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Employment Protection Legislation and Wages

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Employment Protection Legislation and Wages

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Abstract
In a perfect labor market severance payments can have no real effects as they can be undone by a properly designed labor contract (Lazear 1990). We give empirical content to this proposition by estimating the effects of EPL on entry wages and on the tenure-wage profile in a quasi-experimental setting. We consider a reform that introduced unjust-dismissal costs in Italy for firms below 15 employees, leaving firing costs unchanged for bigger firms. Estimates which account for the endogeneity of the treatment status due to workers and firms sorting around the 15 employees threshold show no effect of the reform on entry wages and a decrease of the returns to tenure by around 20% in the first year and by 8% over the first two years. We interpret these findings as broadly consistent with Lazear's (1990) prediction that firms make workers prepay the severance cost.
Keywords: Costs of Unjust Dismissals, Severance Payments, Regression Discontinuity Design.

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1 Introduction

Since the work of Lazear (1990), it is well-known that in a Coasean world firing costs which involve a transfer from firms to workers in case of dismissal have no employment effects and are reflected only into changes of the individual earnings profiles. In presence of government-mandated severance pay, firms require workers to pay a fee upon entry that is equal to the value of the stream of the future severance payments. Under risk neutrality this scheme leaves the expected present value of the cumulative wage bill (inclusive of the severance payment) unchanged and has no effects on employment. Alternatively, if dismissal protections cannot be undone by Coasean bargaining, theory predicts that Employment Protection Legislation (EPL) acts as a tax on firing and reduces both accessions and separations with an ambiguous effect on the employment level.

This paper focusses on the wage effects of EPL and evaluates whether and to which extent stricter EPL affects both entry wages and the tenure profile using a natural experiment from Italy. In 1990, Italy introduced a labour market reform which increased the severance payment (i.e. the transfer part of the firing costs) of workers employed in firms with fewer than 15 employees from zero to between 2.5 and 6 months of pay, leaving firing costs unchanged for workers employed in firms with more than 15 employees.

Previous literature mostly concentrates on the effects of EPL on employment flows, often using the cross-state variation of EPL within the US. Autor (2003) looks at the effect of EPL on the use of temporary help agencies. Autor et al. (2004 and 2006) study the effect on employment. Kugler and Saint-Paul (2004) consider re-employment probabilities. To our knowledge the only paper which looks at the effect of EPL on productivity (measured as value added per worker) at the establishment-level data is Autor et al. (2007). They find that the adoption of wrongful discharge protections reduced total factor productivity in the adopting US states. 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. Kugler and Pica (2007) exploit the differential change in firing costs for unfair dismissals in large and small firms after 1990 in Italy to look at the effects of changes in EPL on job and workers flows.

This paper uses the variation of EPL both across firms (below and above 15 employees) and over time (before and after 1990) in Italy. We identify the effects of employment protection legislation on wages through a Regression Discontinuity Design (RDD) and compare wages of individuals who work in firms in a neighborhood of the 15 employees threshold before and after the reform. Our identification assumption is essentially that the average outcome for individuals employed in firms marginally above the 15 employees threshold represents a valid counterfactual for the treated group employed in firms just below the threshold. One natural concern, in our case, is the endogeneity of the treatment status. Both firms and workers may sort above and below the 15 employees threshold. 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.

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. Consistently with previous literature (Borgarello, Garibaldi and Pacelli 2002, and Schivardi and Torrini, 2004), we find that the propensity to grow of firms at the 15 employees threshold increases after the 1990 reform. Moreover, we find that the increase in the propensity to grow is larger for more productive firms. On the other side, also workers may sort around the 15 employees threshold. Individuals with different preferences over a menu of employment protection and wages may move from big to small firms (or viceversa) *because* of the EPL reform. The evidence points to an increase in the probability of moving to large firms after the reform for workers of small firms. We also find that most productive workers have a higher propensity to move away from small firms.

In order to identify the causal effect of EPL on wages, we purge the empirical analysis from the composition effects due to the sorting of workers and firms into the treatment status. To address the sorting of workers, we look at exogenously displaced workers. We identify all plant closings in the dataset and look at post-displacement wages of workers involuntary displaced due to plant closings in the two previous years. We show that the allocation of displaced workers in firms below and above the 15 employees threshold is random both before and after the reform. An instrumental variable strategy is adopted to address the sorting of firms: the current firm size is instrumented with firm size dummies in the pre-reform period (1989-1988-1987), when the reform was not in place and was arguably unexpected.

We use administrative data from the Italian Social Security Institute (INPS), and exploit a matched employer-employee panel which contains the entire population of workers and firms located in the Italian provinces of Vicenza and Treviso, an area characterized by a tight labor market and a high concentration of small firms. OLS estimates obtained on the "Sample of displaced workers" indicate no effect (or at best a weak negative effect) of the reform on entry wages and a significantly 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, time effects and individual characteristics. The point estimates imply a decrease of two-three percent a year in the returns to tenure in small firms relative to large firms after the reform. The IV results confirm that the reform had a significant negative impact on the tenure-wage profile of displaced workers. Interestingly, it appears that the effect of the reform on the tenure-wage profile is strongest after the first year of tenure and decreases over time. This suggests that firms, possibly unable to lower entry wages because of institutional constraints (e.g. wage minima), shift part of the firing costs onto workers by reducing the returns to tenure right after entry.

Finally, we provide results on industries and occupations with different degrees of wage flexibility captured by an average measure of the wage drift. Lazear's model predicts that the wage effect of EPL is larger where the wage drift is higher, while adjustment through employment flows is larger where the wage drift is lower. Results by industry and occupation do not deliver any clear pattern, however, the existence of a negative correlation between wage and employment adjustment cannot be ruled out at a lower level of aggregation or at the firm level.

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 reviews the theoretical literature. Section 5 explains the identification strategy used to evaluate the impact of EPL on the wage distribution. Section 6 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 7 concludes.

2 The institutional background

2.1 The evolution of Employment Protection regulations in Italy

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.

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.[1]

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.[2] 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]

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 covered by our sample.[4] 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.[5] 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 the possibility for EPL changes to affect individual wages. It has to be noticed that the available estimates of the firm-specific part of the wage are average measures while what is strictly relevant for Lazear's (1990) model to work is contractual flexibility at the moment of entry in the firm.

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

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

[3] 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).

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

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

3 Data description

The VWH data set 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 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.[6] 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. Moreover, the availability of information on the universe of workers and firms allows to build suitable instruments for firm size and apply IV techniques. 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 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.

The only reason of dropping out of the dataset is exit from the private sector or from the 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, and retirement.

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

[6] 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).

of closure.

3.1 Sample selection rules

We select all males of age between 21 and 55 and, 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, and to preserve the comparability of treatment and control groups, we eliminate all firms with fewer than 10 employees and with more than 20 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. For the cases of multiple individual spells in the same year we keep the longest spell.

4 Theoretical background

Lazear's (1990) competitive model posits that any "state mandated severance pay can be undone in a perfect market by an appropriately designed labor contract. Thus, without frictions severance pay can have no effect" (Lazear, 1990). Suppose that the government imposes a requirement that all workers who sign a contract in period 1 be paid Q as a severance pay if they are not employed in period 2. Let A^* be the reservation wage of the marginal worker and M^* the reservation wage of the marginal firm. After the introduction of severance pay the same equilibrium is maintained if the wage in period 2 is equal to $W' = A^* + Q = M^* + Q$, i.e. the wage in period two increases by the amount of the severance pay. To offset this, in period 1 firms require workers to pay a fee such that the expected compensation on signing the contract for any given worker is the same as it was before. The fee in period 1 is exactly equal to the amount of the severance pay. Thus, total compensation remains unchanged because in period 2, the worker receives the higher wage $W' = A^* + Q$ if employed, and Q if not employed. In summary, with perfectly flexible wages and risk neutrality, EPL raises the cost of employment and leads to an inward shift in labor demand but wages fall and shift labor supply outwards to offset the increase in cost; as a result employment levels are unchanged.

Even under risk neutrality, Coasean bargaining may not be feasible in presence of contractual rigidities or of market imperfections like binding minimum wages (Bertola and Rogerson, 1997), and government-mandated severance pay may have real effects. In this case, theory predicts that firms hire and fire less with an ambiguous effect on the employment level. EPL has real effects also if workers are risk averse and value job security. In this case they accept a lower expected present value of the cumulative wage bill in return for greater stability.

Differently from competitive models, much work in the macroeconomics of EPL is based on matching models where rents are split by Nash bargaining. Nash bargaining implies different wages for insiders and outsiders because they have different outside options and only insiders are protected by firing costs. 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.[7] Finally Güell (2000) shows that in an efficiency wage framework where workers' effort can only be monitored imperfectly, severance payments increase wages of insiders in equilibrium. Since the transfer increases the value of being unemployed and makes the punishment for shirking less effective, firms reduce labor demand and raise wages to restore the incentives to work.

Although macroeconomic models focus on how the relative importance of insiders and outsiders wage setting might reflect on equilibrium employment rates, they share the basic prediction with the competitive model: in presence of wage flexibility at entry, the market perfectly offsets the employment effects of EPL. These theoretical considerations motivate our focus on the analysis of the wage effects of EPL separately at entry and on subsequent wages.

5 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 Regression Discontinuity Design (RDD) requires a dichotomous treatment i.e. a deterministic function of a single variable (S) with a known point of its support ($\bar{s}$) where the probability of being treated changes from 0 to 1.

[7] The key difference between 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 reform in EPL which increases severance pay obligations (a transfer) on small firms. Nevertheless, the reform also entails a tax in as much there is an additional possibility to go to court.

The key condition for identification requires that in the counterfactual world no discontinuity takes place at the threshold for selection (see Battistin and Rettore, 2006, and Hahn et al., 2001).

In our case, EPL varies both among firms and over time. The treatment status depends on firm size but participation to the treatment group changes discontinuously at the 15 employees threshold after the 1990 reform. Thus, the identifying assumption requires that the relationship between wages and firm size around the threshold $\bar{s}$ would not change in absence of the reform, i.e. the difference between wages paid in firms slightly above and below the threshold $\bar{s}$ is constant over time. Formally, the identification assumption is written as:

$$\left[E\left\{w_0 \mid \bar{s}^-\right\} - E\left\{w_0 \mid \bar{s}^+\right\}\right]_{post\ 1990} = \left[E\left\{w_0 \mid \bar{s}^-\right\} - E\left\{w_0 \mid \bar{s}^+\right\}\right]_{pre\ 1990} \tag{1}$$

where w_0 is the counterfactual 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 the size of the discontinuity (if any) is identical before and after the 1990 reform.

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

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

The identification assumption in a RDD is essentially that the average outcome for individuals marginally above the threshold represents a valid counterfactual for the treated group just below the threshold. In this regard, we have an advantage with respect to RDD studies which compare different groups around the threshold in that we 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. Moreover, as the identification assumption of the RDD implies that close to the threshold all variables determined prior to assignment are independent of treatment status (Lee, 2007), we will be able to further assess the validity of the RDD by comparing the means of predetermined variables conditional on treatment around the threshold. Table 9, discussed in section 6.3, provides such evidence.

The strategy to identify the impact of the change in dismissal costs is illustrated in Figures 1 and 2. Figure 1 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.

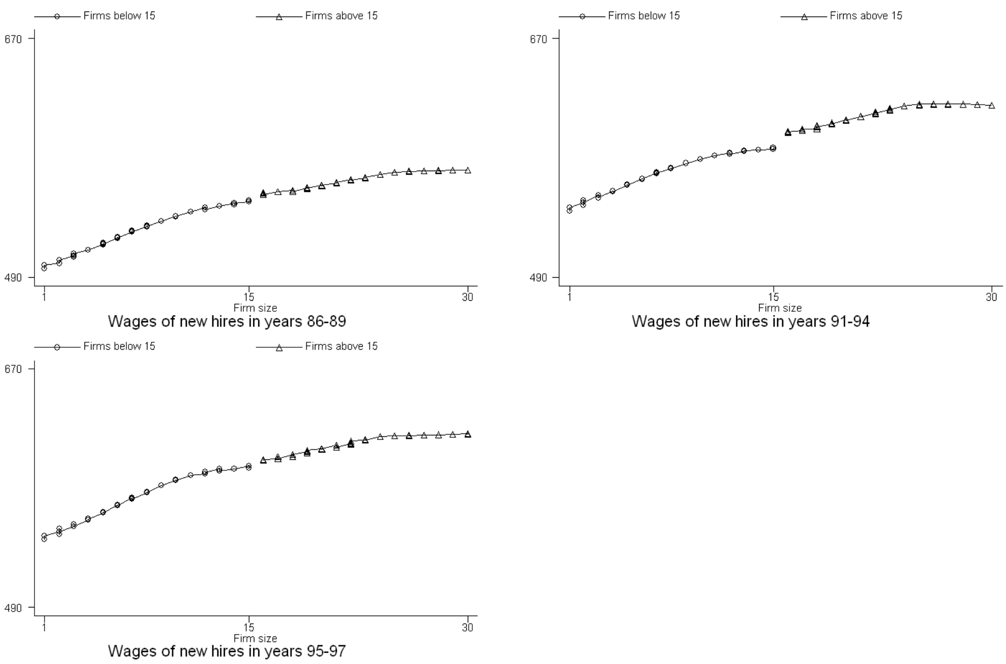

Figure 1: Wages of all new hires. Non parametric prediction of the average real wage from a weighted local linear regression smoother with bandwith 0.8, estimated separately for each side of 15 employees threshold.

It plots the mean wage (including both entry wages and subsequent wages) against firm size before (1986-1989) and after the reform in the period (1991-1994) and in the period (1995-1997). The mean is estimated non parametrically separately for each side of the threshold. The RDD identifies the effect of EPL on wages as the difference between average wages of firms in a neighborhood of the 15 employees threshold before and after the reform.

Figure 1 shows no discontinuity at the 15 employees threshold, neither before nor after 1990. According to Lazear's (1990) model with homogeneous workers and firms, one should expect a discontinuous downward jump at 15 in Figure 1 before the reform (1986-1989) and a reduction of the gap after the reform (1991-1994) and (1995-1997).[8] Lazear (1990) predicts that in a Coasean market with flexible wages the market offsets the severance payment and leaves the cumulative wage bill (inclusive of the severance payment) unchanged. The full wage offset implies a discontinuity at 15 *before* the reform in Figure 1 (which plots wages of new entrants and does not include the severance payment[9]), with a wage-penalty for workers employed in firms just above the 15 employees threshold that pay higher severance payments.

[8] Recall that the 1990 reform only *reduces* the gap in job security provisions between small and large firms.

[9] Our data do not include the severance payment which should be calculated on workers' entire working life.

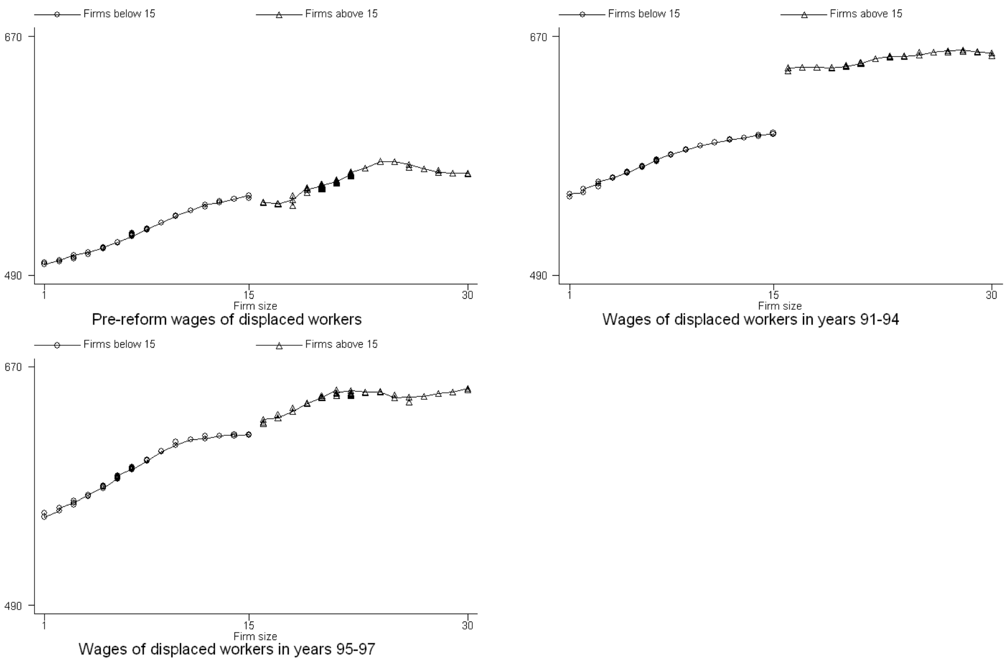

Figure 2: Wages of displaced workers. Non parametric prediction of the average real wage from a weighted local linear regression smoother with bandwith 0.8, estimated separately for each side of 15 employees threshold.

After the reform, one should expect a reduction in the hypothetical gap in Figure 1 (but not full continuity), because although the reform narrows the gap between employment security provisions guaranteed in firms above and below 15 employees, EPL is still stricter in firms with more than 15 employees after 1990.

Notwithstanding Lazear's predictions, the continuity of the relationship before (1986-1989) and much after the reform (1995-1997) does not come entirely as a surprise. If workers (and firms) are heterogenous in terms of preferences (and costs) of EPL, they may select around the 15 employees threshold possibly leaving the wage-firm size relationship continuous at 15. For example, high ability workers, who earn *ceteris paribus* higher wages, may self-select into larger firms (e.g. because of better career prospects). This would act as a confounding factor in our graphical analysis and contribute to restore continuity at 15. On the firms' side, the same companies right above the 15 employees threshold which should in principle pay lower wages because they have higher EPL costs, could be more capital intensive[10] or could use more intensively fixed-term employment contracts which are exempt from EPL (Schivardi and Torrini, 2004). This could bring their wages in line with those paid

[10] Evidence in this direction has been found by for the US by Autor *et al.* (2007).

in firms just below the 15 employees threshold notwithstanding the higher mandated EPL costs. All this implies that the identification of the effects of EPL on wages from the steady state relationship between wages and firm size is made very difficult because of workers' and firms' selection effects.

Our strategy is to identify the effect of an *unexpected change* in EPL (the reform in 1990) on wages. Starting from a steady state where wages are continuous at 15 (as in Figure 1 in the period 1986-1989), theory predicts that wages go down in firms below the threshold to offset the increase in EPL costs. To this extent the continuity of the wage-firm size relationship in the period immediately after the reform (1991-1994) is more troubling. Nonetheless, the evidence of a smooth relationship between firm size and wages in the immediate aftermath of the EPL reform does not rule out the presence of an effect of the EPL reform on wages. The reason is that even after an unexpected reform, the effect of EPL on wages may be confounded by concurrent employment flows.[11]

One possibility is that the 1990 reform induced a wage cut for workers employed in small firms and a concurrent outflow of workers towards firms above 15. This "supply effect" may have depressed wages of firms above the 15 employees threshold and blurred the discontinuity.[12] 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 invalidates the identification assumption in Figure 1 and equation (1).

To assess if workers' sorting has something to do with the smooth relationship between wage and firm size, we replicate the same graphs using a sample of exogenously displaced workers. Similarly to Jacobson, Lalonde, and Sullivan (1993), the idea is that displaced workers are less likely than those who voluntarily change firm, to sort themselves in firms around the threshold only of the reform. To define the sample of displaced workers we identify all plant closings in the dataset and look at post-displacement wages of those who

[11] In Lazear's model workers are risk neutral and the equilibrium after the change in EPL is such that **(1)** the marginal worker and the marginal firm remain the same; **(2)** the same workers work; and **(3)** the same firms employ labor as without state mandated severance pay. In this model the absence of employment flows identifies the effect of EPL on wages. But there are many reasons that can impede full wage adjustment and induce employment flows. One of them is that if workers are risk averse and value job security *per se*, the introduction of severance pay could generate workers sorting, i.e. flows of workers between more protected jobs and less protected jobs. In a sample of new hires (i.e new entrants in the labor market or job changers) workers can choose the firm they move to.

[12] An additionally or alternative explanation of the absence of discontinuity in the wage firm size relationship among new hires is that the reform may have changed the composition of workers flowing to small or big firms in the direction of "better" workers flowing to small firms. This composition effect could have offset the wage cut in small firms caused by the reform. This effect is unlikely to be at work because it would imply that more able workers move to firms which are concurrently lowering their wages.

were displaced due to plant closings in the two previous years and presumably were looking for work before the reform took place.[13]

Figure 2 plots the relationship between post-displacement 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 (at least when one controls for the sorting of workers). Of course, the mere fact that they were exogenously displaced is not enough to guarantee that they do not sort into smaller or bigger firms according to their preferences. The post-displacement wage-firm size relationship may still be affected by composition effects. But the fact that displaced workers were looking for work before the (unexpected) reform took place, makes them less likely to influence their treatment status in the aftermath of the reform. In section 6.3, we provide a formal test, in the spirit of Lee (2007), showing that after the reform displaced workers are indeed randomly allocated in small and large firms. Thus, in our empirical exercise identification essentially comes from displaced workers being taken by surprise in the immediate aftermath of the reform.

With the passage of time, there is no particular reason to consider the sample of displaced workers more exogenous than the sample of new hires. As we argue that the identification of the effect of EPL on the wages of the displaced comes from the discontinuity surrounding the passage of the 1990 reform, we would be concerned to see the effects of the reform on post-displacement wages before the reform. To verify if the discontinuity picks up the causal effect of the 1990 reform rather than other coincidental trends we plot the wage firm-size relationship in earlier years. The result of this falsification exercise indicates that there is no discontinuity in the years (1986-1989). Moreover, we plot the wage-firm size relationship in later years after the reform (1995-1997) and find no sign of discontinuity. Overall Figure 2 is consistent with a *causal short-term* effect of the reform on post displacement wages. The presence of the discontinuity at 15 in Figure 2 and its absence in Figure 1 points to the necessity to take into account workers' sorting when estimating the effect of EPL on wages. For this reason we will rely on the sample of displaced workers.

So far we have focussed on workers' sorting because its effects are evident in the comparison between Figure 1 and 2. However the identification of equation (1) is also threatened by the sorting of firms. Firms in the neighborhood of the 15 employees' threshold may vary their size in response to the 1990 reform of EPL, thus biasing the estimates. Firms which

[13] Table 1 (described later in more detail) contains descriptive statistics for the sample of Displaced Workers and shows that the treatment and control groups are similar in terms of observable characteristics around the threshold, both before and after the reform.

kept their size just below 15 before the reform to avoid strict EPL rules, may have increased their size *because* of the reform. It is not easy to sign the bias due to firms' sorting.

If firms which were keeping their size below 15 before the reform for fear of incurring in 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.

To account for firms' sorting in a formal regression framework we will use an IV strategy which basically identifies the effect on wages of those firms which did not cross the 15 employees threshold. In the next two sections we will provide direct evidence on workers' and firms' sorting around the 15 employees threshold, in the attempt to shed light on the importance of the bias.

5.1 Workers' sorting

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. These flows may in turn bias the estimate of the impact of the EPL reform on wages. Figure 3 shows the conditional 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 each panel of Figure 3 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 $y_{ij't} = 1$ if worker i moves in year t from firm j of size S_{jt-1} (in the horizontal axis in Figure 3) to a firm $j^{'}$ with more than 15 employees (upper panel of Figure 3) or a firm with fewer than 15 employees (lower panel of Figure 3). We split the sample in two periods, the pre-reform period (from 1986 to 1989) and the post-reform period (from 1991 to 1994). Figure 3 depicts both the raw proportion of movers and the fitted probability against firm size.

Figure 3 shows a smooth pattern of between-firm mobility both before and after the reform. At first glance, no discontinuities appear after the reform: the graphs show no evidence of exceptional workers' flows either to big or small firms around the time of the reform. The results above are conditional on moving and the two panels, before and after the reform, sum vertically to unity.

However, the reform may have had an effect on the unconditional probability of moving. Moreover, workers could sort according to unobservable characteristics. In order to check

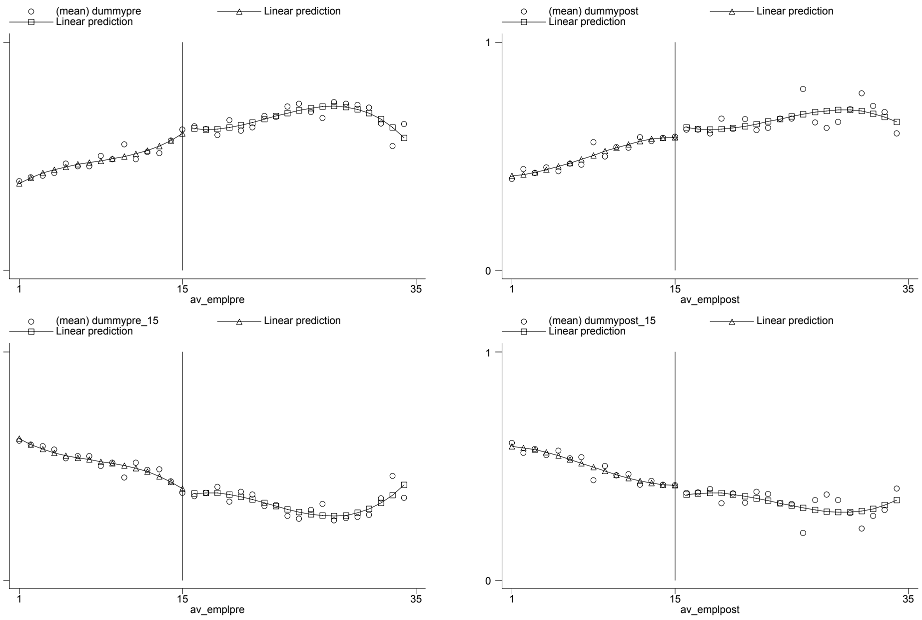

Figure 3: Upper panel: probability of moving to firms with 16 or more employees in the pre-reform period (upper left) and the post reform period (upper right). Lower panel: probability of moving to firms with 15 or fewer employees in the pre-reform period (lower left) and the post reform period (lower right).

this hypothesis, we calculate workers' fixed effects on the basis of their average wages before 1990 and run regressions of their probability of moving to a big firm or to a small firm on an indicator of small firm size, year dummies and interactions with workers' fixed effects. The regression is of the form:

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

where $d_{ij't} = 1$ if worker i moves in year t from firm j to a firm j' with more than 15 employees (Table 3, columns 1 and 2) or to a firm j' with fewer than 15 employees (Table 3, columns 3 and 4). D^S_{jt-1} indicates the firm size before moving, $D^S_{jt-1} = 1$ if employees ≤ 15 and 0 otherwise. T_t is a set of year dummies and FE_i is workers fixed effects. The variable FE_i in the regression above and indicated as Workers Fixed Effect in Table 3 is equal to the individual's average wage between 1986 and 1989 purged of age, a third degree polynomial in firm size, year and sector dummies.[14] The matrix X_{ijt-1} includes a quadratic in workers' age, sector dummies and a polynomial in the size of the firm of origin.

[14] We purge wages from the effect of firm size because we want to account for the fact that larger firms pay higher wages.

Columns 1 and 2 of Table 3 show that there is a larger probability of moving to firms larger than 16 coming from a small firm after the reform (positive and significant coefficient on $T_{1991} \times D^S_{jt-1}$). This effect could explain why in Figure 1 we do not observe a discontinuity at 15 in the wage-firm size relationship in the aftermath of the reform: because the larger flow towards bigger firms has depressed their wage and restored the continuity of the wage-firm size relationship. The Table also shows that the probability of moving from a small to a large firm in the aftermath of the reform is larger for "good" workers with higher than average wages before 1990 (positive and significant coefficient on $T_{1991} \times D^S_{jt-1} \times FE_i$). This effect however is quantitatively modest: one-standard-deviation increase in the workers' fixed effect increases the propensity to move from small to big firms in 1991 by only about 0.011 which is 1/25 of one standard deviation).[15] There is no evidence of a change after the reform in the probability of moving from small to small firms (columns 3 and 4).

Overall the evidence is consistent with a larger than average flow of workers towards firms bigger than 15 and a pattern of better workers moving out of small firms which were reducing wages in the face of higher government-mandated EPL. While a larger than normal flow of workers form small to big firms in 1991 may have depressed wages of firms above the threshold and may have contributed to hide the discontinuity at 15 in Figure 1, the evidence on "higher" than average ability of those movers would point in the opposite direction of increasing the gap between wages in firms at 15 and 16 in 1991. The outflow of high ability workers out of small firms which were reducing wages makes economic sense but the estimates indicate a quantitatively very modest impact which is unlikely to have affected the wage-firm size relationship of Figure 1. The evidence of some form of workers' sorting justifies our attempt of controlling for workers sorting using displaced workers.

5.2 Firms' sorting

The average firm size in Italy is approximately half of that of the European Union. Expensive EPL on firms larger than 15 is often indicated as one of the factors responsible for such a skewed size distribution. As in the case of the effect of EPL on wages (Figures 1 and 2), the identification of the effect of EPL on firm size in equilibrium is hampered by selection effects. Figure 4 shows the firm size distribution before and after the reform. Both graphs are smooth at 15 and show no evidence of lumping at 15. Yet one should not observe any firm at 16 because the marginal cost of hiring the 16th worker in Italy is huge and affects all previous 15 workers. The non-existence of lumps at 15 can be explained with the fact that firms choose

[15] Calculated as $(-0.029 + 0.043 + 0.057)/1000 \times 166$. The standard deviation of workers' fixed effect is 166, the standard deviation of the propensity to move to a big firm is 0.25.

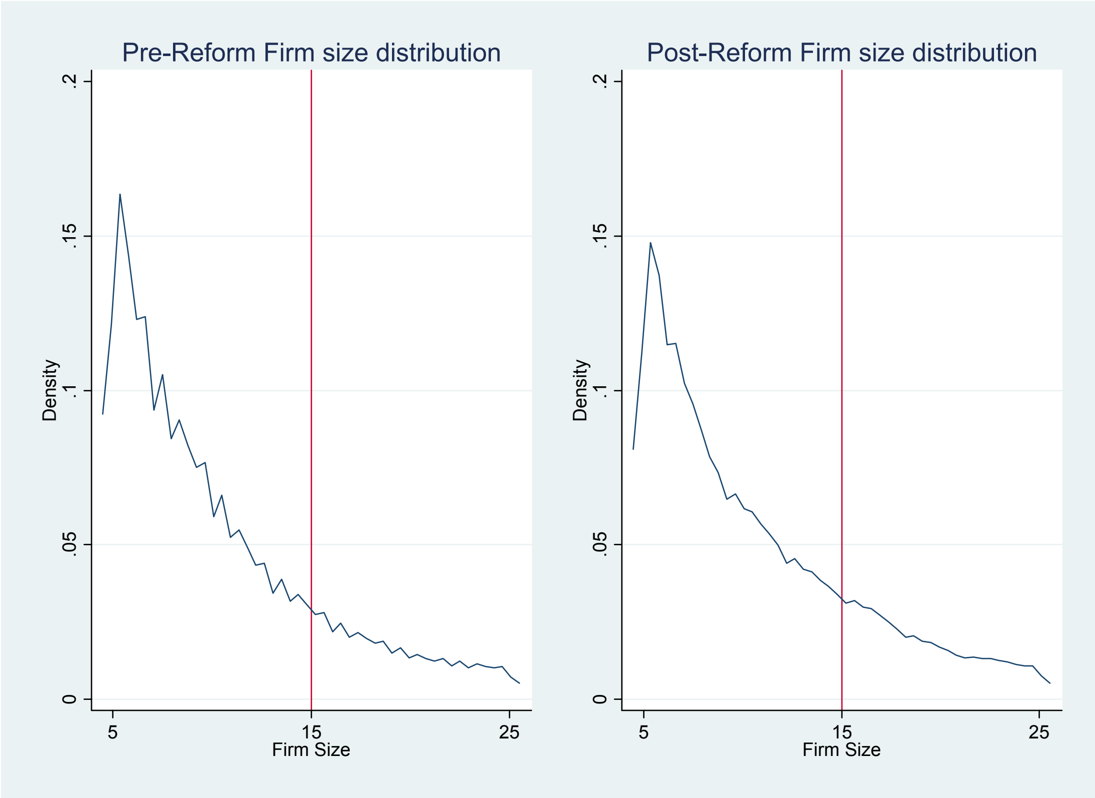

Figure 4: Firm Size Distribution. Pre-reform period: 1986-1989; Post-reform period: 1991-1994

their size on the basis of several factors and not only on the basis of EPL. A more promising approach is to look at the effect of EPL on the propensity to grow. Schivardi and Torrini (2004) and Borgarello, Garibaldi and Pacelli (2004) 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 employees before the reform to avoid strict EPL rules, increased their size *because* of the reform. This behavior would bias the identification in equation (1).

The observation that there is virtually no change in the firm size distribution before and after the reform does not rule out that some firms may have (partially) offset the increase in EPL costs with lower wages and that some other may have moved across the threshold. Firms may sort around the threshold according to observable and unobservable characteristics. To verify if sorting happens according to pre-existing unobservable characteristics, we calculate firm fixed effects on the basis of firms' average wages paid before the reform and then we regress the firms probability of growing on their size. The regression is of the form:

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

where $d_{jt} = 1$ if firm j in year t has a larger size than in $t-1$. $dummyS_{jt-1}$ is a set of firm size dummies. FE_j is firm j fixed effect and $Post = 1$ if year$\geqslant$1991. FE_j is the residual of a regression of firms' average wages in 1986-1989 on firm age, firm size, sector and year dummies. The matrix X_{jt-1} 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 not less likely to grow than larger firms. However when we look at the post reform period we see that, after 1991, firms of 15 employees are more likely to grow than before 1991 (column 2, positive significant coefficient on $dummy15 \times Post$). And among firms of 15 employees, the best firms which pay on average higher wages (before 1990) are more likely to grow (column 3, positive significant coefficient on $dummy15 \times Post \times FE_j$). These results are consistent with the following pattern: "good" firms, which were keeping their size just at 15 for fear of incurring in high EPL before 1991, were more willing to grow over 15 after the reform because the distance of EPL costs between small and large firms was reduced.

Notice that this pattern has different effects on the discontinuity depending on whether the "good" firms of 15 employees that passed the threshold because of the reform were paying, on average, higher or lower wages than the firms above the threshold. In the first (second) case, the discontinuity gets larger (smaller) and OLS estimates are upward (downward) biased, because the flow of firms raises (lowers) wages in large firms relative to small firms after the reform. In any case, the existence of some form of firms' sorting, justifies our attempt of instrumenting firm size in the regressions.

5.3 Regression model

Figure 1 and 2 show that an effect on wages is likely to be found in the sample of displaced rather than in the sample of the new hires. Before proceeding to the formal regression we verify whether the hypothesis of independence of the observable characteristics from the treatment status is valid in our sample. 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.[16]

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 ideal experiment to measure the effect of a change in EPL would have firms and workers exogenously assigned to the treatment status. If this was the case, simple OLS estimates of the following model would identify 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. Since the EPL reform may have affected wages differently at entry and during the employment relationship, we will estimate the effect of the reform both on entry wages and on the tenure-earnings profile. The variable $Post$ is a dummy that takes the value of 1 after 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

[16] 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.

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), industry and year effects. The reported standard errors account for possible error correlations at the individual level.

5.4 Firms' sorting and the IV model

In a perfect Lazear world, there is no sorting neither of workers nor of firms and every increase in EPL is adjusted through changes in wages. Yet the existence of firms' and workers' sorting is an empirical question, and potentially biases our estimates of the relationship between wages and firm size. To deal with this problem we use an IV strategy. As an instrument for the firm size dummy, we use firm size dummies (above/below 15 employees) in 1987, 1988 and 1989. This instrument is not affected by the reform as long as the reform was unexpected.[17] The formal specification looks as follows:

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

where $\mathbf{D}_{jpre}^S$ is a vector of firm size dummies in 1987, 1988 and 1989. All terms interacted with D_{jt}^S (respectively $D_{jt}^S \times Post$, $Ten_{ijt} \times D_{jt}^S$ and $Ten_{ijt} \times D_{jt}^S \times Post$) are also instrumented using the interaction with $\mathbf{D}_{jpre}^S$.

6 The effects of the 1990 reform

6.1 Results on the sample of New Hires

Table 4 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.

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.[18]

[17] 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.

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

Columns 1 and 2 refer to estimates on the full sample without and with controls respectively. Columns 3 to 5 present the IV estimates. To the traditional controls we add a measure of firm-level job creation with the intent of controlling for the firm-specific growth trend. The use of this variable should reassure us that the coefficients are not reflecting omitted trends which may predate the introduction of the reform and could otherwise be confounded with the effect of the reform. It may be the case that firms that are growing strongly are also paying more to guarantee workers' effort (Belzil 2000). The variable is defined as: $\frac{2(e_{jt}-e_{jt-1})}{(e_{jt}+e_{jt-1})}$ where e_{jt} is employment of firm j at time t.

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

However, 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.

6.2 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. 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 to 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 5 shows the results from OLS estimates on the sample of displaced workers. Columns 1 and 2 show no significant effects of the EPL reform on entry wages and a significant negative effect of as much as 3% on the returns to tenure. A comparison of Tables 4 and 5 seems to indicate that the presence of workers sorting leads to a positive bias of the OLS estimates. If better workers moved out of small firms towards larger firms after the reform, this would lead to higher estimates of the effect of the reform on wages in small firms because those workers whose wages would be reduced had moved away. The sample of displaced workers is meant to address, in the best possible way, the issue of workers' sorting. However the OLS estimates on the sample of the displaced may still be biased by the endogeneity of the treatment status on the firm side.

6.3 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 for the sorting of firms, we instrument the treatment status (the dummy firm size lower than 15 employees) using firm size dummies in 1987, 1988 and 1989 when the reform was not in place and was arguably unexpected.

Table 5 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 the wage-tenure profile (columns 3 to 5). The magnitude of the results is unchanged with respect to the OLS results of columns 1 and 2.

Our results rest on the validity of the continuity condition (1). We test the validity of this condition in the sample of displaced workers following Lee (2007). The test is implemented by running the same OLS and IV regressions (2) and (3) on the sample of displaced workers using as a dependent variable a pre-intervention outcome, namely the wage at the time of displacement. Such outcome should not be affected by the size of the firm after displacement neither before nor after the 1990 reform, while still depending on the same unobservables (e.g. ability) likely to affect post-displacement wages. This exercise allows to test not only the validity of the continuity condition but also the conclusion that even if displaced workers are able to choose in which firm to work after displacement, they do not sort around the 15 employees threshold. A negative coefficient on the interaction between the small firm dummy and the post-reform dummy would indicate that, after the reform, individuals employed in small firms have a disproportionately lower income in their pre-displacement job. This would suggest the possibility that lower ability workers self-select, after the reform, into small firms. Results are reported in Table 9. Columns 1 and 2 show the OLS estimates. Once we control for workers and firms characteristics, OLS estimates indicate that pre-displacement wages are not significantly different for workers subsequently employed in small firms, neither before nor after the 1990 reform. Results from IV regressions reported in columns 3 and 4 confirm this conclusion. Thus, Table 9 supports the validity of the continuity condition (1) on which our identification strategy is based and the absence of sorting of displaced workers.

We are finally worried about the endogeneity of tenure. It could be the case that the increase in EPL costs also changes the incentives to fire workers and therefore increases tenure in small firms. To the extent of verifying the robustness of the results with respect to the inclusion of tenure, we re-estimate the model on a sample of workers with short tenure (between 0 and 2 years in Table 6 and between 0 and 1 years in Table 7). The results of Table 6 and 7 seem to confirm that workers pay a higher penalty in early years of tenure. The coefficient on returns to tenure is reduced in small firms with respect to big firms by almost 20% between the first and the second year and by 9% between the first and the third year. We view this result as consistent with Lazear's theory according to which workers pre-pay at entry the increase in EPL cost. The fact that the reduction in wages occurs in the early years of tenure rather than at entry may be explained by the existence of binding minimum wages at entry.

6.4 Results by industry and occupation

The results of Table 4 and Table 5 show that there is no effect on wages of new hires but there is a robust negative effect on returns to tenure for the displaced. This evidence is in favor of a Lazear effect which posits that wages adjust to offset the additional costs of EPL. However a better test of the Lazear model would also exploit the differences in firm-level wage flexibility across industry and occupation. For the Lazear model to work, we need wage setting at the firm level which is capable to undo the effects of EPL. To measure the extent of firm-level bargaining we use a measure of wage drift defined as the difference between average total compensation and the centrally bargained contractual wage. Unfortunately we do not have a measure of the wage drift at the firm level but we have average measures at the industry and occupation level from nationally representative data (see Devicienti, Maida and Sestito, 2005). In construction and among blue collars the wage drift is substantially lower than in manufacturing and white collar occupations respectively. The Lazear model predicts that where wages are free to adjust, there should be less employment adjustment and viceversa. Employment flows are measured as the rate of accessions and separations.

Table 8 shows the impact of the reform on wages (of displaced workers) and employment flows for blue collars, white collars, the manufacturing and the construction sector separately. The table indicates no clear inverse relationship between the effect on wages and employment. The reform seems to have had the expected negative effect on accessions and separations but only among blue collars there is a negative wage adjustment. This is not what we would expect according to the theory that wage adjustment is easier in sectors or occupations where the wage drift is higher, since blue collars enjoy a relative low wage drift. The explanations we offer for this evidence is that the wage drift is not a perfect measure of contractual flexibility at the firm level and that the negative correlation between adjustment through wages and quantities may be present at finer definition of industry and occupation or at the firm level.

7 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 employees. This may also have created incentives for workers to move across the threshold according to their preferences for the mix of EPL and wages prevalent in small rather than big firms.

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 returns to tenure of displaced male workers declined by around 3% in firms below 15 employees, relative to larger firms, because of the 1990 EPL. The decline is concentrated in the early years of tenure.

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 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 partially consistent with Lazear's delayed-payment scheme: using our estimates 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 (on average 16 weeks) of the last wage. On the basis of our data, the post-reform average weekly wage of an employee of 3.5 years of tenure amounts to approximately 287 euros. Therefore, the severance pay transferred to the worker amounts to 287×16 weeks$= 4,604$ euros, excluding the legal expenses that can be roughly calculated to equal 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 off-court agreement to be around 0.5 and probability that the dismissal is ruled unfair to be about 0.5. Assuming that, in case of an off-court agreement, the employer pays approximately the sum due in form of severance pay, firms below 15 employees can expect a firing cost equal to $4,604 \times 0.5 = 2,302$ euros excluding legal expenses.

On the basis of our estimates in Table 5, after 3.5 years of tenure the cumulative wage loss amounts to 19.5 euros per week or $1,011$ euros per year. This implies that firms translate around 44% of the expected firing cost onto lower wages. Of course, this calculation is valid only at the average tenure of 3.5 years.

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Table 1: Descriptive statistics (Displaced Workers)

Variables	**Small firms**	**Large firms**	**Small firms**	**Large firms**	
	Pre-reform		**Post-reform**		***Diff-in-Diff***
Log Wage	6.271	6.267	6.372	6.431	*-0.063*
	(0.237)	(0.237)	(0.234)	(0.284)	*[0.013]*
Age	36.948	36.680	38.311	39.299	
	(9.37)	(9.727)	(8.995)	(9.163)	
White collars %	0.142	0.111	0.168	0.204	
	(0.35)	(0.314)	(0.374)	(0.403)	
Firm size	12.342	17.742	12.386	17.930	
	(1.707)	(1.402)	(1.715)	(1.499)	
Tenure	0.821	0.973	2.634	2.558	
	(0.96)	(1.01)	(2.391)	(2.333)	
N	1601	1019	2178	1300	

Notes: Only firms between 10 and 20 workers are included. Standard deviations in parentheses. Standard errors in square brackets

Table 2: Firm sorting (Years 1986-1994)

	(1)	(2)	(3)
Dependent Variable: employment growth dummy			
Dummy 13	-0.008	-0.007	-0.005
	(0.008)	(0.011)	(0.011)
Dummy 14	-0.014	**-0.021**	**-0.022**
	(0.009)	(0.011)	(0.012)
Dummy 15	-0.013	**-0.029**	**-0.032**
	(0.009)	(0.012)	(0.012)
Post 1990	-	**0.049**	**0.057**
	-	(0.007)	(0.008)
Post 1990 × Dummy 13	-	-0.002	0.009
	-	(0.015)	(0.017)
Post 1990 × Dummy 14	-	0.016	**0.034**
	-	(0.016)	(0.018)
Post 1990 × Dummy 15	-	**0.035**	**0.054**
	-	(0.018)	(0.02)
Firms Fixed Effect (× 1000)	-	-	**0.280**
	-	-	(0.025)
Firms Fixed Effect × Dummy 13 (× 1000)	-	-	0.083
	-	-	(0.105)
Firms Fixed Effect × Dummy 14 (× 1000)	-	-	0.062
	-	-	(0.114)
Firms Fixed Effect × Dummy 15 (× 1000)	-	-	-0.017
	-	-	(0.118)
Post 1990 × Firms Fixed Effect (× 1000)	-	-	**-0.090**
	-	-	(0.037)
Post 1990 × Firms Fixed Effect × Dummy 13 (× 1000)	-	-	0.105
	-	-	(0.086)
Post 1990 × Firms Fixed Effect × Dummy 14 (× 1000)	-	-	**0.360**
	-	-	(0.177)
Post 1990 × Firms Fixed Effect × Dummy 15 (× 1000)	-	-	**0.455**
	-	-	(0.19)
N	104061	104061	97858

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. Only 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.

Table 3: Workers sorting (Years 1986-1994)

	(1)	(2)	(3)	(4)
Dependent Variable: mover dummy	P>15		$P \leq 15$	
Dummy 1989	**0.026**	**0.015**	**0.004**	0.000
	(0.001)	(0.001)	(0.001)	(0.001)
Dummy 1990	**0.031**	**0.022**	**0.004**	0.000
	(0.001)	(0.001)	(0.001)	(0.001)
Dummy 1991	**0.011**	0.000	**0.004**	0.001
	(0.001)	(0.001)	(0.001)	(0.001)
Small firm dummy	**-0.008**	0.0001	**0.041**	0.041
	(0.001)	(0.002)	(0.001)	(0.001)
Workers Fixed Effect (× 1000)	-	**-0.029**	-	**-0.025**
	-	(0.005)	-	(0.004)
Workers Fixed Effect × Small firm dummy (× 1000)	-	**0.043**	-	**-0.014**
	-	(0.012)	-	(0.009)
Small firm dummy × Dummy 1989	**-0.008**	**-0.017**	**-0.004**	**-0.007**
	(0.002)	(0.002)	(0.001)	(0.002)
Small firm dummy × Dummy 1990	**-0.011**	**-0.019**	**-0.006**	**-0.007**
	(0.002)	(0.002)	(0.001)	(0.002)
Small firm dummy × Dummy 1991	**0.007**	0.003	**-0.006**	**-0.008**
	(0.002)	(0.002)	(0.001)	(0.002)
Workers Fixed Effect × Dummy 1989 (× 1000)	-	-0.007	-	0.003
	-	(0.006)	-	(0.005)
Workers Fixed Effect × Dummy 1990 (× 1000)	-	**-0.028**	-	-0.001
	-	(0.007)	-	(0.005)
Workers Fixed Effect × Dummy 1991 (× 1000)	-	-0.003	-	-0.003
	-	(0.007)	-	(0.005)
Workers FE × Dummy 1989 × Small Firm Dummy (× 1000)	-	0.001	-	-0.014
	-	(0.015)	-	(0.012)
Workers FE × Dummy 1990 × Small Firm Dummy (× 1000)	-	**0.058**	-	0.001
	-	(0.016)	-	(0.012)
Workers FE × Dummy 1991 × Small Firm Dummy (× 1000)	-	**0.057**	-	0.002
	-	(0.016)	-	(0.012)
N	1603117	1412427	1603117	1412427

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 of all sizes included. All specifications include a quadratic in workers' age, year dummies, sector dummies and a polynomial in the size of the firm of origin.

Table 4: New Hires in years 1986-1994 (excl. 1990). OLS and IV estimates.

	(1)	(2)	(3)	(4)	(5)
Dependent Variable: log wage	OLS		IV		
Tenure	**0.038**	**0.020**	**0.032**	**0.033**	**0.015**
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Post 1990	**0.059**	**0.091**	0.020	**0.033**	**0.039**
	(0.004)	(0.005)	(0.012)	(0.013)	(0.012)
Small firms	**0.013**	**0.015**	0.169	0.038	-0.183
	(0.005)	(0.005)	(0.123)	(0.132)	(0.117)
Post 1990 × Small firms	-0.003	-0.002	**0.043**	0.025	0.001
	(0.005)	(0.004)	(0.02)	(0.021)	(0.019)
Post 1990 × Tenure	**-0.015**	-0.002	**-0.011**	**-0.011**	0.002
	(0.003)	(0.003)	(0.004)	(0.004)	(0.003)
Small firms × Tenure	-0.0003	-0.0001	**0.009**	**0.010**	**0.011**
	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)
Small firms × Post 1990 × Tenure	-0.002	-0.002	-0.008	-0.008	**-0.009**
	(0.003)	(0.003)	(0.005)	(0.005)	(0.005)
N	122954	122954	89248	87989	87989
Job creation/job destruction	NO	NO	NO	YES	YES
Additional controls	NO	YES	NO	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. In columns 4 and 5 we control for job creation/job destruction at the firm level. Additional controls added in columns 2 and 5 are: year dummies, sector dummies, age, age squared, and occupation. The treatment status (above/below 15 employees) is instrumented with size dummies in the pre-reform period (1987, 1988 and 1989).

Table 5: Displaced workers in years 1986-1994 (excl. 1990). OLS and IV estimates.

	(1)	(2)	(3)	(4)	(5)
Dependent Variable: log wage	OLS		IV		
Tenure	**0.015**	-0.004	0.013	0.010	0.000
	(0.008)	(0.009)	(0.01)	(0.01)	(0.01)
Post 1990	**0.160**	**0.135**	**0.219**	**0.194**	**0.106**
	(0.018)	(0.016)	(0.04)	(0.037)	(0.038)
Small firms	-0.020	-0.023	-0.357	-0.208	0.037
	(0.021)	(0.019)	(0.196)	(0.21)	(0.188)
Post 1990 × Small firms	**-0.040**	0.002	**-0.132**	-0.098	-0.002
	(0.021)	(0.018)	(0.061)	(0.059)	(0.053)
Post 1990 × Tenure	-0.008	0.009	-0.010	-0.006	0.006
	(0.008)	(0.009)	(0.011)	(0.011)	(0.011)
Small firms × Tenure	**0.030**	**0.035**	**0.033**	**0.033**	**0.029**
	(0.01)	(0.008)	(0.013)	(0.013)	(0.012)
Small firms × Post 1990 × Tenure	**-0.030**	**-0.038**	**-0.027**	**-0.029**	**-0.032**
	(0.01)	(0.009)	(0.015)	(0.015)	(0.013)
N	6098	6098	4702	4638	4638
Job creation/job destruction	NO	NO	NO	YES	YES
Additional controls	NO	YES	NO	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. In columns 4 and 5 we control for job creation/job destruction at the firm level. Additional controls added in columns 2 and 5 are: year dummies, sector dummies, age, age squared, and occupation. The treatment status (above/below 15 employees) is instrumented with size dummies in the pre-reform period (1987, 1988 and 1989).

Table 6: Displaced workers with at most three years of tenure. Years 1986-1994 (excl. 1990) OLS and IV estimates.

	(1)	(2)	(3)	(4)	(5)
Dependent Variable: log wage	OLS		IV		
Tenure	0.006	-0.013	0.001	-0.001	-0.010
	(0.01)	(0.01)	(0.011)	(0.011)	(0.012)
Post 1990	**0.144**	**0.148**	**0.139**	**0.119**	0.073
	(0.019)	(0.018)	(0.046)	(0.045)	(0.044)
Small firms	0.020	-0.001	-0.005	0.126	0.181
	(0.023)	(0.021)	(0.18)	(0.191)	(0.164)
Post 1990 × Small firms	**-0.041**	0.002	-0.034	-0.009	0.032
	(0.023)	(0.02)	(0.07)	(0.068)	(0.06)
Post 1990 × Tenure	0.020	0.029	0.052	0.054	0.044
	(0.017)	(0.016)	(0.032)	(0.032)	(0.03)
Small firms × Tenure	**0.041**	**0.052**	**0.048**	**0.043**	**0.043**
	(0.013)	(0.011)	(0.014)	(0.015)	(0.014)
Small firms × Post 1990 × Tenure	**-0.043**	**-0.051**	**-0.089**	**-0.087**	-0.067
	(0.021)	(0.018)	(0.047)	(0.047)	(0.042)
N	4322	4322	3165	3101	3101
Job creation/job destruction	NO	NO	NO	YES	YES
Additional controls	NO	YES	NO	NO	YES

Notes: Only firms between 10 and 20 workers and workers with at most three years of tenure are included. Robust standard errors in parentheses allow for clustering by individual. All specifications include a third degree polynomial in firm size. In columns 4 and 5 we control for job creation/job destruction at the firm level. Additional controls added in columns 2 and 5 are: year dummies, sector dummies, age, age squared, and occupation. The treatment status (above/below 15 employees) is instrumented with size dummies in the pre-reform period (1987, 1988 and 1989).

Table 7: Displaced workers with at most two years of tenure. Years 1986-1994 (excl. 1990) OLS and IV estimates.

	(1)	(2)	(3)	(4)	(5)
Dependent Variable: log wage	OLS		IV		
Tenure	**-0.027**	**-0.037**	**-0.028**	**-0.033**	-0.033
	(0.014)	(0.015)	(0.015)	(0.016)	(0.018)
Post 1990	**0.135**	**0.144**	0.084	0.073	0.049
	(0.02)	(0.021)	(0.049)	(0.048)	(0.046)
Small firms	0.033	0.009	0.031	0.080	0.113
	(0.025)	(0.023)	(0.161)	(0.172)	(0.159)
Post 1990 × Small firms	-0.032	0.010	0.038	0.045	0.087
	(0.024)	(0.02)	(0.07)	(0.069)	(0.063)
Post 1990 × Tenure	**0.051**	**0.060**	**0.167**	**0.166**	**0.166**
	(0.027)	(0.025)	(0.051)	(0.051)	(0.048)
Small firms × Tenure	**0.082**	**0.084**	**0.081**	**0.076**	**0.065**
	(0.017)	(0.016)	(0.021)	(0.023)	(0.023)
Small firms × Post 1990 × Tenure	**-0.072**	**-0.087**	**-0.226**	**-0.212**	**-0.216**
	(0.033)	(0.029)	(0.073)	(0.074)	(0.068)
N	3444	3444	2480	2416	2416
Job creation/job destruction	NO	NO	NO	YES	YES
Additional controls	NO	YES	NO	NO	YES

Notes: Only firms between 10 and 20 workers and workers with at most two years of tenure are included. Robust standard errors in parentheses allow for clustering by individual. All specifications include a third degree polynomial in firm size. In columns 4 and 5 we control for job creation/job destruction at the firm level. Additional controls added in columns 2 and 5 are: year dummies, sector dummies, age, age squared, and occupation. The treatment status (above/below 15 employees) is instrumented with size dummies in the pre-reform period (1987, 1988 and 1989).

Table 8: Effects by sector and occupation in years 1986-1994 (excl. 1990). IV estimates.

		(1)	(2)	(3)
Dependent Variable		Log wage (Displaced workers)	Accession dummy	Separation dummy
All Sectors	Post 1990 × Small Firm	-0.002	**-0.129**	**-0.081**
		(0.053)	(0.017)	(0.013)
	Small firms × Post 1990 × Tenure	**-0.032**	**-**	**-**
		(0.013)	**-**	**-**
	N	4638	156901	156901
Manufacturing	Post 1990 × Small Firm	-0.102	**-0.078**	-0.032
		(0.131)	(0.032)	(0.023)
	Small firms × Post 1990 × Tenure	-0.008	-	-
		(0.03)	-	-
	N	1189	42124	42124
Constructions	Post 1990 × Small Firm	-0.143	**-0.143**	**-0.055**
		(0.115)	(0.036)	(0.025)
	Small firms × Post 1990 × Tenure	-0.025	-	-
		(0.04)	-	-
	N	634	19015	19015
White Collars	Post 1990 × Small Firm	-0.234	**-0.119**	**-0.124**
		(0.157)	(0.033)	(0.029)
	Small firms × Post 1990 × Tenure	0.068	-	-
		(0.049)	-	-
	N	673	27306	27306
Blue Collars	Post 1990 × Small Firm	-0.041	**-0.124**	**-0.062**
		(0.053)	(0.019)	(0.014)
	Small firms × Post 1990 × Tenure	**-0.035**	-	-
		(0.014)	-	-
	N	3894	129595	129595

Notes: The dependent variables in columns 3 and 4 are, respectively, a dummy that takes the value of 1 if a match (repsectively, a separation) between worker *i* and firm *j* occurs at time *t* and 0 otherwise. Only firms between 10 and 20 workers are included. All specifications include a third degree polynomial in firm size, job creation/job destruction at the firm level, year dummies, sector dummies, age, age squared, tenure and occupation. The treatment status (above/below 15 employees) is instrumented with size dummies in the pre-reform period (1987, 1988 and 1989).

Table 9: Effects of the reform on workers wages and firm size at displacement. IV estimates

	(1)	(2)	(3)	(5)
Dependent Variable: wage at displacement	OLS		IV	
Post 1990	**0.164**	**0.112**	**0.103**	**0.108**
	(0.023)	(0.026)	(0.059)	(0.059)
Small firms	-0.001	-0.013	0.267	0.380
	(0.032)	(0.03)	(0.298)	(0.315)
Post 1990 × Small firms	**-0.055**	-0.018	0.025	0.063
	(0.028)	(0.024)	(0.086)	(0.083)
N	2092	2092	1445	1445
Controls	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. Controls added in columns 2 and 4 are: year dummies, sector dummies, age, age squared, and occupation. The treatment status (above/below 15 employees) is instrumented with size dummies in the pre-reform period (1987, 1988 and 1989).