



Effects of employment protection on worker and job flows: Evidence from the 1990 Italian reform[☆]

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Abstract

This paper uses the Italian Social Security employer–employee panel to study the effects of the Italian reform of 1990 on worker and job flows. We exploit the fact that this reform increased unjust dismissal costs for businesses below 15 employees, while leaving dismissal costs unchanged for bigger businesses, to set up a natural experiment research design. We find that the increase in dismissal costs decreased accessions and separations for workers in small relative to large firms, especially in sectors with higher employment volatility, with a negligible impact on net employment. We also find some evidence suggesting that the reform reduced firms' entry rates and employment adjustments, but had no effect on exit rates.

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1. Introduction

Theory makes clear-cut predictions on the effects of employment protection on labor reallocation in the medium– and long-run (see, e.g., Bertola, 1990; Hopenhayn and Rogerson, 1993). While a number of cross-country studies examine the impact of dismissal costs on aggregate and sectoral employment adjustments, few empirical studies use individual and firm

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level data to examine the impact of dismissal costs on worker and job flows. Moreover, the few studies which rely on microdata focus on the intensive margin of adjustment, and often ignore adjustments on the extensive margins through entry and exit.

In this paper, we use an Italian employer–employee panel to examine the effects of dismissal costs on worker and job flows. Italy is an interesting country to study these effects because it is one of the strictest countries in terms of employment protection legislation. More importantly, in 1990 Italy introduced a labour market reform which increased employment protection for workers employed under permanent contracts in businesses with fewer than 15 employees relative to those in businesses with more than 15 employees. We exploit the differential increase in the costs of unfair dismissals in small relative to large businesses after 1990 to study the impact of dismissal costs on labour flows. We use a differences–in–differences approach by comparing worker and job flows in small and large firms before and after the reform.

Our empirical analysis uses administrative data from the Italian Social Security Institute (INPS). Our data set is an employer–employee panel which includes information on: dates of hiring and separation of the worker; dates of incorporation and termination of the firm, and the annual average number of employees. We use these data to examine worker flows (i.e., accessions and separations), and job flows on the intensive (i.e., employment changes) and extensive (i.e., entry and exit) margins. We find that accessions and separations decreased by about 13% and 15% in small relative to large firms after the reform. The results are robust to the inclusion of time, sector and region effects, sector– and region-specific time effects, and size-specific cyclical effects. Moreover, we find evidence of a bigger effect of the reform in sectors where the variance of employment growth was larger prior to the reform, and thus where the increase in dismissal costs was likely to be binding. On the other hand, a simulation exercise based on a steady-state condition shows that the reform hardly affected net employment.

We then examine the impact on job flows and find evidence suggesting that the reform had some effect on the intensive and extensive margins of adjustment. In particular, we find some evidence that the reform decreased employment adjustments in small relative to large firms. We also find that small firms were less likely to enter the market compared to large firms after the reform. This is consistent with a rise of dismissal costs lowering the present value of the future stream of profits and leading to less entry. On the other hand, we find no differential effects on exit rates of small relative to large firms after the reform.

A number of studies have exploited the variation in employment protection legislation across firms of different sizes within Italy, but ours is the first study to fully exploit both the temporal and cross sectional variation in employment protection legislation with regards to the 15 employee threshold. [Boeri and Jimeno \(2005\)](#) assess the effect of employment protection on lay-off probabilities by comparing small and large firms. [Borgarello, Garibaldi and Pacelli \(2004\)](#), and [Schivardi and Torrini \(2004\)](#) evaluate the effects of employment protection on the size distribution of Italian firms, by looking at the probability of firm size adjustments around the 15 employee threshold.¹ These papers identify the effect of employment protection by exploiting the fact that Italian establishments with fewer than 15 employees are subject to lower dismissal costs than firms with more than 15 employees. This identification strategy assumes that small and large establishments are the same along any dimensions not observed by the econometrician (like, for example, different costs of capital due to the differential impact of borrowing constraints on firms of different sizes). By contrast, our differences–in–differences approach exploits the variation in

¹ [Borgarello, Garibaldi and Pacelli \(2004\)](#) also present some results exploiting the 1990 temporal variation in employment protection, but their sole focus is on the effect of employment protection on firm size.

employment protection between small and large firms as well as the temporal variation in the legislation. Our baseline approach thus allows small and large firms to differ in terms of time-invariant unobservables and, instead, makes the weaker assumption that small and large firms are affected similarly by macro-economic shocks.² Moreover, we also rely on a differences-in-differences framework which allows for small and large firms to be affected differently by the business cycle. This approach makes the weaker assumption that the effect of the business cycle on small firms is the same before and after the reforms, although different from the effect of the business cycle on large firms.

The rest of the paper is organized as follows. Section 2 describes the evolution of dismissal costs in Italy. Section 3 explains the identification strategy used to evaluate the impact of dismissal costs in Italy. Section 4 describes the Social Security data and presents estimates of the effects of the rise in dismissal costs on worker and job flows in Italy.

2. Employment protection regulations in Italy

Italy, together with the other Southern European countries, is considered one of the strictest countries in terms of employment protection legislation (EPL). For example, a study by [Lazear \(1990\)](#) for the period 1956–84 and a study by [Bertola \(1990\)](#) for the late 1980s rank Italy as the strictest country in terms of EPL. A study by the OECD's Employment Outlook for the late 1980s, ranks Portugal as the strictest country followed by Italy, Spain, and Greece.

A similar study by the OECD's Employment Outlook for the late 1990s, which includes Turkey, North America, and transition economies as well, continues to rank Portugal as the strictest, followed by Turkey, Greece, Italy and Spain. Similarly, the study by [Nicoletti et al. \(2000\)](#), which does not include some of the countries in the OECD's Employment Outlook study, ranks Italy third, after Portugal and the Netherlands, in terms of the strictness of regulations on permanent contracts.

Individual dismissals were first regulated in Italy in 1966 through Law No. 604, which established that employers could freely dismiss workers either for economic reasons (considered as fair objective motives) or in case of misconduct (considered as fair subjective motives).³ However, in these cases workers could take employers to court and judges would determine if the dismissals were indeed fair or unfair.⁴ In cases when judges ruled these dismissals as unfair, employers had the choice of either reinstating the worker or paying a dismissal cost which depended on tenure and firm size. Payments for unfair dismissals ranged between 5 and 8 months per year of service for workers with fewer than two and a half years of tenure; between 5 and 12 months per year of service for those between two and a half and 20 years of tenure; and between 5 and 14 months per year of service for workers with more than 20 years of tenure in establishments with more than 60 employees. Establishments with

² Other studies exploiting within-country variation across groups and over time to examine the effect of dismissal costs on labor reallocation include: [Kugler \(1999\)](#), [Hunt \(2000\)](#), [Oyer and Schaeffer \(2000\)](#), [Acemoglu and Angrist \(2001\)](#), [Kugler, Jimeno and Hernanz \(2003\)](#), [Autor \(2003\)](#), [Autor, Donohue and Schwab \(2002\)](#), [Kugler and Saint-Paul \(2004\)](#), [Kugler and Pica \(2006\)](#), [Friesen \(2005\)](#), and [Autor, Kerr and Kugler \(2006\)](#).

³ According to [Ichino, Polo and Rettore \(2003\)](#), economic dismissals are very rare. In case of fair dismissals (either due to economic reasons or due to misconduct), firms have to pay severance of 2/27 of annual salary per year of service (known as *Trattamento Fine Rapporto*), which is paid out of workers' retained earnings ([OECD, 1999](#)).

⁴ For the period 1990–2001, 56% of the dismissal cases taken to court were ruled as unfair. This number is very stable over this period (personal communication with [Maia Guell](#) who estimated this using ISTAT data).

fewer than 60 employees had to pay half the amount paid by those with more than 60 employees, and establishments with fewer than 35 workers were completely exempt.⁵

In 1970, the *Statuto dei Lavoratori* (Law No. 300) decreed that all establishments with more than 15 employees had to reinstate workers and pay their foregone wages in case of un-fair dismissals. Establishments with fewer than 15 employees, however, remained completely exempt.⁶ A number of recent studies provide evidence on the effect of this law for firms at the 15 employee threshold. For example, the 2002 annual report by the Italian Statistical Office, *ISTAT* (2002), shows a larger fraction of firms moving to a smaller size category for firms around the 15 employee threshold than for firms of any other size. Similarly, [Borgarello, Garibaldi and Pacelli \(2004\)](#) and [Schivardi and Torrini \(2004\)](#) find a (slightly) higher probability of inaction and a higher probability of reducing than increasing firm size for firms at the 15 employee threshold.

Given the high costs of unfair dismissals for larger businesses, in 1987 the Italian government liberalized the use of temporary contracts in an attempt to provide more flexibility to employers. Prior to 1987, temporary contracts could be used for specific projects, seasonal work, or for replacement of permanent workers temporarily on leave (e.g., on leave due to maternity or injury). After 1987, temporary contracts could be used more widely subject to collective agreements but only for certain target groups. While the extended use of temporary contracts allowed for more flexibility in the labour market, these contracts could only be renewed up to two times and could only have a maximum length of 15 months. Consequently, even though temporary contracts were liberalized after this reform, the use of temporary contracts remained heavily regulated in Italy compared to other countries.⁷

Soon after the 1987 reform, Law No. 108 was introduced in 1990 further restricting dismissals of permanent workers. In particular, this law introduced costs for unfair dismissals of between 2.5 and 6 months pay per year of service for unfair dismissals in establishments with fewer than 15 employees.⁸ By contrast, unfair dismissal costs in establishments with more than 15 employees remained unchanged.⁹ This meant that, after 1990, the cost of unfair dismissals for establishments with fewer than 15 employees increased. However, the firing cost borne by establishments with more than 15 employees remained larger, as they were still obliged to reinstate unfairly dismissed workers. Thus, the reform narrowed the gap between employment security provisions guaranteed in establishments above and below 15 employees. This is the reform we are studying in our paper.

In 1991, the Italian government introduced a new procedure concerning collective dismissals for economic reasons, i.e. for those cases where individual dismissals are allowed for fair

⁵ Discriminatory dismissals based on race, gender, or political opinions are forbidden independently of establishments size.

⁶ [Boeri and Jimeno \(2005\)](#) present a theoretical explanation of why these exemptions may be in place to begin with. They argue that exempting small firms reduces the disemployment effect of dismissal costs because, in their model, smaller firms subject to dismissal costs have to pay much higher efficiency wages to discourage shirking than larger firms.

⁷ Note that, according to the [OECD's Employment Outlook \(1999\)](#), Italy ranked first in terms of strictness of the regulation of temporary contracts during the 1980s and 1990s.

⁸ In general, firing costs consist of a transfer from the employer to the employee and administrative deadweight costs. Since [Lazear's \(1990\)](#) seminal work, it is well known that the transfer part of EPL can be undone through lower wages when these are flexible. The EPL reform we are focusing on, however, is unlikely to have been completely undone by offsetting wage agreements, as it increased not only the transfer part of EPL but also the administrative costs associated with EPL (by adding the costs of going to court). For evidence on the wage effects of EPL see [Leonardi and Pica \(2006\)](#).

⁹ The law applies to establishments, but in the empirical analysis we focus on firms because our data only includes information on firms. However, our sample consists of firms with fewer than 35 employees, which are also likely to represent establishments given that these are most likely to be single-unit as opposed to multi-unit firms.

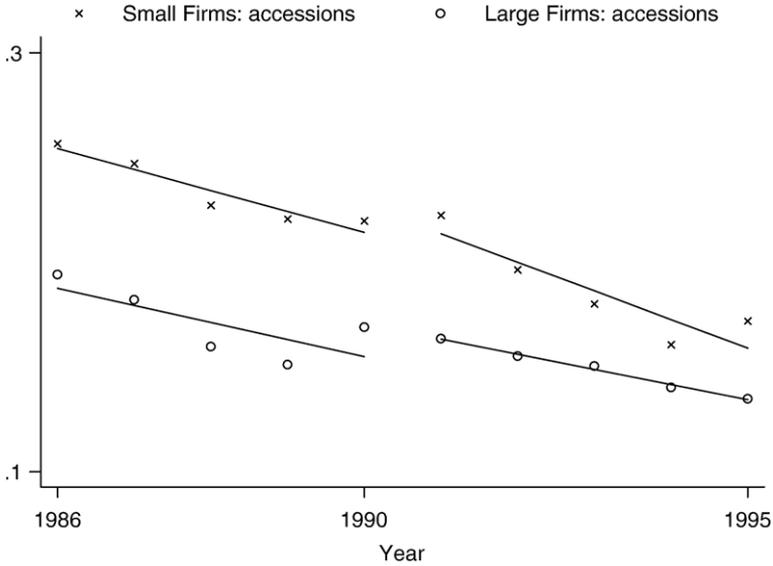


Fig. 1. Yearly accession probabilities by firm size (below/above 15 employees).

objective motives. According to this procedure, firms needing to dismiss more than five employees in establishments with more than 15 employees within 120 days are required to engage in negotiations with unions and government to reach an agreement on the dismissals. However, if public administration officials determine that an agreement cannot be reached, the firm is free to

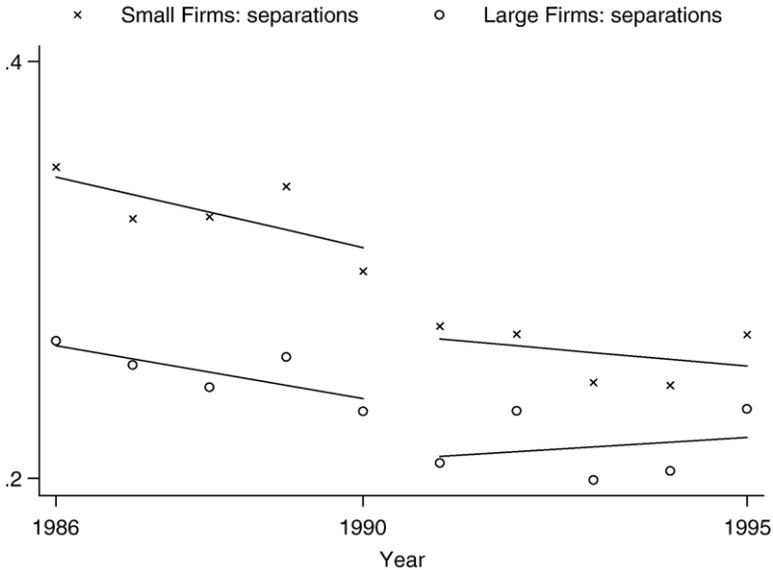


Fig. 2. Yearly separation probabilities conditional on firm size by (below/above 15 employees).

downsize and the employees are not allowed to take the firm to court.¹⁰ Even though it is plausible that this reform affected dismissal costs, it is not clear in which direction. On the one hand, it reduced the uncertainty associated with judicial decisions. On the other hand, the firm's decision is still subject to a process of evaluation by public authorities. Below, we do a triple differences analysis, before and after 1991 for firms laying off more and less than five employees in firms with more and less than 15 employees in order to distinguish the 1990 and the 1991 reforms. While we discuss this in more detail below, we found no additional significant difference between firms with more than 15 employees laying off more and less than five employees after 1990.

In addition, in 1992 two other legislative changes were introduced. The first was a pension reform which changed retirement ages and reference periods for calculating pensions.¹¹ The second was a legislative initiative which eliminated a wage indexation mechanism (*Scala Mobile*) that had been in place since 1945 for firms of all sizes. Since these reforms do not apply differentially to firms of different sizes, our differences-in-differences strategy should control for differences in outcomes between the 1980s and 1990s due to the pension and wage indexation reforms.

Finally, in 1997, Italy moved again in the direction of trying to provide firms with a margin of flexibility by legalizing the use of temporary help agencies. While the 1990 reform increased the costs of unfair dismissals for permanent contracts in businesses with fewer than 15 employees relative to businesses with more than 15 employees, the 1987 and 1997 reforms introduced flexibility at the margin by deregulating the use of temporary contracts and temporary layoffs for businesses of all sizes. Since our data is for the period of 1986 to 1995, in this paper we exploit the temporal change in dismissal costs generated by the 1990 reform for permanent workers, which applied differently for small and large firms.¹²

3. Identification strategy

The goal of this paper is to identify the impact of dismissal costs on worker and job flows. In order to do so, we compare the change in flows for firms with fewer and more than 15 employees before and after the 1990 reform. The strategy to identify the impact of the change in dismissal costs is illustrated in Figs. 1–4. Figs. 1 and 2 show accession and separation probabilities in firms with fewer and more than 15 employees for the period 1986 to 1995. These figures show higher accessions and separations in small than in large firms throughout the period of analysis, but a closing of the worker flow gap between small and large firms after the 1990 reform. Similarly, Figs. 3 and 4 show higher entry and exit rates for small than large firms, but a closing of the gap in firms flows between small and large firms after the reform.

¹⁰ See *Schivardi and Torrini (2004)* for a description of this reform.

¹¹ In particular, the pension reform introduced the following changes. First, it raised the normal retirement age from 55 to 60 for women and from 60 to 65 for men employed in the private sector. Second, the reference period for calculating pensions was extended (over a ten years transition) from 5 to 10 years. Third, for younger workers (less than 15 years of contributions in 1992) the reference period was extended to the whole working life. Fourth, past earnings were to be converted to real terms at a rate equal to the growth in the cost of living index plus one percentage point per year. Finally, the minimum number of years of contributions for eligibility to an old-age pension was raised (over a ten years transition) from 15 to 20.

¹² Given that our focus in the empirical analysis is on permanent workers, we are unlikely to capture the effect of the 1987 reform. In addition, both the 1987 and the 1997 reforms applied to firms of all sizes, so that our differences-in-differences strategy should control for any difference in outcomes generated by the 1987 reform. Moreover, we also tried limiting the sample to the period from 1987 to 1995 to eliminate any possible effect of the liberalization of temporary contracts in 1987 and we found similar results.

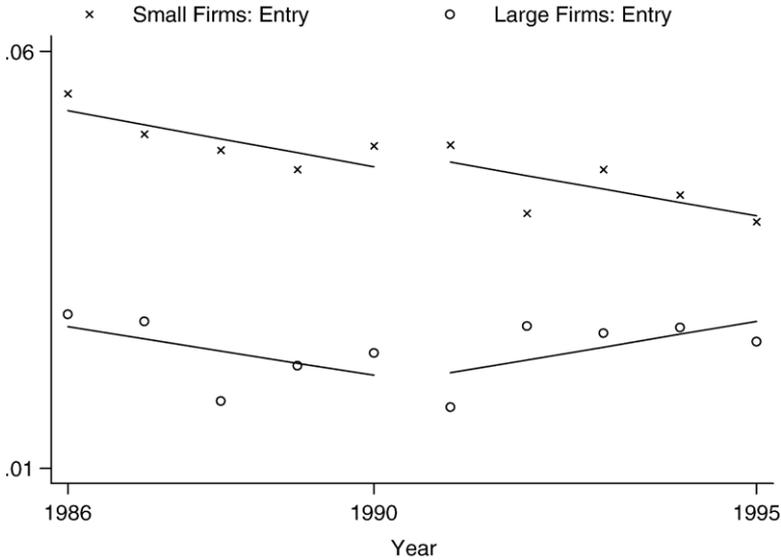


Fig. 3. Yearly entry probabilities by firm size (below/above 15 employees).

3.1. Worker flows: accessions and separations

3.1.1. Baseline specification

To increase precision and to control for the possibility that reduced accessions and separations are the result of changing characteristics of workers and firms of different sizes, we estimate the following linear probability model:

$$E[m_{ijt} = 1 | X_{ijt}, D_j^S, Post_t] = \tau_t + \gamma_k + \phi_r + \beta'X_{ijt} + \delta_1 D_j^S + \delta_2 (D_j^S \times Post_t) \quad (1)$$

where the dependent variable m_{ijt} is a dummy variable that takes the value of 1 if a match is created or destroyed, i.e., if there is either an accession or a separation, between worker i and firm j at time t .¹³ τ_t , γ_k and ϕ_r are time, sector and region effects, respectively. The matrix X_{ijt} includes worker and firm characteristics, including age, occupation, gender, average firm size and, the starting date of the job when estimating the probability of a separation. $Post_t$ is a dummy that takes the value of 1 after 1990 and zero otherwise.¹⁴ D_j^S is a dummy that takes the value of 1 if the worker is employed in a small firm (i.e., a firm with less than 15 employees) and 0 if the worker is employed in a large firm (i.e., a firm with more than 15 employees).¹⁵ The interaction term

¹³ Note that we can estimate separation rates but not hiring rates because not all workers at risk of being hired are included in our sample. However, this is true both before and after 1990, so this will only bias our estimation if the number of potential applicants to small and large firms changes before and after the reforms. Since applicants are likely to apply to jobs in firms of all sizes, it is unlikely that incomplete data should affect our measure of accessions in small and large firms differently before and after the reform.

¹⁴ Since the reform was introduced in May 1990 and public discussion started at the beginning of that year, we leave out the year 1990 in our analysis to avoid capturing the effects of anticipation of the reform.

¹⁵ We define firms as small if they have less than 15 employees in all years and large if they have more than 15 employees in all years. We also defined small/large firms as having less/more than 15 employees during the pre-reform period. The results we report below are robust to this alternative definition of firm size.

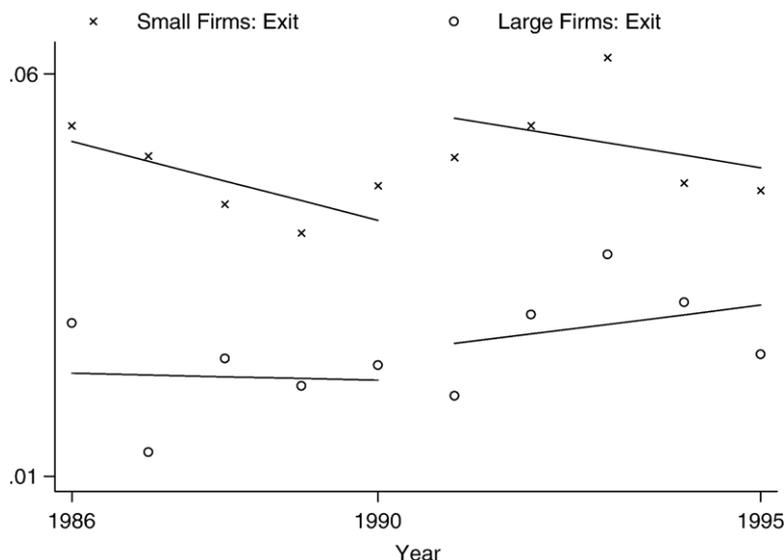


Fig. 4. Yearly exit probabilities by firm size (below/above 15 employees).

between the small firm dummy and the post-reform dummy captures the effect of interest. We also estimate models which allow for sector- and region-specific time effects in order to control for the possibility that differences in flows in certain sectors or regions at different times may be driving the differences in flows in small firms after 1990.

We choose to estimate linear probability models because all covariates in Eq. (1), with the exception of firm size and age, are dummy variables, so the model is almost fully saturated. It is well-known that in the extreme case of a fully saturated model, i.e. one where all independent variables are discrete variables for mutually exhaustive categories, the linear probability model is completely general and the fitted probabilities lie within the interval $[0, 1]$.¹⁶ In addition to being fairly general in our context, the linear probability model also has the advantage of allowing a straightforward interpretation of the regression coefficients. Moreover, since the error term of the linear model is heteroskedastic, we compute the White (1980) heteroskedasticity-robust variance matrix. Also, to take into account the occurrence of repeated observations of individuals, we allow standard errors to be clustered at the individual level. Below, however, we also present results of a Probit model and our results are robust to this alternative specification.

3.1.2. Additional specifications

Our differences-in-differences strategy controls for macro shocks coinciding with the reform but assumes that these shocks have similar effects on small and large firms. It is possible, however, that the business cycle affects small and large firms differently. For example, if small firms are affected more by downturns, then we should have observed both reduced accessions and increased separations in small relative to large firms during the post-reform period due to the

¹⁶ In this case, the fitted probabilities are simply the average of the dependent variable within each cell defined by the different values of the covariates (see Angrist, 2001; Wooldridge, 2001). Note that in the estimations reported below, the number of cases in which the predicted probabilities is smaller than zero and larger than one lies between 150 and 250, or less than 0.4% of all cases.

strong recession of 1992 and 1993. Instead, Figs. 1 and 2 show both reduced accessions and separations for small firms. Nonetheless, to make sure we control for differential cyclical effects on firms of different sizes, we also estimate the following alternative specification:

$$E[m_{ijt} = 1 | X_{ijt}, D_j^S, Post_t, G_t] = \tau_t + \gamma_k + \phi_r + \beta'X_{ijt} + \phi_0 G_t + \phi_1 (D_j^S \times G_t) + \delta_1 D_j^S + \delta_2 (D_j^S \times Post_t) \quad (2)$$

where G_t is the growth rate of GDP. The size-specific cyclical effect is captured by the interaction between the small firm dummy D_j^S and G_t .

In addition, we estimate the following specification to allow for the possibility that the introduction of dismissal costs may have had stronger effects in more volatile sectors, where these costs are likely to bind:

$$E[m_{ijt} = 1 | X_{ijt}, D_j^S, Post_t, V_k^S] = \tau_t + \gamma_k + \phi_r + \beta'X_{ijt} + \delta_1 D_j^S + \delta_2 (D_j^S \times Post_t) + \gamma_0 V_k^S + \gamma_1 (V_k^S \times D_j^S) + \gamma_2 (V_k^S \times Post_t) + \gamma_3 (V_k^S \times D_j^S \times Post_t) \quad (3)$$

where V_k^S denotes the sectoral variance of employment growth of small and large firms during the pre-reform period.¹⁷ The coefficient γ_3 captures the differential effect of dismissal costs on sectors with different degrees of volatility.

3.2. Firms' employment changes, entry and exit

We also estimate the following model to examine whether the annual change in permanent employment differed in small relative to large firms after the reform:

$$E[|\Delta L_{jt}| | Z_{jt}, S_j, D_j^S, Post_t] = \pi_t + \kappa_k + \theta_r + \alpha'Z_{jt} + \rho_1 D_j^S + \rho_2 (D_j^S \times Post_t) \quad (4)$$

where π_t , κ_k , and θ_r are time, sector and region effects. Like in the analysis of worker flows, we also try specifications with sector- and region-specific time effects. Z_{jt} is a vector including firm characteristics. $Post_t$ and D_j^S are, as before, the post-reform dummy and the small firm dummy. We run the above regression for the sample of all firms adjusting their workforce.

While the above regression captures the effect of dismissal costs on the intensive margin of adjustment, we are also interested on the effects on the extensive margins. On the one hand, one may expect the 1990 rise in dismissal costs to reduce entry and increase exit, as larger firing costs lower the present value of the future stream of profits, making production activity less attractive both for potential entrants and for incumbents. On the other hand, insofar as larger firing costs make it relatively less expensive to adjust employment through firm closings and re-openings, one may expect both larger entry and exit (Blanchard and Portugal, 2001).

In order to assess empirically the impact of dismissal costs on the extensive margins of adjustments, we estimate the effects of the 1990 reform on firms' entry and exit rates. We use the longitudinal information on firms to estimate the following linear probability model:

$$E[e_{jt} = 1 | W_{jt}, D_j^S, Post_t] = \xi_t + \mu_k + \psi_r + \lambda'W_{jt} + \rho_1 D_j^S + \rho_2 (D_j^S \times Post_t) \quad (5)$$

¹⁷ Note that this variance is estimated on the full sample of firms with more and less than 15 employees in each period.

where the dependent variable e_{jt} is a dummy variable that takes the value of 1 if firm j enters or exits the market at time t . ξ_t , μ_k , and ψ_r are time, sector, and region effects, respectively, and W_{jt} includes the average firm size. We also try specifications with sector and region-specific time effects. $Post_t$ and D_j^S are as described above. As in the analysis for accessions and separations, we extend the specifications above to control for size-specific cyclical effects, as in Eq. (2). Below, we also report results from probit models for the entry and exit probabilities and find that our results are robust to this specification.

4. Effects of dismissal costs

4.1. Data description

The data set is drawn from the Italian Social Security Administration (INPS) archives for the years 1986–1995. The original data is a 1/90 random sample from the stock of employed workers with social security records drawn every year, with workers born on the 10th of March, June, September, and December of every year being sampled. The original archives only include information on private sector firms in the manufacturing and service sectors, so that it excludes all workers in the public sector and agriculture. We use a 10% random sample from this original data set.

The data set is a panel of workers, which includes individual longitudinal records generated using social security numbers. Since the INPS collects information on private sector employees for the purpose of computing retirement benefits, employees are only followed through their employment spells. This means that individuals who move into self-employment, the public sector, the agricultural sector, the underground economy, unemployment, or retirement will exit our sample.

The data set also includes longitudinal records of the firms employing the randomly selected workers in the sample using the firms' name, address, and social security and fiscal codes. While

Table 1
Workers descriptive statistics by firm size, before and after the reform

Variables	Pre-reform	Post-reform	Pre-reform	Post-reform
	Small firms		Large firms	
Age	34.35 (11.096)	35.784 (10.491)	35.455 (11.09)	36.884 (10.497)
% males	0.629 (0.483)	0.653 (0.476)	0.675 (0.469)	0.673 (0.469)
% white collar workers	0.333 (0.471)	0.315 (0.465)	0.311 (0.463)	0.305 (0.46)
Yearly average size of the firm	6.154 (3.857)	6.233 (3.867)	25.252 (5.639)	25.257 (5.669)
Accession rate	0.239 (0.427)	0.193 (0.395)	0.188 (0.391)	0.176 (0.381)
Separation rate	0.354 (0.478)	0.264 (0.441)	0.289 (0.453)	0.241 (0.428)
<i>N</i>	14848	16938	5055	6626

Notes: Only permanent workers and firms below 35 workers are included. The pre-reform period goes from 1986 to 1989, the post-reform period from 1991 to 1995. Standard deviations in parentheses.

Table 2
Firms descriptive statistics by firm size, before and after the reform

Variables	Pre-reform	Post-reform	Pre-reform	Post-reform
	Small firms		Large firms	
Entry rate	0.05 (0.217)	0.045 (0.207)	0.029 (0.167)	0.031 (0.174)
Exit rate	0.05 (0.217)	0.052 (0.223)	0.027 (0.161)	0.033 (0.179)
Yearly average size of the firm	5.88 (3.837)	5.938 (3.815)	25.335 (5.632)	25.298 (5.651)
<i>N</i>	17625	20288	5067	6793
Absolute value of the firm-level employment growth rate	0.359 (0.428)	0.328 (0.37)	0.134 (0.133)	0.126 (0.127)
<i>N</i>	6037	7514	2741	4044

Notes: Only firms below 35 workers are included. The pre-reform period goes from 1986 to 1989, the post-reform period from 1991 to 1995. The absolute value of the firm-level employment growth rate is computed for all firms adjusting their workforce. Standard deviations in parentheses.

the data set includes a random sample of workers, the probability that a firm is selected increases with size. When using the panel of firms, i.e. when estimating Eqs. (4) and (5), we account for this by weighting each observation by the probability that it appears in the sample, which is given by the average number of employees divided by 90.

The data set is, thus, an employer–employee unbalanced panel with information on the characteristics of workers and firms. In particular, the data includes information on employees' age, gender, occupation, dates of accession and separation with each firm, and type of contract, and information on firms' location, sector of employment, number of employees, and firms' dates of incorporation and termination. A key advantage of this administrative data for the purpose of studying the effects of dismissal costs on worker transitions is that, contrary to most survey data which measures transitions by matching quarterly data and using tenure information to identify job changes, in our data we can identify the exact dates of accessions and separations according to when social security contributions began and ended. Moreover, the exact dates of incorporation and termination of the employing firm are recorded in the INPS archives, so that we do not have to proxy entry into and exit out of the market with entry into and exit out of the sample as we would have to do if we were using survey data.

Table 1 presents descriptive statistics separately by firm size, before and after the 1990 reform. Workers' accessions and separations decrease after 1990 in both small and large firms, but more in small firms. These simple double differences of raw means suggest that the Italian 1990 reform appears to have reduced accessions and separations. Notice that, as workers are followed for some time, their average age grows over time. However, ageing in large firms is not statistically different from ageing in small firms.

Table 2 presents descriptive statistics on firms' characteristics. The table shows that entry rates fell in small firms after the reform, while (slightly) going up in large firms. Analogously, exit rates increased more in large than in small firms after the reform. Finally, Table 2 also suggests a reduction in job flows due to the reform, as the percentage change in employment growth (in absolute value) decreased more in small than in large firms after the reform.

Table 3
Effects of the 1990 reform on accessions and separations

Regressors	Linear Probability Model						Probit	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<i>A. Accessions – N=43423</i>								
Post 1990	0.003 (0.010)	0.013 (0.025)	-0.005 (0.029)	-0.005 (0.029)	-0.024 (0.020)	-0.024 (0.020)	0.008 (0.025)***	-0.026 (0.020)
Small firms	-0.020 (0.012)*	-0.021 (0.012)*	-0.021 (0.012)*	-0.021 (0.017)	-0.033 (0.021)	-0.031 (0.024)	-0.027 (0.018)	-0.019 (0.026)
Post 1990 × Small firms	-0.033 (0.009)***	-0.032 (0.009)***	-0.031 (0.009)***	-0.031 (0.011)***	0.024 (0.019)	0.023 (0.020)	-0.029 (0.012)**	0.010 (0.020)
Post 1990 × Small firms × Pre-reform variance of employment growth by sector and size	-	-	-	-	-0.258 (0.092)**	-0.258 (0.092)***	-	-0.167 (0.083)**
<i>B. Separations – N=43423</i>								
Post 1990	-0.213 (0.012)***	-0.281 (0.027)***	-0.285 (0.031)***	-0.282 (0.032)***	-0.219 (0.022)***	-0.218 (0.023)***	-0.217 (0.029)***	-0.248 (0.024)***
Small firms	-0.012 (0.012)	-0.011 (0.012)	-0.010 (0.012)	-0.004 (0.019)	0.001 (0.022)	0.003 (0.026)	-0.008 (0.02)	0.018 (0.027)
Post 1990 × Small firms	-0.048 (0.010)***	-0.050 (0.010)***	-0.050 (0.010)***	-0.053 (0.013)***	0.013 (0.020)	0.011 (0.021)	-0.052 (0.014)***	-0.008 (0.022)
Post 1990 × Small firms × Pre-reform variance of employment	-	-	-	-	-0.324 (0.095)***	-0.324 (0.095)***	-	-0.207 (0.090)**
Sector and Region Effects	YES	YES	YES	YES	YES	YES	YES	YES
Year Effects	YES	YES	YES	YES	YES	YES	YES	YES
Year × Sector Interactions	NO	YES	YES	YES	NO	NO	YES	NO
Year × Region Interactions	NO	NO	YES	YES	YES	YES	YES	YES
Size-specific Cyclical Effects	NO	NO	NO	YES	NO	YES	YES	YES

Notes: Only permanent workers and firms below 35 workers are included. Pre-reform period 1986-1989. Post-reform period 1991-1995. Huber/White heteroskedasticity-robust standard errors (in parentheses) allow for clustering by individual. All specifications control for sector and region effects, age, occupation and total number of employees in the firm. Specifications in panel B also control for the starting date of the job. Columns (4), (6), (7) and (8) control for size-specific cyclical effects by interacting the small size dummy with GDP growth. Columns (5), (6) and (8) include interactions between the small firm dummy, the post reform dummy and the pre-reform variance of employment growth by sector and size. In columns (7) and (8) the marginal effects from probit estimates are for discrete change from 0 to 1 in case of dummy variables. *** denotes significance at the 1% level, ** denotes significance at the 5% level and * denotes significance at the 10% level.

Though these simple comparisons of means suggest that the increase in dismissal costs as a result of the reform may have affected worker and job flows and firm entry and exit, some of these changes could be due to changes in worker characteristics, sectoral composition or location of small firms. The following sections present results which control for covariates.

4.2. *Effects on accessions and separations*

Table 3 reports marginal effects of linear probability and probit models for accessions and separations estimated using Eqs. (1)–(3). The dependent variable takes the value of 1 if the person joined a firm in a given year and zero otherwise. These linear probability and probit models are estimated using a sample of permanent workers in firms with fewer than 35 employees. The sample is restricted to firms with fewer than 35 employees to increase comparability between the treatment group of firms with less than 15 employees and the control group of firms with more than 15-employees.¹⁸

Panel A in Table 3 reports results for accessions. Columns (1)–(6) report results from a linear probability model. The baseline specification in Column (1) includes controls for worker's age and occupation, firm size, and sector, region, and year effects. Columns (2) and (3) control for sector-specific and region-specific time effects and Column (4) controls for size-specific cyclical effects. The reported heteroskedasticity-robust standard errors allow for clustering at the individual level to account for the presence of repeated observations on the same worker. The results show a large and statistically significant decline in permanent accessions in small relative to large firms after the 1990 reform was introduced. For example, the results in Column (4) show a reduction of -0.031 or 13% in accessions in small firms after the reform.¹⁹

Columns (5) and (6) report results which include the third-level interaction of the post-reform dummy, the small firm dummy and the pre-reform variance of employment growth, as described in Eq. (3).²⁰ The results show that the probability of an accession falls drastically after the reform for workers employed in small firms which produce in highly volatile sectors, where dismissal costs are more likely to bind. For example, accessions fall by about 31% in small relative to large firms in sectors where the variance of employment is three standard deviations above the mean, and by about 21% and 10% in sectors where the variance of employment are two and one standard deviations above the mean. This confirms that workers employed in firms where dismissal costs are likely to bind appear to be most affected by the 1990 reform.

In Columns (7) and (8) we also report results from probit models which include size-specific cyclical effects, without and with the interaction with the variance term. The probit results show similar though somewhat smaller effects compared to those estimated with the linear probability model. For example, the result in Column (7) comparable to the one in Column (4) shows a reduction of about 12% in accessions in small firms after the reform. Similarly, accessions fall by about 20%, 13% and 7% in small relative to large firms in sectors where the variance of employment is three, two, and one standard deviations above the mean, respectively.

Panel B in Table 3 reports marginal effects of linear probability and probit models for separations. The dependent variable is now a variable that takes the value of 1 if the person

¹⁸ Results are robust to changes in the sample, which include firms larger or smaller than 35.

¹⁹ In some specifications (not reported but available upon request) we control for unobserved worker heterogeneity. Overall the results are similar. However, when including worker-fixed effects the estimates are, not surprisingly, less precise as the fixed-effects methodology disregards the variability between workers. When treating worker unobserved heterogeneity as random we obtained more precise estimates. This is because random effects allow exploiting variation both across individuals and over time, although it does require the additional assumption of orthogonality between the individual effects and the covariates. In any case, given that accessions and separations are moving in the same direction, it is unlikely that changes in workers' unobservable characteristics after 1990 are behind the observed patterns. For example, if less-employable individuals look for work in small firms during the 1990s, this could explain the decrease in accessions in small firms after 1990, but it could not explain the decrease in separations.

²⁰ In these specifications time-varying sector characteristics are accounted for by the interaction between the sectoral pre-reform variance of employment growth and the post-reform dummy.

separated from the firm in a given year and zero otherwise. In these specifications, we include all the controls of the linear probability models for accessions and, in addition, we include the starting date of the job to account for the effect of tenure. The results show a decrease in separation probabilities in small relative to large firms after the reform. For example, the basic specification shows a decrease in separation probabilities of 0.048 or about 14%. Controlling for sector- and region-specific time effects yields slightly larger coefficients on the interaction term. Results controlling for size-specific cyclical effects suggest that the effect of the reform is even larger as separation probabilities now fall by 15%.

Results of specifications including the third-level interaction between the post-reform dummy, the small firm dummy and the pre-reform variance of employment growth are reported in Columns (5) and (6). As for accessions, we find a greater decline in separations in highly volatile sectors. For example, separations fall by about 26% in small relative to large firms in sectors where the employment variance is three standard deviations above the mean, and by about 18% and 7% in sectors where the employment variance are two and one standard deviations above the mean.

Columns (7) and (8) report marginal effects of probit models. The results show very similar effects to those found with the linear probability model. As before, Column (7) shows a reduction in separations of about 15%. Similarly, the results in Column (8) show that separations fall by about 17%, 11% and 6% in small relative to large firms in sectors where the variance of employment is three, two, and one standard deviations above the mean, respectively.

Our results show decreased accessions and separations in small relative to large firms after 1990. Moreover, the results suggest that the declines in accessions and separations in small relative to large firms after 1990 were greatest in more volatile sectors, where dismissal costs are more likely to bind. This is reassuring since it suggests that we are capturing the effect of the 1990 reform rather than the effect of some other contemporaneous shock or legislative change, such as the pension reform or the elimination of the *scala mobile*, which should not have affected differently sectors with different volatilities. In addition, we check whether the procedures introduced in 1991 concerning collective dismissals may have been behind the fall in worker flows by estimating triple differences models where the post-1991 and small firm dummies are interacted with a mass-layoff dummy (i.e., a dummy of whether the firm laid off more than 5 workers). Contrary to what one would expect if the 1991 reform was driving the results, we find that the triple interaction terms are not significant neither in the regression for accessions nor in the one for separations, while the interaction term between the post 1990 reform dummy and the small firm dummy remains significant.

Since both accessions and separations fell after the 1990 reform, then employment could have either increased or decreased. In order to simulate the effect of the reform on net employment, we use a steady state condition requiring the flows into and out of employment from small and large firms to be the same. The estimates suggest that the reform hardly affected employment, as they imply an increase in employment of less than a half of a percentage point.

4.3. *Effects on employment changes*

We then analyze whether the increase in dismissal costs affected employment adjustments in the intensive and extensive margins in small relative to large firms. To do this we extract a sample of firms from the data set in order to work at the firm level. However, while the data set includes a

Table 4
Effects of the 1990 reform on the change in firm-level employment

Regressors	(1)	(2)	(3)	(4)
Post 1990	-0.011 (0.012)	0.003 (0.033)	-0.013 (0.04)	-0.017 (0.04)
Small firms	-0.272 (0.015)***	-0.265 (0.015)***	-0.265 (0.015)***	-0.287 (0.019)***
Post 1990 × Small firms	-0.017 (0.009)*	-0.022 (0.009)**	-0.023 (0.009)**	-0.01 (0.012)
<i>N</i>	20317	20317	20317	20317
Sector and Region Effects	YES	YES	YES	YES
Year Effects	YES	YES	YES	YES
Year × Sector Interactions	NO	YES	YES	YES
Year × Region Interactions	NO	NO	YES	YES
Size-specific Cyclical Effects	NO	NO	NO	YES

Notes: Only firms below 35 adjusting their workforce workers are included. Pre-reform period 1986-1989. Post-reform period 1991-1995. Huber/White heteroskedasticity-robust standard errors (in parentheses) allow for clustering by firm. Observations are weighted for the probability that a firms actually enters the sample (given by the average number of employees/90). All specifications control for sector and region effects and total number of employees in the firm. Column (4) controls for size-specific cyclical effects by interacting the small size dummy with GDP growth. *** Denotes significance at the 1% level, ** denotes significance at the 5% level and * denotes significance at the 10% level.

random sample of workers, the probability that a firm is selected increases with size. We account for this by weighting each observation by the probability that the firm actually appears in the sample, which is given by average number of employees divided by 90.²¹

To look at the effects of dismissal costs on the intensive margin, we regress the absolute value of firms' annual employment change (in percentage terms) on the interaction between the post-reform dummy and the small firm dummy, controlling for firm characteristics. Table 4 reports results for all firms adjusting their workforce. Column (1) reports the results of the baseline specification, which controls for time, sector, and region effects, while Columns (2) and (3) add sector- and region-specific time effects. All these specifications show a decline in year-to-year employment changes ranging from 4.7% to 6.4%. Controlling for size-specific cyclical effects in Column (4) continues to show a negative but insignificant effect.²²

This result is in line with previous cross-country studies like Blanchard and Portugal (2001) and Bertola and Rogerson (1997) which look at annual rates of job reallocation and find that EPL has little effect on job flows. Blanchard and Portugal (2001) conjecture that only shorter-frequency job reallocation is impaired by employment protection, as they find the quarterly rate of reallocation, but not the yearly rate of reallocation, to be relatively lower in Portugal than in the U.S.²³ Likewise, it may be that the lack of significance of our results once we control for cyclical factors may be driven by the use of annual as opposed to quarterly data.

²¹ As for accessions and separations, only firms below 35 workers are included and the effect of interest is captured by the interaction between the post-reform dummy and a dummy for firms under 15 employees. In this case, the reported heteroskedasticity-robust standard errors allow for clustering at the firm-level.

²² We have estimated these same regressions without the weights and we find slightly larger effects. However, as pointed out above, these regressions require the use of weights because small firms are under-sampled in our data. By weighting the regressions, we are estimating the effect of the reform on the average firm in the economy.

²³ Wolfers (2005) also provides evidence in this direction.

Table 5
Effects of the 1990 reform on firms' entry and exit

Regressors	Linear Probability Model				Probit
	(1)	(2)	(3)	(4)	(5)
<i>A. DEPENDENT VARIABLE: ENTRY DUMMY – N=49723</i>					
Post 1990	–0.007 (0.007)	0.002 (0.011)	0.007 (0.017)	0.007 (0.017)	0.026 (0.024)
Small firms	–0.084 (0.008)***	–0.083 (0.008)***	–0.083 (0.008)***	–0.082 (0.011)***	–0.155 (0.034)***
Post 1990 × Small firms	–0.013 (0.005)***	–0.014 (0.005)***	–0.014 (0.005)***	–0.015 (0.007)**	–0.017 (0.009)*
<i>B. DEPENDENT VARIABLE: EXIT DUMMY – N=49723</i>					
Post 1990	–0.01 (0.008)	0.024 (0.018)	0.002 (0.023)	0.008 (0.023)	0.016 (0.024)
Small firms	–0.128 (0.009)***	–0.126 (0.009)***	–0.126 (0.009)***	–0.117 (0.012)***	–0.258 (0.043)***
Post 1990 × Small firms	2.00E–04 (0.005)	–0.0012 (0.006)	–0.0007 (0.006)	–0.0057 (0.007)	–0.0082 (0.01)
Sector and Region Effects	YES	YES	YES	YES	YES
Year Effects	YES	YES	YES	YES	YES
Year × Sector Interactions	NO	YES	YES	YES	YES
Year × Region Interactions	NO	NO	YES	YES	YES
Size-specific Cyclical Effects	NO	NO	NO	YES	YES

Notes: Only firms below 35 workers are included. Pre-reform period 1986–1989. Post-reform period 1991–1995. Huber/White heteroskedasticity-robust standard errors (in parentheses) allow for clustering by firm. Observations are weighted for the probability that a firm actually enters the sample (given by the average number of employees/90). All specifications control for sector and region effects and total number of employees in the firm. Column (4) controls for size-specific cyclical effects by interacting the small size dummy with GDP growth. In column (5) the marginal effects from probit estimates are for discrete change from 0 to 1 in case of dummy variables. ***Denotes significance at the 1% level, ** denotes significance at the 5% level and * denotes significance at the 10% level.

4.4. Effects on entry and exit

To look at the effects on the extensive margins, we estimate linear probability and probit models of entry and exit. Table 5 reports results from estimating Eq. (5). The dependent variable is a dummy variable that takes the value of 1 if the firm entered (Panel A) or exited (Panel B) the market. As for employment changes, we weight each observation by the probability that the firm appears in the sample and adjust heteroskedasticity-robust standard errors to allow for clustering at the firm level.

The baseline specification in Column (1) of Panel A shows a decrease in the entry rate of small relative to large firms after the 1990 reform. This result is robust to the inclusion of sector- and region-specific time effects in Columns (2) and (3), as well as to the inclusion of size-specific cyclical effects in Column (4).²⁴ The results range between a 26% and a 30% reduction. The results from a Probit model in Column (5) show an even bigger reduction of 34%.²⁵ This is

²⁴ We also ran other specifications including the third-level interaction of the post-reform dummy, the small firm dummy and the pre-reform variance of employment growth. No differential effects of the reform on entry and exit rates of more volatile sectors were found.

²⁵ As for the year-to-year employment changes, we also estimated the entry and exit regressions without the weights and we find somewhat smaller effects.

consistent with theory (see, e.g., [Hopenhayn and Rogerson, 1993](#)), as a rise in dismissal costs should lower the present value of the future stream of profits and reduce entry. By contrast, all specifications in Panel B show no change in the exit rate of small relative to large firms after the 1990 reform.

5. Conclusion

In this paper, we present new evidence on the impact of dismissal costs on worker and job flows. We use an employer–employee panel drawn from the Italian Social Security records to examine the effects of dismissal costs on workers' accessions and separations, and on firms' employment changes and entry and exit rates. We exploit the fact that dismissal costs increased in Italy after the 1990 reform for firms with fewer than 15 employees relative to larger firms. The adoption of this reform allows us to use a differences–in–differences approach that helps to overcome identification problems in previous studies, which only exploit cross-country variation and cross-sectoral variation in dismissal costs within a country.

We find that both accession and separation probabilities decreased in small relative to large firms after the 1990 reform. In particular, our results imply a decrease of as much as 13% in accessions and 15% in separations. Moreover, we find that the effect is more pronounced in those sectors that were more volatile before the reform, and where dismissal costs are likely to bind. On the other hand, the decreased accessions and separations appear to generate only a negligible increase in net employment.

Aside from the effect of the reform on worker flows, we look at the effects of the reform on employment adjustments at the intensive and extensive margins. We find some evidence that the 1990 reform decreased year–to–year employment changes, but the results are imprecise when size-specific cyclical effects are included. This hints at the need to analyze shorter-frequency job reallocation rates. On the other hand, our estimates suggest that entry rates of small relative to large firms declined by as much as 34% after the 1990 reform, while having no impact on exit rates.

Overall, these results support the view that dismissal costs lower mobility by reducing workers flows into and out of employment. At the same time, we find some evidence suggesting that dismissal costs reduce employment changes and entry. While the impact of dismissal cost on the extensive margin has hardly been studied, our results are consistent with a recent study by [Autor, Kerr and Kugler \(2006\)](#) which uses establishment-level data and also finds reductions in employment changes and entry rates following the introduction of employment protection doctrines in the U.S.

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