Robustness - Effects on job and worker flows - 'All' firms** (Differences-in-Differences Matching)

Variable	*Year*	*ATT*	*t(ATT)*	*Treated*	*Control*
Net job creation rate					
	1991	-0.009	-0.97	5849	2328
	1992	-0.000	-0.01	5577	2259
	1993	0.012	1.40	5578	2274
	1994	0.016	1.51	5590	2243
	1995	0.026	2.75	5459	2186
	1996	0.019	1.96	5177	2043
	1997	0.031	3.18	5039	1982
	1998	0.003	0.35	4818	1885
	1999	0.027	2.76	4732	1878
Hirings rate					
	1991	-0.005	-0.81	5849	2328
	1992	0.007	1.10	5577	2259
	1993	0.010	1.69	5578	2274
	1994	0.011	1.68	5590	2243
	1995	0.013	1.98	5459	2186
	1996	0.007	1.09	5177	2043
	1997	0.008	1.15	5039	1982
	1998	0.001	0.19	4818	1885
	1999	0.015	2.12	4732	1878
Separations rate					
	1991	0.004	0.66	5849	2328
	1992	0.007	1.08	5577	2259
	1993	-0.002	-0.36	5578	2274
	1994	-0.005	-0.64	5590	2243
	1995	-0.013	-1.78	5459	2186
	1996	-0.011	-1.51	5177	2043
	1997	-0.022	-2.74	5039	1982
	1998	-0.002	-0.27	4818	1885
	1999	-0.012	-1.54	4732	1878

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 the t-ratio. The outcome variable is measured by the difference between the value of the variable in the year under analysis and the base year, 1989. *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 13: **Robustness - Effects on wages and firm performance - 'All' firms** (Differences-in-Differences Matching)

Variable	*Year*	*ATT*	*t(ATT)*	*Treated*	*Control*
Wages					
	1991	-0.015	-2.57	6749	2606
	1992	-0.027	-4.17	6789	2621
	1993	-0.028	-3.90	6673	2593
	1994	-0.025	-3.46	6709	2606
	1995	-0.015	-2.17	6423	2480
	1996	-0.028	-3.58	6074	2333
	1997	-0.028	-3.56	5959	2293
	1998	-0.023	-2.96	5673	2195
	1999	-0.014	-1.72	5581	2174
Sales per worker					
	1991	0.014	0.81	5662	2181
	1992	0.035	1.88	5615	2181
	1993	0.035	1.67	5564	2160
	1994	0.027	1.26	5328	2067
	1995	0.039	1.68	4989	1914
	1996	0.056	2.34	4779	1829
	1997	0.037	1.48	4618	1772
	1998	0.026	0.97	4522	1752
	1999	0.008	0.30	4454	1749
Surplus per worker					
	1991	0.030	1.47	5457	2115
	1992	0.042	1.93	5406	2109
	1993	0.025	1.04	5295	2056
	1994	0.024	0.90	5106	1979
	1995	0.035	1.27	4808	1846
	1996	0.049	1.74	4581	1753
	1997	0.023	0.81	4455	1701
	1998	0.031	1.04	4360	1682
	1999	0.006	0.21	4305	1684

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 the *t*-ratio. The outcome variable is measured by the difference between the value of the variable in the year under analysis and the base year, 1989. *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 14: **Robustness - Effects on job and worker flows - 'Permanent' firms (10-40)** (Differences-in-Differences Matching, 'Permanent' Firms)

Variable	*Year*	*ATT*	*t(ATT)*	*Treated*	*Control*
Net job creation rate					
	1991	0.006	0.66	3491	2292
	1992	0.003	0.41	3316	2198
	1993	0.009	1.16	3325	2186
	1994	0.021	2.11	3344	2197
	1995	0.016	2.00	3243	2156
	1996	0.014	1.63	3122	2006
	1997	0.022	2.57	3037	1970
	1998	0.004	0.45	2889	1879
	1999	0.017	1.87	2854	1851
	1991	0.008	1.48	3491	2292
Hirings rate					
	1992	0.012	2.30	3316	2198
	1993	0.011	2.10	3325	2186
	1994	0.015	2.67	3344	2197
	1995	0.009	1.57	3243	2156
	1996	0.005	0.82	3122	2006
	1997	0.013	1.84	3037	1970
	1998	0.006	0.94	2889	1879
	1999	0.011	1.68	2854	1851
	1991	0.002	0.28	3491	2292
Separations rate					
	1992	0.009	1.44	3316	2198
	1993	0.002	0.23	3325	2186
	1994	-0.006	-0.74	3344	2197
	1995	-0.007	-0.98	3243	2156
	1996	-0.009	-1.17	3122	2006
	1997	-0.009	-1.05	3037	1970
	1998	0.002	0.27	2889	1879
	1999	-0.007	-0.77	2854	1851

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 the *t*-ratio. The outcome variable is measured by the difference between the value of the variable in the year under analysis and the base year, 1989. *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 15: **Robustness - Effects on wages and firm performance - 'Permanent' firms (10-40)** (Differences-in-Differences Matching, 'Permanent' Firms)

Variable	*Year*	*ATT*	*t(ATT)*	*Treated*	*Control*
Wages					
	1991	-0.019	-2.98	3903	2487
	1992	-0.030	-4.24	3929	2480
	1993	-0.035	-4.45	3874	2470
	1994	-0.033	-4.02	3893	2491
	1995	-0.025	-3.23	3724	2388
	1996	-0.036	-3.99	3530	2235
	1997	-0.037	-4.15	3483	2228
	1998	-0.030	-3.44	3312	2125
	1999	-0.025	-2.80	3276	2077
Sales per worker					
	1991	0.033	1.77	3278	2078
	1992	0.030	1.50	3262	2063
	1993	0.037	1.68	3218	2061
	1994	0.000	0.02	3083	1986
	1995	0.026	1.11	2895	1847
	1996	0.033	1.32	2780	1779
	1997	0.022	0.84	2675	1711
	1998	0.052	1.90	2627	1666
	1999	0.027	1.01	2588	1668
Surplus per worker					
	1991	0.056	2.44	3170	2019
	1992	0.049	2.07	3143	2000
	1993	0.032	1.24	3074	1960
	1994	0.014	0.50	2957	1902
	1995	0.030	1.00	2797	1777
	1996	0.023	0.73	2668	1703
	1997	0.020	0.63	2582	1635
	1998	0.080	2.45	2536	1600
	1999	0.036	1.12	2502	1608

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 the *t*-ratio. The outcome variable is measured by the difference between the value of the variable in the year under analysis and the base year, 1989. *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).