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

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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 entry wages were around 10% lower on average in small firms because of the 1990 EPL reform and the returns to tenure increased by as much as 5%.

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

JEL Classification: E24, J63, J65.

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1 Introduction

Employment Protection Legislation (EPL) is a set of laws which rules the dismissal of employees and determines the total firing cost paid upon workers' dismissal. Many papers have studied the effect of changes in EPL on employment and on job flows from both a theoretical and an empirical perspective. While it is by now well understood that the general equilibrium effects of EPL depend crucially on the modelling assumptions concerning wages (Ljunqvist (2002), Garibaldi and Violante (2005)), scant attention has been devoted to the investigation of the empirical effect of EPL on wages. The primary purpose of this paper is to estimate the effect of EPL on wages using a natural experiment from Italy. We focus on the effect of the change in EPL on entry wages and on the tenure-earnings profile.

The firing cost consists of two distinct parts: a transfer from the employer to the employee (severance payment) and a tax (e.g. trial costs). Lazear (1990) pointed out that, while the tax part of the firing cost cannot be part of the negotiations between employers and employees, the transfer part of EPL can be undone by changes in wages in a flexible wage framework. In absence of contractual or market rigidities, the firm raises wages one for one with the severance payment and requires the worker to pay a fee (post a bond) upon entry that reflects the value of the stream of the future severance payments. This scheme leaves the expected present value of the cumulative wage bill (inclusive of the severance payment) unchanged. Thus, Lazear (1990) predicts that EPL lowers entry wages and raises the tenure-wage profile in a way that generates no real effects, i.e. no effects on employment. However, in presence of contractual rigidities or of market imperfections this scheme may not be feasible and EPL may have real effects.

Guell (2003), for instance, shows that in an efficiency wages framework, where the worker's effort can only be monitored imperfectly, severance payments increase wages in equilibrium. Since the transfer increases the value of being unemployed, and therefore makes the punishment for shirking less effective, firms reduce labor demand and raise wages to restore the incentives to work. Garibaldi and Violante (2005) show that the impact of severance payments on employment differs according to the bite of wage rigidity. If entry wages are not responsive to EPL (for example because of minimum wages), severance payments may increase unemployment. Differently, if entry wages are flexible, EPL reduces unemployment even if insiders' wages are rigid (for example because of union bargaining). Ljunqvist (2002) shows that the effects of firing taxes on employment depend on the model of employment and wage determination (competitive, matching or search model) and on the specific assumption on how lay-off costs affect the bargaining game between firms and workers.[1] A common prediction of Ljunqvist (2002) and Garibaldi and Violante (2005) is that flexibility of wages at entry is sufficient for firing costs (severance payments or taxes) to reduce unemployment in matching models. Thus, the general equilibrium effects of EPL crucially depend on how wages react to EPL. However, no empirical evidence on the effect of EPL on wages is available.

This paper addresses this question and provides a test of Lazear's theoretical result – typically named "bonding critique"[2] – by assessing the effect of EPL on entry wages and on the tenure-earnings profile. We do so by using a natural experiment which increased the transfer part of EPL in 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 firm size. The identification assumption, further discussed in section 4, is essentially that the average outcome for individuals marginally above the 15 employees threshold represents a valid counterfactual for the treated group just below the threshold.

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. 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. In the same way workers with different preferences over a menu of employment protection and wages may have been induced to move from a

[1] The key difference bewteen Ljunqvist (2002) and Garibaldi and Violante (2005) is that the former focuses on firing taxes and the latter on the transfer part of EPL. In the empirical analysis, it is difficult to distinguish a transfer from a tax. In our case, we consider a change in EPL that obliges small firms to pay severance. Nevertheless, the reform also entails a tax in as much there is an additional possibility to go to court.

[2] The term bonding critique is originally due to Lazear (1981) and refers to the literature on compensation design. Firms initially pay lower wages than the outside option and later pay higher wages than the outside otion to discourage shirking when monitoring is imperfect. Such delayed payment/bonding contracts are efficient because they leave the present discounted value of compensation unchanged. If legal limits or minimum wages impede the up-front bond, firms have to pay efficiency wages to motivate workers.

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 deal with 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 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.

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.

Our results are easy to summarize. We first define a sample of new hires i.e. we follow those workers who are newly hired in the years 1986-1994 as long as they stay with the same firm. OLS results on the sample of new hires show an insignificant effect of the reform both on the starting wage and on the tenure-wage profile. However, identification on this sample implies that the treatment assignment to a firm smaller than 15 is independent of the EPL reform.

We therefore move to a sample of displaced workers due to plant closings i.e. of workers who were displaced due to plant closing in the previous two years. Displaced workers who presumably were looking for work before the reform took place are more unlikely to sort themselves in the treatment group *because* of the reform.

The estimates obtained on the "Sample of Displaced" indicate no effect on the starting wage and a flatter tenure-wage profile. These estimates are robust to the inclusion among the regressors of polynomials of various orders in firm size 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 two percent a year in the returns to tenure in small firms relative to large firms after the reform.

The OLS estimates do not address the endogeneity of treatment status on firms' side.

Therefore, we complement the OLS results with IV estimates where we instrument the treatment status with the size of the firm in year 1989, when the reform was still unexpected. The IV results indicate that the reform had a significative negative impact on the entry wages of displaced workers around 10%. In particular, the entry wages of blue collar males decreased by as much as 15%. Moreover, the IV estimates suggest a positive effect of the EPL reform on the tenure-wage profile of as much as 5%. On the basis of these estimates, we quantify the extent to which higher firing costs are translated onto lower wages, and find it to range between 0 and 34%.

1.1 Previous Literature

Previous literature has studied the effects of EPL in Italy comparing the different firing costs regimes that apply to firms below and above 15 employees. 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.[3] 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. Section 6 concludes.

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

2 The institutional background

2.1 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 reinstate 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 by firms with more than 60 employees, and firms with fewer than 35 workers were completely exempt.[4]

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 reinstate workers and pay foregone wages in case of unfair dismissals.[5] 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.[6]

2.2 Wage Formation in Italy

The effect of EPL on wages depends on the diffusion of company-level bargaining and on the importance of the firm-specific wage components. In Italy there are three levels of wage bargaining, economy-wide, industry-wide and company-level agreements. In terms of diffusion, half of Italian workers were involved in firm-level negotiations in the period

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

[5] 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)).

[6] 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).

covered by our sample.[7] The presence of a company-agreement increases with firm size. In terms of magnitude of the firm-specific part of the wage, estimates based on data in the metal products, machinery and equipment industry indicate that between one sixth and one quarter of the compensation is firm-specific.[8] Overall, an important part of the compensation of employees (company-level wage increments, production bonuses and other variable compensations) is determined at the firm level (Guiso et al. (2005)). This opens up the possibility for EPL changes to affect firm-level wages. 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 VWH data set is an employer-employee panel with information on workers and firm characteristics in the private sector. The longitudinal panel is constructed from the administrative records of the Italian Social Security System (Inps). It refers to the entire population of employers and workers of the private sector in two provinces, Treviso and Vicenza, of the Italian region of Veneto. The two provinces are located in the North-eastern part of the country. In year 2000 GDP per capita was 22,400€, 20% higher than the national average and accounted for 3.3% of the Italian GDP. The overall population was 1.6 million people (2.7% of the total Italian population) as of the 2001 Population Census. Although limited to two relatively small provinces, the data are well suited for studying the effect of the 1990 EPL reform because the Italian North-East is characterized by a high concentration of small firms and tight labor market.[9]

The data include universal information on all plants and employees working at least one day in any plant of the two provinces from 1984 to 1994. The unit of observation is the employer-day; such information is used to build a monthly history of the working life of each employee. Once they are in the dataset, employees are followed, independently of their place of residence, even in their occupational spells out of Treviso and Vicenza.

[7] Yearly report of CESOS, an association of trade unions.

[8] See Erickson and Ichino (1995) for further details on wage formation in Italy for the period covered by our data.

[9] The average establishment size in Veneto is 13 employees. Half of the employment stock is not subject to protection against dismissal as stated by art. 18 of the Statuto dei Lavoratori. For a decade Veneto has been also a full employment region with a positive rate of job creation in manufacturing, compared to a negative national rate and positive migration flows. Typical manufacturing activities are garments, mechanical goods, goldsmiths, leather, textile, furniture and plastics. The stock of manufacturing workers in the two Veneto provinces of Treviso and Vicenza has varied between 194.000 employees in the early eighties and 233.000 employees in 1996, with a yearly positive average rate of variation of 1.4%. The average rate of growth in employment is the result of a marked increase in white collars and women (see Tattara and Valentini, 2005).

The only reason of dropping out of the dataset is exit from the private sector or from the employment status altogether. In fact, the individual longitudinal records are 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 stops following individuals who move into self-employment, the public sector, the agricultural sector, the underground economy, unemployment, and retirement.

Among firm and workers characteristics, the data include 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.[10]

3.1 Sample selection rules

We select all males of age between 20 and 60. 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.

In a classical randomized experiment, a sharp RD design requires a dichotomous treatment that is a deterministic function of a single variable with a known point of its support where the probability of being treated changes from 0 to 1. In our case, the treatment status depends on firms size and participation to the treatment group changes discontinuously at the 15 employees threshold after the 1990 reform. The key identifying assumption is that

[10] Employers are identified by their identification number, which changes when ownership of the firm changes. Therefore the variable "date of closure" does not always correspond to a real closure as it may also capture an ownership transfer. We label false closures (e.g. closures due mergers or acquisitions) all episodes where more than 50% of the employees of the closing firm are found in another firm.

the relation between wages and firm size, in absence of the reform, would be continuous at the threshold. Formally, the mean value of wages is a continuous function of firm size S at the threshold $\bar{s}$. The continuity assumption is written as:

$$E\left\{w_0 \mid \bar{s}^+\right\} = E\left\{w_0 \mid \bar{s}^-\right\} \tag{1}$$

where w_0 is the wage in the absence of the reform and $\bar{s}^+$ and $\bar{s}^-$ refer to units marginally above or below $\bar{s}$. This condition for identification requires that in the counterfactual world, no discontinuity takes place at the threshold.

In the empirical analysis, we identify the mean effect of the 1990 EPL reform on wages for a worker of a firm in a neighborhood of the cut-off point as:

$$\left\{E\left\{w \mid \bar{s}^-\right\} - E\left\{w \mid \bar{s}^+\right\}\right\}_{post\ 1990} - \left\{E\left\{w \mid \bar{s}^-\right\} - E\left\{w \mid \bar{s}^+\right\}\right\}_{pre\ 1990}$$

The identification assumption in a RD design is essentially that the average outcome for individuals marginally above the threshold represents a valid counterfactual for the treated group just below the threshold.[11]

The identification assumption in RD implies that close to the threshold all variables determined prior to assignment will be independent of treatment status. Table 1 shows that the treatment and control groups are similar in terms of observable characteristics around the threshold.

The strategy to identify the impact of the change in dismissal costs is illustrated in Figure 1. The upper panel of the Figure considers a sample of new hires, 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. The upper panel of figure 1 plots the mean wage (including both entry wages and subsequent wages) 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.

The upper panel of 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

[11] 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. In this regard, 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.

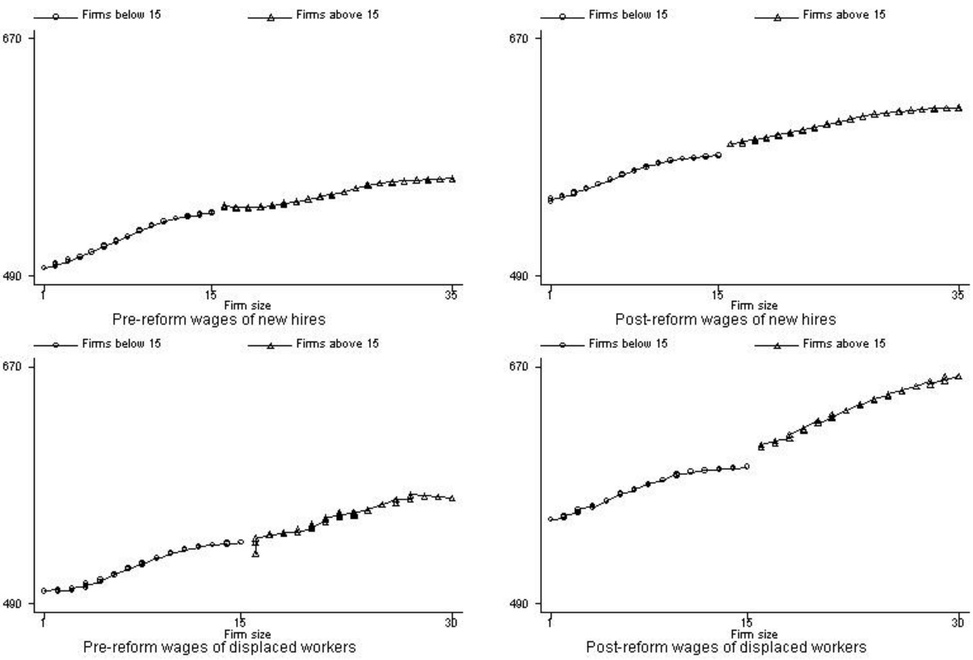

Figure 1: Non parametric prediction of the average log real wage from a weighted local linear regression smoother with bandwith 0.8, estimated separately for each side of 15 employees threshold. Pre-reform period: 1986-1989. Post-reform period: 1991-1994.

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.

4.1 Workers' sorting

Figure 2 shows the probability that workers move to firms bigger or smaller than 15 employees (and therefore subject to different firing costs regimes) before and after the reform. In the Figure, in each panel we estimate the following linear probability model:

$$y_{ij't} = \alpha + \beta_1 S_{jt-1} + \beta_2 S_{jt-1}^2 + \beta_3 S_{jt-1}^3 + \varepsilon_{ijt}$$

where $y_{ijt} = 1$ if worker i moves in year t from firm j of size S_{jt-1} (in the horizontal axis in Figure 2) to a firm j' with more than 15 employees (upper panel of figure 2) or a firm

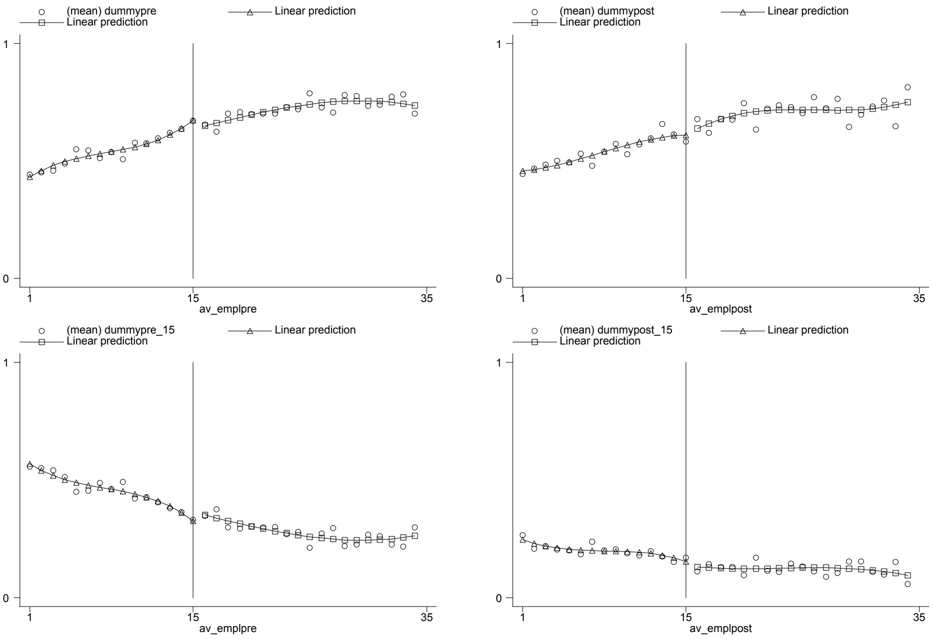

Figure 2: Upper panel: probability of moving to firms with 16 or more employees in the pre-reform period (upper left) and the post reform period (upper right). Lower panel: probability of moving to firms with 15 or fewer employees in the pre-reform period (lower left) and the post reform period (lower right).

with fewer than 15 employees (lower panel of Figure 2). We split the sample in two periods, the pre-reform period (from 86 to 89) and the post-reform period (from 91 to 94). Figure 2 depicts both the raw proportion of movers and the fitted probability against firm size.

Figure 2 shows a smooth pattern of between-firm mobility both before and after the reform. If the reform lowers the wage in small firms relative to big firms, one may expect larger flows of workers from small to big firms and smaller flows from big to small firms after the reform. At first glance, no discontinuities appear after the reform. However, it may be the case that the movers before and after the reform differ according to unobservable characteristics. For example, after the reform higher ability workers have larger incentives to move to large firms.

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 are less likely than those who voluntarily change firm to sort themselves in firms around the threshold only because of the reform.

The bottom panel of figure 1 plots the relationship between wages and firm size for a sample of displaced workers. It shows evidence of a discontinuous jump in the relationship between firm size and wages right at the 15 employees threshold after the reform. This 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 (discussed in section 4.4).

4.2 Descriptive statistics

Table 1 considers the sample of displaced male workers in firms between 10 and 20 employees and provides descriptive statistics of the covariates for firms above and below the 15 employees threshold before and after the reform. It shows that the age, the percentage of blue collars and the tenure 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.[12]

Table 1 also shows a significant difference between the average real wages in small and large firms, before and after the reform. The mean wage paid in small firms after the reform is 2% lower than the wage paid in large firms. 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 in firm size. In particular, wages are regressed on polynomials of various orders in firm size. The next section illustrates the regression model.

4.3 Regression model

The previous discussion suggests that the EPL reform may have affected wages differently at entry and during the employment relationship. The empirical analysis will therefore estimate the effect of the reform on entry wages and on the tenure-earnings profile. The ideal experiment to measure the effect of a change in EPL would have firms and workers

[12]Tenure is measured in years starting from zero upon entry in a new firm after displacement, thus it ranges from 0 to 3 in the pre-reform period and from 0 to 8 in the post reform period.

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:

$$
\begin{aligned}
\log w_{ijt} &= \beta' X_{ijt} + \delta_0 Post + \delta_1 D_{jt}^S + \delta_2 \left(D_{jt}^S \times Post\right) + \alpha_0 Ten_{ijt} + \alpha_1 \left(Ten_{ijt} \times D_{jt}^S\right) \\
&\quad + \alpha_2 \left(Ten_{ijt} \times Post\right) + \alpha_3 \left(Ten_{ijt} \times D_{jt}^S \times Post\right) + u_{ijt} \qquad (2) \\
D_{jt}^S &= 1\left[\text{firm size } \leq 15 \text{ in year } t\right] \\
Post &= 1\left[\text{year} \geq 1991\right]
\end{aligned}
$$

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.

The variable $Post$ is a dummy that takes the value of 1 from 1991 and zero otherwise; D_{jt}^S is a dummy that takes the value of 1 if the worker is employed in year t in a small firm and 0 if the worker is employed in a big firm. Ten_{ijt} is tenure of worker i in firm j at time t starting from 0 in the year of entry in the new firm. The interaction term $D_{jt}^S \times Post$ between the small firm dummy and the post-reform dummy is included to capture the effect of the EPL reform on entry wages (i.e. at zero tenure: $Ten_{ijt} = 0$). Similarly, the term $Ten_{ijt} \times D_{jt}^S \times Post$ identifies the effect of interest on the tenure-earnings profile, i.e. it measures the effect of a one year increase in tenure on the post-reform wages of small firms workers relative to large firms workers. The matrix X_{ijt} contains a polynomial of third degree in firm size. In some specifications, baseline covariates are included in the regression to reduce the sampling variability of the estimates. Our most complete specification includes a quadratic in workers age and occupation (white collar/blue collar dummy), the geographical location of the firm (four dummies) and industry effects. We also control for time effects.

4.4 Firms' sorting and the IV model

The average firm size in Italy is approximately half of that of the European Union. EPL is often indicated as the main responsible for such a skewed size distribution. Schivardi and Torrini (2004) and Borgarello, Garibaldi and Pacelli (2003) find that more stringent job security provisions hampers firm growth. They find that the discontinuous change in EPL at the 15 employees threshold reduces by 2% the probability that firms pass the threshold. Although the effect is quantitatively modest, this finding suggests that firms in a neighborhood of the threshold may vary their size in response to the 1990 change in EPL. The increase in EPL in 1990 applied only to firms with fewer than 15 workers, therefore 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 deal with this problem we use an IV strategy. As an instrument for the firm size dummy, we use firm size (above/below 15 employees) in 1989. This instrument is not affected by the reform as long as the reform was unexpected.[13] The first stage regression (also called Intention-to-Treat) of the firm size dummy on the firm size dummy in 1989 (see equation 4 below) yields a coefficient of 0.84 with a standard error of 0.004. In a LATE interpretation (Angrist, Imbens and Rubin (1996)), this indicates that the average effect of EPL on wages is identified over a large proportion of the total population. In fact, the IV model identifies the *causal* effect of EPL on wages only for *compliers*, i.e. workers in firms whose treatment status (the firing cost regime) depends on the assignment (firm size in 1989). Formally, the causal effect of EPL on wages is the following:

$$\frac{E\left\{w \mid \bar{s}^{-}\right\} - E\left\{w \mid \bar{s}^{+}\right\}}{E\left\{D^S_{j89} \mid \bar{s}^{-}\right\} - E\left\{D^S_{j89} \mid \bar{s}^{+}\right\}}$$

The above ratio is the mean impact on those subjects in a neighborhood of $\bar{s}$ who would switch their treatment status if the threshold for participation (the firm size dummy in 1989) switched from just above the threshold to just below it. Notice that the denominator identifies the causal effect of the instrument on the treatment status, i.e. the proportion of compliers at $\bar{s}$.

Equivalently, in an IV framework the formal specification looks as follows:

$$\begin{aligned}
\log w_{ijt} &= \beta' X_{ijt} + \delta_0 Post + \delta_1 D^S_{jt} + \delta_2 \left(D^S_{jt} \times Post\right) + \alpha_0 Ten_{ijt} + \alpha_1 \left(Ten_{ijt} \times D^S_{jt}\right) \\
&\quad + \alpha_2 \left(Ten_{ijt} \times Post\right) + \alpha_3 \left(Ten_{ijt} \times D^S_{jt} \times Post\right) + \upsilon_{ijt} && (3) \\
D^S_{jt} &= \gamma_0' X_{ijt} + \gamma_1 Post + \gamma_2 D^S_{j89} + \gamma_3 \left(D^S_{j89} \times Post\right) + \gamma_4 Ten_{ijt} + \gamma_5 \left(Ten_{ijt} \times D^S_{j89}\right) \\
&\quad + \gamma_6 \left(Ten_{ijt} \times Post\right) + \gamma_7 \left(Ten_{ijt} \times D^S_{j89} \times Post\right) + \nu_{jt} && (4)
\end{aligned}$$

All terms interacted with D^S_{jt} (respectively $D^S_{jt} \times Post$, $Ten_{ijt} \times D^S_{jt}$ and $Ten_{ijt} \times D^S_{jt} \times Post$) are also instrumented using the interaction with 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 (2) estimated on the sample of New Hires. This sample includes all male workers starting a new job, coming either from another firm or from outside the sample.

[13] We checked that the first published news of the intention to change the EPL rules for small firms appeared in the main Italian financial newspaper – *Il Sole 24 Ore* – at the end of January 1990.

We focus on two coefficients. The effect of interest on entry wages is captured by the interaction $\left(D_{jt}^{S} \times Post\right)$. The effect on the tenure profile is captured by the term $\left(Ten_{ijt} \times D_{jt}^{S} \times Post\right)$. All specifications include a third degree polynomial in firm size.[14] Columns 1 and 2 refer to estimates on the full sample without and with controls respectively. Column 3 to 6 restrict the focus on blue collars and young blue collars.

All specifications, except for blue collar workers, show no significant effects of the 1990 EPL reform neither on entry wages of male workers nor on the tenure-wage profile.[15] The addition of the covariates (year dummies, sectoral and regional dummies, a quadratic in age and occupation) in columns 2, 4 and 6 does not change the results with respect to the corresponding columns 1, 3 and 5. Indeed, if the covariates are independent of the treatment status the estimates are expected to be insensitive to the inclusion of those covariates.

These results are obtained on a sample where the treatment status is unlikely to be exogenous. We focus next on the sample of displaced workers.

5.2 Results on the Sample of Displaced Workers

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. As already explained, for this reason we select a sample of workers exogenously displaced as a consequence of plant closings. Plant closings are defined using information on the firms' date of closure. However, the variable "date of closure" does not always correspond to a real closure as it may also capture an ownership transfer. "False" closures (e.g. closures due mergers or acquisitions) are defined as all those episodes where more than 50% of the employees of the closing firm are found in another firm.

Table 3 shows the results from OLS estimates on the sample of displaced workers. Columns 1 and 2 provide estimates on the full sample and show no significant effects of the EPL reform on entry wages and a negative effect of as much as 2% on the returns to tenure. The same holds for the sample of blue collars in columns 3 and 4 and for the sample of young blue collars in columns 5 and 6. The effect of the reform of EPL on the tenure profile of young blue collars in small firms is twice as large as the effect on the full sample.

The sample of displaced workers is meant to address, in the best possible way, the issue of workers' sorting. However the results may still be biased by the endogeneity of the treatment status on the firm side.

[14] The results carry over to the inclusion of quadratic and quartic polynomials in firm size.

[15] The reported robust standard errors allow for clustering by individual.

5.3 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 the standard errors obtained from the estimation of equations (3) and (4) estimated on the sample of displaced workers. Once we address all sources of endogeneity, we find that the reform affects negatively entry wages and positively the wage-tenure profile in the full sample (columns 1 and 2) and in the subsample of blue collars (columns 3 and 4). The results on the subsample of young blue collars (columns 5 and 6) are insignificant.

Columns 1 and 2 indicate a decrease of entry wages in small firms after the reform of between 9% and 12% and an increase in the tenure profile between 0 and 5%. To put the magnitude of this coefficients in perspective, these results imply that after the reform workers in small firms earn lower wages (relative to big firms' workers) for the first 3.5 years and higher wages thereafter.[16]

At this point it is possible to calculate how much of the increase in the firing cost is translated onto lower wages. We start by considering the situation of a employer-initiated separation of a worker of average tenure in a small firm after the reform. If the separation is ruled unfair by the judge, the firing cost will range between 2.5 and 6 months of the last wage. On the basis of our IV estimates (Column 1, Table 4), the post-reform average weekly wage of an employee of 3.5 years of tenure amounts to $557,000$ Italian liras (approximately 287€). Therefore, the transfer to the worker amounts to $557,000 \times 16$ weeks$= 8,916,221$ Italian liras (approximately $4,604$€) excluding the legal expenses that can be roughly calculated to equal as much as $5,000$€. The above computation results in a very high firing cost, but we should keep in mind that this is the worst possible scenario for the firm. Ex-ante, the firm does not know with certainty whether the separation will be ruled unfair by the court. Furthermore, firms and workers may find a settlement out of court. Galdon Sanchez and Guell (2000), using data based on actual court sentences, estimate that in Italy the probability of reaching an off-court agreement to be around 0.5 and probability that the dismissal is ruled unfair to be about 0.5. Therefore, firms below 15 employees can expect a firing cost equal to $223,000$ Italian liras excluding legal expenses.

[16] This is calculated as the crossing point of the tenure profile before and after the reform, assuming a linear profile.

On the basis of our estimates in Table 4 (Column 1), small firms reduce entry wages by 12.5% and increase the annual returns to tenure by 5%. Thus, after 3.5 years of tenure the cumulative wage loss amounts to $75,940$ Italian liras per week (around 39€). This implies that firms translate around 34% of the expected firing cost onto lower wages. Of course, this calculation is valid only at the average tenure of 3.5 years.[17]

6 Conclusion

This paper provides evidence on the impact of a change in dismissal costs on 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 threshold. 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. The 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 firms' sorting by instrumenting the treatment status using firm size prior to the reform. We find that average entry wages of displaced male workers declined by around 10% in firms below 15 employees, relative to larger firms, because of the 1990 EPL. Moreover, our estimates suggest a positive effect of the EPL reform on the tenure-wage profile of as much as 5%.

These findings may be interpreted within the Lazear's neutrality framework. The Lazear bonding critique predicts that, in absence of contractual or market frictions, a firm can undo

[17] The effect of the reform is to tilt the wage-tenure profile so that workers at low tenure are penalized and high-tenure workers gain higher wages. In particular, a comparison of the pre- and post-reform wage-tenure profile shows that workers earn lower wages for the first 3.5 years and higher wages thereafter. Therefore, the cumulative wage loss with respect to the pre-reform period is at its peak precisely after 3.5 years. Since average tenure in our sample of displaced workers is 3.5 years our back of the envelope calculation represents an upper bound estimate.

a government-mandated transfer (severance payment) reducing the wages of new entrants by an amount equal to the expected increase in the future transfer. Our empirical results are consistent with Lazear's delayed-payment scheme: in the IV estimates where we try to account for the endogeneity problems, we find a reduction in the entry wage in small firms after the reform and an increase in the tenure profile.

These results suggest that firms exploit the existence of wage flexibility at entry in order to make workers pre-pay (part of) the severance payment, possibly anticipating that stricter EPL raises the bargaining power of the insiders.

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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*
Log wage	6.254	6.351	*0.098*	6.259	6.382	*0.123*	*-0.025*
	(0.248)	(0.227)	*[0.008]*	(0.25)	(0.262)	*[0.01]*	*[0.013]*
Age	38.896	40.217		37.925	40.751		
	(10.75)	(10.02)		(10.504)	(10.152)		
White collars %	0.116	0.136		0.127	0.142		
	(0.321)	(0.343)		(0.333)	(0.35)		
Yearly average size of the firm	12.177	12.319		17.827	17.893		
	(1.704)	(1.735)		(1.376)	(1.393)		
Tenure	0.807	2.593		0.816	2.694		
	(0.956)	(2.326)		(0.942)	(2.431)		
N	1497	2178		1221	1397		

Notes: Standard deviations in parentheses. Standard errors in square brackets. Tenure is measured in years starting from zero since entry in a new firm after displacement.

Table 2: New hires 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	
Tenure	0.038	0.018	0.033	0.020	0.029	0.012
	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)
Post 1990	0.062	0.095	0.057	0.089	0.051	0.047
	(0.004)	(0.004)	(0.003)	(0.004)	(0.004)	(0.005)
Small firms	0.003	-0.002	-0.005	-0.005	-0.009	-0.009
	(0.005)	(0.005)	(0.005)	(0.005)	(0.006)	(0.006)
Post 1990 × Small firms	0.004	0.005	**0.010**	**0.008**	0.008	0.008
	(0.005)	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)
Tenure × Post 1990	-0.014	0.001	-0.011	-0.002	-0.006	0.005
	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)
Tenure × Small firms	0.001	0.002	0.002	0.001	0.000	-0.001
	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)
Tenure × Post 1990 × Small firms	-0.004	-0.006	-0.005	-0.004	-0.003	-0.002
	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)
N	120693	120693	100567	100567	44046	44046
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. Young below 30. Controls: year dummies, sectoral and regional dummies, age, age squared, occupation (white collar/blue collar dummy).

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	
Tenure	0.018	0.002	0.009	-0.001	-0.006	-0.014
	(0.009)	(0.009)	(0.008)	(0.008)	(0.014)	(0.016)
Post 1990	0.119	0.133	0.109	0.110	0.094	0.110
	(0.015)	(0.014)	(0.013)	(0.014)	(0.028)	(0.033)
Small firms	-0.002	0.001	-0.030	-0.020	-0.009	-0.020
	(0.021)	(0.018)	(0.019)	(0.018)	(0.038)	(0.036)
Post 1990 × Small firms	-0.014	-0.016	-0.011	-0.013	-0.052	-0.032
	(0.019)	(0.017)	(0.017)	(0.016)	(0.033)	(0.031)
Tenure × Post 1990	-0.011	0.003	-0.004	0.005	0.009	0.014
	(0.009)	(0.009)	(0.008)	(0.008)	(0.016)	(0.017)
Tenure × Small firms	0.027	0.026	0.036	0.031	0.077	0.074
	(0.011)	(0.01)	(0.01)	(0.009)	(0.019)	(0.018)
Tenure × Post 1990 × Small firms	**-0.023**	**-0.023**	**-0.029**	**-0.027**	**-0.052**	**-0.051**
	(0.011)	(0.01)	(0.01)	(0.009)	(0.021)	(0.02)
N	6293	6293	5468	5468	1158	1158
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. 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)	(4)	(5)	(6)
Dependent Variable: log wage	Full sample		Blue Collars		Young Blue Collars	
Tenure	0.046	0.016	0.042	0.028	0.130	0.058
	(0.017)	(0.013)	(0.016)	(0.014)	(0.353)	(0.122)
Post 1990	0.162	0.141	0.168	0.177	0.087	0.007
	(0.044)	(0.034)	(0.04)	(0.036)	(0.357)	(0.399)
Small firms	1.438	0.694	1.085	0.828	3.510	2.794
	(0.655)	(0.5)	(0.597)	(0.545)	(12.554)	(7.772)
Post 1990 × Small firms	**-0.127**	**-0.090**	**-0.150**	**-0.136**	-0.057	0.034
	(0.068)	(0.05)	(0.059)	(0.052)	(0.517)	(0.469)
Tenure × Post 1990	-0.056	-0.022	-0.053	-0.039	-0.100	-0.037
	(0.021)	(0.015)	(0.018)	(0.016)	(0.243)	(0.084)
Tenure × Small firms	-0.029	0.001	-0.028	-0.022	-0.187	-0.137
	(0.029)	(0.021)	(0.028)	(0.025)	(0.684)	(0.428)
Tenure × Post 1990 × Small firms	**0.057**	0.020	**0.058**	**0.050**	0.125	0.089
	(0.032)	(0.023)	(0.03)	(0.027)	(0.353)	(0.22)
N	5357	5357	4639	4639	974	974
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. Young below 30. Controls: year dummies, sectoral and regional dummies, age, age squared, occupation (white collar/blue collar dummy). The treatment status (above/below 15 employees) is instrumented with the size dummy in 1989 (above/below 15 employees in 1989) .