The Effects of Employment Protection Legislation on Wages: Evidence from Italy*

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Abstract

This paper uses Italian Social Security data to study the wage effects of a reform that introduced unjust dismissal costs for firms below 15 employees, while leaving firing costs unchanged for bigger firms. Difference-in-difference estimates show that male average wages were 2% lower because of the 1990 EPL reform. The effect is concentrated among new entrants in the firms below 15 employees after the reform, while no effect is found on the wages of the insiders. Among male new entrants, white collars and young workers bore the burden of the wage reduction. No effect is found on female wages. IV and quantile regression estimates yield similar results.

Keywords: Costs of Unjust Dismissals, Severance Payments, Employment Protection.

JEL Classification: E24, J63, J65.

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1 Introduction

Employment Protection Legislation (EPL) is a set of laws which rule the dismissal of employees. Many papers have studied the effect of changes in EPL on employment and job flows. This paper studies the effect of EPL on the distribution of wages.

The firing cost consists of a transfer from the employer to the employee (severance payment) and a tax (e.g. the trial costs). While the tax part of the firing cost cannot be part of the negotiations between employers and employees, it is known since the work of Lazear (1990) that the transfer part of EPL can be undone by changes in wages in a flexible wage framework: The firm reduces the entry wage of a worker by an amount equal to the expected present value of the future severance payment and leaves the cumulative wage bill unchanged. Thus the theory predicts a wage decrease for new entrants. Unlike new entrants, insiders should gain from the introduction of stricter EPL, in fact in a bargaining framework a higher EPL improves the outside option of the insiders and their bargaining power.

We test this theoretical result- typically named "bonding critique"- using a natural experiment form Italy. In 1990, Italy introduced a labour market reform which increased employment protection for workers employed under permanent contracts in firms with less than 15 employees relative to those in firms with more than 15 employees. The reform increased the severance payment (i.e. the transfer part of firing costs) of workers in firms below 15 employees from zero to between 2.5 and 6 months of pay. This reform allows the identification of the effects of employment protection legislation changes on wages through a difference-in-difference approach, i.e. the comparison of workers wages in firms of the same size, before and after the reform.

Most previous work on Italy has studied the effects of EPL comparing the different firing costs regimes that apply to firms below and above 15 employees. Among these papers, Boeri and Jimeno (2003) assess the effect of EPL on lay-off probabilities. Borgarello, Garibaldi and Pacelli (2002), and Schivardi and Torrini (2004) evaluate the effects of EPL on the size distribution of Italian firms.[1] Comparing firms above and below the 15 employees threshold, however, may lead to biased results in case small and large firms differ along dimensions not observable to the econometrician (like, for example, different costs of capital due to the different impact of borrowing constraints on firms of different size).

[1] Borgarello, Garibaldi and Pacelli (2002) also exploit the temporal variation in EPL but they look at firm size effects.

We exploit the temporal variation in EPL, which affected differentially small and large firms. Thus we are able to control for time-invariant unobservable differences in the two groups of firms. Kugler and Pica (2004) also exploit the differential change in firing costs for unfair dismissals in large and small firms after 1990 to look at the effects of changes in EPL on job and workers flows. In this paper we extend their work and look at the effects of EPL changes on the wage distribution.

Our empirical analysis uses administrative data from the Italian Social Security Institute (INPS). The dataset is an employer-employee panel reporting, among other information, the firm yearly average number of employees, the workers yearly wage and the number of paid weeks as well as other individual characteristics.

Our results are easy to summarize. We select all workers who have positive weekly wages continuously in the years 1989, 1990 and 1991, i.e. before and after the reform. Following the indication of Lazear's theory we distinguish two groups, the insiders i.e. those workers who were continuously employed in the same firm (in the text "Sample of Stayers") and the sample which also includes the new entrants i.e. those workers who change firm at least once during the three years (in the text "Sample of Movers").

Difference-in-difference estimates on the "Sample of Movers", controlling for industry fixed effects, region and time effects, industry-specific time trends and firm-specific net job creation (to capture firm-specific cyclical effects), indicate that average male wages decreased by about 2% after the reform in small firms relative to large firms. The point estimates imply a decrease of as much as 7% for white collar males and 2.6% for male young workers (less than 35 years old).

The difference-in-difference estimates are complemented with IV estimates. The increase in EPL in 1990 applied only to firms with less than 15 workers. However, it is possible that marginal firms increased their size with the purpose of escaping from the new rules. To control for the possible endogeneity of firm size, we instrument the treatment status with the size of the same firms in year 1988. The instrument is available only for the subset of workers who stayed in the same firm throughout the period i.e. the "Sample of Stayers". The IV results (and the difference-in-difference OLS results on the same sample) indicate that the reform had no significative impact on the wages of the workers who stayed in the same firm throughout the period.

The difference in the results obtained on the "Sample of Movers" and on the "Sample of Stayers" fits well with the Lazear prediction that changes in the tranfer part of EPL reduces the wages of new entrants. Differently from Lazear's theory, incumbent workers wages do not seem to respond significantly to the change in EPL, at least in the year immediately following the reform. This is a plausible result as long as the wage bargaining for insiders takes place at uneven periods.

Finally, and in overall accordance with the OLS-IV results, quantile regression estimates find evidence of an effect of EPL at the 25^{th} percentile of the wage distribution of male workers of as much as -3.4%. An even stronger effect, of as much as -5.1%, is found at the 25^{th} percentile of the wage distribution of young male workers. The OLS-IV and quantile estimates on either the sample of "Stayers" and "Movers" yield no significative effect on female wages.

The rest of the paper is organized as follows. Section 2 describes how firing restrictions evolved in Italy. Section 3 explains the identification strategy used to evaluate the impact of EPL on the wage distribution. Section 4 describes the dataset and the sample selection rules. Section 5 presents OLS and IV estimates of the impact of increased strictness of employment protection in small firms in Italy after 1990 on average log wages and the wage distribution. Section 6 concludes.

2 The evolution of Employment Protection Regulations in Italy

Over the years the Italian legislation ruling unfair dismissals has widely varied. Both the magnitude of the firing costs and the coverage of firms subject to the restrictions have been subject to extensive legislative revisions.

Dismissals were first regulated in Italy in 1966 through Law 604, which established that, in case of unfair dismissal, employers had the choice to either hire back workers or pay severance, which depended on tenure and firm size.[2] Severance pay for unfair dismissals ranged between 5 and 8 months for workers with less than two and a half years of tenure, between 5 and 12 months for those between two and a half and 20 years of tenure, and between 5 and 14 months for workers with more than 20 years of tenure in firms with more than 60 employees. Firms with less than 60 employees had to pay half the severance paid by firms with more than 60 employees, and firms with less than 35 workers were completely

[2] By contrast, severance pay for fair dismissals is paid from workers' retained earnings, so they entail no cost to employers.

exempt.[3]

In 1970, the *Statuto dei Lavoratori* (Law 300) established that all firms with more than 15 employees had to hire back workers and pay their foregone wages in case of unfair dismissals. Firms with less than 15 employees remained exempt.

Finally, Law 108 was introduced in July 1990 restricting dismissals for permanent contracts. In particular, this law introduced severance payments of between 2.5 and 6 months pay for unfair dismissals in firms with less than 15 employees. Firms with more than 15 employees still had to hire back workers and pay foregone wages in case of unfair dismissals.[4] This means that the cost of unfair dismissals for firms with less than 15 employees increased relative to the cost for firms with more than 15 employees after 1990.[5] Next section explains how we identify the impact of job security provisions on wages exploiting the differential change in dismissal costs that occurred in 1990.

3 Identification strategy

In order to identify the impact of dismissal costs on the wage distribution, we compare the change in mean wages paid by firms with less than 15 employees before and after the 1990 reform to the change in mean wages paid by firms with more than 15 employees.

The strategy to identify the impact of the change in dismissal costs is illustrated in Figures 1 and 2 and in Tables 1 and 2. Figure 1 shows the pattern of average log wages of male blue-collars on a permanent contract in small and large firms (index 1990 = 1). The picture suggests the presence of a steeper wage profile in small relative to large firms before 1990. After 1990 the growth rate of wages in small firms declined and became similar to the growth rate of wages in large firms. Figure 2 shows the time profile of the 10^{th} percentile of the log distribution of wages (index 1990 = 1) of male blue-collars on a permanent contract in small and large firms. Again, a steeper profile appears for small firms only before 1990. Overall, the two pictures suggest that (the growth rate of) both average log wages and wages at 10^{th} percentile of the log distribution of male blue collars have declined more markedly

[3] Boeri and Jimeno (2003) 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 EPL, because small firms subject to EPL have to pay much higher efficiency wages to discourage shirking than large firms.

[4] Notice that this change in EPL concerned the transfer part of EPL (severance payments). Overall, the transfer part has been estimated at 80% of the total firing cost (Garibaldi and Violante, 2005).

[5] Overall, Italy, together with other Southern European countries, is considered one of the strictest countries in terms of employment protection legislation. See, for instance, Lazear (1990), Bertola (1990), OECD's Employment Outlook (1999) and Nicoletti, Scarpetta and Boylaud (2000).

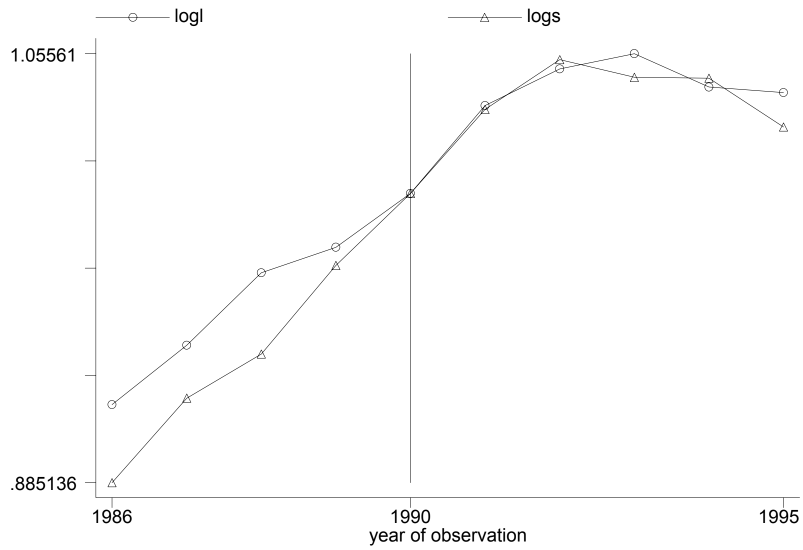

Figure 1: Average log wages of male blue-collars (index 1990 = 1) on a permanent contract in small (less than15 employees) and large firms (between 15 and 35 employees).

after 1990 in small than in large firms.

The figures in table 1, relative to the "Sample of Movers" described below, confirm this interpretation. The table shows a smaller increase of average log wages in small firms after 1990 relative to large firms. The T test indicates that the double difference of the means is not significant. The same happens at the 25^{th}, 50^{th} and 75^{th} percentiles.[6]

The figures in table 2 show a similar pattern for women. Average log wages seem to go up more in small than in large firms after 1990, but the diff-in-diff is not significant.

3.1 Workers average wages

To control for the possibility that the pattern highlighted in Figures 1 and 2 and in Tables 1 and 2 is the result of other shocks occurring during the post-reform period, we estimate formal regression models using the panel of workers described in section 4. The baseline specification that controls for year effects, industry fixed-effects, and for observable worker and firm characteristics looks as follows:

$$E\left(\log w_{ijt} \mid X_{ijt}, D_j^S, Post_t\right) = \beta' X_{ijt} + \delta_0 Post_t + \delta_1 D_j^S + \delta_2 \left(D_j^S \times Post_t\right) \tag{1}$$

The dependent variable, $\log w_{ijt}$, is the log of the weekly wage paid to worker i by firm j in year t, and is given by the yearly wage divided by the number of paid weeks. If a worker

[6]The standard errors for the quantiles of the distribution are constructed assuming normality of the log-wage distribution.

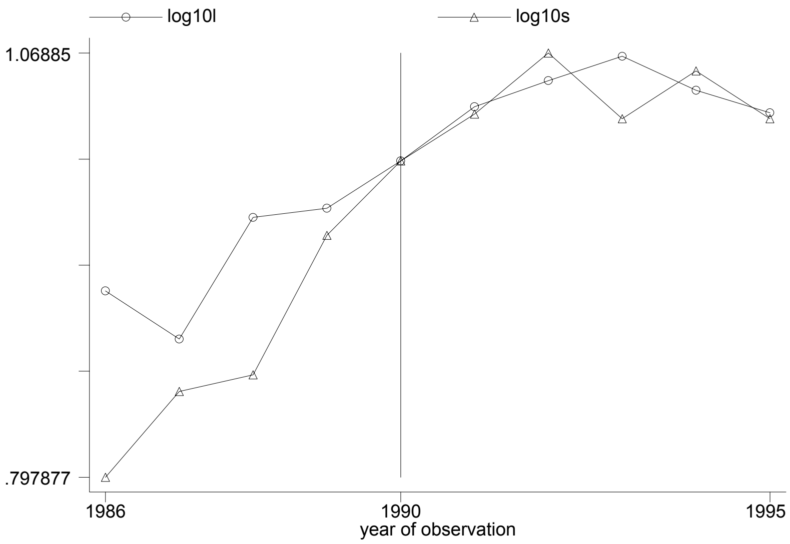

Figure 2: 10^{th} quantile of log distribution of male blue-collars wages (index 1990 = 1) on a permanent contract in small (less than15 employees) and large firms (between 15 and 35 employees).

appears more than once in a given year, a possible event given the administrative nature of our dataset, the wage with the longest spell is selected.

The matrix X_{ijt} includes worker characteristics such as a quadratic in age and tenure, and occupation (white collar/blue collar dummy); firm characteristics such as the geographical location, industry effects, the yearly average number of employees. We also control for time-varying industry productivity, time effects and, in some specifications, for workers fixed effects.

While the inclusion of time effects allows controlling for the possibility that the change in wages after the post-reform period was due to macro shocks, it is possible that the cycle affects firms differently. Following Belzil (2000), we include in the regressions the net job-creation rate, as it is possible that firms expanding faster than average pay more (in order to attract or retain workers). Failure to account for the different sensitivity of wages to firm expansion may bias the estimates of δ_2. The matched employer-employee nature of our dataset allows to compute the net job-creation rate of firm j at time t:

$$NJC_{jt} = \frac{2(e_{jt} - e_{jt-1})}{e_{jt} + e_{jt-1}} \tag{2}$$

where e_{jt} is average employment of firm j at time t.

Finally, $Post_t$ is a dummy that takes the value of 1 after 1990 and zero otherwise; D_j^S is a dummy that takes the value of 1 if the worker is employed in a small firm and 0 if the worker is employed in a big firm. The interaction term between the small firm dummy and the post-reform dummy is included to capture the effects of interest.

The difference-in-difference estimates are complemented with IV estimates. The increase in EPL in 1990 applied only to firms with less than 15 workers. However it is possible that marginal firms increased their size with the purpose of escaping the new rules. As an instrument for the firm size dummy, we use firm size (above/below 15 employees) in 1988, which is correlated with size in subsequent years but is not affected by the reform. We also need to instrument the interaction term between the small firm dummy and the post-reform dummy. We do so by using as an instrument the product of the *fitted value* of the firm size dummy with the Post 1990 dummy. Formally, the IV specification looks as follows:

$$E\left(\log w_{ijt} \mid X_{ijt}, D_j^S, Post_t\right) = \beta' X_{ijt} + \delta_0 Post_t + \delta_1 \widehat{D}_j^S + \delta_2 \left(\widehat{D_j^S \times Post_t}\right) \tag{3}$$

the instruments being the firm size dummy in 1988 and the term $\widetilde{D}_j^S \times Post_t$, where $\widetilde{D}_j^S$ is the fitted value obtained regressing the firm size dummy on all (included and excluded) exogenous variables.

3.2 Workers wage distribution

To study the effect of the reform on the wage distribution we use quantile regression techniques. Let $Q_\theta(\log w_{ijt} | X_{ijt}, D_j^S, Post_t)$ for $\theta \in (0,1)$ denote the θ^{th} quantile of the distribution of $\log w_{ijt}$, the log wage of individual i at time t in firm j given the covariates. The model of the conditional quantiles is:

$$Q_\theta\left(\log w_{ijt} \mid X_{ijt}, D_j^S, Post_t\right) = X_{ijt}\beta_\theta + \delta_\theta Post_t + \delta_\theta D_j^S + \gamma_\theta\left(D_j^S \times Post_t\right) \tag{4}$$

where X_{it} is as described above. The parameter of interest is γ_θ. Estimating the equation at the 25^{th}, 50^{th} and 75^{th} quantile measures the impact of the 1990 reform at different quantiles of the wage distribution. Since we are dealing with panel data, the standard errors on γ_θ are clustered at the individual level and are calculated by bootstrapping methods with 200 replications of samples of size $N =$ number of observations.

4 Data description

The data set is drawn from the Italian Social Security Administration (INPS) archives for the years 1986-1995. The original data set collects social security forms of a 1/90 random sample employees every year, with employees 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 includes individual longitudinal records generated using social security numbers. However, since the INPS collects information on private sector employees for the purpose of computing retirement benefits, employees are only followed through their employment spells. The data, thus, stops following individuals who move into self-employment, the public sector, the agricultural sector, the underground economy, unemployment, and retirement. The data set also includes longitudinal records for firms employing the randomly selected workers in the sample using the firms' name, address, and social security and fiscal codes.

The data set is, thus, an employer-employee panel with information on workers and firm characteristics. In particular, the data includes information on employees' age, gender, occupation (blue collar-white collar), yearly wage, number of paid weeks, and type of contract (permanent-temporary), and information on firms' location, sector of employment, and average number of employees.

4.1 Sample selection rules

We keep in the sample all workers who have a valid yearly wage continuously before and after the reform. In order to preserve sample size we focus on the years 1989, 1990 and 1991. We remove the years after 1991 also because in 1992 a wage compressing institution − called *scala mobile* − was abolished and its effects may act as a confounding factor. Since we are interested in the relative trend in wages in firms smaller and larger than 15 before and after the reform, we eliminate all firms with more than 35 workers to preserve the comparability of treatment and control groups. We also eliminate few observations where the daily wage (the yearly wage divided by the reported days of work) is larger than the yearly wage. In the course of the paper we use weekly wages. For the cases of double individual spells in the same year we keep only the longest spell.

As reported above, we deal with the potential endogeneity of firm size, by instrumenting the treatment status with the size of the same firm in year 1988. Arguably, size in 1988 is correlated with size in subsequent years and is not affected by the EPL reform. However, since our dataset is a panel of *workers*, a given firm j appears in year t only if (at least) one of its worker is selected in that year. In our sample this implies that the instrument is available only for the subset of workers who stayed in the same firm throughout the period 1988-1991.

Therefore, we estimate the effect of the 1990 reform on two distinct samples: a "Sample of Stayers", made up of all permanent workers working in the same firm in 1989-1990-1991, and a "Sample of Movers", which strictly includes the latter, made up of all permanent workers appearing in 1989-1990-1991 that *may* (or may not) have moved from one firm to another. IV estimates are implementable only on the "Sample of Stayers" because the instrument "*firm size in 1988*" is not available for workers who changed firm.

Table 1 presents descriptive statistics relative to the "Sample of Movers" by firm size, before and after the 1990 reform. As for men, the table shows that, after the reform, there is a smaller increase in average wages in small firms relative to large firms. The table also shows an increase in 25^{th}, the 50^{th} and the 75^{th} percentile of earnings in both small and large firms after the reform. Again, the increase at different quantiles of earnings after the reform seems to be larger in large firms. As for women, except for the 25^{th} quantile, the increase in wages seems to be larger in small than in large firms. Anyway, in all cases the diff-in-diff is not significantly different from zero.

The unconditional effects suggest that the Italian 1990 EPL reform might have had no (or, at best, only little) effect on both average (male and female) wages and on the (male and female) wage distribution.

The next section presents regression results which control for covariates.

5 Results: the effects of the 1990 reform

5.1 OLS estimates on the "Sample of Movers"

Table 3 reports the coefficients and standard errors of equation (1) estimated on the "Sample of Movers", i.e. on the sample of all workers with a valid weekly wage in years 1989-1990-1991. The effect of interest is captured by the interaction between the post-reform dummy and a dummy for firms under 15 employees. The reported standard errors allow for clustering by individual.

All specifications control for industry and region effects, industry-specific trends, age, age squared, tenure, tenure squared, occupation (white collar/blue collar dummy), average number of employees in the firm, industry productivity calculated as value-added deflated using a industry-level PPI over the number of workers using 1995 as the base year, and net job creation as defined in equation (2).

Panel A of Table 3 shows the results for male workers. The baseline specification in column 1 shows a significant negative effect of the 1990 EPL reform on the average wages of male workers of as much as -1.9%. The specification that controls for workers fixed effects, in column 2, provides more precise estimates and, similarly, suggests a reduction of the average wage of -1.8%.

As it is plusibly that these results vary across workers types, we look at the effect by occupation (white collar/blue collar) and age. As reported in columns 3 and 4, we find a significant negative effect on white collar workers of as much as -7.26% and on young workers (below 35) of as much as -2.6%. No significant effect seems to be there for blue collars and older workers.

Panel B of Table 3 shows the results for female workers and highlights, in all specifications, the absence of any significant effect of the EPL reform on female workers wages.

5.2 IV and OLS estimates on the "Sample of Stayers"

Firm size is potentially endogenous since marginal firms may decide to cross the 15 employees threshold *because* of the change in EPL. To control for the possible endogeneity, we instrument the treatment status (the dummy firm size lower than 15 employees) using "*firm size in 1988*". As discussed in section 4, this instrument is available only for the subset of workers that stayed in the same firm from 1988 to 1991.

Table 4 provides, in column 2, IV estimates on the "Sample of Stayers" for males (panel A) and females (panel B) using equation (3). For comparison purposes we also report, in column 1, the OLS estimates on the same sample in order to be able to assess the relevance of the endogeneity problem.

Columns 1 and 2 show in a consistent manner for males and females that the average wage of the stayers was not affected by the reform. The fact that IV and OLS estimates lead to the same conclusion suggests that the endogeneity problem is not too much of an issue. This is not unexpected in light of the fact that firms, in order to escape a relatively light increase in EPL, choose to face an even larger firing cost by crossing the 15 employees threshold.

Columns 3 and 4 confirm the absence of any effects of EPL on the wages of the stayers in different occupations and of different ages.

The absence of a serious endogeneity problem makes OLS estimates valid and allows a meaningful comparison of the OLS results obtained with the two different samples: the "Sample of Movers" and the "Sample of Stayers". Evidence from the former suggests that the EPL reform did have a negative effect on wages while the no such evidence emerges from the latter. Hence, it must be that the effect found in the "Sample of Movers" worked through the wages of new hires in 1991.

These findings may be nicely interpreted within the Lazear's neutrality framework that, in an insider-outsider wage regime, takes the following form (Garibaldi and Violante 2005):

$$\begin{aligned} w_o &= \beta p + (1-\beta)\, rU - \lambda S \\ w_i &= \beta p + (1-\beta)\, rU + rS \end{aligned}$$

where $w_{i,o}$ is the wage of insiders and outsiders, p is productivity, rU the worker outside option and β the worker bargaining power. Reducing appropriately the outsider wage, the firm can make the worker pre-pay entirely the severance payment S. The outsider status lasts for $\frac{1}{\lambda}$ periods. As an insider the worker will earn interest on the principal held by the firm and upon separation he will receive the principal back. Given risk neutrality this scheme has no distributive effects.

However, if, for example, insider wages are rigid and are not affected by S, the scheme does have distributive effects and we should expect that entry wages are negatively affected by an increase in EPL while insider wages are unaffected. This is a possible interpretation of our results in view of the fact that in Italy collective bargaining agreements, though binding for both insiders and outsiders, may arguably leave larger room for individual bargaining at the entry stage rather than at a later one. For insiders, the effect of the EPL change may be incorporated in the collective wage agreement itself, however we measured the effects only one year after the reform and collective bargaining takes place at uneven periods.

5.3 Results from quantile regressions

Table 5 reports the effects of the 1990 EPL reform on the wage distribution estimated using equation (4) on the "Sample of Movers" at the 75^{th}, 50^{th} and 25^{th} quantiles.

Panel A reports results on the wage distribution of males and looks at the entire sample of workers, at white collars workers and at young workers. A negative effect is found for the entire sample and for young workers. In both cases at the 25^{th} percentile. Although a negative effect on the average wage was found for white collars, no effect on their wage distribution appears. Panel B shows that the wage distribution of women was not affected by the change in EPL.

The quantile regression exercise completes the picture. The change in EPL affected wages of new hires *at the bottom of the distribution*, while not affecting entry wages at higher quantiles. Thus the increase in EPL seems to widen wage inequalities reducing the wage of new entrants at the bottom of the distribution.

6 Conclusion

The Lazear bonding critique predicts that, in absence of contractual or market frictions, a firm can undo a government-mandated transfer (severance payment) reducing the wages of new entrants by an amount equal to the expected increase in the future transfer. We provide evidence of the impact of changes in dismissal costs on the distribution of wages using a natural experiment from Italy: Severance payments increased after 1990 in Italy for firms with less than 15 employees relative to larger firms.

Difference-in-difference estimates find that average wages of male workers in firms below 15 employees were reduced of around 2% because of the 1990 EPL reform. We find that the negative effects are limited to the sample of new entrants, thus confirming the Lazear theory. We estimate no significant effect on insiders wages and on female wages. Among male new entrants, white collars and young workers bore the burden of the wage reduction.

We address the possible endogeneity of firm size recurring to IV techniques. The instruments are available only for the sample of insiders and not for the new entrants. Instrumenting the treatment status with firm size in 1988, prior to the sample considered, we confirm the insignificant results obtained on the sample of insiders. Finally using quantile regression we find that the negative effect on new entrants male wages is significant at the 25^{th} quantile of the log wage distribution and not significant at higher quantiles. Thus we conclude that the EPL reform may have had an effect on male wage inequality reducing the wage of male new entrants at the bottom of the distribution.

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Table 1: MEN. Descriptive statistics by firm size, before and after the reform

Variables	**Pre-reform**	**Post-reform**		**Pre-reform**	**Post-reform**		
	Small firms		***Diff***	**Large firms**		***Diff***	***Diff-in-Diff***
Age	38.395	40.050		39.308	40.507		
	(10.826)	(10.871)		(10.503)	(10.415)		
White collars %	0.190	0.188		0.247	0.260		
	(0.392)	(0.391)		(0.431)	(0.439)		
Yearly average size of the firm	7.281	7.328		23.421	23.961		
	(4.243)	(4.381)		(5.638)	(5.867)		
Average log weekly real wage	1.637	1.721	*0.084*	1.733	1.844	*0.111*	*-0.027*
	(0.259)	(0.271)	*[0.011]*	(0.297)	(0.308)	*[0.017]*	*[0.02]*
Log Wage - Q25	1.494	1.570	*0.077*	1.566	1.670	*0.104*	*-0.027*
	(0.007)	(0.011)	*[0.013]*	(0.012)	(0.017)	*[0.021]*	*[0.025]*
Log Wage - Q50	1.625	1.709	*0.084*	1.725	1.836	*0.111*	*-0.027*
	(0.007)	(0.011)	*[0.013]*	(0.012)	(0.017)	*[0.021]*	*[0.025]*
Log Wage - Q75	1.767	1.856	*0.090*	1.900	2.014	*0.114*	*-0.024*
	(0.007)	(0.011)	*[0.013]*	(0.012)	(0.017)	*[0.021]*	*[0.025]*
N	1839	915		961	485		

Notes: Only firms below 35 workers are included. Standard deviations in parenthesis. Standard errors in square brackets

Table 2: WOMEN. Descriptive statistics by firm size, before and after the reform

Variables	Pre-reform	Post-reform		Pre-reform	Post-reform		
	Small firms		*Diff*	Large firms		*Diff*	*Diff-in-Diff*
Age	34.288	35.912		33.767	34.955		
	(9.689)	(9.705)		(9.238)	(9.129)		
White collars %	0.591	0.590		0.399	0.410		
	(0.492)	(0.492)		(0.49)	(0.493)		
Yearly average size of the firm	6.601	6.637		23.605	23.594		
	(4.134)	(4.209)		(5.625)	(5.562)		
Average log weekly real wage	1.447	1.542	*0.096*	1.522	1.611	*0.089*	*0.007*
	(0.347)	(0.358)	*[0.018]*	(0.307)	(0.321)	*[0.025]*	*[0.031]*
Log Wage - Q25	1.290	1.340	*0.050*	1.395	1.455	*0.061*	*-0.011*
	(0.012)	(0.019)	*[0.022]*	(0.016)	(0.025)	*[0.029]*	*[0.037]*
Log Wage - Q50	1.481	1.572	*0.091*	1.515	1.592	*0.076*	*0.014*
	(0.013)	(0.018)	*[0.022]*	(0.017)	(0.025)	*[0.03]*	*[0.038]*
Log Wage - Q75	1.643	1.753	*0.110*	1.700	1.795	*0.096*	*0.014*
	(0.013)	(0.019)	*[0.023]*	(0.017)	(0.026)	*[0.031]*	*[0.038]*
N	1138	578		506	244		

Notes: Only firms below 35 workers are included. Standard deviations in parenthesis. Standard errors in square brackets

Table 3: Effects of the 1990 reform on the average wages of permanent workers - Sample of movers in 1989 - 1990 - 1991

	(1)	(2)	(3)	(4)
	Whole sample		White Collars	Young (<35)
Dependent Variable: Log of weekly real wage	*OLS*	*OLS - FE*	*OLS*	*OLS*
	A. MEN			
Post 1990	0.030	0.026	0.094	0.028
	(0.008)	(0.007)	(0.023)	(0.012)
Small firms	0.018	0.005	0.018	0.033
	(0.02)	(0.01)	(0.062)	(0.028)
Post 1990 × Small firms	**-0.019**	**-0.018**	**-0.074**	**-0.026**
	(0.009)	(0.006)	(0.03)	(0.013)
N	4182	4182	882	1623
	B. WOMEN			
Post 1990	0.052	0.059	0.058	0.057
	(0.015)	(0.016)	(0.022)	(0.021)
Small firms	0.108	0.039	0.179	0.113
	(0.034)	(0.025)	(0.058)	(0.037)
Post 1990 × Small firms	-0.006	-0.008	-0.009	-0.005
	(0.02)	(0.01)	(0.02)	(0.02)
N	2487	2487	1278	1398

Notes: Firms below 35 workers are included. Robust standard errors in parenthesis allow for clustering by individual. All specifications control for sectoral and region effects, sector specific trends, age, age squared, tenure, tenure squared, occupation (white collar/blue collar dummy), average number of employees in the firm, sectoral productivity calculated as value-added deflated using a sector-level PPI over the number of workers using 1995 as the base year, and net job creation (defined as specified in the text).

Table 4: Effects of the 1990 reform on the average wages of permanent workers - Sample of stayers in 1989 - 1990 - 1991

	(1)	(2)	(3)	(4)
	Whole sample		White Collars	Young (<35)
Dependent Variable: Log of weekly real wage	*OLS*	*IV*	*IV*	*IV*
		A. MEN		
Post 1990	0.034	0.031	0.056	0.040
	(0.009)	(0.009)	(0.022)	(0.013)
Small firms	0.007	0.072	0.086	0.084
	(0.023)	(0.049)	(0.167)	(0.08)
Post 1990 × Small firms	-0.011	-0.007	-0.017	-0.015
	(0.01)	(0.01)	(0.026)	(0.015)
N	3138	3138	624	1116
		B. WOMEN		
Post 1990	0.058	0.046	0.013	0.079
	(0.022)	(0.021)	(0.028)	(0.029)
Small firms	0.138	0.251	0.357	0.181
	(0.042)	(0.09)	(0.128)	(0.088)
Post 1990 × Small firms	0.005	0.019	0.048	0.006
	(0.02)	(0.02)	(0.03)	(0.03)
N	1821	1821	909	999

Notes: Firms below 35 workers are included. Robust standard errors in parenthesis allow for clustering by individual. All specifications control for sectoral and region effects, sector specific trends, age, age squared, tenure, tenure squared, occupation (white collar/blue collar dummy), average number of employees in the firm, sectoral productivity calculated as value-added deflated using a sector-level PPI over the number of workers using 1995 as the base year, and net job creation (defined as specified in the text). In the IV specifications the firms' size dummy (above/below 15) is instrumented using the size dummy in 1988.

Table 5: Effects of the 1990 reform on the distribution of wages of permanent workers - Sample of movers in 1989 - 1990 - 1991

Quantile regressions	(1)	(2)	(3)
	Quantile		
Dependent Variable: Log of real weekly wage	75th	50th	25th
		A. MEN	
Whole sample (*N* = 4182)	-0.006	-0.009	**-0.034**
	(0.001)	(0.013)	(0.013)
White Collars (*N* = 882)	-0.020	-0.062	-0.028
	(0.044)	(0.039)	(0.033)
Young (*N* = 1623)	-0.020	-0.018	**-0.051**
	(0.024)	(0.019)	(0.017)
		B. WOMEN	
Whole sample (*N* = 2487)	0.003	0.008	-0.019
	(0.017)	(0.016)	(0.026)
White Collars (*N* = 1278)	0.011	0.032	-0.041
	(0.035)	(0.035)	(0.041)
Young (*N* = 1398)	0.024	0.020	-0.002
	(0.021)	(0.018)	(0.027)

Notes: Only firms below 35 workers are included. Bootstrapped standard errors in parenthesis allow for clustering by individual. All specifications control for sectoral and region effects, sector specific trends, age, age squared, tenure, tenure squared, occupation (white collar/blue collar dummy), average number of employees in the firm, sectoral productivity calculated as value-added deflated using a sector-level PPI over the number of workers using 1995 as the base year and net job creation is defined as specified in the text.