Effects of Employment Protection Legislation on Wages: a Regression Discontinuity Approach*

Marco Leonardi†
University of Milan and IZA

Giovanni Pica‡
University of Salerno and CSEF

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Abstract

This paper uses Regression Discontinuity Design 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. We address the endogeneity of the treatment status due to workers and firms sorting around the 15 employees threshold by applying IV techniques on a sample of exogenously displaced workers. The estimates show that male entry wages were around 6% lower on average because of the 1990 EPL reform, implying that as much as 25% of the firing cost was shifted onto lower wages. Among males, blue collars and young blue collars workers bore the burden of the wage reduction. The reform is also found to lower the wages of female blue collar workers by 5%.

Keywords: Costs of Unjust Dismissals, Severance Payments, Regression Discontinuity Design.

JEL Classification: E24, J63, J65.

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†E-mail: marco.leonardi@unimi.it

‡E-mail: gpica@unisa.it

1 Introduction

Employment Protection Legislation (EPL) is a set of laws which rules 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 that entry wages decrease.

We test this theoretical result – typically named "bonding critique" – using a natural experiment from Italy. In 1990, Italy introduced a labour market reform which increased employment protection for workers employed under permanent contracts in firms with fewer 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. Although EPL is still stricter in firms with more than 15 employees, the reform narrows the gap between employment security provisions guaranteed in firms above and below 15 employees.

We identify the effects of employment protection legislation on wages through a Regression Discontinuity Design (RDD). In a classical randomized experiment, RD design involves a dichotomous treatment that is a deterministic function of a single variable (in our case the firm size threshold of 15). The identification assumption is essentially that the average outcome for individuals marginally above the threshold represents a valid counterfactual for the treated group just below the threshold. The identification assumption in RD implies that close to the threshold all variables determined prior to assignment will be independent of treatment status.

For the applied researcher, there are two limitations to this assumption. First, the assumption that the average outcome for individuals marginally below the threshold represents a valid counterfactual for the treated group just above the threshold is fundamentally untestable. Empirical researchers assess the validity of their RDD by comparing the means of the predetermined variables conditional on treatment around the threshold, but of course they cannot compare differences in unobservable variables. Secondly in many instances individuals have some influence over their treatment status i.e. the experiment is not randomized and treatment is endogenous.

Regarding the first limitation of the RDD design, we have an advantage with respect to RDD studies which compare different groups around the threshold in that we can exploit the time dimension of the reform. We essentially compare wages of individuals who work in firms just below the 15 employees threshold with wages of individuals who work in firms just above the 15 employees threshold, before and after the reform. Exploiting the temporal variation in EPL which affected differentially small and large firms, we are able to control for time-invariant unobservable differences in the two groups of firms.

One natural concern in our case is the endogeneity of the treatment status. One may think that both firms and workers sort above and below the threshold according to their preferences. In fact, if there are benefits to receiving the treatment, it is natural to expect those who gain the most to select themselves into the treatment group. Firms which were smaller than 15 before the reform may have been induced to grow bigger than 15 *because* of the EPL reform. In the same way workers with different preferences over a menu of employment protection and wages may have been induced to move from a firm bigger than 15 to a firm smaller than 15 (or viceversa) *because* of the EPL reform.

In the course of the empirical analysis we try to address the sorting of workers and firms into the treatment status. To address the latter, we instrument firm size with firm size in year 1989, when the reform was not in place and arguably unexpected. To try and (partially) address the sorting of workers we look at exogenously displaced workers in the hope that their allocation in firms below or above the threshold is "more random" than the allocation of those who voluntarily change firms. To define the displaced workers we identify all plant closings in the dataset and look at wages of those workers who were displaced due to plant closings in the two years preceding the reform and were presumably looking for work before the reform took place.[1]

Our empirical analysis uses administrative data from the Italian Social Security Institute (INPS) for the Italian provinces of Vicenza and Treviso. 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. Although the data refer only to two Italian provinces, it contains the entire population of workers and firms allowing us to define displaced workers due to plant closings and to apply IV techniques to firm size tracing firms back in time even after the worker left. Vicenza and Treviso are provinces of Northern Italy of particular interest for the analysis of the effects of EPL on wages of small vs big firms of because of the high concentration of small firms.

[1] This identification strategy follows Jacobson, Lalonde, and Sullivan (1993).

Our results are easy to summarize. We first define a sample of new hires as those workers with a valid wage who are newly hired in a firm in the current year. OLS results on the sample of new hires show an insignificant effect of the reform on wages. However, identification on this sample implies that the treatment assignment to a firm smaller than 15 is independent of the EPL reform. We then move to a sample of displaced workers due to plant closings i.e. of workers with a valid wage who are newly hired in a firm in the current year but were displaced due to plant closing in the previous two years. We consider that this group of displaced workers who presumably were looking for work before the reform took place and found a job after the reform are more unlikely to sort themselves in the treatment group *because* of the reform. The estimates obtained on the "Sample of Displaced" indicate that average wages of blue collar males decreased by about 3% after the reform in small firms relative to large firms. These estimates are robust to the inclusion among the regressors of polynomials of various orders in firm size (separately for each side of the threshold) and to the inclusion of industry fixed effects, region and time effects and various individual characteristics thus confirming the validity of the RDD design. The point estimates imply a decrease of more than 7% for young blue collar males (less than 30 years old). No significant effects are found on the sample of displaced females.

The OLS estimates are complemented with IV estimates. The increase in EPL in 1990 applied only to firms with fewer than 15 workers. However, it is possible that marginal firms which kept their size just below 15 before the reform to avoid strict EPL rules, increased their size *because* of the reform. To control for the possible endogeneity of firm size, we instrument the treatment status with the size of the same firms in year 1989 when the reform was still unexpected. The IV results indicate that the reform had a significative negative impact on the wages of displaced workers of 6.7%. In particular, the wages of blue collar and young blue collar males decreased respectively by as much as 9.1% and 10.1%. The IV estimates also suggest a negative effect of the EPL reform on the wages of blue collar females of as much as 5%.

1.1 Previous Literature

Previous literature 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.[2] 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).

Kugler and Pica (2004) exploit the differential change in firing costs for unfair dismissals in large and small firms after 1990 and adopt a difference-in-difference approach to look at the effects of changes in EPL on job and workers flows. Kugler and Pica work on an administrative dataset representative of the Italian economy. In this paper we look at the effects of EPL changes on the wage distribution using a different dataset which refers only to two Italian provinces Vicenza and Treviso but contains the entire population of workers and firms allowing us to apply IV techniques. Vicenza and Treviso are provinces of Northern Italy of particular interest for this type of analysis because of the high concentration of small firms.

The rest of the paper is organized as follows. Section 2 describes how firing restrictions evolved in Italy. Section 3 describes the dataset and the sample selection rules. Section 4 explains the identification strategy used to evaluate the impact of EPL on the wage distribution. 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 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 varied a lot. Both the magnitude of the firing cost and the coverage of the firms subject to the restrictions have gone through extensive changes.

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. 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 fewer than 60 employees had to pay half the severance paid

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

by firms with more than 60 employees, and firms with fewer than 35 workers were completely exempt.[3]

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

The longitudinal panel is constructed from the administrative records of the Italian Social Security System (Inps). It refers to the entire population of employees and workers in two provinces, Treviso and Vicenza, of an Italian region, Veneto.[6]

The database covers each plant and each individual employed in the private sector. It includes information on all plants and employees working at least one day in those plants from 1984 to 1994.[7] The unit of observation is the employer-day. Employers are identified by their identification number.

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

[3] See Boeri and Jimeno (2003) for a theoretical explanation of why these exemptions may be in place.

[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).

[6] The two provinces are located in the North-eastern part of the country. The overall population was 1.6 million people as of the 2001 Population Census. In general, the Italian North-East is characterized by small firm size and tight labor markets. In year 2000 GDP per capita was 22,400 €, 20% higher than the national average. The two provinces account for 3.3% of the Italian GDP.

[7] Data are collected independently of the workers place of residence and take into account also the occupational spells out of Treviso and Vicenza.

sector, the agricultural sector, the underground economy, unemployment, and retirement.

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, average number of employees and date of closure.[8]

3.1 Sample selection rules

In order to preserve sample size we focus on the years 1986-1994. We remove year 1990 because the reform occurred in the month of July and the wages of year 1990 are likely to be a mixture of pre-reform and post-reform wages. Since we are interested in the relative wages in firms close to the threshold, we eliminate all firms smaller than 10 and larger than 20, to preserve the comparability of treatment and control groups. In the course of the paper we use weekly wages. We eliminate the upper and lower 1% of the wage distribution in each year. For the cases of multiple individual spells in the same year we keep only the longest spell.

4 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 just below 15 employees before and after the 1990 reform to the change in mean wages paid by firms just above 15 employees. Lazear's theory indicates that the effect of EPL is more plausibly found on entry wages which are reduced of an amount equal to the present value of the future severance payment. The wages of the insiders are more likely to be insulated because of contractual rigidities.

The RDD strategy to identify the impact of the change in dismissal costs is illustrated in Figure 1. The figure considers a sample of movers, i.e. a sample of workers appearing for the first time in a given firm coming either from another firm or from outside the sample. Figure 1 plots the mean wage against firm size before (1986-1989) and after the reform (1991-1994). The mean is estimated non parametrically separately for each side of the threshold. The RD design identifies the effect of EPL on wages as the difference between average wages of firms above and below the 15 employees threshold before and after the reform.

[8] The data also allow to spot false closures (e.g. closures due mergers or acquisitions). Any time more than 80% of the employees of the closing firm are found in another firm, the episode is not considered a closure.

Figure 1 hardly shows any discontinuity at the 15 employees threshold, neither before nor after 1990. However, the evidence of a smooth relationship between firm size and wages both before and after the 1990 EPL reform does not rule out completely the presence of an effect of EPL on wages. As noticed in the introduction, it is reasonable to believe that the EPL reform induced both firms and workers to sort around the 15 employees threshold. The reform changed the menu of job security and wages in small vs. large firms thus changing the incentives to work in a small (as a opposed to a large) firm. In other words, if workers can influence their own treatment status (and voluntary changers presumably choose the firm they move into), this violates the assumption of random assignment and possibly biases the results.

In the attempt of minimizing the bias due to workers' self-selection in the treatment status, we estimate the RD on a sample of exogenously displaced workers. To define the displaced workers we identify all plant closings in the dataset and look at wages of those who were displaced due to plant closings in the two previous years and presumably were looking for work before the reform took place. Similarly to Jacobson, Lalonde, and Sullivan (1993) the idea is that exogenously displaced workers who were looking for work before the reform took place are less likely than those who voluntarily change firm to sort themselves in firms around the threshold only because of the reform.

Figure 2 plots the relationship between wages and firm size for a sample of displaced workers. The upper panel of figure 2 refers to blue collar displaced workers and shows a discontinuous jump in the relationship between firm size and wages right at the 15 employees threshold after the reform. The lower panel refers to young (below 30 years old) blue collars and shows a similar breakdown of the relationship at the 15 employees threshold after the reform. The evidence of figure 2 is consistent with the hypothesis that after the reform workers in firms smaller than 15 obtained lower wages in exchange of higher employment protection.

Our RRD-based empirical strategy aims at measuring the size of the discontinuity after the reform relative to the pre-reform period. Our first set of estimates focuses on movers and neglects both workers and firms sorting. The second set of estimates looks at displaced workers in an attempt to address workers' sorting. The last set of estimates uses an IV strategy to address firms' sorting.

4.1 Descriptive statistics

To give an idea of the distribution of observed characteristics, Table 1 considers the sample of displaced workers in firms between 10 and 20 employees and provides, separately for males

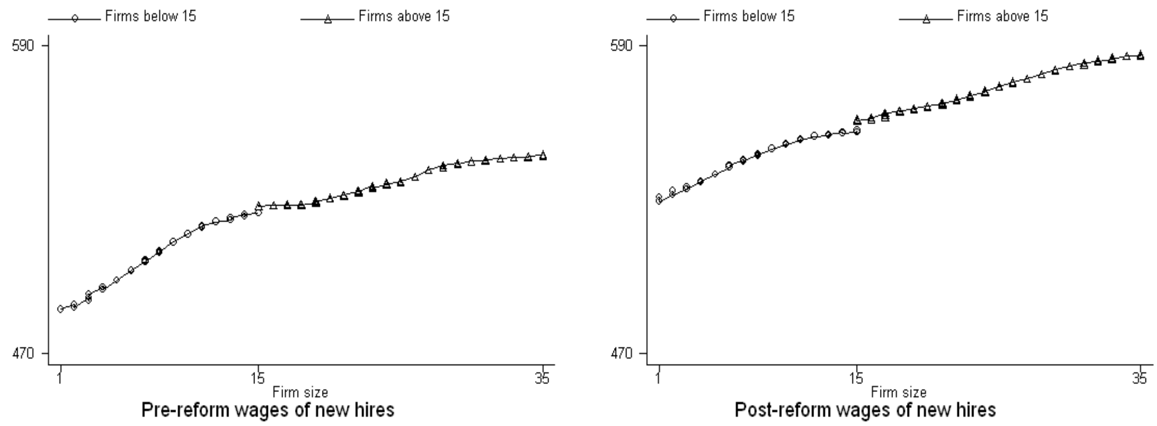

Figure 1: Non parametric prediction of the average log real wage from a weighted local linear regression smoother with bandwith 0.8, estimated separately for each side of 15 employees threshold. Pre-reform period: 1986-1989. Post-reform period: 1991-1994.

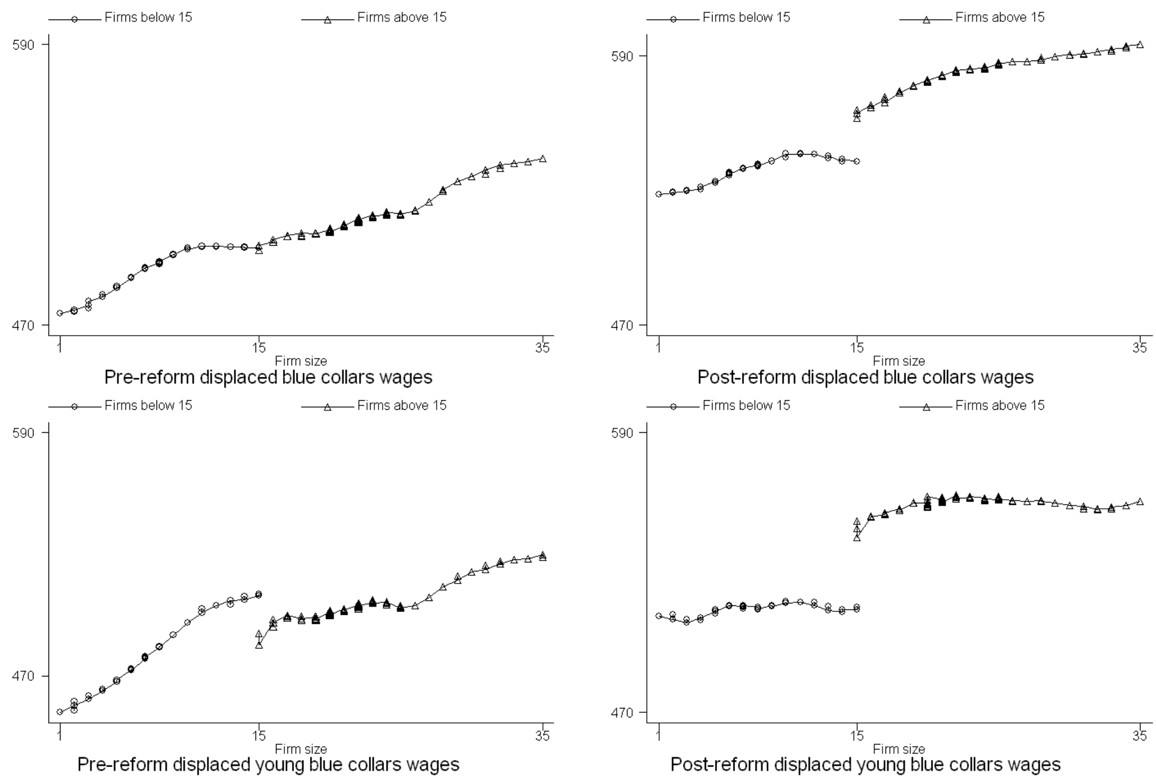

Figure 2: Non parametric prediction of the average log real wage from a weighted local linear regression smoother with bandwith 0.8, estimated separately for each side of 15 employees threshold. Pre-reform period: 1986-1989. Post-reform period: 1991-1994.

and females, descriptive statistics of the covariates for firms above and below the 15 employees threshold before and after the reform. Table 1 shows that the age and the percentage of blue collars in firms above and below the threshold are not significantly different in small and large firms neither before nor after the reform, thus suggesting that the covariates are independent of treatment status, at least around the threshold.

Table 1 also illustrates the difference between the average real wages in small and large firms, before and after the reform. The table pools all years and shows very little difference between small and large firms, both before and after the reform. However, the sample average, even in a narrow neighborhood of the threshold, is in general a biased estimate of the true conditional expectation function at the threshold when the function has non-zero slope. To address this problem we turn to a regression model and estimate the size of the discontinuity including a polynomial approximation. In particular, wages are regressed on polynomials of various orders in firm size separately for each side of the threshold. The next section illustrates the regression model.

4.2 Regression model

The ideal experiment to measure the effect of a change in EPL would have firms and workers exogenously assigned to the treatment status. If this was the case, simple OLS estimates of the following model would deliver the causal effect of EPL on wages:

$$\begin{aligned} \log w_{ijt} &= \beta^{'} X_{ijt} + \delta_0 Post + \delta_1 D^S_{jt} + \delta_2 \left(D^S_{jt} \times Post \right) + u_{ijt} \qquad (1) \\ D^S_{jt} &= 1 \left[\text{firm size } \leq 15 \text{ in year } t \right] \\ Post &= 1 \left[\text{year} \geq 1991 \right] \end{aligned}$$

The dependent variable 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 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 variable $Post$ is a dummy that takes the value of 1 from 1991 and zero otherwise; D^S_j 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 matrix X_{ijt} contains a polynomial in firm size for each side of the threshold. In some specifications, baseline covariates are included in the regression to reduce the sampling variability of the estimates. Our most complete specification includes worker characteristics such as a quadratic in age and occupation (white collar/blue collar dummy); firm characteristics such as the geographical location and industry effects. We also control for time effects.

In any case, if the covariates are independent of treatment status the estimates are expected to be insensitive to the inclusion of those covariates.

4.3 The IV model

The increase in EPL in 1990 applied only to firms with fewer than 15 workers. However it is possible that marginal firms which kept their size just below 15 before the reform to avoid strict EPL rules, increased their size *because* of the reform. As an instrument for the firm size dummy, we use firm size (above/below 15 employees) in 1989, which is strongly correlated with size in other years but is not affected by the reform. Formally, the IV specification looks as follows:

$$\log w_{ijt} = \beta' X_{ijt} + \delta_0 Post + \delta_1 D^S_{jt} + \delta_2 \left(D^S_{jt} \times Post \right) + \upsilon_{ijt} \quad (2)$$

$$D^S_{jt} = \gamma_0 X_{ijt} + \gamma_1 Post + \gamma_2 D^S_{j89} + \nu_{jt} \quad (3)$$

the instrument being D^S_{j89} the firm size dummy in 1989.

5 The effects of the 1990 reform

5.1 Results on the sample of New Hires

Table 2 reports the coefficients and standard errors of equation (1) estimated on the sample of New Hires. This sample includes all workers starting a new job, coming either from another firm or from outside the sample.

Panel A of Table 2 shows the results for male workers. The effect of interest is captured by the interaction between the post-reform dummy and a dummy for firms under 15 employees. The specifications in columns 1-3 also include a third degree polynomial in firm size for each side of the fifteen employees threshold.[9] Column 1 refers to the full sample. Column 2 and 3 restrict the focus on blue collars and young blue collars. All specifications show no significant effects of the 1990 EPL reform on the average wages of male workers.[10]

Panel B of Table 2 shows the results for female workers. The coefficient of interest is positive and significant on the full sample and on the sample of blue collar workers (Columns 1 and 2). This goes contrary to our hypothesis on the effect of EPL on wages of new entrants. We do not have a good explanation of why this is the case. We speculate that the effect

[9] The results carry over to both quadratic and quartic polynomials in firm size.
[10] The reported robust standard errors allow for clustering by individual.

of EPL on female wages is more difficult to interpret because of the changes in labor force participation in Italy and because of maternity-related employment decisions.

In any case, we underline that these results are obtained on a sample where the treatment status is unlikely to be exogenous. We focus next on the sample of the displaced where the coefficients on the sample of females will turn out to be no longer significant.

5.2 Results on the sample of Displaced Workers

As mentioned above, it is plausible that workers sort themselves into (or out of) the treatment group depending on their preferences on the trade-off between wages and job security. This implies that the treatment status is not exogenous.

In an attempt to control for the sorting of workers, we select a sample of workers exogenously displaced as a consequence of plant closings. Arguably, their allocation in firms below or above the threshold is "more random" than the allocation of those who voluntarily change firms. Table 3 shows the results from OLS estimates. Column 1 provides estimates on the full sample and shows no significant effects neither for males (panel A) nor for females (panel B). In column 2, where baseline covariates (year dummies, sectoral and regional dummies, a quadratic in age and occupation) are added, the results are unchanged. In Column 3 and 4 a negative and significant effect of the reform on blue collar wages appears for males (Panel A). The point estimate lies around 3% and is robust to the inclusion of covariates. No effect appears for females (Panel B). Finally, columns 5 and 6 consider the subsample of young (below 30 years old) blue collars and show that average male wages decreased in small relative to large firms after the reform by more than 7%. Again, wages of young blue collar females do not show significant effects.

5.2.1 IV results

Self-selection into the treatment status may also affect firms: marginal firms, which kept their size just below 15 before the reform, may decide to cross the 15 employees threshold *because* of the change in EPL. To control also for the sorting of firms, we instrument the treatment status (the dummy firm size lower than 15 employees) using firm size in 1989, when the reform was not in place and was arguably unexpected.

Table 4 reports the coefficients and standard errors obtained from the estimation of equations (2) and (3) estimated on the sample of displaced workers. Regarding males (Panel A), we find a negative and significant effect of the reform on the full sample (column 1), the subsample of blue collars (column 2) and the subsample of young blue collars (column 3). The point estimates range from 6.7% to around 10%. As to females, a negative and significant effect of about 5% appears only in the subsample of blue collars (Panel B, column 2).

These findings may be nicely interpreted within the Lazear's neutrality framework. In an insider-outsider wage framework, one can think of workers as *Outsiders* with no job security provisions in the first period and then *Insiders* entitled with job security provisions. In this framework steady state wages take 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_j, for $j \in \{i, o\}$, denotes, respectively, the wage of the insiders and the wage of the outsiders, p is productivity, rU the worker outside option and β the bargaining power of the worker. The outsider status lasts for $\frac{1}{\lambda}$ periods. Reducing appropriately the outsider wage, the firm can make the worker pre-pay entirely the severance payment S. 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, along the transition that follows a reform introducing firing costs, insider wages are not affected by S and the scheme does have distributive effects. We should expect that entry wages are negatively affected by an increase in EPL while insider wages are unaffected. This theoretical prediction is also reinforced by the fact that in Italy collective bargaining agreements, though binding for both insiders and outsiders, arguably leave larger room for individual bargaining at the entry stage rather than at a later one.[11]

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 average wages using a reform of EPL in Italy which increased severance payments after 1990 for firms with fewer than 15 employees relative to larger firms.

If workers and firms are exogenously assigned to the treatment status, a Regression Discontinuity Design will identify the causal effect of EPL on wages measuring the difference between wages in small and large firms around the threshold before and after the reform. However the results may be biased if firms and workers sort around the fifteen employees

[11] For details on collective bargaining agreements in Italy see Dell'Aringa and Lucifora (1994).

threshold. The direction of the sorting of firms around the threshold following the EPL increase should be in principle clear. The increase in EPL in 1990 applied only to firms with fewer than 15 workers, thus closing the gap in employer protection provision between firms below and above the 15 employees threshold. This may have induced firms, which kept their size marginally below the threshold to avoid expensive EPL, to increase their size above 15. Unlike firms, the key mechanisms determining the sorting of workers are more complex. In general workers' choices for firms below or above the threshold will depend on their preferences for the mix of EPL and wages prevalent in small rather than big firms and on a host of unobserved characteristics of firms and workers.

We address workers sorting by considering a sample of workers exogenously displaced due to plant closings, and firm sorting by instrumenting the treatment status using firm size prior to the reform. We find that average wages of male workers in firms below 15 employees declined by around 6.7% because of the 1990 EPL reform.

These estimates suggest that the wage cuts do not undo the effect of EPL completely. The present discounted value of the expected firing cost for a worker with a (residual) tenure of $T = 8$ years is given by $w \times$firing cost$\times \left(\frac{1}{1+r}\right)^T$.[12] The wage effect of EPL, i.e. the estimated reduction of the entry wage due to the increase in EPL $w \times 12 \times 0.067$. Thus, if $r = 0.03$ and, on average, the number of monthly wages paid as firing costs is equal to 4, the ratio of the wage cuts due to EPL to expected firing cost is given by: $\frac{0.067 \times 12}{4 \times \left(\frac{1}{1.03}\right)^8} = 0.254$. In other words, our estimates suggest that as much as 25% of the firing cost is shifted onto a lower wage.[13]

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[12] w denotes the monthly wage; firing cost is the number of monthly wages to be paid as firing costs; r is the interest rate; T is the average residual tenure in the sample, calculated as the end date of the job minus the current date.

[13] If the residual tenure is 6 years and the number of monthly wages paid as firing costs is equal to 3, the ratio of the wage cuts due to EPL to expected firing cost is a very similar 24%.

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

	Pre-reform	**Post-reform**		**Pre-reform**	**Post-reform**		
	Small firms (10-15)		*Diff*	Large firms (16-20)		*Diff*	*Diff-in-Diff*
				PANEL A. MALES			
Average log wage	6.23	6.32	*0.089*	6.245	6.361	*0.115*	*0.026*
	(0.25)	(0.237)	*[0.011]*	(0.219)	(0.253)	*[0.013]*	*[0.017]*
Age	38.503	37.69		37.168	38.604		
	(10.911)	(10.135)		(10.541)	(10.378)		
White collars %	0.118	0.14		0.12	0.132		
	(0.322)	(0.347)		(0.325)	(0.339)		
Yearly average size of the firm	12.17	12.355		17.809	17.913		
	(1.708)	(1.738)		(1.34)	(1.457)		
N	1002	844		832	560		
				PANEL B. FEMALES			
Average log wage	6.067	6.127	*0.060*	6.075	6.126	*0.051*	*-0.009*
	(0.224)	(0.258)	*[0.014]*	(0.216)	(0.211)	*[0.014]*	*[0.02]*
Age	30.961	31.411		29.316	31.625		
	(8.275)	(7.709)		(7.513)	(7.816)		
White collars %	0.231	0.284		0.201	0.219		
	(0.422)	(0.451)		(0.401)	(0.414)		
Yearly average size of the firm	12.663	12.528		17.978	18.083		
	(1.678)	(1.756)		(1.361)	(1.424)		
N	641	504		497	411		

Notes: Standard deviations in parentheses. Standard errors in square brackets.

Table 2: New hires in years 1986-1994 (excl. 1990). OLS estimates.

	(1)	(2)	(3)
Dependent Variable: log wage	Full sample	Blue Collars	Young Blue Collars
		PANEL A. MALES	
Post 1990	0.056	0.052	0.050
	(0.004)	(0.004)	(0.004)
Small firms	-3.292	-3.634	-0.404
	(6.829)	(6.043)	(7.376)
Post 1990 × Small firms	0.003	0.009	0.007
	(0.005)	(0.004)	(0.006)
N	51274	43056	22620
		PANEL B. FEMALES	
Post 1990	0.051	0.034	0.026
	(0.004)	(0.004)	(0.004)
Small firms	14.527	7.723	0.483
	(6.601)	(6.471)	(6.44)
Post 1990 × Small firms	**0.023**	**0.009**	0.007
	(0.005)	(0.005)	(0.005)
N	35139	23374	16404

Notes: Only firms between 10 and 20 workers are included. Robust standard errors in parentheses allow for clustering by individual. All specifications include a third degree polynomial in firm size for each side of the threshold. Young below 30.

Table 3: Displaced workers in years 1986-1994 (excl. 1990). OLS estimates

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable: log wage	Full sample		Blue Collars		Young Blue Collars	
			PANEL A. MALES			
Post 1990	0.118	0.031	0.113	0.031	0.117	0.161
	(0.014)	(0.019)	(0.013)	(0.019)	(0.029)	(0.041)
Small firms	-13.909	-5.429	-5.327	1.276	-4.326	3.051
	(21.275)	(19.135)	(19.416)	(19.136)	(37.868)	(36.19)
Post 1990 × Small firms	-0.029	-0.025	**-0.034**	**-0.030**	**-0.098**	**-0.070**
	(0.018)	(0.017)	(0.017)	(0.017)	(0.034)	(0.031)
N	3238	3238	2723	2723	708	708
			PANEL B. FEMALES			
Post 1990	0.052	-0.009	0.023	-0.005	0.018	0.021
	(0.016)	(0.02)	(0.013)	(0.02)	(0.015)	(0.026)
Small firms	-32.799	-29.928	-4.545	-5.368	-4.814	-9.419
	(25.108)	(21.651)	(22.68)	(22.153)	(21.586)	(21.929)
Post 1990 × Small firms	0.008	-0.009	-0.004	-0.016	-0.026	-0.043
	(0.022)	(0.018)	(0.019)	(0.018)	(0.025)	(0.025)
N	2053	2053	1520	1520	806	806
Controls	NO	YES	NO	YES	NO	YES

Notes: Only firms between 10 and 20 workers are included. Robust standard errors in parentheses allow for clustering by individual. All specifications include a third degree polynomial in firm size for each side of the threshold. Young below 30. Controls: year dummies, sectoral and regional dummies, age, age squared, occupation (white collar/blue collar dummy).

Table 4: Displaced workers in years 1986-1994 (excl. 1990). IV estimates.

	(1)	(2)	(3)
Dependent Variable: log wage	Full Sample	Blue Collars	Young Blue Collars
	PANEL A. MALES		
Post 1990	0.134	0.144	0.110
	(0.02)	(0.019)	(0.037)
Small firms	-0.243	-0.357	-0.064
	(0.029)	(0.027)	(0.05)
Post 1990 × Small firms	**-0.067**	**-0.091**	**-0.101**
	(0.029)	(0.027)	(0.05)
N	2547	2215	627
	PANEL B. FEMALES		
Post 1990	0.036	0.035	0.024
	(0.019)	(0.016)	(0.019)
Small firms	0.796	0.828	0.683
	(0.458)	(0.461)	(0.664)
Post 1990 × Small firms	0.039	**-0.05**	-0.037
	(0.033)	(0.026)	(0.036)
N	1581	1191	650

Notes: Only firms between 10 and 20 workers are included. Robust standard errors in parentheses allow for clustering by individual. The treatment status (above/below 15 employees) is instrumented with the size dummy in 1989 (above/below 15 employees in 1989) .