Who pays for it? The Heterogeneous Wage Effects of Employment Protection Legislation*

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November 23, 2010

Abstract

Theory predicts that the wage effects of government-mandated severance payments depend on workers' and firms' relative bargaining power. This paper estimates the effect of employment protection legislation (EPL) on workers' individual wages in a quasi-experimental setting, exploiting a reform that introduced unjust-dismissal costs in Italy for firms below 15 employees and left firing costs unchanged for bigger firms. Accounting for the endogeneity of the treatment status, we find that high-bargaining power workers (stayers, white collar and workers above 45) are almost left unaffected by the increase in EPL, while low-bargaining power workers (movers, blue collar and young workers) suffer a drop both in the wage level and its growth rate.

Keywords: Costs of Unjust Dismissals, Severance Payments, Policy Evaluation, Endogeneity of Treatment Status.
JEL Classification: E24, J3, J65.

*This is a revised version of a paper previously circulated under the title 'Employment Protection Legislation and Wages'. We are grateful to Giuseppe Bertola, David Card, Ken Chay, Enrico Moretti, and Michele Pellizzari for useful suggestions. Comments from seminar participants at the University of California at Berkeley, Boston College, Georgetown University, the University of Milan, the University of Salerno, the University of Padova, the University of Venezia, the Fifth IZA/SOLE Transatlantic Meeting, and the 7^{th} ECB/CEPR Labour Market Workshop are also gratefully acknowledged. We thank Giuseppe Tattara and Marco Valentini for providing us with the VWH (Veneto Workers History) dataset (Miur Projects 1999–2001 #9913193479 and 2001–2003 #2001134473). The usual disclaimer applies.

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1 Introduction

Since the work of Lazear (1990), it has been well-known that in a perfectly competitive labour market the cost of employment protection legislation (EPL) is fully shifted onto lower wages if dismissal costs entail a transfer from firms to risk neutral workers. Wages are also expected to fall if risk averse workers value job security and are willing to pay for an increase in EPL (Pissarides, 2001; Bertola, 2004). Potentially offsetting the negative effect of EPL on wages, job security provisions may strengthen the bargaining position of workers vis-à-vis employers, allowing them to reap a larger share of the surplus and obtain higher wages in markets where individual or collective negotiation takes place (Mortensen and Pissarides, 1999; Ljungqvist, 2002). Moreover, stricter EPL may raise firms' incentives to invest in training, thereby fostering the accumulation of firm-specific human capital and increasing both productivity and wages (Autor et al., 2003; Wasmer, 2006; Cingano et al., 2010).

Thus, theory predicts an ambiguous impact of EPL on wages, with heterogenous effects possibly stemming from differences in the bargaining positions of workers vis-à-vis employers (Dolado et al., 2007).

This paper attempts to provide evidence for the effects of EPL on workers' individual wages. The analysis is based on data from Italy, one of the strictest countries in terms of employment protection legislation. Italy is an interesting country to study for two additional reasons: First, although in Italy wage determination is to a large extent centralized, an important component of workers' compensation is determined at the firm level in the form of company-level wage increments, production bonuses and other variable benefits (Guiso et al., 2005).[1] Second, the changes in the Italian institutional framework allow us to achieve a clean identification, exploiting EPL variation both across firms and over time. In fact, until 1990 the Italian labour code, enacted in 1970, provided a sharp discontinuity in the application of EPL at the 15 employee threshold, with no protection for workers in small firms and high protection for those in large firms. In July 1990, severance payments were increased from zero to between 2.5 and 6 months of pay for firms with fewer than 15 employees, and left unchanged for firms with more than 15 employees.

We are therefore able to identify the effects of employment protection legislation comparing wages of small versus large firms workers before and after the law change in a neighbour-

[1] In terms of the magnitude of the firm-specific part of the wage, between one sixth and one quarter of the compensation is firm-specific. In terms of diffusion, half of Italian workers were involved in firm-level negotiations in the period covered by our sample. These estimates, based on data in the metal products, machinery and equipment industry are reported by CESOS, an association of trade unions. See Erickson and Ichino (1995) for further details on wage formation in Italy for the period covered by our data.

hood of the 15 employees threshold, thus combining a regression discontinuity design (RDD) with a difference in difference (DID) approach. Our identification assumption is essentially that, after conditioning, the average wages of individuals employed in firms marginally above the 15 employees threshold (16–25) represents a valid counterfactual for the wages of workers employed in firms just below the threshold (5–15) both before and after the reform, i.e., we expect conditional wages in the treated and control groups to diverge after the law change for no other reason than the reform itself.

One natural concern, in our case, is the endogeneity of the treatment status. On the one side, 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 firms sorting into the large or small group according to time-invariant characteristics, we estimate firm fixed effects models. Additionally, we instrument the treatment status with firm size in 1989 and 1988, when the reform was not in place and was unexpected. On the other side, workers may also sort around the 15 employees according to their preferences over the mix of employment protection and wages. To control for workers sorting into large or small firms according to fixed characteristics, we estimate the model using worker fixed effects.

This paper uses administrative data from the Italian Social Security Institute (INPS), and exploits a matched employer–employee panel which contains the entire population of workers and firms located in the Italian provinces of Vicenza and Treviso. Baseline OLS estimates indicate a significant wage loss in small relative to large firms after the 1990 reform that ranges, on average, between 0.5 and 1 percent. The negative effect is, however, highly heterogeneous. Movers suffer a drop in the wage rate in small relative to large firms after the reform of about 2 percent, while incumbent workers seem not to be harmed by stricter EPL. Blue collar in small firms experience a reduction of the wage rate after the reform of about 1.5 percent, whereas white collar are left unaffected. Similarly, wages of workers below 45 in small firms go down by 1 to 2 percent after the reform, while older workers suffer no wage loss. The negative effect is robust to the inclusion of worker fixed effects, firm fixed effects and appears also when firm size is instrumented, suggesting that the sorting of firms may not be a big issue.

In order to investigate whether job security provisions affect not only the level of the wage but also its growth rate, we look at the effects of EPL on year-to-year log wage changes. Results show that small firms workers lose on average 1 percent in terms of wage growth after the reform relative to large firms workers. The specification in changes exhibits a similar pattern as the specification in levels: even though all groups of workers (except white collar) seem to be negatively affected by the reform, the losses are quantitatively stronger among movers, young workers and blue collar.

This pattern of results favours the interpretation that the ability of the employers to shift the cost of EPL onto wages depends on workers' bargaining power. Firms are better able to negotiate lower wages with new entrants rather than to renegotiate incumbents wages. At the same time, it is plausible to think that young and blue collar workers are in a weaker position within the firm compared to older and white collar workers.

While there is a large literature on the effects of EPL on job flows, relative little empirical evidence is available on the wage effects of dismissal costs.[2] Autor et al. (2007) and Cingano et al. (2010) study the effect of EPL on firm-level productivity. Bertola (1990) shows that in high job security countries wages tend to be lower. More recently, using firm-level data, Martins (2009) shows that EPL raises wages in Portugal while Bird and Knopf (2009) find evidence of a relationship between the adoption of wrongful-discharge protections and the increase in labor expenses of U.S. commercial banks.

More related to this paper are the studies conducted on individual data which reach disparate conclusions. Autor et al. (2006) find no evidence that wrongful-discharge laws had a significant impact on wage levels in the U.S.. Cervini Plá et al. (2010) analyse the 1997 reform of Spanish severance pay and payroll taxes and conclude that decreased firing costs and payroll taxes have a positive effect on wages. Van der Wiel (2010) finds opposite results for the Netherlands using a reform that affected differently high- and low-tenured workers.

Our paper is distinct from the available studies in many respects. First, identification is cleanly achieved by means of a Regression Discontinuity Design combined with a Difference in Difference approach. Second, differently from Martins (2008) and Bird and Knopf (2009) we look at individual wages rather than average firm wages. Finally, we look at all workers and not only at displaced workers as Cervini Plà et al. (2010) and we address explicitly the issue of endogeneity of the treatment status with instrumental variables.

The rest of this 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 dis-

[2]Previous empirical literature mostly concentrates on the effects of EPL on employment flows, often using the cross-state variation of EPL within the U.S.. Autor (2003) looks at the effect of EPL on the use of temporary help agencies. Kugler and Saint-Paul (2004) consider re-employment probabilities. Some papers exploit the discontinuities in firing costs regimes that apply to firms of different sizes within countries. Boeri and Jimeno (2005) assess the effect of EPL on lay-off probabilities by comparing firms below and above 15 employees in Italy, while Kugler and Pica (2006) examine the joint impact of EPL and product market regulation on job flows in Italy using both the firm size threshold and a law change. Using a difference-in-differences approach, Bauer et al. (2007) investigate the impact of granting employees the right to claim unfair dismissal on employment in small German firms. Also related is the literature on the effects of changes in payroll tax rates on earnings (Gruber, 1997 and Matsaganis et al. 2010).

tribution. 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 discusses the results and concludes with a back of the envelope calculation of the share of EPL costs translated into lower wages.

2 Institutional background

The Italian labour code favours open-ended contracts over fixed-term or temporary contracts. As a form of worker protection for open-ended contracts, labour codes specify which causes are considered justified causes for dismissal, and establish workers' compensation depending on the reason for the termination. In contrast, temporary contracts can be terminated at no cost provided that the duration of the contract has expired. Labour codes also limit trial periods—that is, the period of time during which a firm can test and dismiss a worker at no cost (in Italy 3 months) and mandate a minimum advance notice period prior to termination (1 month).

Over the years the Italian legislation ruling unfair dismissals has changed several times. Both the magnitude of the firing cost and the coverage of the firms subject to the restrictions have gone through extensive changes. Individual dismissals were first regulated in Italy in 1966 through Law 604, which established that employers could freely dismiss workers either for economic reasons (considered as fair "objective" motives) or in case of misconduct (considered as fair "subjective" motives). However, in these cases workers could take employers to court and judges would determine if the dismissals were indeed fair or unfair. In case of unfair dismissal, employers had the choice to either reinstate the worker 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.

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. The law prescribes that the 15 employees threshold should refer to establishments rather than to firms. In the data we only have information at the firm level. However, this is not likely to be a concern as in the empirical analysis we focus on firms between 5 and 25 employees that are plausibly singleplant firms. Although severance pay in case of dismissal is due only to permanent workers, the labour code computes the 15 employees threshold in terms of full-time equivalents rather than in terms of heads in order to avoid firms bypassing EPL regulations by hiring workers under fixed-term contracts.

Finally, Law 108 was introduced in July 1990 restricting dismissals for permanent contracts. 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. 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.[3] For our purposes, this reform has two attractive features. First, it was largely unexpected: 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. Second, it imposed substantial costs on small firms. Kugler and Pica (2008) look at the effect of this reform on job and workers flows and find that accessions and separations decreased by about 13% and 15% in small relative to large firms after the reform.

3 Data description

This paper uses the VWH data set which is an employer–employee panel with information on the characteristics of both workers and firms. 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 the year 2000, GDP per capita was 22,400 euros, 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.[4] Although limited to two relatively small provinces, the data are well suited for

[3] A further reform was passed in 1991 concerning *collective* dismissals. A special procedure was introduced for firms with more than 15 employees willing to dismiss five or more workers within 120 days because of plant closure or restructuring. According to this procedure, firms were required to engage in negotiations with unions and government to reach an agreement on the dismissals. However, if public administration officials determine that an agreement cannot be reached, the firm is free to downsize and the employees are not allowed to take the firm to court. Kugler and Pica (2008) empirically distinguish the 1990 and the 1991 reforms and find no additional significant effect of the 1991 reform on workers and job flows. Hence, this reform is unlikely to cloud our results. Paggiaro, Rettore and Trivellato (2008) also examine aspects of the 1991 law concerning active labour market policies and find limited effects only on workers aged 50+.

[4] 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

studying the effect of the 1990 EPL reform because the Italian north-east is characterized by a high concentration of small firms and a tight labour market. Moreover, the availability of information on the universe of workers and firms allows of building suitable instruments for firm size and of applying IV techniques.[5] The use of a random sample of the Italian working population would only allow OLS estimates (available upon request).

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 1997. The data include information on employees' age, gender, occupation (blue collar/white collar), yearly wage, number of paid weeks, type of contract (permanent/temporary), and information on firms' location, sector of employment, average number of employees and date of closure. Unfortunately, we have no information on education. The unit of observation is the employer-day; such information is used to build a complete 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.

The only reason of dropping out of the dataset is exit from the private sector or from employment status altogether. Since the individual longitudinal records are generated using social security numbers and collect information on private sector employees for the purpose of computing retirement benefits, employees are only followed through their employment spells. The data stop following individuals who move into self-employment, the public sector, the agricultural sector, the underground economy, unemployment, or retirement.

We select all males of ages between 20 and 55 hired on a permanent basis. We exclude females because in their case the trade-off between job security and wages is likely to be affected by fertility decisions on which we have no information. We also exclude temporary workers because employment protection provisions are guaranteed only to workers on a permanent contract.[6]

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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 1980s 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 collar and women (see Tattara and Valentini, 2005).

[5] Card et al. (2010) investigate the evidence on rent-sharing and holdup on the same data.

[6] A further concern is that firms can bypass the EPL regulation based on the 15 employees threshold hiring workers on a fixed term contract. The definition of the threshold is based on full-time equivalents rather than on heads and therefore leaves little room for firms to circumvent the rule. In particular, the labour code excludes apprentices and temporary workers below nine months, and includes part-time workers and all other temporary contracts in proportion to their actual time. Our dataset records the type of contract (full-time, part-time, apprentices and temporary workers) but does not contain information on the number of hours worked. For this reason, in our estimates the threshold is calculated on the basis of full-time workers on permanent contracts.

We focus on 1989–1993 and remove 1990 because the reform occurred in the month of July and the wages of 1990 are likely to be a mixture of pre-reform and post-reform wages. To preserve the comparability of treatment and control groups, we further select the sample to firms within the interval 5–25 employees. In the course of the paper we use weekly wages after eliminating the upper and lower 1% of the wage distribution in each year. In case the same individual has multiple employment spells in different firms in the same year we keep the longest spell. The final sample is of 9,914 firms and 29,177 workers.

Descriptive statistics for the main variables used in the analysis are shown in Table 1. The number of small firms (5–15) is higher than the number of large firms (16–25), so is the number of workers working in small firms, both before and after the reform. The real weekly wage of workers in large firms is around 312 (331) euros per week before (after) the reform vs. a significantly lower wage of 297 (312) euros per week in small firms before (after) the reform. The year-to-year average wage change is however similar in the two groups in the range of 4-5% per year before the reform and 2.4-2.9% after the reform. The average age of workers is not significantly different across the two groups while larger firms employ a slightly higher proportion of white collar workers.

4 Identification strategy

The estimand of interest is the average treatment effect of EPL on wages. The conditional comparison of wages in small and big firms does not generally provide an unbiassed estimate of the average treatment effect, because firms with different unobservable characteristics may endogenously choose their size and their wages. The fact that in Italy the level of EPL depends on firm size, coupled with the reform of EPL which affected only small firms, can be exploited to build an RDD combined with a DID strategy to estimate the causal effect of EPL on wages.

In order to identify the impact of dismissal costs on wages, 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 other words, the assumption that guarantees that the effect of EPL on wages can be interpreted as causal is that the characteristics of workers and firms should not display any discontinuity at the threshold. Another identification assumption is that the average wage of individuals employed in firms marginally below the 15 employees threshold (5–15) is expected to diverge from the wage of the control group employed in firms just above the threshold (16–25) for no other reason than the law change.

If workers and firms were exogenously assigned to the treatment and control groups, OLS estimates of the following model would identify the causal effect of EPL on wages:

$$
\begin{aligned}
Y_{ijt} &= \beta' X_{ijt} + \delta_1 D^S_{jt} + \delta_2 \left(D^S_{jt} \times Post\right) + \sum_{k=1}^{3} (\gamma_k fsize^k_{jt}) + e_{ijt} \qquad (1) \\
D^S_{jt} &= 1\,[\text{firm size } \leq 15 \text{ in year } t] \\
Post &= 1\,[\text{year} \geq 1991]
\end{aligned}
$$

The dependent variable is the log of the weekly wage paid to worker i by firm j in year t (or the first difference of log wages in the specification in changes, see Table 6) 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 starting from 1991 and zero otherwise; D^S_{jt} is a dummy that takes the value of 1 if the worker is employed in year t in a firm with fewer than 15 employees and 0 if the worker is employed in a firm with strictly more than 15 employees. The interaction term $D^S_{jt} \times Post$ between the small firm dummy and the post-reform dummy is included to capture the effect of the EPL reform. All specifications contain a polynomial of third degree in firm size.[7] The matrix X_{ijt} includes age dummies, an occupation (white collar/blue collar) dummy, nine industry dummies and year dummies which account for macro shocks and predate the main effect of the post-reform dummy. The reported standard errors account for possible error correlations at the individual level.

Equation (1) gives unbiassed estimates only if workers and firms are exogenously assigned to the treatment status. However, individuals may decide to work in small or large firms, and firms in turn may decide to grow above or shrink below the 15 employees threshold. Thus, a fundamental concern of this paper is the non-random selection of workers and firms above and below the fifteen employees threshold. To control for the sorting of workers into large or small firms according to time-invariant workers characteristics, we estimate the model using worker fixed effects. In the same way, to control for the sorting of firms into the large or small group, we estimate a model that includes firm fixed effects and, in an alternative specification, we use an instrumental variable approach to which we now turn.

4.1 Firm sorting: the instrumental variable model

Identification in (1) is threatened by the possibility that firms sort around the 15 employees threshold. Regressions using firm fixed effects control for all time-invariant unobserved factors that may affect the propensity of firms to self-select into (or out of) treatment. However,

[7] Results are robust to this functional form assumption. Alternatively, a split polinomial approximation (as in Lee, 2008) or a local linear regression can be used in RDD regressions. See Imbens and Lemieux (2008) for an overview of different alternatives.

they do not account for the selection due to the reform itself. Firms in the neighbourhood of the 15 employees' threshold may change their size in response to the 1990 reform of EPL, thus biassing the estimates. For example, firms which kept their size just below 15 before the reform to avoid strict EPL rules, may have increased their size *because* the reform made the gap in EPL provisions narrower. The sign of the bias due to firms' sorting is not easy to establish. If firms which were keeping their size below 15 before the reform for fear of incurring a much higher EPL were those with bad growth perspectives and lower wages, then presumably OLS estimates understate the effect of the reform on wages. But it may also be the case that the firms which were keeping under the threshold were instead those which were paying higher wages.

In this section, we assess the validity of the identification strategy discussed in Section 4 with two different testing procedures. First, to formally check for the absence of manipulation of the running variable at 15 (violated if firms were able to alter their size and sort above or below the threshold), we test the null hypothesis of continuity of the density of firm size at 15 as proposed by McCrary (2008). Second, we regress the probability of firm growth on pre-existing firm characteristics.

In Figure 1, we plot the frequency of firms with less than 25 employees, using different bin sizes (0.5 and 1) for 1989 (before the reform) and for 1991 (after the reform). Visual inspection does not reveal any clear discontinuity at the 15 employees threshold.[8] In the right panel of the same figure, we zoom in on the shape of the running variable around the 15 employees threshold. There, no evidence of manipulative sorting can be detected. We formally test for the presence of a density discontinuity at this threshold with a McCrary test by running kernel local linear regressions of the log of the density separately on both sides of the threshold (McCrary, 2008). As we can see from the figure, the log-difference between the frequency to the right and to the left of the threshold is not statistically significant. In fact, the point estimate is -0.007 (with a standard error of 0.236). However, density tests have low power if manipulation has occurred on both sides of the threshold. In that case, there might be non-random sorting not detectable in the distribution of the running variable.

For this reason we perform a further test. Firms may sort around the threshold according to both observable and unobservable characteristics. To verify if sorting happens according to pre-existing unobservable characteristics, we first estimate a regression of firms' average wages paid in 1986–1989 (before the reform) on firm size, firm age, year dummies and firm

[8] The average firm size in Italy is approximately half that of the European Union and expensive EPL for firms larger than 15 is often wrongly indicated as one of the factors responsible for such a skewed size distribution. The non-existence of lumps at 15 can be explained by the fact that firms choose their size on the basis of several factors and not only on the basis of EPL (Schivardi and Torrini, 2008).

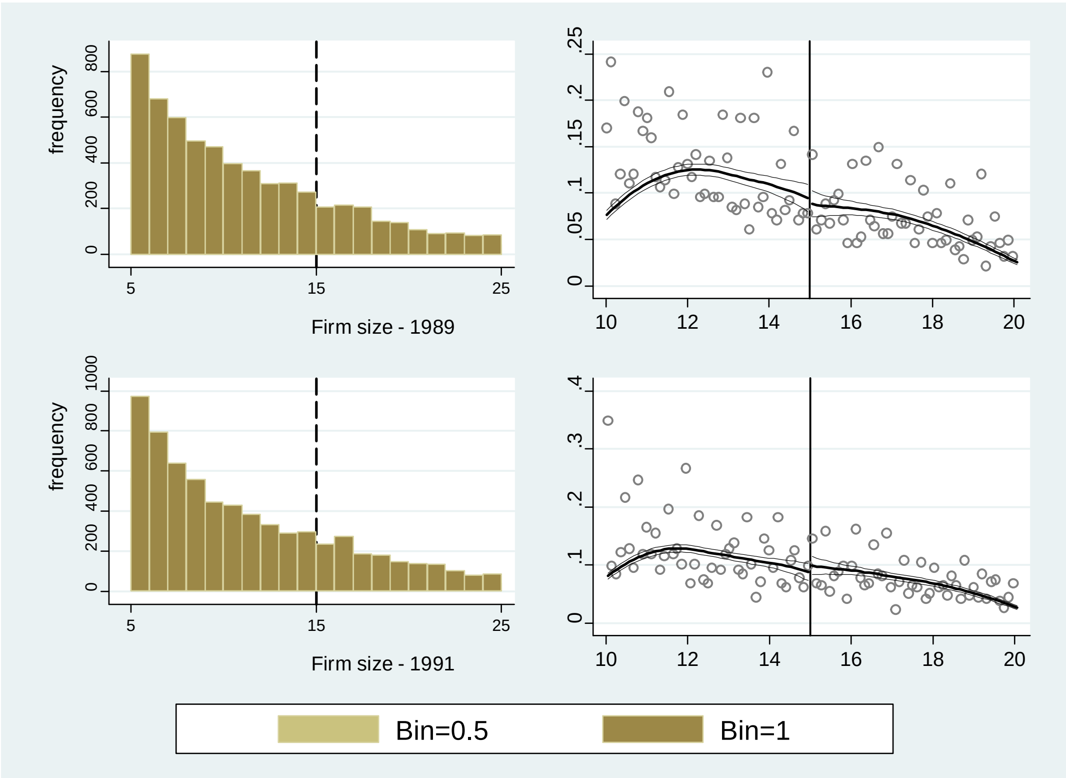

Figure 1: Frequency of firm size and McCrary test of density continuity. Weighted kernel estimation of the log density, performed separately on either side of the threshold. Optimal binwidth and binsize as in McCrary (2008).

fixed effects. We then use the time-invariant portion of the residual as one of the determinants of the firm probability of growing. The probit regression is of the form

$$\begin{aligned} d_{jt} &= \beta' X_{jt} + \delta_0 Post + \delta_1 dummyS_{jt-1} + \delta_2 FE_j + \alpha_0 \left(dummyS_{jt-1} \times Post \right) \\ &\quad + \alpha_1 \left(FE_j \times Post \right) + \alpha_2 \left(dummyS_{jt-1} \times Post \times FE_j \right) + \varepsilon_{jt}, \end{aligned} \tag{2}$$

where $d_{jt} = 1$ if firm j in year t has a larger size than in $t-1$. The term $dummyS_{jt-1}$ denotes a set of firm size dummies while the variable $Post$ takes the value of one from 1991. The term FE_j denotes the estimated firm fixed effects. The matrix X_{jt} includes a quadratic in firms' age, year dummies, sector dummies and a polynomial in lagged firm size.

Column 1 of Table 2 shows that on average firms just below 15 employees are about 3% less likely to grow of one unit than larger firms. These results are consistent with Schivardi and Torrini (2008) and Borgarello, Garibaldi and Pacelli (2004) who find that more stringent job security provisions hamper 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. Column 2 shows that the effect is not significantly different before and after the reform (insignificant coefficient on Post 1990 $\times$ Dummy 15). Finally, column 3 indicates that the effect is similar for firms with different average pre-reform wages, as the coefficient of the triple interaction Post 1990 $\times$ Firms Fixed Effect $\times$ Dummy 15 is not significantly different from zero.

While the fact that there is little evidence of firm sorting is reassuring, we also show results adopting an IV strategy to address the further concern that residual unobserved heterogeneity may drive firms' sorting behaviour. As an instrument for the treatment status (the firm size dummy), we use firm size in 1989 and in 1988. These instruments are not affected by the reform as long as the reform was unexpected (see Section 2). The formal specification is

$$\begin{aligned} \log w_{ijt} &= \beta' X_{ijt} + \delta_1 D_{jt}^S + \delta_2 \left(D_{jt}^S \times Post \right) + \sum_{k=1}^{3} (\gamma_k fsize_{jt}^k) + \upsilon_{ijt} \\ D_{jt}^S &= \gamma_0' X_{ijt} + \gamma_2 \mathbf{S}_{jpre}^S + \gamma_3 \left(\mathbf{S}_{jpre}^S \times Post \right) + \sum_{k=1}^{3} (\gamma_k fsize_{jt}^k) + \nu_{jt}, \end{aligned} \tag{3}$$

where $\mathbf{S}_{jpre}^S$ is a vector that includes firm size in 1989 and in 1988. The term $D_{jt}^S \times Post$ is also instrumented using as an instrument $\mathbf{S}_{jpre}^S \times Post$. The matrix X_{ijt} contains the same controls as in equation (1).

4.2 Worker sorting

Identification in (1) may be threatened also by workers non-randomly sorting around the 15 employees threshold. The idea is that workers (particularly job-to-job movers) may be able to choose their own EPL regime by selecting the size of the firm they work for. This may bias our results as long this selection process is driven by worker characteristics that we are not able to control for. Suppose, for example, that low-productivity workers disproportionately apply to (and are subsequently hired in) more protected jobs. In this case, a negative association between wages and job protection cannot be interpreted as the causal effect of EPL on wages, as it rather reflects the different composition of the pool of workers in protected and non protected jobs.

Including worker fixed effects into (1) helps address this concern to the extent that it allows of controlling for all time-invariant unobservable worker attributes that affect the choice of the workers regarding their EPL regime. Of course, worker fixed effects do not allow of controlling for the time-varying factors that affect worker self-selection, including the reform itself.

It is therefore desirable to test whether workers non-randomly sort into firms above and below the 15 employees threshold. We do so adopting two strategies. First, we check whether firms observable characteristics, such as industry, age, and occupation (white collar/blue collar) composition of the workforce, are balanced in the neighbourhood of the 15 employees threshold. If non-random workers sorting were to occur, we would expect these characteristics to differ systematically between treated and untreated firms around the 15 employees threshold. The balance tests are performed running the firm-level regression:

$$X_{jt} = \delta_0 Post + \delta_1 D^S_{jt} + \delta_2 \left(D^S_{jt} \times Post\right) + \sum_{k=1}^{n} (\gamma_k fsize^k_{jt}) + e_{jt}. \tag{4}$$

Notice that this test gives also insights on whether other (unobserved) policies differentially affect small and large firms since 1990. Indeed, our empirical strategy may be hampered by the presence of unobserved factors (for example another policy change) that are also discontinuous at the threshold exactly at the time of the reform, thus confounding the effect of the reform itself. Although we cannot directly test this assumption, we can investigate whether firms observable characteristics have discontinuities at the threshold after 1990. Table 3 shows the coefficients and standard errors of δ_2. No pre-treatment characteristics show a significant discontinuity at the 15 employees threshold after the reform in the 3nd degree polynomial specification. In particular, the age, occupation, and industry composition of firms across the two sides of the threshold is not significantly different after the reform.

The only weakly significant coefficients belong to three industry dummies in the case of the 2rd degree polynomial specification. These results are also suggestive that the effect of the change in EPL is unlikely to be confounded with the effect of another policy that depends on firm size and shares the same threshold.

We further test for non-random selection of workers by explicitly looking at their flows across firms. If the reform lowers the wage in small firms relative to big firms after the reform, one may expect larger flows of workers from small to big firms and smaller flows from big to small firms after the reform. In order to assess the extent of worker sorting we run regressions of the probability of workers moving to a big firm or to a small firm on a number of determinants that include a small firm dummy interacted with year dummies. The probit regression is of the form:

$$\begin{aligned} d_{ij't} &= \beta' X_{ijt} + \delta_0 D^S_{jt-1} + \delta_1 T_t + \delta_2 FE_i + \alpha_0 \left(T_t \times D^S_{jt-1}\right) + \alpha_1 \left(T_t \times FE_i\right) + \\ &\quad + \alpha_2 \left(T_t \times D^S_{jt-1} \times FE_i\right) + \varepsilon_{ijt}, \end{aligned} \tag{5}$$

where $d_{ij't}$ equals 1 if in year t worker i moves from firm j to a firm j' with more than 15 employees (Table 4, columns 1 and 2) or to a firm j' with fewer than 15 employees (Table 4, columns 3 and 4). The dummy D^S_{jt-1} indicates the size of the firm of origin and it equals 1 if the firm has fewer than 15 employees. The term T_t denotes a set of year dummies. The variable FE_i (indicated as Workers Fixed Effect in Table 4) is the time-invariant component of the individual's average pre-reform wage (between 1986 and 1989) purged of age, a third degree polynomial in firm size and year dummies. The matrix X_{ijt} includes a quadratic in worker age, sector dummies and a polynomial in the size of the firm of origin.

Columns 1 and 2 of Table 4 show that there is a lower probability of moving to firms larger than 15 coming from a small firm after the reform, i.e. in 1990, 1991 and 1992 (negative and significant coefficients on $T_{1990} \times D^S_{jt-1}$, $T_{1991} \times D^S_{jt-1}$ and $T_{1992} \times D^S_{jt-1}$). However, column 2 of Table 4 shows that the drop in the probability of moving from a small to a large firm is smaller for high-wage workers in 1991 (positive and significant coefficient on $T_{1991} \times D^S_{jt-1} \times FE_i$), while it is independent of (the time-invariant component of) workers wages in 1990 and 1992 (insignificant coefficients on $T_{1990} \times D^S_{jt-1} \times FE_i$ and $T_{1992} \times D^S_{jt-1} \times FE_i$). Thus, except for 1991, the probability of moving from a small to a large firm after the reform is apparently not driven by workers' attributes correlated with their productivity. Results for the probability of moving from small to small firms (columns 3 and 4) indicate that there are no differential effects around 1990 (insignificant coefficients on both $T_{1990} \times D^S_{jt-1}$ and $T_{1990} \times D^S_{jt-1} \times FE_i$).

Even though Tables 3 and 4 suggest that non-random sorting of workers around the 15 employees threshold should not be a major issue, for robustness purposes the next section will show results from workers' fixed effects regressions that control for any time-invariant worker attributes that may affect their behaviour.

5 Results

Theory delivers ambiguous predictions on the wage effects of EPL as workers are subject to two offsetting forces: on the one hand according to the Coasean–Lazear model we should expect EPL to lower wages as firms try and translate part of their cost increase onto workers. On the other hand insider–outsider theories suggest that EPL strengthens workers' bargaining position and possibly leads to an increase in wages. This is why in what follows we cut our sample into high- (stayers, white collar, old) and low-bargaining power subsamples (movers, blue collar, young).

Before turning to the estimates, let us provide a visual summary of the relationship between EPL and wages. Figure 2 draws a scatter plot of the difference between post-reform and pre-reform log wages against firm size in 1989. Each point is the average log wage difference within firms of the same size.[9] The figure also reports the fitted values of a third degree polynomial regression of the average log wage differences with respect to firm size in 1989. As firm size is taken to be that of 1989 to minimize endogeneity issues, the picture can be thought of as representing the reduced form of the IV specification. The figure shows a positive jump in the difference between post- and pre-reform log wages at the 15 employees threshold, meaning that in the neighbourhood of the threshold wages in small firms decrease after 1990 relative to wages in large firms. This is consistent with the interpretation that small firms translate part of the increased cost of EPL into lower wages. The general patterns presented in the figure are also borne out in the regression results to which we now turn.

Table 5 reports regression results from the estimation of (1). Panel A focusses on the full sample which includes all workers with a valid wage between 1989 and 1993. The sample is otherwise unrestricted and is therefore unbalanced. We then move to the subsample of firm movers (Panel B), i.e., the sample of individuals with a valid wage between 1989 and 1993 who change firm at least once. Next, we consider the sample of stayers (Panel C), i.e., the sample of all individuals with a valid wage between 1989 and 1993 who stay in the same firm for the whole period. Of course, the full sample is the sum of the firm stayers and the firm movers samples. We finally report separate estimates for blue collar workers (Panel D), white collar workers (Panel E), young workers below 30 (Panel F), and old workers above 45 (Panel

[9] The variable firm size varies almost continuously as it measures the average size of the firm during the year.

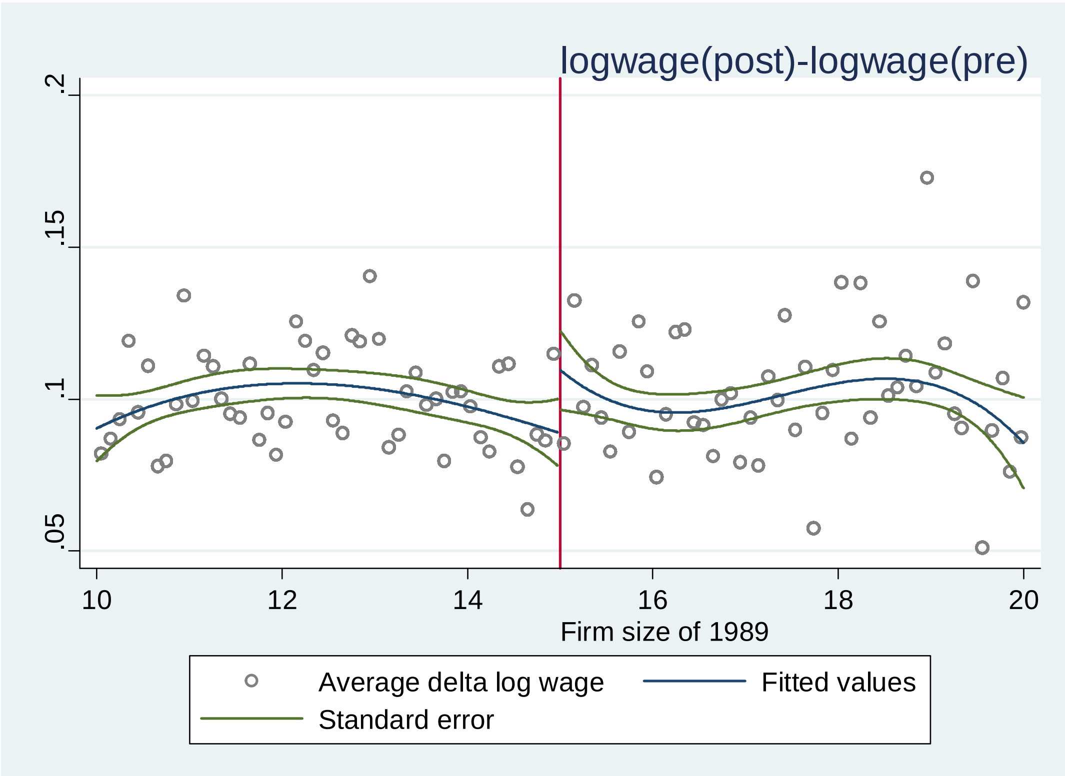

Figure 2: The solid line is a fitted regression of the variable on the vertical axis on a 3rd degree polynomial in firm size in 1989, performed separately on either side of the threshold. The dots are the observed log wage differences averaged in intervals of 0.1 firm size in 1989. Log wage (post) is the average individual wage in 1991, 1992 and 1993, log wage (pre) is the average in 1988 and 1989.

G). For the sake of brevity we only show the coefficient of interest on the interaction term between the small firm dummy and the post-reform dummy, which measures the average increase in log wages in small firms after the reform.

OLS results for the full sample (Column 1, Panel A) suggest that workers in firms just below the firm size threshold of 15 employees are paid 1.1 percent less than workers in firms immediately above the cutoff after 1990. Columns 2 and 3 refer to worker and firm fixed effects estimates, respectively, and show that the negative OLS result is robust (albeit lower in magnitude) to the inclusion of worker fixed effects but does not survive the inclusion of firm-specific dummies. This may be due to the heterogeneity of the effects across different workers within the same firm, as the within-firm variation (that identifies the firm fixed effects coefficients) derives from the aggregation of potentially highly heterogeneous and off-setting worker effects. Finally, columns 4 and 5 refer to IV and IV with worker fixed effects estimates, respectively. Both specifications deliver negative and significant coefficients of approximately the same magnitude as the OLS results.[10] This is reassuring as it confirms the impression that firm sorting is not a major source of bias.

Panels B and C of Table 5 look at movers and stayers separately and indicate that the results obtained for the full sample are mainly driven by the former. Results for the sample of stayers in Panel C show in fact low and insignificant effects once we control for worker and firm fixed effects. These results fit the interpretation that the strength of the wage effects of EPL is inversely related to the bargaining power of workers, as newly hired workers are often endowed with low bargaining power and firms can more easily lower their wages than those of incumbent workers.

In Panels D and E of Table 5 we analyse the subsamples of blue collar and white collar, respectively. Results show that the effect found on the total sample is driven by blue collar workers: the coefficients in Panel D are of the same magnitude as those shown in Panel A, while the results for white collar are smaller in magnitude and insignificant. We also find that the effects of EPL on wages are stronger among young workers aged less than 30 (Panel F) and insignificant among old workers aged more than 45 (Panel G). The negative effect on young workers' wages is almost twice as large as the effect found on the full sample in the OLS and the IV specification (columns 1 and 4) and even larger in the other cases (columns 2, 3 and 5). These results are again in line with a bargaining power type of interpretation, as blue collar and young workers have arguably low bargaining power vis-à-vis employers. Of course, the strength of bargaining power does not refer necessarily to unionization rates

[10]The power of the instruments is strong as indicated by the F-test of the excluded instruments, equal to 8.04 and 40727.76.

(which are very low among small firms workers in Veneto, as in the whole country, while coverage is very high) but to the option value of workers in the market.

Finally, we look at effects of the EPL reform on log wage changes to investigate whether stricter EPL affects wage growth on top of levels. Results in Table 6 show that indeed this is the case. Full sample estimates in Panel A show that workers lose on average 1% in terms of wage growth after the reform in small firms relative to large firms. The effect is negative and significant in all specifications (worker fixed effects, firm fixed effects and IVs). It is worth noticing that the fact that the specification in changes yields a negative and significant coefficient even including firm fixed effects (and the coefficient of interest is identified by within firm variation) seems to indicate that the negative effects on wage growth are more evenly spread across different workers types, contrary to the case of wage levels.

This impression is confirmed in the analysis of the different subsamples. Both movers (Panel B) and stayers (Panel C) in small firms suffer significant wage losses after the reform relative to large firms workers. While the negative effect on movers is larger in magnitude, the fact that also stayers are hit by the reform suggests that they also pay part of the increase in EPL in the form of lower wage growth (or, given that these workers stay in the same firm, in the form of a lower tenure profile). As long as a large part of the wage is decided at the firm level, incumbents have higher bargaining power and can impede renegotiation of wages to the bottom but probably cannot avoid a slower wage growth for the part not included in union contracts. Consistently with this story, blue collar (Panel D) and young workers (Panel F) lose more in terms of percentage wage changes than white collar (Panel E) and older workers (Panel G).

5.1 Contractual minimum wages and quantile regression

Similarly to many other European countries, Italy has a system of sectoral minimum wages bargained at the national level (every 2 years, with exceptions) which extends also to non-signatory workers. In this section we exploit information on the sectoral minimum wage to construct a measure of the "wage drift", i.e. the difference between the actual wage and the sectoral minimum. Thus, the wage drift is $y_{ijt} = w_{ijt} - w_{jt}^{\min}$.where $w_{jt}^{\min}$ is the contractual minimum in sector j.[11] The average wage premium is 247 Euros per week or in percentage terms the average premium is 40%. The average wage drift differs by sector and goes from

[11] We have information on minimum wages for about 52% of the observations present in the estimation sample. The difference is due to missing contract information for firms in the chemical industry and in industries covered by narrow sectoral agreements. The distribution of the characteristics of the workers (age, gender, and wages) in the resulting subsample is similar to the distribution in the overall estimation sample.

29% in the insurance sector to 45% of employees of law firms.[12]

The wage drift can be interpreted as a measure of bargaining power of the workers: the higher is the actual compensation with respect to the contractual minimum the higher is the bargaining power of the workers (Card et al., 2010). Following this reasoning – and consistently with the previous results – we should expect larger wage cuts for low-bargaining power workers with small wage premia over the minimum. Of course, wages at (or very close to) the minimum should be insensitive to changes in EPL because of the binding legal floor. To investigate these hypotheses we run a quantile regression at different points of the distribution using as a dependent variable the log of the wage drift $\log y_{ijt} = \log(w_{ijt} - w\min_{jt})$. Let $Q_\theta(\log y_{ijt}|X_{ijt})$ for $\theta \in (0,1)$ denote the θ^{th} quantile of the distribution of $\log y_{ijt}$ conditional on individual and firm characteristics included in the matrix X_{ijt} (same controls as in equation (1)). The model of the conditional quantile is:

$$Q_\theta(\log y_{ijt}|X_{ijt}) = \beta'_\theta X_{ijt} + \delta_{1\theta} D^S_{jt} + \delta_{2\theta}\left(D^S_{jt} \times Post\right) + \sum_{k=1}^{3}(\gamma_{\theta k} fsize^k_{jt}) \tag{6}$$

Bootstrapped standard errors are obtained from individual resampling.

Table 7 reports the estimates of the coefficient of the interaction term $\delta_{2\theta}$ obtained at the 5th, 10th, 50th and 90th quantile. Results show that the negative effect of the reform on the wages of small firm workers is stronger at the bottom of the wage drift distribution and weaker at the top. Panel A shows results for the full sample of workers for whom we have information on the contract in the benchmark period 1989-1993. While the coefficient of interest is negative and significant at all percentiles, the effect at the 5th percentile of the wage drift distribution is more than four times larger than the effect at the top of the distribution (and than the average effect obtained in Table 5 on all workers). Panel B on blue collar workers confirms this pattern.

These results are in accordance with the interpretation that firms were able to translate the increased EPL costs onto workers with low bargaining power. The fact that we find a strong effect also on wages very close to the minimum (the 5th percentile of the wage drift) is explained by the fact that even at the 5th percentile there is still a 31% premium on average,

[12] Collective contracts do not correspond exactly to sectors but vary according to firm size and type of firm. We have information on 27 types of contracts: employees of manufacturing firms, small and medium size manufacturing firms, artisan firms in the manufacturing sector, food manufacturing, insurance firms, shoemaking firms, paper products, retail industry, employees of cooperative firms in the retail industry, leather products, construction firms, small construction firms, cooperative construction firms, construction artisans, toys and personal products, wooden products, artisan wooden products, equipment instalment firms, equipment instalment state-owned firms, equipment instalment small firms, equipment instalment artisan firms, metal products small firms, cleaning services, cleaning services small firms, transport firms, professional firms, textiles products, tourism.

or 140 euro per week over the minimum, i.e. wage minima are hardly binding in this sample.

5.2 Robustness checks and placebo tests

This section shows that our results are robust to a number of checks. In Table 8 we implement placebo tests by estimating the treatment effect at fake thresholds, where there should be no effect. In particular, we look at firms below and above the fake 10 employees threshold and we estimate the treatment effect before and after 1993 and before and after 1988. The results are reassuring in that the coefficients of the treated group are always insignificant.

In Table 9 we run robustness checks with respect to the time span of the sample—enlarging the sample from the benchmark 1989–1993 to 1988–1993, 1987–1994 and 1986–1996—and with respect to the window of firm size—from the benchmark 5–25 to 5–20, 10–20 and 10–25. Both panels show that our results are robust to changes in the sample.

In Table 10 we also show that the results are robust to a different specification. To test the robustness of the estimates to the polynomial specification we also fit a linear regression function to the observations distributed within a distance Δ on both sides of the threshold:

$$\log w_{ijt} = \beta' X_{ijt} + \delta_1 D^S_{jt} + \delta_2 \left(D^S_{jt} \times Post\right) + \varepsilon_{ijt} \quad \text{for firm size} \in [15-\Delta, 15+\Delta] \tag{7}$$

where X_{ijt} contains the same controls as in equation (1). We choose Δ with the cross-validation method of Imbens and Lemieux (2008). The cross-validation method consists in choosing Δ so as to minimize the loss function: $L(\Delta) = \frac{1}{N}\sum_{i=1}^{N}(\log w_i - \widehat{\log w}_{\Delta}(fsize_j))^2$ where, for every $fsize_j$ to the left (right) of the threshold 15, we predict $\widehat{\log w}_{\Delta}(fsize_j)$ as if it were at the boundary of the estimation using only observations in the interval $fsize_j \in [15-\Delta, 15+\Delta]$. We choose the optimal Δ between 1 and 15. The optimal $\Delta^* = 10$ with $L(10) = 0.03762235$. Table 10 shows that the results are robust to the specification change: the local linear regression estimator yields a negative significant coefficient for OLS and weaker but still negative significant results for workers' and firms' fixed effects and IVs. Panel B of Table 10 tests successfully the results with samples taken over different years.

6 Conclusion

This paper examined the effect of employment protection legislation (EPL) on wages, a largely underexplored issue in the wide literature on EPL. It exploited, as a natural experiment, a reform of EPL in Italy which increased severance payments after 1990 for firms with fewer than 15 employees relative to larger firms.

We find that average wages of male workers declined by around 0.7%–1.5% in firms below 15 employees, relative to larger firms, because of the 1990 EPL reform. Our result are consistent with an explanation based on bargaining power because the effect is concentrated on new hires (rather than stayers), on blue collar and on young workers, all groups with a relatively low bargaining power. Stayers suffered a moderate reduction of wage growth after the reform. We showed that our results are robust to many changes in the sample time span and in the window of firm size considered around the 15 employees threshold. Placebo tests confirmed that the results apply only in 1990 for firms at the 15 employees threshold and not in other cases.

It is important to stress that our empirical exercise—which is local in nature as any RDD—cannot help determining whether *any* increase in EPL would be offset by lower wages. However, the Italian case offers not only a clean natural experiment which involved a vast quantity of firms and workers, but may also provide insights on the effects of EPL in the many countries which have firm-level thresholds in the application of EPL.

Using our estimates, it is possible to calculate how much of the increase in the firing cost is translated onto lower wages.[13] We start by considering the situation of a employer-initiated dismissal of a worker of average tenure in a small firm after the reform. If the dismissal is ruled unfair by the judge, the firing cost will range between 2.5 and 6 months (on average 16 weeks) of the last wage. On the basis of our data, the post-reform average weekly wage amounts to approximately 313 euros. Therefore, the severance pay transferred to the worker amounts to 313×16 weeks $= 5{,}008$ euros, excluding the legal expenses that can be roughly calculated to be as much as 5,000 euros. 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. Galdón-Sánchez and Güell (2000), using data based on actual court sentences, estimate that in Italy the probability of reaching an out-of-court agreement to be around 0.5 and the probability that the dismissal is ruled unfair to be about 0.5. If we assume that in case of an out-of-court agreement the employer pays approximately the same sum that would be paid in the form of severance pay, firms below 15 employees can expect a firing cost equal to $5{,}008 \times 0.5 = 2{,}504$ euros excluding legal expenses. If we assume a probability of 10% of the occurrence of individual firing for economic reasons, the total expected cost ex-ante is $(5{,}000 + 2{,}504)/10 = 750.4$ euros.

[13] This quantification exercise is, as our empirical approach, purely partial equilibrium and assumes any additional effects of EPL away. We leave the quantification of the general equilibrium impact of EPL within a dynamic GE model for future work.

Heckman and Pagès (2004) develop a measure of the expected present discounted cost to the firm, at the time a worker is hired, associated with severance payments to that worker in the future (they also take into account notice period, which is not of interest here). Adopting an analogous approach, one could use the estimates in the paper to compute the effect of severance payments on the expected present discounted value of wages, also at the time the worker is hired. On the basis of our estimates in Table 5 (columns 1), the wage loss for an average worker in a small firm (with average tenure 3.5 years) after the reform ($\widehat{\delta_2} = -0.011$) amounts to about 3.4 euros per week (313×0.011) or approximately 179 euros per year.

We use an annual discount rate of 8%, i.e., a discount factor of $\beta = 0.92$. To match an average tenure of 3.5 years, we use an annual survival probability of $\rho = 0.71$. Let W be the present discounted value of the wage loss due to the reform $W(\widehat{\delta_2}|\beta,\rho) = 179 \times \sum_{t=0}^{\infty}[\beta\rho]^t = 516.1$. This implies that around 68.8% ($516/750 = 0.688$) of the expected firing cost is translated into lower wages. Notice that a 68.8% offsetting effect is not inconsistent with the view that severance payments may actually strengthen the bargaining power of (incumbent) workers: in fact the average hides heterogenous effects across different workers.

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Table 1: Descriptive statistics

	Pre-reform		Post-reform	
	Small firms	Large firms	Small firms	Large firms
Real weekly wages	297.004	312.041	312.923	331.243
	(72.688)	(83.89)	(78.545)	(90.367)
Real weekly wage growth rate	0.049	0.04	0.024	0.029
	(0.121)	(0.114)	(0.123)	(0.127)
Employment	9.595	19.478	9.541	19.551
	(2.956)	(2.805)	(2.958)	(2.83)
White collar dummy	0.134	0.163	0.133	0.165
	(0.34)	(0.37)	(0.34)	(0.371)
Age	35.06	35.514	37.489	37.918
	(8.598)	(8.525)	(8.675)	(8.623)
Sector dummies:				
Agriculture	0.006	0.005	0.006	0.005
	(0.079)	(0.069)	(0.077)	(0.071)
Gas-water-oil	0.001	0	0.001	0
	(0.03)	(0)	(0.03)	(0)
Extraction-minerals-chemical	0.08	0.094	0.077	0.103
	(0.271)	(0.292)	(0.267)	(0.305)
Metal	0.272	0.322	0.271	0.311
	(0.445)	(0.467)	(0.445)	(0.463)
Manufacturing	0.242	0.297	0.237	0.292
	(0.429)	(0.457)	(0.425)	(0.455)
Construction	0.156	0.111	0.163	0.109
	(0.363)	(0.314)	(0.369)	(0.312)
Wholesale-retail-hotel	0.182	0.11	0.184	0.118
	(0.386)	(0.313)	(0.388)	(0.323)
Transportation	0.032	0.026	0.034	0.026
	(0.177)	(0.158)	(0.18)	(0.158)
Banks-insurance	0.011	0.013	0.01	0.014
	(0.105)	(0.112)	(0.099)	(0.118)
Observations	31505	17121	45848	26178

Notes: sample of years 1989-1993, all males aged 20-55 hired on a permanent basis in firms of between 5 and 25 employees.

Table 2: Firm sorting

	(1)	(2)	(4)
Dummy 13	-0.012	0.014	0.005
	(0.014)	(0.028)	(0.028)
Dummy 14	-0.026	-0.041	-0.041
	(0.014)*	(0.027)	(0.027)
Dummy 15	-0.029	-0.005	-0.001
	(0.015)*	(0.030)	(0.030)
Post 1990 × Dummy 13		-0.034	-0.030
		(0.030)	(0.031)
Post 1990 × Dummy 14		0.021	0.030
		(0.033)	(0.034)
Post 1990 × Dummy 15		-0.031	-0.035
		(0.033)	(0.033)
Firms Fixed Effect			0.242
			(0.033)***
Firms Fixed Effect × Dummy 13			0.348
			(0.151)**
Firms Fixed Effect × Dummy 14			-0.087
			(0.139)
Firms Fixed Effect × Dummy 15			-0.302
			(0.165)*
Post 1990 × Firms Fixed Effect			-0.220
			(0.036)***
Post 1990 × Firms Fixed Effect × Dummy 13			-0.254
			(0.173)
Post 1990 × Firms Fixed Effect × Dummy 14			0.011
			(0.162)
Post 1990 × Firms Fixed Effect × Dummy 15			0.297
			(0.183)
Observations	29315	29315	27720

Notes: The dependent variable is a dummy that takes the value of 1 if in firm *j* employment at time *t* is larger than employment at time *t*-1, and 0 otherwise. Firms between 5 and 25 workers are included. All specifications include a third degree polynomial in lagged firm size, a quadratic in firms' age, sector dummies and year dummies. One asterisk denotes significance at 10%; two asterisks denote significance at 5%; three asterisks denotes significance at 1%.

Table 3: Balanced test of firm characteristics

	Age	White collar	Agri-culture	Gas Water Oil	Extraction Minerals Chemical	Metal	Manu-factur-ing	Con-struc-tion	Whole-sale Retail Hotel	Trans-porta-tion
					2^{nd} degree polynomial					
Post 1990 × Small Firm	-3.760	-0.473	-0.112	-0.001	0.101	-0.699	1.218*	-0.990*	0.823	0.240
	(10.816)	(0.515)	(0.131)	(0.028)	(0.441)	(0.733)	(0.737)	(0.535)	(0.620)	(0.251)
					3^{rd} degree polynomial					
Post 1990 × Small Firm	-41.268	1.355	0.531	0.002	3.533	1.333	-2.234	-1.770	-1.707	-1.541
	(83.991)	(3.996)	(1.014)	(0.216)	(3.420)	(5.691)	(5.721)	(4.155)	(4.816)	(1.952)
Obs.	28043	28043	28043	28043	28043	28043	28043	28043	28043	28043

Data collapsed at firm-year level. One asterisk denotes significance at 10%; two asterisks denote significance at 5%; three asterisks denotes significance at 1%.

Table 4: Workers' sorting

Dependent Variable: mover dummy (probit)	P > 15		P ≤ 15	
Small firm dummy	0.009	0.009	-0.000	0.000
	(0.003)***	(0.003)***	(0.004)	(0.004)
Small firm dummy × Dummy 1990	-0.010	-0.010	-0.003	-0.003
	(0.003)***	(0.003)***	(0.004)	(0.004)
Small firm dummy × Dummy 1991	-0.013	-0.013	0.001	0.001
	(0.003)***	(0.003)***	(0.005)	(0.005)
Small firm dummy × Dummy 1992	-0.014	-0.014	0.024	0.023
	(0.003)***	(0.003)***	(0.006)***	(0.006)***
Small firm dummy × Dummy 1993	-0.003	-0.003	0.014	0.014
	(0.003)	(0.003)	(0.005)***	(0.005)***
Workers Fixed Effect		-0.010		-0.061
		(0.012)		(0.014)***
Workers Fixed Effect × Small firm dummy		0.001		0.022
		(0.015)		(0.017)
Workers Fixed Effect × Dummy 1990		-0.008		-0.012
		(0.016)		(0.019)
Workers Fixed Effect × Dummy 1991		-0.020		-0.001
		(0.016)		(0.020)
Workers Fixed Effect × Dummy 1992		-0.019		0.044
		(0.017)		(0.021)**
Workers Fixed Effect × Dummy 1993		-0.008		-0.005
		(0.015)		(0.023)
Workers Fixed Effect × Dummy 1990 × Small Firm Dummy		0.008		0.018
		(0.021)		(0.024)
Workers Fixed Effect × Dummy 1991 × Small Firm Dummy		0.050		0.003
		(0.021)**		(0.024)
Workers Fixed Effect × Dummy 1992 × Small Firm Dummy		0.024		-0.033
		(0.022)		(0.025)
Workers Fixed Effect × Dummy 1993 × Small Firm Dummy		0.016		0.024
		(0.018)		(0.027)
Observations	120652	120652	120583	120583

Notes: In the first (last) two columns the dependent variable is a dummy that takes the value of 1 if worker *i* moves to a firm with more (less) than 15 employees and 0 otherwise. Firms between 5 and 25 employees included. All specifications include a quadratic in workers' age, year dummies, sector dummies and a polynomial in the size of the firm of origin. Standard errors in brackets. One asterisk denotes significance at 10%; two asterisks denote significance at 5%; three asterisks denote significance at 1%.

Table 5. Log wage levels

	(1)	(2)	(3)	(4)	(5)
			Panel A: Full sample		
Post 1990 × Small Firm Dummy	-0.011	-0.004	-0.002	-0.013	-0.008
	[0.003]***	[0.002]*	[0.002]	[0.004]***	[0.003]***
Observations	96333	96333	96333	83592	83592
R-squared	0.26	0.16	0.22		
F-test of excluded instr. (p-value)				0.00; 0.00	0.00; 0.00
			Panel B: Movers		
Post 1990 × Small Firm Dummy	-0.021	-0.019	-0.011	-0.025	-0.024
	[0.006]***	[0.005]***	[0.005]**	[0.008]***	[0.008]***
Observations	28451	28451	28451	19074	19074
R-squared	0.20	0.13	0.17		
F-test of excluded instr. (p-value)				0.22; 0.00	0.70; 0.00
			Panel C: Stayers		
Post 1990 × Small Firm Dummy	-0.008	0.002	0.001	-0.011	-
	[0.003]***	[0.002]	[0.002]	[0.005]**	-
Observations	67882	67882	67882	64518	-
R-squared	0.28	0.19	0.24		
F-test of excluded instr. (p-value)				0.00; 0.00	-
			Panel D: Blue collars		
Post 1990 × Small Firm Dummy	-0.014	-0.004	-0.001	-0.015	-0.006
	[0.003]***	[0.002]*	[0.002]	[0.003]***	[0.003]**
Observations	82413	82413	82413	71526	71526
R-squared	0.13	0.14	0.09		
F-test of excluded instr. (p-value)				0.00; 0.00	0.00; 0.00
			Panel E: White collars		
Post 1990 × Small Firm Dummy	0.005	-0.001	-0.004	0.003	-0.006
	[0.009]	[0.006]	[0.006]	[0.019]	[0.007]
Observations	13920	13920	13920	12066	12066
R-squared	0.18	0.20	0.20		
F-test of excluded instr. (p-value)				0.37; 0.00	0.13; 0.00
			Panel F: Young (age < 30)		
Post 1990 × Small Firm Dummy	-0.019	-0.012	-0.007	-0.024	-0.028
	[0.005]***	[0.005]**	[0.004]*	[0.007]***	[0.010]***
Observations	23579	23579	23579	19934	19934
R-squared	0.17	0.18	0.16		
F-test of excluded instr. (p-value)				0.00; 0.00	0.02; 0.00
			Panel G: Old (age > 45)		
Post 1990 × Small Firm Dummy	-0.006	-0.001	-0.001	-0.006	0.001
	[0.007]	[0.005]	[0.005]	[0.008]	[0.009]
Observations	19784	19784	19784	17337	17337
R-squared	0.29	0.11	0.22		
F-test of excluded instr. (p-value)				0.00; 0.00	0.00; 0.00
Workers FE	NO	YES	NO	NO	YES
Firms FE	NO	NO	YES	NO	NO
IV	NO	NO	NO	YES	YES

Robust standard errors clustered by individual in brackets. All specifications include a third degree polynomial in the size of the firm, age dummies, sectoral dummies, year dummies and a blue collar dummy (except in panels D and E). One asterisk denotes significance at 10%; two asterisks denote significance at 5%; three asterisks denotes significance at 1%.

Table 6. Log wage changes

	(1)	(2)	(3)	(4)	(5)
			Panel A: Full sample		
Post 1990 × Small Firm Dummy	-0.013	-0.011	-0.010	-0.016	-0.009
	[0.002]***	[0.002]***	[0.002]***	[0.002]***	[0.004]**
Observations	93435	93435	93435	81391	81391
R-squared	0.01	0.03	0.01		
F-test of excluded instr. (p-value)				0.00; 0.00	0.00; 0.00
			Panel B: Movers		
Post 1990 × Small Firm Dummy	-0.022	-0.021	-0.017	-0.033	-0.034
	[0.005]***	[0.006]***	[0.007]**	[0.007]***	[0.013]**
Observations	27322	27322	27322	18251	18251
R-squared	0.02	0.03	0.01		
F-test of excluded instr. (p-value)				0.33; 0.00	0.52; 0.00
			Panel C: Stayers		
Post 1990 × Small Firm Dummy	-0.009	-0.006	-0.007	-0.012	-
	[0.002]***	[0.002]***	[0.002]***	[0.002]***	-
Observations	66113	66113	66113	63140	-
R-squared	0.02	0.05	0.01		
F-test of excluded instr. (p-value)				0.00; 0.00	-
			Panel D: Blue collars		
Post 1990 × Small Firm Dummy	-0.014	-0.012	-0.011	-0.016	-0.009
	[0.002]***	[0.002]***	[0.002]***	[0.002]***	[0.004]**
Observations	79967	79967	79967	69662	69662
R-squared	0.01	0.03	0.01		
F-test of excluded instr. (p-value)				0.00; 0.00	0.00; 0.00
			Panel E: White collars		
Post 1990 × Small Firm Dummy	-0.004	-0.003	-0.003	-0.010	-0.010
	[0.005]	[0.006]	[0.006]	[0.007]	[0.008]
Observations	13468	13468	13468	11729	11729
R-squared	0.02	0.04	0.01		
F-test of excluded instr. (p-value)				0.30; 0.00	0.09; 0.00
			Panel F: Young (age < 30)		
Post 1990 × Small Firm Dummy	-0.018	-0.021	-0.020	-0.026	-0.029
	[0.004]***	[0.006]***	[0.005]***	[0.005]***	[0.010]***
Observations	22028	22028	22028	18717	18717
R-squared	0.01	0.03	0.01		
F-test of excluded instr. (p-value)				0.01; 0.00	0.03; 0.00
			Panel G: Old (age > 45)		
Post 1990 × Small Firm Dummy	-0.007	-0.011	-0.009	-0.006	-0.021
	[0.004]	[0.005]**	[0.005]*	[0.005]	[0.011]*
Observations	19535	19535	19535	17169	17169
R-squared	0.01	0.07	0.01		
F-test of excluded instr. (p-value)				0.00; 0.00	0.00; 0.00
Workers FE	NO	YES	NO	NO	YES
Firms FE	NO	NO	YES	NO	NO
IV	NO	NO	NO	YES	YES

Robust standard errors clustered by individual in brackets. All specifications include a third degree polynomial in the size of the firm, age dummies, sectoral dummies, year dummies and a blue collar dummy (except in panels D and E). One asterisk denotes significance at 10%; two asterisks denote significance at 5%; three asterisks denotes significance at 1%.

Table 7: Quantile regression: Log of wage drift (firm size 5-25)

	(1)	(2)	(3)	(4)
	Panel A: full sample 1989-1993			
	Q05	Q10	Q50	Q90
Post 1990 × Small Firm Dummy	-0.062	-0.035	-0.021	-0.014
	[0.012]***	[0.008]***	[0.007]***	[0.007]*
Observations	50207	50207	50207	50207
	Panel B: blue collars 1989-1993			
	Q05	Q10	Q50	Q90
Post 1990 × Small Firm Dummy	-0.042	-0.026	-0.018	-0.013
	[0.008]***	[0.004]***	[0.006]***	[0.015]
Observations	43539	43539	43539	43539

The wage drift is the difference between the actual wage and the sectoral minimum wage. All specifications include a third degree polynomial in the size of the firm, age dummies, sectoral dummies, year dummies and a blue collar dummy. Bootstrapped standard errors clustered by individual in brackets. One asterisk denotes significance at 10%; two asterisks denote significance at 5%; three asterisks denotes significance at 1%.

Table 8: Placebo tests on log wage levels

	(1)	(2)	(3)
	Firm Threshold =10	Reform in year 1992	Reform in year 1988
Post 1990 × 10 employees threshold dummy	-0.003		
	[0.002]		
Post 1992 × Small Firm Dummy		0.000	
		[0.000]	
Post 1988 × Small Firm Dummy			0.002
			[0.002]
Observations	96333	96458	157513
R-squared	0.16	0.15	0.30

Robust standard errors clustered by individual in brackets. All specifications include workers fixed effects, a third degree polynomial in the size of the firm, age dummies, sectoral dummies, year dummies and a blue collar dummy. One asterisk denotes significance at 10%; two asterisks denote significance at 5%; three asterisks denotes significance at 1%.

Table 9: Robustness check: Log wage levels

	(1)	(2)	(3)	(4)
	Panel A: different time spans			
	1989-93	1988-93	1987-94	1986-96
Post 1990 × Small Firm Dummy	-0.011	-0.006	-0.006	-0.008
	[0.003]***	[0.003]**	[0.002]***	[0.002]***
Observations	96333	117630	158116	211267
R-squared	0.26	0.27	0.28	0.28
	Panel B: different firm-size windows			
	5-25	5-20	10-20	10-25
Post 1990 × Small Firm Dummy	-0.011	-0.008	-0.008	-0.012
	[0.003]***	[0.004]**	[0.004]*	[0.003]***
Observations	96333	81713	47481	62101
R-squared	0.26	0.25	0.24	0.25

Robust standard errors clustered by individual in brackets. All specifications include a third degree polynomial in the size of the firm, age dummies, sectoral dummies, year dummies and a blue collar dummy. One asterisk denotes significance at 10%; two asterisks denote significance at 5%; three asterisks denotes significance at 1%.

Table 10: Local linear regression: Log wage levels (firm size 5-25)

	(1)	(2)	(3)	(4)
	Panel A: different estimators			
	OLS	Worker FE	Firm FE	IV
Post 1990 × Small Firm Dummy	-0.011	-0.004	-0.003	-0.006
	[0.002]***	[0.001]***	[0.002]*	[0.003]*
Observations	96333	96333	96333	96333
R-squared	0.24	0.88	0.70	
F-test of excluded instr. (p-value)				0.00; 0.00
	Panel B: OLS different time spans			
	1989-93	1988-93	1987-94	1986-96
Post 1990 × Small Firm Dummy	-0.011	-0.007	-0.008	-0.011
	[0.002]***	[0.002]***	[0.002]***	[0.002]***
Observations	96333	117630	158116	211267
R-squared	0.24	0.26	0.27	0.28

The optimal symmetric bandwidth is chosen with cross-validation methods Δ=10. All specifications include age dummies, sectoral dummies, year dummies and a blue collar dummy. Robust standard errors clustered by individual in brackets. One asterisk denotes significance at 10%; two asterisks denote significance at 5%; three asterisks denotes significance at 1%.