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

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Marco Leonardi[*] and Giovanni Pica[**]

Abstract

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 the slight average wage reduction (between –0.4 and –0.1 percent) that follows the increase in EPL hides highly heterogeneous effects. Workers who change firm during the reform period suffer a drop in the entry wage, while incumbent workers are left unaffected. Results also indicate that the negative wage effect of the EPL reform is stronger on young blue collars and on workers at the low-end of the wage distribution. Finally, workers in low-employment regions suffer higher wage reductions after the reform. This pattern suggests that the ability of the employers to shift EPL costs onto wages depends on workers' and firms' relative bargaining power.

Keywords: Costs of Unjust Dismissals, Severance Payments, Policy Evaluation, Endogeneity of Treatment Status.

JEL Classification: E24, J3, J65.

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1 Introduction

Since the work of Lazear (1990), it is well-known that, in a perfectly competitive labour market and in the absence of contractual frictions, a government-mandated pure transfer (e.g., a severance payment) from firms to risk neutral workers can be neutralised by an appropriately designed wage contract that lowers the entry wage by an amount equal to the expected present value of the future transfer. A negative effect of Employment Protection Legislation (EPL) on the entry wage is also predicted by models with labour market frictions and decentralized bargaining in which job security provisions weaken the threat point of the firm in negotiations with already employed workers. In both contexts, the impact of EPL on the wage profile is tantamount to a newly-hired worker posting a bond equal to her share of the future severance payment (Mortensen and Pissarides, 1999; Ljungqvist, 2002; Garibaldi and Violante, 2005).

Thus, theory predicts a differential impact of firing costs across incumbent and newly-hired workers – *insiders* and *outsiders* in Lindbeck and Snower (1988) terminology. The impact of EPL on wages is also likely to be heterogeneous across workers with different characteristics potentially correlated with their bargaining position and across local labour markets. For instance, workers with a high degree of risk aversion who value job security may be more willing to accept a wage cut in exchange for an increase in EPL (Pissarides, 2001; Bertola, 2004), while workers in tight labour markets may be able to avoid paying for the increased protection thanks to a better outside option (Pissarides, 2000).

Summing up, the impact of EPL on wages is likely to depend on the bargaining position of workers vis-à-vis employers, which in turn depends on the labour market status of the worker (incumbent/new-hire), on her individual characteristics and on the aggregate labour market conditions that determine firms' and workers' outside options.

This paper provides an empirical analysis of the effects of EPL on wages, exploiting the variation in EPL induced by an Italian reform which raised dismissal costs for firms with 15 or fewer employees and left costs unchanged for larger firms. We provide complementary evidence to Kugler and Pica (2008) who use the same reform episode to estimate, as most of the empirical literature on EPL, the effect of firing costs on worker and job flows. We focus, instead, on the adjustment through the wage rate, a margin on which the available evidence is scant and provides ambiguous results, as will be discussed in Section 2.1. The contribution of this paper is not only to quantify the causal effect of EPL on wages, but also and foremost to highlight the heterogeneity of the effect and to relate it to the role of the relative bargaining power of workers and firms. This allows us to draw a general lesson, which goes arguably beyond the Italian experience, on the heterogeneity of the effects of EPL across workers with different bargaining positions and on the extent to which firms are able to translate higher EPL costs onto lower wages.

The analysis is based on 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 in the north-eastern region of Veneto. 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 a tight labour market which makes it similar to many manufacturing regions in Europe. Moreover, as explained below, the richness of this dataset allows us to build suitable instruments and apply IV techniques.

Our identification strategy exploits a reform of EPL rules which provides variation both across firms and over time. Until 1990 the Italian labour code provided a sharp discontinuity in the application of EPL, with no protection for workers employed in small firms below the 15 employee threshold and high protection for those employed in firms above the threshold. In July 1990, severance payments were increased from zero to between 2.5 and 6 months of pay for firms with 15 or fewer employees, and left unchanged for firms with more than 15 employees.

We identify the effects of EPL comparing wages of workers employed in firms in the neighbourhood of the 15 employees threshold before and after the law change, thus combining a regression discontinuity design (RDD) with a difference-in-difference (DID) approach. Our identification assumption is essentially that 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.

In the course of the paper we address the potential endogeneity of the treatment status. On the one side, it is possible that marginal firms, which kept their size just below the 15 employees threshold to avoid strict EPL rules, increased their size because of the reform. To control for firms' sorting into (or out of) the treatment group according to time-invariant characteristics, we estimate models with firm and match (worker-firm) fixed effects. Additionally, we instrument the treatment status with firm size in 1988 and 1987, when the reform was not in place and was unexpected. On the other side, workers may also sort in firms below or above the 15 employees threshold according to their preferences over the mix of employment protection and wages. To control for workers' sorting into firms around the threshold according to fixed individual characteristics, we estimate the model using worker fixed effects.

Another concern is the "common time effects" assumption implicit in the DID approach, that assumes that the pre-reform wage trends are parallel in the treatment and control groups. Even though close to the threshold this assumption sounds reasonable, we also adopt a triple-difference strategy that exploits the different relevance of the constraints imposed by EPL on firms in different sectors. In particular, we look at whether the impact of EPL is greater in industries in which the need for labour reallocation is higher.

Our analysis leads to the following results. Baseline estimates indicate a small but significant wage loss in firms below the 15 employees threshold relative to larger firms after the 1990 reform that ranges, on average, between 0.4 and 1 percentage point. Additionally, we find that wages fell more markedly in sectors with greater pre-reform employment volatility. This rules out the concern that results are driven by small firm wages being on a declining time path with respect to wages in larger firms.

The negative effect is not uniformly spread across workers. As suggested by theory, we first check whether the EPL reform has a different impact on insiders and outsiders. To this aim, we identify two groups of workers:

(i) a group of *incumbent* workers, which includes all individuals who are already employed in a firm at the time of the introduction of the reform in 1990 and stay continuously in the same firm over our sample period; and (ii) a group of *movers*, i.e. a group of workers who change firm at least once, before or after 1990, for whom we observe at least one entry wage. The evidence indicates that after the reform movers suffer a drop in the wage rate of about 2 percent in treated firms below the threshold relative to larger firms, while incumbent workers are left unaffected. Additionally, the estimates show that the negative effect on movers is concentrated upon entry in the new firm, with a reduction of the entry wage of between 3.4 and 6.3 percent. Post-entry wages of movers are unaffected by the introduction of the EPL reform. Thus, on the one hand firms seem to be able to translate (part of) the cost of EPL onto workers before the match is formed, when they do not incur in any severance payment if there is no agreement. On the other hand, incumbent workers and movers (in the years after entry in the new firm) do not seem to be able to renegotiate their wages upwards (McLeod and Malcomson, 1993).

We then look at whether the impact of EPL on wages depends on individual characteristics presumably correlated with workers bargaining power in the employment relationship, and find that the negative wage effect of the EPL reform is stronger among low-bargaining power workers, such as young blue collars workers.[1] Furthermore, but unfortunately only for part of the sample, we have a direct measure of bargaining power, given by the individual wage premium over the sectoral contractual minimum – the wage drift.[2] Results from quantile regressions suggest that workers at the 5^{th} percentile of the distribution of the wage drift suffer a wage reduction five times as large as workers at the 90^{th} percentile and three times as large as the average effect.

Finally, we exploit the theoretical insight that the bargaining power of workers should also depend on the local labour market conditions that affect workers' and firms' relative outside options, through the probabilities of finding a job and filling a vacancy. To test this idea we look at a sample of workers over the whole Italian territory and proxy the tightness of the local labour markets using the regional employment rates of males aged 25-64. We find that workers in low-employment regions suffer higher wage reductions in firms below the threshold relative to larger firms after the reform.

The rest of this paper is organized as follows. Section 2 lays out the theoretical (Section 2.1), empirical (Section 2.2) and institutional (Section 2.3) background of our empirical analysis. Section 3 describes the dataset and the sample selection rules. Section 4 explains the identification strategy used to evaluate the impact of EPL on wages. Section 5 presents estimates of the impact of increased strictness of employment protection in small firms in Italy after 1990 on average wages. Section 6 investigates the heterogeneity of the wage effects along multiple dimensions: insiders/outsiders, workers with different observable characteristics correlated with their bargaining position, and wage effects at different point of the wage drift distribution and in regions with

[1] Dolado et al. (2007) analyse the heterogeneous effect of stricter dismissal procedures for different groups of workers within a matching framework and show that the wages of low- and high-productivity workers react differently to different types of EPL reforms.

[2] Although at the time of the reform wages in Italy were set through a highly centralized bargaining process at the sectoral level, there was still room left for firms to react to the law change because contractual minimum wages were in most cases hardly binding and an important component of workers' compensation was determined at the firm and individual level in the form of company-level wage increments, production bonuses and other variable benefits (Guiso et al., 2005). In terms of the magnitude of the firm-specific part of the wage, between one sixth and one quarter of the compensation was 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.

different labour market conditions. Finally, Section 7 discusses the results and concludes with a back of the envelope calculation of the share of EPL costs translated into lower wages.

2 Background

2.1 Theoretical background

Under Nash bargaining, firing costs affect firms' outside option in bilateral wage negotiations with workers because, in the absence of a wage agreement, firms have to pay severance with an associated drop in profits.[3]

In their standard matching model with a two-tier wage structure, Mortensen and Pissarides (1999) assume that the firm does not incur in any firing costs if there is no agreement on the wage in the first encounter because no employment relationship yet exists. In this framework workers prepay for higher EPL *via* lower entry wages and enjoy a subsequent one-shot increase in post-entry wages.[4] Ljungqvist (2002) shows that this formulation is formally equivalent to assuming that the relative split of the surplus of the match is left unaffected by firing costs throughout the employment relationship. The equivalence arises because the wage profile under the Mortensen and Pissarides (1999) set-up is homomorphic to new workers posting a bond equal to their share of any future expected firing costs (as in Lazear (1990), discussed in the introduction). Therefore, the model predicts a negative effect of EPL on workers' cumulative wage bill (net of firing costs) and a distinction between the effect on entry and post-entry wages.

In order to better understand the mechanism, consider the choice facing the firm and the worker when they first meet (Pissarides, 2000). If they sign the contract the pay-off to the firm is J_0. Therefore the initial wage w_0 is chosen to maximize the product $B_0 = (W_0 - U)^{\beta}(J_0 - V)^{1-\beta}$ where V and J_0 are the firm's net worth from a vacancy and from a job paying w_0, and W_0 and U are the worker's net worth from a job paying w_0 and unemployment. After the worker is taken on, if the firm fails to agree on a continuation wage its loss will be $J_+ + F$ where F is the firing cost. Therefore, the post-entry wage maximizes $B_+ = (W_+ - U)^{\beta}(J_+ + F - V)^{1-\beta}$. Of course, a crucial point is whether workers are able to force a wage renegotiation after entry. MacLeod and Malcomson (1993) argue that they cannot because they have no credible threat if the firm refuses to renegotiate. However, if renegotiation takes place, the model predicts tighter employment protection to increase wages in post-entry negotiations to reflect the fact that, once the job has started, the firm is locked into the relationship by the firing tax. Anticipating this, employers reduce wages at entry when the worker is still unprotected.[5]

While the model described above offers predictions on the effect of the introduction of EPL on the wage level, it is not equally helpful in understanding the effects of EPL on workers' wage-tenure profile. In that model, an

[3]Recent theory has highlighted the crucial role of wage determination mechanisms for the analysis of the employment effects of firing costs in matching models. On this, see especially Ljungqvist (2002) but also Garibaldi and Violante (2005) and Cahuc and Koeniger (2007) in the *Economic Journal* special issue on EPL.

[4]In this formulation an increase in EPL leads to an increase of equilibrium employment. Alternatively, firing costs may be assumed to weaken the firm's threat point from the first negotiation with a newly hired worker, thereby increasing the worker's relative share of the surplus of the match. In this case, firms are not able to undo the detrimental effect of firing costs on profits by reducing worker's wages. Workers become insiders – and extract the associated rents – since the first encounter with the firm and suffer no wage losses. Ljungqvist (2002) shows that, under this hypothesis, EPL reduces the equilibrium employment level.

[5]Lindbeck and Snower (1988) reject the plausibility that firms can make outsiders fully prepay the EPL cost via a lower wage for a number of reasons that limit the downward flexibility of wages, such as the presence of minimum wages. This implies that firms will fail to translate the entire costs of EPL on workers. We will return on this issue in Section 7.

increase in EPL implies a stepwise increase in the wage of incumbent workers (and also of movers' post-entry wages) if workers can force a wage renegotiation, but does not change the *slope* of the wage-tenure profile. Indeed, virtually all theoretical studies in the literature (with some exceptions, see for example Cozzi, Fella and Violante, 2011) assume that productivity – and therefore wages – does not vary with tenure. This assumption makes these models unsuitable to study how EPL affects the evolution of workers' wages with respect to their tenure in the firm.

However, results from the empirical literature suggest at least two contrasting channels through which EPL may affect workers' tenure profile. On the one side, stricter EPL may raise the incentives to invest in firm-specific human capital (Autor et al., 2003; Wasmer, 2006; Belot et al., 2007; Cingano et al., 2010) thus raising productivity growth and the slope of the wage-tenure profile. On the other side, higher EPL may reduce the incentives to exert effort and may therefore imply a lower growth rate of productivity and a flatter wage-tenure profile (Ichino and Riphahn, 2005). Eventually, the effect of EPL on the relationship between wages and tenure is an empirical question that we will address in Section 5.

2.2 Related empirical literature

While there is a large empirical literature on the effects of EPL on job flows, relatively little empirical evidence is available on the wage effects of dismissal costs.[6] Using aggregate data Bertola (1990) shows that in high job security countries wages tend to be lower. More recently, using firm-level data, Martins (2009) shows that lower 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 labour expenses of U.S. commercial banks. Autor et al. (2007) and Cingano et al. (2010) look at the effect of EPL on firm-level productivity.

More related to this paper are the studies conducted on individual data. These papers reach disparate conclusions: Autor et al. (2006) find no evidence that wrongful-discharge laws had a significant negative 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 a reduction in firing costs and payroll taxes had 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 these studies in many respects. First, it is the only study that looks explicitly at the heterogeneity of the wage effects of EPL in relation to workers' bargaining power. Second, identification is cleanly achieved by means of a Regression Discontinuity Design combined with a Difference-in-Difference approach. Third, differently from Martins (2009) and Bird and Knopf (2009) we look at individual wages rather than average firm-level wages. Finally, we look at all workers and not only at displaced workers as Cervini Plà et al. (2010) and address explicitly the issue of endogeneity of the treatment status with instrumental variables.

[6] Previous empirical literature mostly concentrates on the effects of EPL on employment flows. Among the many papers in the literature, only few 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.

2.3 Institutional background

As a form of worker protection for open-ended contracts, labour codes specify the causes for fair dismissal, and establish workers' compensation depending on the reason for termination.[7]

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 either as fair "subjective" motive or as just cause). However, in any case 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 loosely on tenure and firm size. 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.[8]

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 small firms. This law introduced severance payments of between 2.5 and 6 months pay for unfair dismissals in firms with 15 or fewer employees.[9] 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.

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. At the end of the paper we will comment on the relative importance of the adjustment through employment and wages.

The 15 employees threshold is not only relevant for EPL provisions but also for the establishment of the so called "Rappresentanze Sindacali Aziendali" (RSA). Workers of firms with more than 15 employees can elect trade union representatives at firm level (RSA), who can call general meetings and referendum and affix posters on union activities. They also have the right to vote for a worker representative for safety related issues. The practical relevance of this rule, however, is likely to be minor, as collective agreements which set minimum wage by worker qualification also apply to workers and firms that do not belong to unions. In any case, what is

[7] 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). Differently from open-ended contracts, temporary contracts can be terminated at no cost provided that the duration of the contract has expired.

[8] 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.

[9] The law prescribes that the 15 employees threshold should refer to establishments rather than firms. Although in our data we only have information at the firm level, this is unlikely to be a concern because in the empirical analysis we focus on firms between 5 and 25 employees that are plausibly single-plant firms.

relevant for our identification strategy is that there are no reforms on this matter over our sample period.

In addition, in 1992 two other legislative changes were introduced. The first was a pension reform which changed retirement ages and reference periods for calculating pensions. The second initiative eliminated a wage indexation mechanism (*Scala Mobile*) that had been in place since 1945 for firms of all sizes. Since these reforms do not apply differentially to firms of different sizes, our identification strategy fully controls for differences in outcomes due to the pension and wage indexation reforms.

The only reform that may potentially confound our results is the collective dismissals reform that took place in 1991. 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. The collective dismissals' procedures require a credible risk of bankruptcy and firms are required to engage in negotiations with unions and the 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 i.e. collective dismissals do not impose additional firing costs on firms. In Section 5 we will test whether our results are confounded by the effects of this other reform.

3 Data description

This paper uses the VWH dataset which is an employer–employee panel with information on the characteristics of both workers and firms.[10]

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 located in the north-eastern part of the country. The overall population in the two provinces was 1.6 million people (2.7% of the total Italian population) as of the 2001 Population Census. Starting from a relatively backward economic condition after World War II, Veneto enjoyed fast growth in the post-war period first reaching the national average GDP per capita and then outgrowing it: in 2000 GDP per capita in Veneto was 20% higher than the national average.

There are two reasons to use these data. The first is that, although limited to two 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 a tight labour market which makes it similar to many manufacturing regions of France or Germany. Therefore the results may be relevant for other labour markets outside Veneto.[11] The second reason is that we need the information on the universe of workers and firms to be able to build suitable instruments for firm size and apply IV techniques. A random sample of the whole Italian working population is available from the same administrative source, but it is a representative sample of workers and

[10] Cingano and Rosolia (2012) investigate network effects on job finding probabilities using the same data.

[11] 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*. Over the last decades, Veneto has been 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 (Tattara and Valentini, 2005).

not of firms. Therefore, it neither allows us to follow firms over time nor to build appropriate instruments for firm size to estimate IV models. For this reason, we only use the nationwide random sample in Section 6.4 to look at the differential effect of the reform across local labour markets.

The VWH dataset includes universal information on all firms and employees working at least one day in any firm of the two provinces from 1985 to 1997. In particular, it includes 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 and firm size measured as the average (weighted by number of worked months), number of employees.[12] 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 (in the private sector) out of Treviso and Vicenza.

The original archives only include information on private sector firms in the manufacturing and service sectors, therefore all workers in the public sector, agriculture and self-employment are excluded. This selection is common for administrative data which typically include the private sector only. 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 only reason of dropping out of the dataset is exit from the private sector or from employment status altogether. The data stop following individuals who move into self-employment, public sector, agricultural sector, underground economy, unemployment, or retirement.[13]

3.1 Sample selection

We select (*i*) all males between 20 and 55 years of age, (*ii*) hired on an open-ended contract, (*iii*) with a valid wage between 1989-1993. We exclude females because the trade-off between job security and wages is likely to be affected by fertility decisions on which we have no information. For instance, Prifti and Vuri (2011) find that the strengthening of the EPL regime imposed by the 1990 reform positively affected the propensity to childbearing for Italian working women and may have changed their incentives to participate to the labour market. They find that young female workers in small firms below the 15 employee threshold after the reform had higher incentives to both give birth and return back to work afterwards. Depending on whether this compositional effect is stronger for low- or high-wage women this may generate an upward or downward bias on our estimates.[14]

[12]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. In particular, the labour code excludes from the threshold's calculation apprentices and temporary workers below nine months, and includes part-time workers and all other temporary contracts in proportion to their actual time worked during the week. For this reason firm size is computed as the average number of employees weighted by number of worked months in the firm during the year.

[13]Using Bank of Italy survey data (SHIW), we find that at the beginning of our sample period in 1989 the private sector constitutes 52 percent of total employment of males aged 20 to 55; agriculture represents only 2 percent while public employment and self employment represent 23 percent each. The vast majority of private sector workers do not move out of private sector employment: after two years 83% of males aged between 20 and 55 employed in the private sector in 1989 are still private sector workers; 6.7% move to the public sector, only 2.3% to self employment and to agriculture and 5.7% become unemployed or retire early. These figures are stable over time and transition rates for different years are very similar.

[14]Additional selection issues make the analysis on Italian women difficult. The female employment rate in Italy over our sample period (1989-1993) was a very low 35%, about half of the male employment rate and well below northern European figures. Olivetti

We also exclude workers on temporary contracts because employment protection provisions are guaranteed only to workers on open-ended contracts. In order to stay as close as possible to the reform year 1990, we focus on the period 1989–1993 excluding years 1988 and 1987 to be able to use firm size in those pre-reform out-of-sample years as instruments for current firm size. We also remove year 1990 because the reform occurred in the month of July and the annual wages of year 1990 are likely to be a mixture of pre-reform and post-reform wages. However, we will test the robustness of our results both to different time periods (Table B.8) and to the inclusion of 1990 (Table 2). To preserve comparability between 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 (annual wages divided by the number of weeks worked) 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 includes 9,914 firms and 29,177 workers for a total of 96,333 observations. We observe an entry wage for about one third of the sample, namely for the 9,667 workers (28,451 observations) who changed firm at least once over our sample period. The remaining 19,510 workers (67,882 observations) stayed with the same firm throughout the sample period.

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 employed in small firms, both before and after the reform. The real weekly wage of workers in large firms is around 307 (331) Euro at 1995 prices per week before (after) the reform vs. a significantly lower wage of 293 (313) Euro per week in small firms before (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 and, as expected, have a slightly lower turnover (i.e. they employ a lower proportion of movers). Tenure is on average equal to 3.5 years before the reform and 5.5 years after the reform in both small and large firms.

4 Identification strategy

The estimand of interest is the average treatment effect of EPL on wages. We exploit both the discontinuity in EPL at the 15 employees threshold and the reform of EPL which affected only small firms 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 any variable that affects wages is either continuous at the threshold (as in standard RDD) or its discontinuity is constant over time (as in standard DID). Another identification assumption is

and Petrongolo (2008) document that in countries in which female employment rates are low, female selection into employment is non-random as only high-productivity females self-select into employment. A further and more serious selection problem may be induced by the reform itself. Before Prifti and Vuri (2011), Adserà (2004) documents that unstable employment relationships depress fertility and Bratti et al. (2005) find that Italian women who enjoy a greater amount of employment protection have a higher incentive to return to work in the three years following childbirth compared to female workers in less protected jobs.

Table 1. Descriptive Statistics

	Pre-reform		Post-reform	
	Small firms	Large firms	Small firms	Large firms
Real weekly wages	293.738	307.454	312.923	331.243
	(72.011)	(82.479)	(78.545)	(90.367)
Firm size	9.604	19.496	9.541	19.551
	(2.953)	(2.824)	(2.958)	(2.830)
White collar dummy	0.134	0.161	0.133	0.165
	(0.340)	(0.368)	(0.340)	(0.371)
Movers (proportion)	0.309	0.258	0.313	0.268
	(0.462)	(0.438)	(0.464)	(0.443)
Age	34.565	34.990	37.489	37.918
	(8.556)	(8.498)	(8.675)	(8.623)
Tenure	3.50	3.61	5.51	5.68
	(1.62)	(1.59)	(2.52)	(2.47)
Sectoral dummies:				
Agriculture	0.007	0.004	0.006	0.005
	(0.080)	(0.065)	(0.077)	(0.071)
Gas-water-oil	0.001	0.000	0.001	0.000
	(0.030)	(0.000)	(0.030)	(0.000)
Extraction-minerals-chemical	0.079	0.091	0.077	0.103
	(0.270)	(0.288)	(0.267)	(0.305)
Metal	0.274	0.330	0.271	0.311
	(0.446)	(0.470)	(0.445)	(0.463)
Manufacturing	0.244	0.297	0.237	0.292
	(0.430)	(0.457)	(0.425)	(0.455)
Construction	0.154	0.110	0.163	0.109
	(0.361)	(0.313)	(0.369)	(0.312)
Wholesale-retail-hotel	0.182	0.108	0.184	0.118
	(0.386)	(0.311)	(0.388)	(0.323)
Transportation	0.031	0.025	0.034	0.026
	(0.173)	(0.156)	(0.180)	(0.158)
Banks-insurance	0.012	0.012	0.010	0.014
	(0.107)	(0.110)	(0.099)	(0.118)
N	15965	8342	45848	26178

Notes: Sample of years 1989-1993, all males aged 20-55 with an open-ended contract in firms of between 5 and 25 employees. Real wages are expressed in 1995 Euro. Movers are defined as workers who change firm at least once over the period 1989-1993. Standard deviations in parentheses.

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, i.e. the trend of wages paid in firms above 15 employees represents a good counterfactual for the trend of wages paid in firms with 15 or fewer employees, a reasonable assumption in a neighbourhood of the threshold.

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 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 15 or fewer employees and 0 if the worker is employed in a firm with strictly more than 15 employees (in the following we will refer to this dummy as the *small-firm dummy*). 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.[15]

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 prevent identification of the post-reform dummy). The reported standard errors account for possible error correlations at the individual level in all specifications.

As remarked above, our differences-in-differences strategy relies on the "common time effects" assumption, according to which treated and untreated workers are on the same time path. However, small firms might well be on a declining time path with respect to larger firms above the threshold. Additionally, the differences-in-differences approach allows us to control for macro shocks coinciding with the reform but assumes that these shocks have similar effects on firms on the two sides of the threshold. It is possible, however, that the business cycle affects small and large firms differently. For example, if small firms are affected more by downturns, then we should have observed lower wages in small relative to large firms during the post-reform period due to the strong recession of 1992 and 1993. Violation of these assumptions – though unlikely in a small neighbourhood of the 15 employees threshold – would bias our results. To account for these potential sources of bias, we allow for the possibility that the introduction of dismissal costs may have stronger effects in more volatile sectors,

[15] Results are robust to this functional form assumption. Alternatively, a split polynomial 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. See Section 5.1 for robustness exercises.

where these costs are likely to bind (Micco and Pagés, 2006), as follows:

$$\begin{aligned} Y_{ijt} \quad &= \quad \beta' X_{ijt} + \gamma_1 D^S_{jt} + \gamma_2 \left(D^S_{jt} \times Post \right) + \gamma_3 V^k_S + \gamma_4 \left(V^k_S \times D^S_{jt} \right) + \\ & \gamma_5 \left(V^k_S \times Post \right) + \gamma_6 \left(V^k_S \times Post \times D^S_{jt} \right) + \sum_{k=1}^{3} (\gamma_k fsize^k_{jt}) + e_{ijt}. \end{aligned} \tag{2}$$

The variable V^k_S denotes the variance of employment growth in sector k during the pre-reform period calculated separately for firms above and below the 15 employees threshold. The coefficient γ_6 captures the differential effect of the reform in firms below the threshold relative to larger firms with more than 15 employees in sectors with different pre-reform employment volatility.

Equations (1) and (2) give unbiased estimates only if workers and firms are exogenously assigned to the treatment status. However, the conditional comparison of wages in firms on the two sides of the threshold does not provide an unbiased estimate of the average treatment effect if firms and workers with different unobservable characteristics endogenously choose their treatment or control status: individuals may decide to work in firms below or above the threshold, and firms in turn may decide to grow above or shrink below 15 employees. Thus, a fundamental concern of this paper is the non-random selection of workers and firms above and below the 15 employees threshold to which we now turn.

4.1 The distribution of firm size and firm sorting

Identification in Equations (1) and (2) 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, firm fixed effects do not account for the selection due to the reform itself. Firms in a neighbourhood of the 15 employees threshold may change their size in response to the 1990 reform of EPL, thus biasing the estimates. For example, firms which kept their size just below 15 employees 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 and depends on which type of firms moved because of the reform. If firms which moved from below to above the threshold were those with bad growth perspectives and lower productivity (thus reducing the distance between wages paid in the treatment and in the control group of firms), then presumably OLS estimates understate the negative effect of the reform on wages. On the other hand, OLS would overestimate the negative effects if firms which moved were high-productivity firms.

In this section, we assess the validity of our identification strategy with two different procedures. First, we formally check for the absence of manipulation of the running variable (violated if firms were able to alter their size and sort above or below the threshold) testing the null hypothesis of continuity of the density of firm size at 15 employees as proposed by McCrary (2008). Second, we check whether the probability of firm growth around the 15 employees threshold changes after the reform.

In the left panels of Figure 1, we plot the frequency of firms between 5 and 25 employees in year 1989 (before the reform) and in year 1991 (after the reform). Visual inspection does not reveal any clear discontinuity at the 15 employees threshold, only a small dip in the distribution at 16. In the right panels of Figure 1, we formally test for the presence of a density discontinuity at the threshold by running kernel local linear regressions of the log of the density separately on both sides of the threshold (McCrary, 2008). There, no evidence of manipulative sorting can be detected: 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, neither in 1989 nor in 1991. The point estimate is -0.071(0.090) in 1989 and 0.141(0.087) in 1991.[16]

The reader may be puzzled by the apparent inconsistency between a large difference in EPL at the 15 employees threshold and the continuity of the firm size distribution. The average firm size in Italy is approximately half that of the European Union and expensive EPL for firms with more than 15 employees is often indicated as one of the factors responsible for such a skewed size distribution. This claim does not seem to be confirmed in the data: Schivardi and Torrini (2008) and Borgarello, Garibaldi and Pacelli (2004) find that firms just below 15 employees are only about 2% less likely to grow than larger firms (this result is confirmed in our data, see below). Schivardi and Torrini (2008) explain this finding arguing, first, that firms on a growing (or shrinking) pattern may find themselves temporarily slightly above 15 employees and, second, that firms may adjust other margins to cope with stricter EPL.

One possibility, investigated in this paper, is that protected workers pay for the additional EPL with lower wages. Another possible adjustment margin is through hours, which we partially address looking at weekly wages. Yet another margin – outside the scope and the data availability of this paper – is investment (Cingano et al. 2010): stricter EPL may induce firms to substitute rigid labour with capital and raise the capital-labour ratio. It is noteworthy that the fact that EPL seems to have small or no threshold effects on the firm size distribution holds also for Germany, where a 10 employees threshold applies (Bauer et al., 2007 and Wagner et al., 2001).

The density tests shown in Figure 1 may, however, 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 in Appendix A.1 we perform a further test to verify whether firms sort around the threshold at the time of the reform: we compare the probability of firm growth before and after the reform conditioning on initial firm size and pre-reform average wages. Consistently with Schivardi and Torrini (2008) and Borgarello, Garibaldi and Pacelli (2004), results in Table A.1 of Appendix A.1 show that firms just below 15 employees are about 3% less likely to grow than larger firms, but the effect is not significantly different before and after the reform and for firms with different average pre-reform wages.

While the fact that there is little evidence of sorting according to observables is reassuring, to control for the sorting of firms into the treatment or control group according to unobservable *time-invariant* characteristics, we estimate firm- and match-effect models that control for any permanent differences between firms and worker-firm pairs.

[16] This and the following figures are obtained using the benchmark sample and averaging individual wages by firm size. Note that the variable firm size is not an integer number but varies at each decimal because it measures the average number of employees weighted by number of worked months in the firm during the year

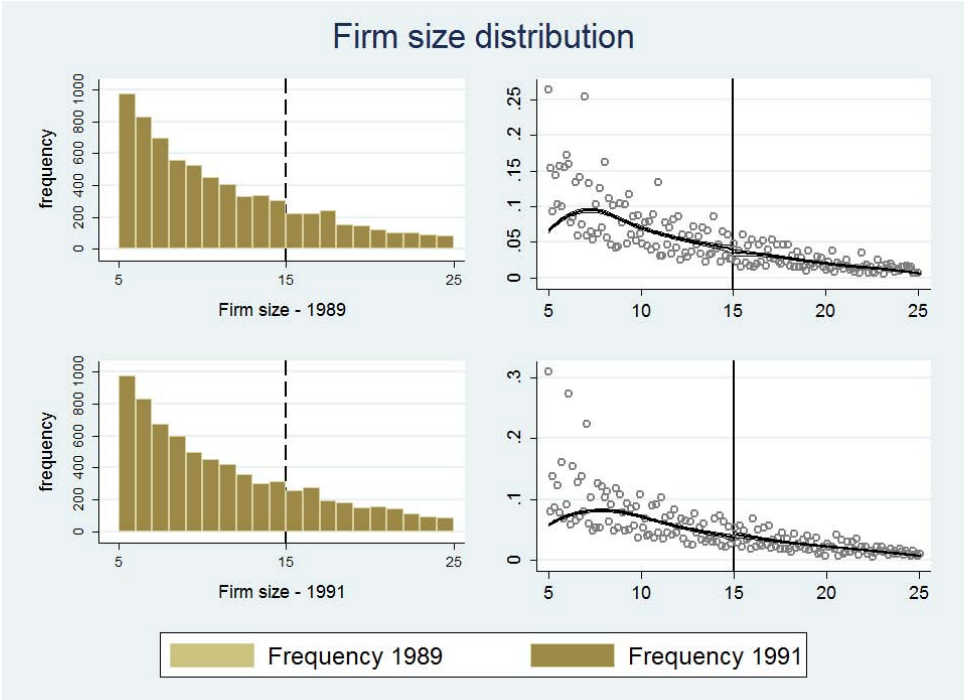

FIGURE 1. Frequency of firm size in 1989 and 1991 (left panels). McCrary test of density continuity (right panels): weighted kernel estimation of the log density, performed separately on either side of the threshold. Optimal binwidth and binsize as in McCrary (2008).

4.2 The IV model

Firm- and match-effect models do not allow to control for the sorting induced by *time-varying* factors, including the reform itself. To this aim, we adopt an IV strategy and instrument the treatment status (the firm size dummy), with firm size in the pre-reform period. To reduce the concern that the instrument is affected by the reform, we disregard the immediate pre-reform year 1989 and use as instruments firm size in years 1987 and 1988, prior to the years considered in the benchmark sample 1989-1993.[17] The formal specification of the IV model is:

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

where $\mathbf{S}^S_{jpre}$ is a vector that includes firm size in 1988 and in 1987. The term $D^S_{jt} \times Post$ is also instrumented using as an instrument $\mathbf{S}^S_{jpre} \times Post$. The matrix X_{ijt} contains the same controls as in Equation (1). Notice that, in order to build the instruments, it is necessary to follow firms over time. For this reason it is crucial to have information on the universe of workers and firms, as the VWH dataset described in Section 3 does.

Figure 2 captures the key element of the relationship between the running variable (the dummy D^S_{jt} in Equation (1) which indicates current firm size equal or smaller than 15 employees) and firm size in 1988, and can be thought of as a plot of the first stage of Equation (3).[18] The Figure shows that firms with 15 or fewer

[17] Results are similar using 1989 and 1988 (see Leonardi and Pica, 2010).

[18] For clarity reasons the figure uses only the pre-reform year 1988 while the actual first stage in Equation(1) uses both 1988 and

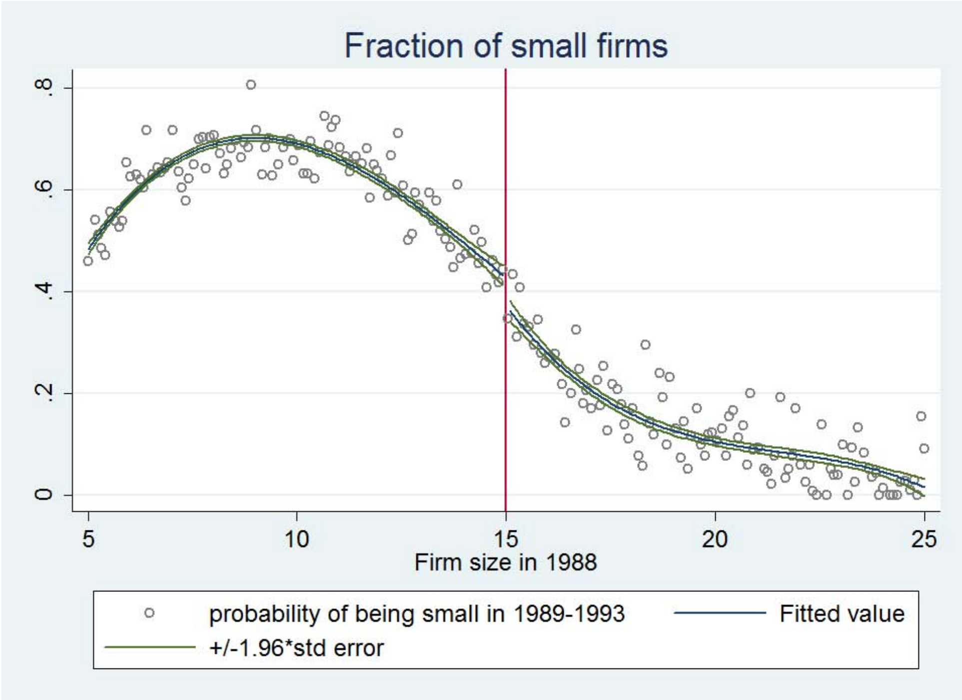

FIGURE 2. The dots represent the probability of being a firm below the 15 employees threshold in the period 1989-1993 averaged in intervals of 0.1 firm size in 1988. The solid line is a fitted regression of the small-firm dummy on a 3rd degree polynomial in the pre-reform firm size, performed separately on either side of the threshold.

employees in 1988 are more likely to have fewer than 15 employees in the following years of the sample 1989-1993 (the average probability of being below the threshold in 1989-1993 across firms between 5 and 15 employees in 1988 is 0.65), while firms above 15 employees in 1988 are more likely to stay larger than 15 in the following years (the average probability of being below the threshold in 1989-1993 across firms between 16 and 25 employees in 1988 is less than 0.1).

While there are transitions across the threshold in both directions, Table A.1 of Appendix A.1 shows that these transitions are not abnormal in years around 1990. As expected, the figure also shows that small firms below the 15 employees threshold in 1988 are more likely to grow in the following years (1 minus the probability of being small in 1989-1993) than are large firms in 1988 to shrink. Overall, Figure 2 shows that past firm size does predict current firm size and is in this sense a good instrument. Additionally, as discussed in Section 2.3, the reform was largely unanticipated and was hastily introduced so it is highly unlikely that firms in 1988 and 1987 were determining their size in view of the new regulations, thus allowing us to control for the sorting due to the reform itself. Of course, the validity of the IV estimates rest on the (untestable) assumption that the instruments satisfy the exclusion restrictions, i.e. that they do not directly affect wages.

4.3 Worker sorting

Identification of Equation (1) may be also threatened by workers non-randomly sorting in firms around the 15 employees threshold and choosing their own EPL regime by selecting the size of the firm they work for. Sorting may bias our results as long the selection process is driven by worker characteristics that we are not

1987. The corresponding first stage equations – one for the main effect D^S_{jt} and one for the interaction $D^S_{jt} \times Post$ – are presented in Table B.5 and discussed in Section 5.

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 could not be interpreted as the causal effect of EPL on wages, because it would rather reflect the different composition of the pool of workers in protected and non protected jobs. We run two different tests of workers' sorting and we show the results in Appendix A.2 for convenience. We first 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 in the post- relative to the pre-reform period. If non-random workers sorting due to the reform were to occur, we would expect these characteristics to differ systematically between treated and untreated firms in the post- relative to the pre-reform period. Results in Table A.2 of Appendix A.2 illustrate that no pre-treatment characteristics show a significant discontinuity at the 15 employees threshold after the reform in the 3nd degree polynomial specification. A few covariates pop up as significantly different from zero in the 2nd degree polynomial specification, but the spotty nature of these gaps and the fact that their significance differs according to the polynomial used supports the notion that our controlled comparisons to the left and right of the 15 employees threshold before and after the reform are indeed a good experiment.[19]

Second, we run regressions of the probability of workers moving to a firm above or below the threshold on a number of determinants that include a small-firm dummy interacted with year dummies. Results in Table A.3 of Appendix A.2 show some evidence of sorting, as the probability of moving to firms larger than 15 employees coming from a firm below the threshold decreases after the reform. However, reassuringly, the same table also shows that this effect is apparently not driven by workers' attributes correlated with their productivity.

Because we cannot rule entirely out the possibility of non-random sorting of workers, the next section will show results including workers fixed effects and match fixed effects. This helps addressing the concern that workers select their most preferred EPL regime to the extent that it controls for all time-invariant unobservable worker- and match-specific attributes that affect workers behaviour. Of course, workers and match effects do not allow us to control for the time-varying factors that affect workers' self-selection.

5 Results

Before turning to the estimates, we provide a visual summary of the relationship between firm size and wages around the threshold. Figure 3 draws a scatter plot of the difference between post-reform (years 1991 to 1993) and pre-reform (year 1989) log wages against firm size in 1988. The figure, as well as the regressions, cover an interval of [-10,+10] around the 15 employees threshold. Firms outside the 10-unit band are either far below or well beyond the relevant cut-off to be relevant. In Section 5.1 we experiment with different bandwidths and with an optimal bandwidth method.

[19] Notice that this test gives also insights on whether other (unobserved) policies differentially affect small and large firms since 1990. In principle 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. Results in Table A.2 of Appendix A.2 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.

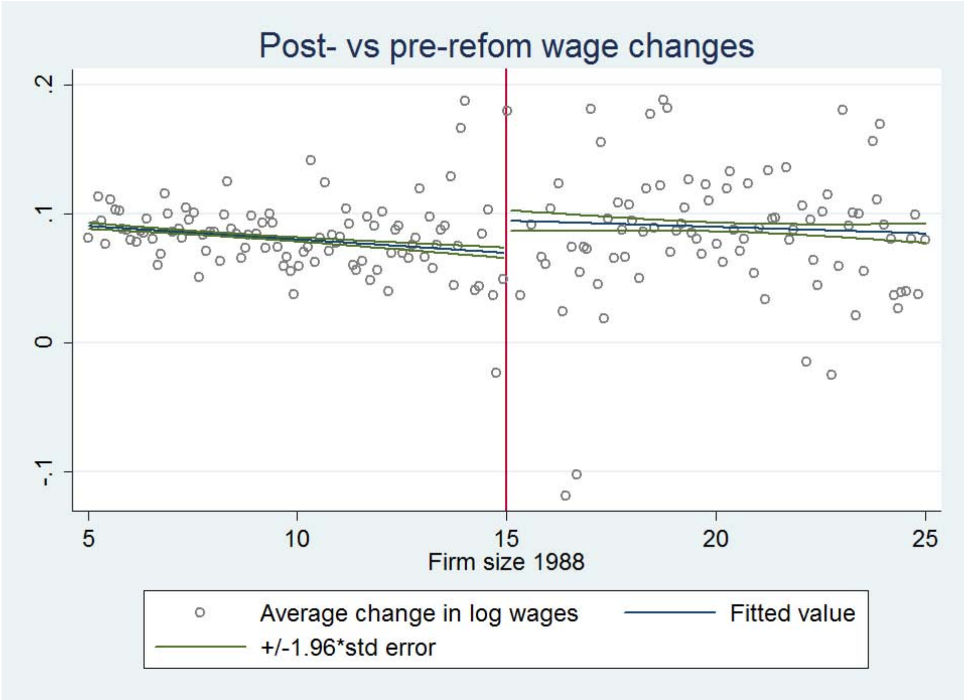

FIGURE 3. Wages are measured at the firm level averaging individual wages in intervals of 0.1 firm size in 1988. The dots are the observed differences between log wages post-reform (averaged in years 1991, 1992 and 1993) minus log wages pre-reform in year 1989. The solid line is a fitted regression of log wage differences on firm size in 1988, performed separately on either side of the threshold.

The figure is obtained averaging individual wages by firm size in 1988. Each point is the difference of log wages post-reform (the average of firm-level log wages in years 1991 to 1993) minus log wages pre-reform in 1989. The figure also reports the fitted values of a regression of log wage differences with respect to firm size in 1988. As firm size is measured in 1988 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. The jump appears to be small but significant, suggesting that small firms translate part of the increased cost of EPL onto lower wages.

The general pattern presented in the figure is also borne out in the regression results to which we now turn. Differently from the figures, the regressions are run on individual wages and allow us to control for both workers and firms characteristics.

Table 2 reports regression results from the estimation of Equation (1). In Panel A the year of the reform, 1990, is excluded. For the sake of space we only show the coefficient of interest on the interaction term between the small-firm dummy and the post-reform dummy.Results in Column 1 of Panel A include workers fixed effects and suggest that individuals employed in firms just below the threshold of 15 employees are paid 0.4 percent less than workers in firms immediately above the cut-off after 1990. Columns 2 and 3 show that the significance of the result does not survive the inclusion of firm and match effects, respectively. Finally, Column 4 refers to IV (with worker fixed effects) estimates in which we instrument the treatment status using firm size in 1987 and 1988. IV results deliver a negative and significant coefficient more than twice as large as the baseline coefficients

Table 2. Average effects of 1990 EPL reform

	(1)	(2)	(3)	(4)
	Panel A: excluding the reform year 1990			
Small firm × Post 1990	-0.004	-0.002	-0.001	-0.011
	[0.002]**	[0.002]	[0.002]	[0.004]***
Observations	96333	96333	96333	76814
R^2	0.16	0.22	0.17	
	Panel B: including the reform year 1990			
Small firm × Post 1990	-0.005	-0.003	-0.002	-0.009
	[0.001]***	[0.001]**	[0.001]	[0.002]***
Observations	120652	120652	120652	99658
R^2	0.15	0.22	0.16	
Worker Effect	YES	NO	NO	YES
Firm Effect	NO	YES	NO	NO
Match Effect	NO	NO	YES	NO
IV	NO	NO	NO	YES

Notes: 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. First-stage results of the IV specification in Panel A are shown in Table B.5. First-stage statistics of the IV specification in Panel B are shown in Table B.14. One asterisk denotes significance at 10%; two asterisks denote significance at 5%; three asterisks denotes significance at 1%.

in Column 1. The larger magnitude of the IV coefficient may be due to measurement error in firm size that produces an attenuation bias in non-instrumented regressions. An alternative interpretation is that the larger size of the IV coefficients confirms the importance of instrumenting to account for the sorting of firms according to unobservable characteristics. In particular, it may suggest that the reform may have induced some firms to cross the threshold, reducing the gap in average wages observed in the treated and control groups after 1990. This can happen, for example, if the reform provided greater incentives to low-productivity firms – that before the reform were keeping their size below the 15 employees threshold to avoid strict EPL rules – to move above the threshold.

In Panel B, year 1990 is included as a pre-reform year (the reform passed in July and wages are unlikely to adjust immediately). It shows that the inclusion of 1990 reinforces the results, as the coefficient of interest is negative and significant in all specifications except for the one with match effects.[20] To be conservative, in what follows we exclude year 1990.

One possible concern is that the negative effect of the EPL reform on wages may be driven by either lower entry or higher exit of firms below the threshold after the reform (or both). The argument is that in the absence of the reform there would be a larger number of small firms and therefore higher labour demand and higher wages in the treatment group of firms. Table B.4 in Appendix B shows that the probability of entry decreases in firms below the threshold relative to larger firms after the reform, though not significantly so. Also the

[20]As we will see, this may be due to the fact that the negative wage effect of EPL is mostly concentrated on the entry wage and is therefore identified off new entrants and not incumbents.

probability of exit is not significantly different in firms below the threshold relative to larger firms after the reform. Thus, our results are unlikely to be driven by differential entry or exit.[21]

Table B.5 displays the first stage of the IV model in Panel A of Table 2, i.e. estimates from the regression of the two endogenous variables D_{jt}^S and $D_{jt}^S \times Post$ on the full set of included and excluded instruments (see Table B.14 for the first stage statistics of the IV model in Panel B). Column 1 reports results for the small-firm dummy D_{jt}^S and shows a negative coefficient on firm size in 1987: as expected, the larger is the firm in 1987, the lower the probability of being below the 15 employees threshold over the sample period 1989-1993. This coefficient is insignificant plausibly for the presence among the controls of the polynomial in current firm size. The interaction term between firm size in 1987 and post-reform dummy is negative and significant. Size in 1988 enters positively in the regression showing evidence of mean reversion: controlling for size in 1987 and for current firm size, firms which experience a positive shock in 1988 tend to switch back to their regular size afterwards. The same pattern of alternating signs applies to the results in Column 2 for $D_{jt}^S \times Post$, in which all coefficients are significant. The mean-reversion effect highlighted by the alternate sign on firm size in 1987 and 1988 suggests that using a single pre-reform year as an instrument may bias the results. In fact, some firms assigned by the single instrument to the small (large) size category may actually correspond to firms that are typically of a larger (smaller) size but that had had a relatively bad (good) year in that period (Martins, 2009). Finally, the overall power of the instruments is strong – as indicated by the F-test of the excluded instruments equal to 5.71 and 11031.65 – and Hansen's J statistic (which becomes under the assumption of conditional homoskedasticity the usual Sargan's statistic) shows that the specification passes the test of overidentifying restrictions.

Table 3 reports regression results from the estimation of Equation (2), in which we exploit the idea that EPL should matter more in highly volatile sectors (Micco and Pagés, 2006). We measure employment volatility as the pre-reform within-sector standard deviation of the firm-level growth rate of employment computed separately for firms below and above the threshold. Results confirm that the reform bites more in more volatile sectors, thus ruling out the concern that results are driven by small firm wages being on a declining path with respect to wages in larger firms above the 15 employees threshold. This is also reassuring since it suggests that we are capturing the effect of the 1990 reform rather than the effect of some other contemporaneous shock or legislative change, such as the pension reform or the elimination of the *Scala Mobile*, which should not have affected differently sectors with different volatilities.

In addition, to we check whether the reform of collective dismissals of 1991 is behind the negative coefficients on wages estimated in Table 2, we augment the baseline specification of Equation (1) with a post-1991 reform dummy and its interaction with the small-firm dummy. Contrary to what one would expect if the 1991 reform was driving our findings, (unreported) regression results show that the interaction term with the post-1991 reform dummy is not significant while the interaction term between the post-1990 reform dummy and the

[21] These results compare fairly well with Kugler and Pica (2008) who find a negative, but significant, effect on entry and no effect on exit on a nationwide Italian sample representative of the population of workers rather than firms. The difference in the entry result may be explained by the fact that Kugler and Pica (2008) use the date of incorporation of the firm as an indicator for firm entry. This measures the incorporation decision and differs from entrepreneurial entry rates (Da Rin et al., 2011) that we are instead able to measure relying on the universe of firms and looking at the first appearance of the firm in the economy.

Table 3. Differential effects of the 1990 reform: pre-reform sectoral employment growth

	(1)	(2)	(3)	(4)
Small firm × Post 1990	-0.004	-0.002	-0.001	-0.008
	[0.002]**	[0.002]	[0.002]	[0.004]**
Pre-reform variability of employment growth	-0.009	-0.009	-0.005	-0.013
	[0.002]***	[0.003]***	[0.002]**	[0.009]
Variability of employment growth × Post 1990	0.010	0.010	0.009	0.011
	[0.002]***	[0.002]***	[0.001]***	[0.003]***
Variability of employment growth × Small firm	0.013	0.014	0.012	0.014
	[0.003]***	[0.004]***	[0.003]***	[0.019]
Variability of employment growth × Small firm × Post 1990	-0.012	-0.014	-0.012	-0.012
	[0.002]***	[0.002]***	[0.002]***	[0.004]***
Observations	96333	96333	96333	76814
R^2	0.16	0.22	0.17	
Worker FE	YES	NO	NO	YES
Firm FE	NO	YES	NO	NO
Match Effects	NO	NO	YES	NO
IV	NO	NO	NO	YES

Notes: Robust standard errors clustered by individual in brackets. The variability of employment growth (standardised to zero mean and unit variance) is the pre-reform within-sector standard deviation of the firm-level growth rate of employment computed separately for small and large firms. All specifications include a third degree polynomial in the size of the firm, age dummies, sectoral dummies, year dummies and a blue collar dummy. IV first-stage statistics are shown in Table B.14. One asterisk denotes significance at 10%; two asterisks denote significance at 5%; three asterisks denotes significance at 1%.

small-firm dummy remains negative and significant.[22]

5.1 Robustness checks and placebo tests

In this section we show that our results are robust to a number of checks. In light of the discussion on firm and workers sorting, we perform our robustness exercises both on the model with workers fixed effects and on the model with IVs and workers fixed effects which allows to control for both types of sorting.

In Table B.6 and B.7 we implement placebo tests by estimating the treatment effect at fake firm size thresholds and fake reform years, where there should be no effect. We first estimate the treatment effect below and above the fake 6, 10, 20 and 23 employees thresholds. The coefficients of interests are mostly insignificant. Table B.6 shows that the fake firm size threshold is significant only at 20 employees, but the significance disappears in the IV specification. In Table B.7 we estimate the treatment effect before and after the fake reform years 1991, 1992, 1988 and 1989 (excluding in turn the fake year of the reform as we did with 1990 in Table 2). A significant negative effect appears only in 1991 (i.e. considering 1989 and 1990 as pre-reform years and 1992 and 1993 as post-reform years). This is not surprising because 1991 is the year immediately after the reform and we may take this result as an indication that 1990, the year of the actual reform, belongs to the pre- rather than the post-reform period. Finally, the interaction between the small firm and the post-reform dummy is not significant when considering 1992 and 1989 as reform years, and turns positive when we pretend that the reform occurred in 1988.

In Table B.8 we run robustness checks with respect to the time span of the sample, enlarging it from the benchmark 1989–1993 to 1988–1993, 1987–1994, 1986–1996 and 1986–1994, and restricting it to 1989–1991 and 1989–1992. In all cases the effect is negative and significant, except for the 1988–1993 sample in the specification with worker fixed effects and the 1987–1994 sample in the specification with worker fixed effects plus IV in which significance is not attained.

Table B.9 shows that the results are robust to alternative specifications of the polynomial in firm size. Results are generally robust to using a first and second degree polynomial, except for two cases where the IV estimates are still negative but insignificant. Finally, in Table B.10 we fit a linear regression function to the observations distributed within a distance Δ on both sides of the threshold:

$$\log w_{ijt} = \beta' X_{ijt} + \delta_1 D^S_{jt} + \delta_2 \left(D^S_{jt} \times Post\right) + \varepsilon_{ijt} \quad \text{for firm size} \in [15-\Delta, 15+\Delta] \tag{4}$$

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 $fsize_j \in [15-\Delta, 15+\Delta]$. The optimal Δ chosen between 1 and 15 is $\Delta^* = 12$ with $L(12) = 0.03762235$.

[22] Kugler and Pica (2008) also empirically distinguish the 1990 and the 1991 reforms and conclude that the latter reform has no differential effect over and above that of the 1990 reform. Paggiaro, Rettore and Trivellato (2008) examine aspects of the 1991 law concerning active labour market policies and find limited effects only on workers aged 50+

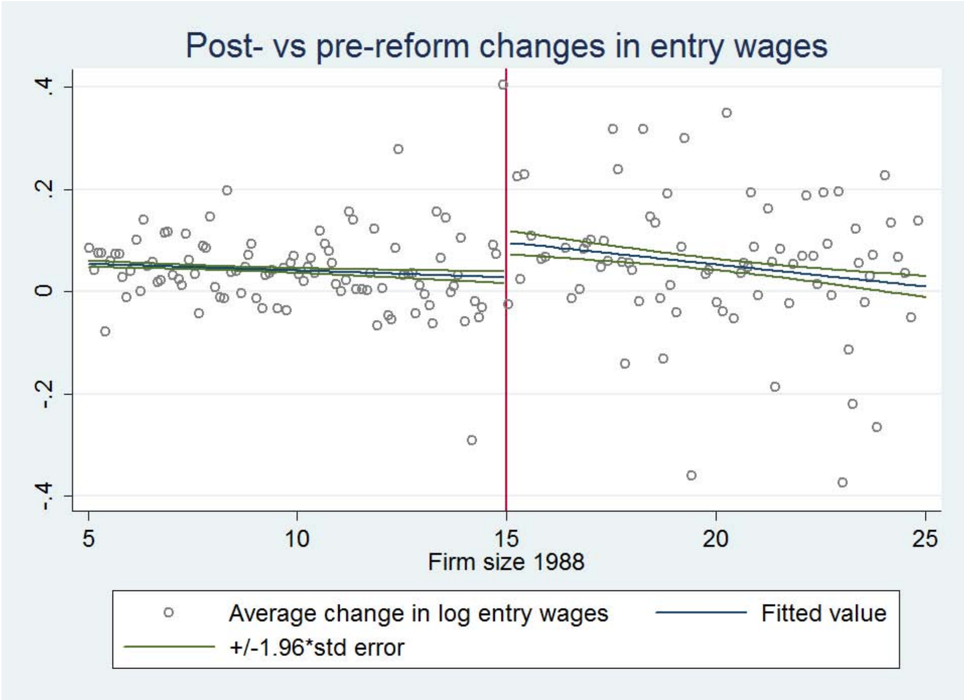

FIGURE 4. Entry wages are the first observed wage in a new firm for workers who change firms at least once in the period 1989-1993. Wages are measured at the firm level averaging individual log entry wages in intervals of 0.1 firm size in 1988. The dots are the observed differences between log entry wages post-reform (averaged in years 1991, 1992 and 1993) minus log entry wages pre-reform in year 1989. The solid line is a fitted regression of log entry wage differences on firm size in 1988, performed separately on either side of the threshold.

Table B.10 shows that the results are robust to the specification change: the local linear regression estimator yields a negative significant coefficient on samples taken over different years, except for the IV model in the 1989–1993 sample.

Overall, these results indicate the presence of a robust, negative and significant (albeit small) effect on wages. However, only on the basis of these coefficients, we cannot tell whether the translation of the EPL cost onto wages was total, partial or negligible: we will attempt an exercise to assess the extent of translation in Section 7.1. Before turning to that, we provide several pieces of evidence that wage changes in consequence of higher EPL are very heterogeneous and depend on the bargaining position of workers.

6 Heterogeneous effects

6.1 Impact at entry

The theoretical considerations made in Section 2.1 suggest that newly-hired workers should be a in weaker bargaining position compared to incumbent workers because firms do not pay severance if there is no agreement on the wage at the first encounter. On the contrary, higher EPL should strengthen incumbent workers' bargaining position and possibly lead to a wage increase if workers are able to renegotiate their wages. In what follows we investigate the different impact of the reform on new entrants' and incumbents' wages.

To this aim, we identify the subsample of incumbent workers, which consists of all workers who stayed in the same firm over the sample period, and the complementary subsample of movers which includes all workers

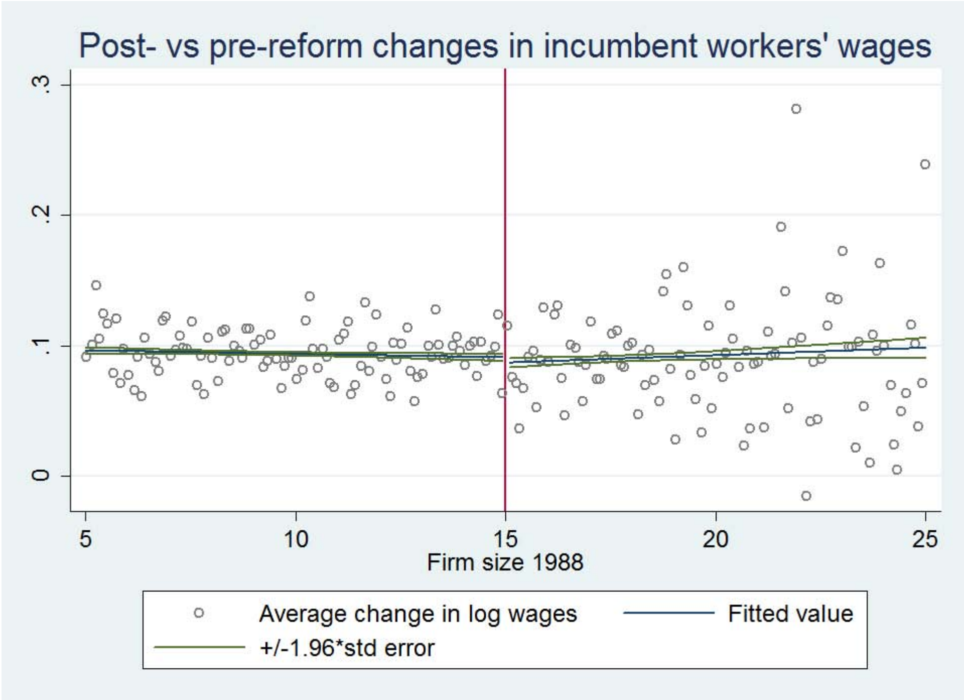

FIGURE 5. The sample of incumbents includes only workers who stayed in the same firm between 1989 and 1993. Wages are measured at the firm level averaging individual log wages in intervals of 0.1 firm size in 1988. The dots are the observed differences between log wages post-reform (averaged in years 1991, 1992 and 1993) minus log wages pre-reform in year 1989. The solid line is a fitted regression of log wage differences on firm size in 1988, performed separately on either side of the threshold.

who changed firm at least once over the same period. Obviously, we observe the entry wage only in the sample of movers. Descriptive statistics in Table B.11 (which includes year 1990 and for this reason displays the same number of observations as Panel B of Table 2) show that movers are a little less than one third of the sample: 35690 observations out of 120652 (the proportion remains the same once year 1990 is dropped as shown in Table 1). Column 1 shows that the proportion of job changes is pretty stable over the sample period at around 9% per year.[23] The wage of incumbent workers is obviously higher than the wage of movers (Columns 2 and 3) because of tenure effects. For the same reason movers' entry wages are lower than movers' post-entry wages (Columns 4 and 5). Before looking at the differential effect of EPL on the wages of new entrants and incumbents using our regression framework, we display movers' entry wages, i.e. the first observed wage of a mover in the new firm, in Figure 4, and the wages of incumbents workers in Figure 5. Individual wages are averaged by firm size in 1988 in the figures.

Figure 4 draws a scatter plot of the difference between log entry wages post-reform (year 1991 to 1993) and log entry wages pre-reform (year 1989). The figure also reports the fitted values of a regression of log wage differences with respect to firm size in 1988. The significant jump at 15 implies that after the reform entry wages are lower in firms below the threshold relative to firms above the threshold. On the contrary, the same relationship plotted for the sample of incumbents (Figure 5) shows no jump, meaning that there is no difference between the average wages of incumbents paid in firms below and above the threshold before and after the reform. This visual evidence is consistent with the idea that most of the burden imposed by higher firing costs

[23]Notice that in Appendix A.2 we analyse the probability of workers moving from firms below the threshold to larger firms above the threshold (and viceversa) *conditional on moving*, while table B.11 reports the unconditional proportion of job changes.

Table 4. Movers and incumbents

	(1)	(2)	(3)	(4)
Small firm × Post 1990	0.001	-0.001	0.001	-0.007
	[0.002]	[0.002]	[0.002]	[0.004]
Mover dummy		-0.036		
		[0.005]***		
Mover dummy × post 1990	0.013	-0.019	0.009	0.009
	[0.005]***	[0.005]***	[0.005]*	[0.008]
Mover dummy × small firm	0.018	0.017	0.018	0.033
	[0.006]***	[0.005]***	[0.006]***	[0.021]
Mover dummy × small firm × post 1990	-0.019	-0.003	-0.015	-0.025
	[0.006]***	[0.006]	[0.006]**	[0.011]**
Observations	96333	96333	96333	76814
R^2	0.16	0.23	0.17	

Notes: Robust standard errors clustered by individual in brackets. Movers are defined as workers who change firm at least once over the period 1989-1993. All specifications include a third degree polynomial in the size of the firm, age dummies, sectoral dummies, year dummies and a blue collar dummy. IV first-stage statistics are shown in Table B.14. One asterisk denotes significance at 10%; two asterisks denote significance at 5%; three asterisks denotes significance at 1%.

is translated onto lower entry wages, with incumbents' wages virtually unaffected.

In Table 4 we report the regression results adding the usual individual and firm characteristics. The regressions confirm that movers account for the overall effect that we see in the baseline regressions in Table 2. Movers suffer an average 2-2.5 percent wage loss in firms below the threshold relative to larger firms after the reform. Incumbents experience no significant change in their wages. This result is consistent with the idea that incumbents are not able to renegotiate their wages. This may be either because no renegotiation round took place in the post-reform period, or because workers have no credible threat if the firm refuses to renegotiate their wage (McLeod and Malcomson, 1993).

We next focus on the subsample of movers in order to analyse whether the EPL reform impacts movers' wages at entry or afterwards. To do so, we augment our baseline specification of Equation (1) with an entry wage dummy, fully interacted with the small-firm dummy and the post-reform dummy. We also add a pre-entry dummy (a dummy equal to one for the observations prior to the first job change observed) fully interacted with the small-firm dummy and the post-reform dummy to be able to interpret the interaction between the small-firm dummy and the post-reform dummy as the effect on the post-entry wages. Table 5 shows that movers experience a wage decline at entry and, consistently with the results on incumbents, no changes in the post-entry period. The decline at entry is sizeable and ranges between 3 and 6.3 percent depending on the different specifications. This result suggests that workers paid for the introduction of EPL with a lower entry wage and were unable to renegotiate their wages upwards in later years.

We finally look at the impact of EPL on wage-tenure profiles: we augment our baseline specification with a full set of interactions between tenure, the small-firm dummy and the post-reform dummy. Table 6 shows a one/two percent decline in the wage-tenure profile of workers in firms below the threshold relative to larger firms after the reform, both in Panel A and in Panel B. In Panel A we keep all workers and calculate their tenure starting from 1985, the first available year in the sample. However, since tenure is properly measured

Table 5. Movers entry and post-entry wages

	(1)	(2)	(3)	(4)
Small firm × Post 1990	-0.004	-0.015	0.005	0.002
	[0.012]	[0.010]	[0.012]	[0.021]
Entry dummy	-0.042	-0.037	-0.053	-0.064
	[0.016]**	[0.013]***	[0.016]***	[0.025]**
Entry dummy × Post 1990	0.034	0.026	0.052	0.054
	[0.017]**	[0.013]*	[0.016]***	[0.027]**
Entry dummy × Small firm	0.036	0.000	0.051	0.064
	[0.019]*	[0.016]	[0.019]***	[0.030]**
Entry dummy × Small firm × Post 1990	-0.034	0.001	-0.048	-0.063
	[0.020]*	[0.016]	[0.019]**	[0.033]*
Observations	28451	28451	28451	16140
R^2	0.13	0.17	0.11	
Worker Effect	YES	NO	NO	YES
Firm Effect	NO	YES	NO	NO
Match Effect	NO	NO	YES	NO
IV	NO	NO	NO	YES

Notes: Robust standard errors clustered by individual in brackets. The sample includes only movers. All specifications include a third degree polynomial in the size of the firm, age dummies, sectoral dummies, year dummies, a blue collar dummy and a pre-entry dummy (a dummy equal to one for the observations prior to first job change observed) fully interacted with the small-firm dummy and the post-reform dummy. IV first-stage statistics are shown in Table B.14. One asterisk denotes significance at 10%; two asterisks denote significance at 5%; three asterisks denotes significance at 1%.

only for employment relationships that start in 1985 or later, in Panel B we keep only workers who start a new job spell after 1985. The small difference in the results between Panel A and Panel B, notwithstanding the large difference in the number of observations, is reassuring that tenure is not endogenous to EPL. The negative effects of EPL on wage-tenure profiles are consistent with Scoppa (2010) who finds that the 1990 EPL reform increased absenteeism in firms with 15 or fewer employees, and may be explained by the fact that EPL reduces effort and productivity growth (Ichino and Riphahn, 2005).[24]

6.2 Occupation and age

The bargaining power of workers may differ not only across insiders and outsiders. There are other possible dimensions – that relate to the individual characteristics – along which the bargaining power may be heterogeneous. In what follows we cut our dataset into high- (white collar, old) and low-bargaining power subsamples (blue collar, young).

Table 7 reports regression results from the estimation of a version of Equation (2) in which the triple interaction term identifies the differential effect of the reform on the above mentioned subgroups, which arguably have different bargaining power. Panel A looks at blue collars: we find no significant difference relative to white collars. In Panel B we find a significant negative effect for young workers aged less than 30, both in the baseline specification with workers fixed effects and in the IV specification. Finally, the subgroup of young blue collars

[24]Unreported results show that the effect of the reform on the tenure profile is driven by the impact on incumbents' tenure profiles. However, the fact that movers' tenure profiles seem unchanged may be due to the very short spells of tenure observed for those who moved in the last years of the sample.

Table 6. Effect of the 1990 EPL reform on the tenure profile

	(1)	(2)	(3)	(4)
	Panel A: full sample			
Small firm × Post 1990	-0.007	0.001	-0.006	-0.019
	[0.003]***	[0.003]	[0.003]**	[0.009]**
Tenure	0.003	0.049	-0.010	0.010
	[0.003]	[0.003]***	[0.010]	[0.007]
Tenure × Post 1990	-0.002	-0.008	0.001	-0.003
	[0.003]	[0.003]***	[0.003]	[0.004]
Tenure × small firm	0.005	-0.002	0.008	0.009
	[0.003]	[0.003]	[0.003]**	[0.008]
Tenure × small firm × Post 1990	-0.003	-0.004	-0.008	-0.004
	[0.003]	[0.003]	[0.003]**	[0.005]
Observations	96333	96333	96333	76814
R^2	0.16	0.25	0.17	
	Panel B: censored sample			
Small firm × Post 1990	-0.009	-0.003	-0.003	-0.044
	[0.005]*	[0.005]	[0.005]	[0.016]***
Tenure	0.002	0.032	-0.017	-0.014
	[0.005]	[0.004]***	[0.010]*	[0.013]
Tenure × Post 1990	0.002	0.001	0.007	0.016
	[0.005]	[0.004]	[0.004]*	[0.009]*
Tenure × small firm	0.008	0.007	0.008	0.034
	[0.006]	[0.005]	[0.005]	[0.014]**
Tenure × small firm × Post 1990	-0.007	-0.010	-0.009	-0.019
	[0.005]	[0.005]**	[0.005]*	[0.010]*
Observations	56912	56912	56912	38943
R^2	0.15	0.21	0.16	
Worker Effect	YES	NO	NO	YES
Firm Effect	NO	YES	NO	NO
Match Effect	NO	NO	YES	NO
IV	NO	NO	NO	YES

Notes: Robust standard errors clustered by individual in brackets. Tenure is standardised to zero mean and unit variance. In Panel B we drop individuals for whom the starting date of the job is not observed. All specifications include a third degree polynomial in the size of the firm, age dummies, sectoral dummies, year dummies and a blue collar dummy. IV first-stage statistics are shown in Table B.14. One asterisk denotes significance at 10%; two asterisks denote significance at 5%; three asterisks denotes significance at 1%.

TABLE 7. Heterogeneous wage effects of the 1990 EPL reform: blue/white collars, young/old, and young blue collars

	(1)	(2)	(3)	(4)
	Panel A: Blue/white collars			
Small firm × Post 1990	0.002	0.004	0.005	0.000
	[0.006]	[0.006]	[0.005]	[0.010]
Blue collar × Small firm × Post 1990	-0.006	-0.006	-0.006	-0.012
	[0.006]	[0.007]	[0.006]	[0.0011]
Observations	96333	96333	96333	76814
R^2	0.16	0.22	0.17	
	Panel B: Young (< 30)/old (> 40)			
Small firm × Post 1990	0.002	0.002	0.002	-0.009
	[0.003]	[0.003]	[0.003]	[0.007]
Young × Small firm × Post 1990	-0.012	-0.002	-0.008	-0.020
	[0.005]**	[0.005]	[0.005]	[0.011]*
Observations	61899	61899	61899	46622
R^2	0.15	0.23	0.16	
	Panel C: Young blue collars			
Small firm × Post 1990	0.002	0.005	0.002	-0.008
	[0.003]	[0.003]	[0.003]	[0.007]
Young blue collar × Small firm	-0.013	-0.009	-0.008	-0.021
× Post 1990	[0.005]**	[0.005]*	[0.005]	[0.011]*
Observations	61899	61899	61899	46622
R^2	0.15	0.26	0.16	
Worker Effect	YES	NO	NO	YES
Firm Effect	NO	YES	NO	NO
Match Effect	NO	NO	YES	NO
IV	NO	NO	NO	YES

Notes: Robust standard errors clustered by individual in brackets. Young workers are defined as under the age of 30 and old workers over the age of 40. All specifications include a third degree polynomial in the size of the firm, age dummies, sectoral dummies, year dummies and a blue collar dummy. IV first-stage statistics are shown in Table B.15. One asterisk denotes significance at 10%; two asterisks denote significance at 5%; three asterisks denotes significance at 1%.

in Panel C displays significant negative effects in all specifications except for the one with match effects.

Overall the results presented in this section fit the interpretation that the negative wage effects of EPL are inversely related to the bargaining power of workers, measured indirectly with workers' observable characteristics. We investigate further this issue moving to a subsample of workers for which we have a more direct measure of bargaining power, namely the individual wage premium over the contractual minimum wage.

6.3 Contractual minimum wages and quantile regression

Similarly to many other European countries, Italy has a system of contractual minimum wages bargained every 2 years (with many delays and exceptions) at the industry level which extends also to non-signatory workers. Employers can negotiate supplemental wage premiums over and above the contractual minimum wage at the firm level and employees also receive individual premiums and bonuses. In this section we exploit information on contractual minimum wages to construct a measure of the "wage drift", i.e. the difference between the actual wage and the contractual minimum. The contractual minimum wages are defined by industry and occupation category (typically 5 or more categories according to tasks performed and tenure). Thus, the wage drift in percentage terms is $y_{ijzt} = (w_{ijt} - w_{jzt}^{\min})/w_{jzt}^{\min}$ where $w_{jzt}^{\min}$ is the contractual minimum in sector j for a worker of occupation category z.

Our data allow us to identify the sectoral contracts and the occupation category for a subsample of workers. For this subsample we know the contractual minimum wage that applies to their jobs, however, the matching of each worker to her contractual minimum has a cost in terms of observations due to missing information either on sectoral contracts (for example firms in the chemical industry and in industries covered by narrow sectoral agreements) or on the occupation category of the worker. We can match only around 40% of the observations present in the benchmark sample to their respective contractual minimum wages.

We have information on contractual minima in 21 types of contracts in Table B.12.[25] Notice that collective contracts do not correspond exactly to sectors but vary also according to firm size and type of firm. The distribution of the characteristics of the workers (proportion of white collar workers, average age and wages) in the resulting subsample (13.7%, 38.7 years and 321.2 Euro per week at 1995 prices) is similar to the distribution in the overall estimation sample (14.4%, 36.9 years and 314.29 Euro per week). This is suggestive that the loss of observations due to missing information is not endogenous to the variables of interest.

Table B.12 reports the distribution of the wage drift for all available contracts and shows that there is a wide variation across contracts in the incidence of the contractual minimum as a percentage of the full compensation. The table reports the 5^{th}, 10^{th}, 25^{th}, 50^{th}, 75^{th}, 90^{th} and 95^{th} quantile of the wage drift distribution by each contract. The median wage drift differs by sector and goes from 25.2% in textile artisanal firms to 91.8% of the professional services sector. Notice that even at the 5^{th} percentile the wage drift is on average a sizeable 12.5%, suggesting that wage minima are hardly binding.

As long as individual-specific premia and firm-wide premia paid above the minimum are a result of bargaining in local contracts, the wage drift can be interpreted as a measure of bargaining power of the workers: the higher the actual wage with respect to the contractual minimum, the higher 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 contractual (and legal) floor. To investigate these hypotheses we run a quantile regression at different points of the distribution using as a dependent variable the wage drift $y_{ijzt} = (w_{ijt} - w_{jzt}^{\min})/w_{jzt}^{\min}$. Let $Q_\theta(y_{ijzt}|X_{ijt})$ for $\theta \in (0,1)$ denote the θ^{th} quantile of the distribution of y_{ijzt} 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:

$$Q_\theta(y_{ijzt}|X_{ijt}) = \beta_\theta' X_{ijt} + \delta_{1\theta} D_{jt}^S + \delta_{2\theta}\left(D_{jt}^S \times Post\right) + \sum_{k=1}^{3}(\gamma_{\theta k} fsize_{jt}^k) \tag{5}$$

[25]The contracts for firms in the insurance sector and in cooperative firms in the construction sector cover only a very small number of workers in the sample (10 and 17 observations) and have been dropped.

Table 8. Heterogeneous wage effects of 1990 EPL reform: quantile regression

	Q05	Q10	Q25	Q50	Q75	Q90	Q95
				Log wages			
Small firm × Post 1990	-0.014	-0.009	-0.009	-0.009	-0.010	-0.008	-.011
	[0.006]***	[0.004]**	[0.003]***	[0.003]***	[0.004]***	[0.007]	[0.008]
Observations	96333	96333	96333	96333	96333	96333	96333
				Wage drift			
Small firm × Post 1990	-0.032	-0.025	-0.021	-0.009	-0.010	-0.006	-0.020
	[0.010]***	[0.008]***	[0.006]***	[0.007]	[0.011]	[0.018]	[0.024]
Observations	38895	38895	38895	38895	38895	38895	38895

Notes: Bootstrapped standard errors (100 replications) clustered by individual. The wage drift is defined as (wage-contractual wage)/contractual wage. Contractual wages are bargained at the national level by sector and occupation category (typically 5 or more categories according to tasks performed and tenure). All specifications include a third degree polynomial in the size of the firm, age dummies, sectoral dummies, year dummies, a blue collar dummy. One asterisk denotes significance at 10%; two asterisks denote significance at 5%; three asterisks denotes significance at 1%.

Bootstrapped standard errors are obtained from individual resampling.

Table 8 reports the estimates of the coefficient of the interaction term $\delta_{2\theta}$ obtained at the 5^{th}, 10^{th}, 25^{th}, 50^{th}, 75^{th}, 90^{th} and 95^{th} quantile of the log wage and of the wage drift distributions. Panel A shows the results on log wages in the benchmark period 1989-1993, Panel B on the wage drift as defined above in the same sample period. Results show that the negative effect of the reform on the log wages of workers of firms below the 15 employees threshold is stronger at the bottom of the distribution and weaker at the top. Panel B exhibits an even stronger pattern using the wage drift as a dependent variable. In particular, the effect at the 5^{th} percentile of the wage drift distribution is more than five times larger than the effect at the 90^{th} of the distribution and is three times as large as the average effect obtained in Table 2.

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 5^{th} percentile of the wage drift) is explained by the fact that even at the 5^{th} percentile, as remarked above, there is room for adjustment.

6.4 Local labour markets

According to models with labour market frictions, the tightness of the local labour market affects workers' outside option and eventually their wages. As a final exercise, we therefore test whether the effect of the reform differs across local labour market. To do so we exploit a sample of workers which covers the whole Italian territory. The dataset is drawn from the same administrative source as our VWH dataset, namely the Italian Social Security Administration (INPS) archives. The original data is a 1/90 random sample from the stock of employed workers with social security records drawn every year, with workers born on the 10th of March, June, September, and December of every year being sampled. We use a 10% random draw from this orginal dataset,

Table 9. Heterogeneous wage effects of 1990 EPL reform: local labour markets

	(1)	(2)	(3)	(4)
	1989-1993	1988-1993	1987-1994	1986-1995
	Panel A: firm size 5-25			
Small firm × Post 1990	-0.010	-0.010	-0.017	-0.018
	[0.011]	[0.009]	[0.008]**	[0.007]**
Employment rate	0.006	0.001	0.008	0.021
	[0.011]	[0.011]	[0.009]	[0.009]**
Employment rate × Post 1990	0.000	0.007	0.006	-0.005
	[0.012]	[0.012]	[0.009]	[0.008]
Employment rate × Small firm	-0.016	-0.017	-0.024	-0.020
	[0.018]	[0.013]	[0.012]*	[0.010]*
Employment rate × Small firm × Post 1990	0.019	0.017	0.019	0.020
	[0.017]	[0.012]	[0.010]*	[0.009]**
Observations	7323	9147	12894	16637
R^2	0.63	0.69	0.68	0.71
	Panel B: firm size 10-20			
Small firm × Post 1990	-0.015	-0.015	-0.023	-0.022
	[0.011]	[0.008]*	[0.007]***	[0.009]**
Employment rate	0.019	0.020	0.019	0.033
	[0.013]	[0.011]*	[0.010]*	[0.009]***
Employment rate × Post 1990	-0.012	-0.014	-0.010	-0.018
	[0.013]	[0.011]	[0.010]	[0.007]**
Employment rate × Small firm	-0.039	-0.050	-0.054	-0.043
	[0.014]**	[0.010]***	[0.011]***	[0.010]***
Employment rate × Small firm × Post 1990	0.036	0.046	0.047	0.038
	[0.014]**	[0.011]***	[0.010]***	[0.011]***
Observations	3727	4622	6502	8362
R^2	0.64	0.70	0.70	0.72
Match effects	YES	YES	YES	YES

Notes: Robust standard errors clustered by region in brackets. Estimates are based on a random 10% sample of a data set drawn from the Italian Social Security Administration (INPS) archives. Employment rate is the regional rate of employment of males aged 25-64 (standardised to zero mean and unit variance). All specifications include a third degree polynomial in the size of the firm, age dummies, sectoral dummies, year dummies, a blue collar dummy, regional dummies and regional trends. One asterisk denotes significance at 10%; two asterisks denote significance at 5%; three asterisks denotes significance at 1%.

as in Kugler and Pica (2008). Contrary to our main sample, it is a representative sample of workers and not of firms, therefore we can neither use IVs nor can we follow firms over time. To control for firms', workers' and match unobserved heterogeneity, we will show results using match effects.

Before moving to regression results, Table B.13 compares the main characteristics of males aged 20-55 in the Italian sample and in the VWH sample (Columns 1 and 2). It shows that the average characteristics of workers in the two datasets are similar, except for a higher shares of white collar workers in the Italian sample (20% against 14%), due to the fact that Veneto is a predominantly manufacturing region. In particular, in 1990 the Veneto region has the same average wage as the national average. Columns 1 and 3 of Table B.13 show that figures for the Veneto region from the two data sources compare fairly well. As a measure of regional labour market tightness we use the (standardized) regional employment rate of males aged 25-64 because there is wide variation in labour force participation across regions in Italy. For this reason employment, rather than unemployment, is a better measure of labour market tightness. In 1989-1993, the employment rate among prime age men (aged 25-64) in Veneto is slightly above the Italian average equal to 80.22 percent.

Table 9 shows the results for firms in the range 5-25 and 10-20 employees. We estimate regressions with a triple interaction term where the small-firm dummy is interacted with the period post reform and with the average regional employment rate. We also control for regional fixed effects and region-specific trends. The average effect on the Italian sample (captured by the double interaction Small $\times$ Post) ranges between -0.01 and -0.02 and is generally significant. The coefficient on the triple interaction is instead always positive (and typically significant) meaning that workers of firms below the threshold in regions with higher employment rate suffer lower wage losses after 1990. Being Veneto a region with labour market tightness slightly above average, the results in Table 2 represent presumably a lower bound of the results in other regions. Indeed, the average negative effect of the reform over the whole Italian territory is larger in magnitude than the effect obtained on the VWH sample using the same specification.

7 Discussion and Conclusions

This paper investigates the effects on wages of an Italian reform which introduced severance payments for firms with 15 or fewer employees in case of unfair dismissal, and left larger firm unaffected. On average, we find a small but significant negative effect on wages in firms below 15 employees, between minus 0.7%–1.5%. Interestingly, the effect is highly heterogeneous depending on the relative bargaining power of workers vs. firms. These findings complement those in Kugler and Pica (2008) who find a significant reduction in worker flows of around 13-15% induced by the same 1990 reform: we can presume that the substantial effects on worker flows would have been even higher had part of the adjustment not taken place through wages.

It is important to stress that this empirical exercise – which is local in nature as any RDD – cannot help determining whether *any* increase in EPL would be (partially) offset by lower wages. However, the first advantage of the Italian EPL reform is to offer a clean natural experiment which involved a vast quantity of firms and workers. Second, being Veneto a relatively rich region with small-sized firms not dissimilar to many manufacturing regions in Europe, the estimated effects are unlikely to be peculiar of the area under scrutiny and may therefore provide useful insights on the effects of EPL reforms in the many countries which have firm-level thresholds in the application of EPL.

Consistently with the theoretical predictions of models with labour market frictions and Nash bargaining, we find that the wage effect of EPL is concentrated on the entry wage of newly-hired workers; furthermore the negative effect is stronger (*i*) on young blue collar workers, (*ii*) on low-bargaining power workers at the low end of the wage drift distribution and (*iii*) in regions with low employment rates in which workers have a worse outside option. Results partially diverge from theory on one account: the two-tier wage model structure *à la* Mortensen and Pissarides (1999) predicts that higher EPL improves the bargaining position of workers who are already locked in an employment relationship. Differently, we find that the wage of workers already employed when the reform was introduced (who stayed in the same firm afterwards) did not increase after the reform; the same happens to the wage of newly-hired workers in the periods subsequent to entry. These results seem to suggest that workers are not able to renegotiate wages possibly because, if the firm refuses to renegotiate wages, the workers' threat point is not credible (McLeod and Malcomson, 1993).

Finally, we use our estimates to try and calculate – albeit roughly – how much of the increase in the firing cost is translated onto lower wages.[26] The Mortensen and Pissarides (1999) model predicts that the wage-offset of the EPL costs should be total, and pre-paid at entry. In practice, Lindbeck and Snower (1988) list many reasons why employers may not be able to reduce entry wages by the full extent of the increase in EPL: among them, liquidity constraints, minimum wage laws or social norms.[27] Therefore the extent to which firms can actually translate EPL costs onto lower entry wages is an empirical question and in the following final section we use our results to produce a plausible range of estimates.

7.1 Basic calculations of the translation of EPL costs on wages

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 Euro. Therefore, the severance pay transferred to the worker amounts to 313×16 weeks = 5,008 Euro, excluding the legal expenses that can be roughly calculated to be as much as 5,000 Euro. 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 Italian court sentences, estimate both the probability of reaching

[26] This quantification exercise is, as our empirical approach, purely partial equilibrium. We leave the quantification of the general equilibrium impact of EPL within a dynamic GE model for future work.

[27] Furthermore, firms often sustain some costs in hiring new workers (e.g., advertising, screening). If these costs are incurred before the firm negotiates the wage with the entrant then they are sunk and therefore the firm cannot shift them onto the wage. There are other reasons why entrants wages may be higher than their reservation wages: if entrants fear that the firm may dismiss them before they turn into insiders and gain bargaining power then their reservation wage will be higher to make up for this risk. Or if insiders can bargain on both their wages and entrants' wages, then they may use their power (through unions for example) to push entrant wages above their reservation wage, thus reducing hiring and potential competition in the workplace.

an out-of-court agreement and the probability that the dismissal is ruled unfair to be around 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 $5{,}008 \times 0.5 = 2{,}504$ Euro excluding legal expenses. If we assume a probability of 10% of the occurrence of individual firing for economic reasons during a typical employment relationship, the total expected cost ex-ante for the employer is $(5{,}000 + 2{,}504)/10 = 750.4$ Euro.

Heckman and Pagés (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, we use the estimates in the paper to compute the effect of severance payments on the expected present discounted value of wages calculated at the time of hiring. On the basis of our estimates in Table 2 (Column 4), the wage loss for an average worker (with average tenure 3.5 years) in a firm below the threshold of 15 employees after the reform ($\widehat{\delta_2} = -0.011$) amounts to about 3.4 Euro per week (313×0.011) or approximately 179 Euro 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 onto lower wages.

This percentage is increasing in the worker estimated wage loss (which depends on the estimated point coefficient and on average worker tenure) and decreasing in the expected firing costs for employers (which depends on the assumed probability of firing a worker and on legal expenses). In any case, we have shown in this paper that the estimated extent of the translation of EPL costs on lower wages is an average that hides heterogeneous effects across workers with different bargaining positions.

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A Evidence on Firm and Workers Sorting

A.1 Firms

To verify if firms sort according to pre-existing observable and 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 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

$$\begin{aligned} d_{jt} &= \beta' X_{jt} + \delta_0 Post + \delta_1 dummyS_{jt-1} + \delta_2 FE_j + \alpha_0 \left(dummyS_{jt-1} \times Post\right) \\ &\quad + \alpha_1 \left(FE_j \times Post\right) + \alpha_2 \left(dummyS_{jt-1} \times Post \times FE_j\right) + \varepsilon_{jt}, \end{aligned} \tag{6}$$

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 A.1 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 × 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 × Firms Fixed Effect × Dummy 15 is not significantly different from zero.

A.2 Workers

We test whether workers non-randomly sort into firms above and below the 15 employees threshold adopting two strategies. First, we check whether firms observable characteristics X_{jt}, such as industry, age, and occupation (white collar/blue collar) composition of the workforce, are balanced in the neighbourhood of the 15 employees threshold. The balance tests are performed running the firm-level regression:

$$X_{jt} = \delta_0 Post + \delta_1 D^S_{jt} + \delta_2 \left(D^S_{jt} \times Post\right) + \sum_{k=1}^{n} (\gamma_k fsize^k_{jt}) + e_{jt}. \tag{7}$$

Table A.2 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.

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:

$$\begin{aligned} d_{ij't} &= \beta' X_{ijt} + \delta_0 D^S_{jt-1} + \delta_1 T_t + \delta_2 FE_i + \alpha_0 \left(T_t \times D^S_{jt-1}\right) + \alpha_1 \left(T_t \times FE_i\right) + \\ &\quad + \alpha_2 \left(T_t \times D^S_{jt-1} \times FE_i\right) + \varepsilon_{ijt}, \end{aligned} \tag{8}$$

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

Table A.1. Firm Sorting

	(1)	(2)	(3)
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%.

A.3, Columns 1 and 2) or to a firm $j^{'}$ with fewer than 15 employees (Table A.3, 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 A.3) 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 A.3 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 A.3 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 $T_{1990} \times D^S_{jt-1} \times FE_i$).

TABLE A.2. Balanced test of firm characteristics

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

Notes: 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 A.3. Worker Sorting

Dependent Variable: mover dummy (probit)	$P > 15$		$P \leq 15$	
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%.

B Additional Tables

Table B.4. Effects of the reform on entry and exit

	Entry		Exit	
	LPM	Probit	LPM	Probit
Small firm dummy	-0.004	-0.087	-0.008	-0.401
	(0.009)	(0.079)	(0.002)***	(0.241)*
Small firm dummy × post 1990	-0.010	-0.012	0.001	-0.070
	(0.008)	(0.066)	(0.002)	(0.209)
Observations	28043	28043	28043	28043
R^2	0.01		0.00	

Notes: Columns 1 and 3 report results from Linear Probability Models and columns 2 and 4 from Probit models. Entry models include a third degree polynomial in firm size, sectoral dummies and year dummies. Exit models additionally include a second degree polynomial in firm age. Robust standard errors clustered by firm in brackets. One asterisk denotes significance at 10%; two asterisks denote significance at 5%; three asterisks denote significance at 1%.

Table B.5. First stage of IV model in Panel A of Table 2

	Small	Small × Post 1990
Excluded instruments		
Size in 1987	-0.0015	-0.0185
	[0.0015]	[0.0020]***
Size in 1988	0.0020	0.0840
	[0.0016]	[0.0021]***
Size in 1987 × 1990	-0.0018	0.0153
	[0.0008]**	[0.0011]***
Size in 1988 × 1990	0.0009	-0.0843
	[0.0008]	[0.0011]***
Observations	76814	76814
R^2	0.55	0.82
F-Test of excluded instruments	5.71	11031.65
(p-value)	(0.000)	(0.000)
Hansen's J statistic	0.53	
(p-value)	(0.77)	

Notes: 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 B.6. Falsification exercise: fake firm size threshold

Fake firm size threshold	6 employees	10 employees	20 employees	23 employees
	Panel A: Worker fixed effects			
Small firm × post 1990	-0.003	-0.003	-0.009	-0.001
	[0.003]	[0.002]	[0.003]***	[0.005]
Observations	96333	96333	96333	96333
R^2	0.16	0.16	0.16	0.16
	Panel B: IV + Worker fixed effects			
Small firm × post 1990	-0.064	-0.007	0.001	0.043
	[0.052]	[0.006]	[0.015]	[0.081]
Observations	76814	76814	76814	76814

Notes: 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 B.7. Falsification exercise: fake reform year

Fake reform year	1991	1992	1988	1989
	Panel A: Worker fixed effects			
Small firm × post reform year	-0.004	-0.001	0.003	0.002
	[0.001]***	[0.002]	[0.002]*	[0.001]
Observations	96627	96458	139794	136784
R^2	0.15	0.15	0.24	0.28
	Panel B: IV + Worker fixed effects			
Small firm × post reform year	-0.004	0.000	0.006	0.000
	[0.003]*	[0.003]	[0.004]	[0.004]
Observations	75852	75852	113884	113238

Notes: the sample of columns 1 and 2 is 1989-1993 excluding the year of the falsified reform. The sample of columns 3 and 4 is 1987-1993 excluding the year of the falsified reform. 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 B.8. Robustness to different time periods

Time periods	1989-93	1989-91	1989-92	1988-93	1987-94	1986-96	1986-94
	Panel A: Worker fixed effects						
Small firm × post 1990	-0.004	-0.006	-0.005	-0.002	-0.003	-0.005	-0.003
	[0.002]**	[0.002]***	[0.002]***	[0.002]	[0.002]*	[0.002]***	[0.002]**
Observations	96333	48332	72526	117630	158116	211267	175835
R^2	0.16	0.25	0.2	0.24	0.26	0.29	0.3
	Panel B: IV + Worker fixed effects						
Small firm × post 1990	-0.011	-0.015	-0.015	-0.006	-0.008	-0.018	-0.012
	[0.004]***	[0.004]***	[0.005]***	[0.003]**	[0.005]	[0.008]**	[0.006]*
Observations	76814	36320	57593	98840	134191	176648	151189

Notes: 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 B.9. Robustness to polynomials of different degrees

Time periods	1989-93	1989-91	1989-92	1989-93	1989-91	1989-92
	Polynomial of degree 1			Polynomial of degree 2		
	Panel A: Worker fixed effects					
Small firm × post 1990	-0.004	-0.006	-0.005	-0.004	-0.006	-0.005
	[0.002]**	[0.002]***	[0.002]***	[0.002]***	[0.002]***	[0.002]***
Observations	96333	48332	72526	96333	48332	72526
R^2	0.16	0.25	0.2	0.16	0.25	0.2
	Panel B: IV + Worker fixed effects					
Small firm × post 1990	-0.005	-0.015	-0.005	-0.008	-0.021	-0.01
	[0.003]	[0.004]***	[0.006]	[0.003]**	[0.009]**	[0.003]***
Observations	76814	36320	57593	76814	36320	57593

Notes: 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 B.10. Local linear regression with optimal bandwidth

Time periods	1989-93	1989-91	1989-92
	Panel A: Worker fixed effects		
Small firm × post 1990	-0.004	-0.006	-0.005
	[0.002]***	[0.002]***	[0.002]***
Observations	118308	59450	89030
R^2	0.15	0.24	0.19
	Panel B: IV + Worker fixed effects		
Small firm × post 1990	-0.004	-0.011	-0.008
	[0.003]	[0.003]***	[0.002]***
Observations	94396	46160	71347

Notes: Robust standard errors clustered by individual in brackets. Local Linear Regression (LLR) with optimal symmetric bandwidth $\Delta = 12$. All specifications include 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 B.11. Movers and incumbents characteristics

Year	Proportion of job changes	Incumbents wages	Movers wages	Movers entry wages	Movers post-entry wages
1989	0.0863	302	290	284	291
	(0.281)	(78)	(70.3)	(70.2)	(70.3)
1990	0.105	310	297	295	298
	(0.307)	(80.7)	(70.7)	(69.8)	(71.2)
1991	0.0999	322	309	305	311
	(0.3)	(83.6)	(74.6)	(72.1)	(75.7)
1992	0.0927	324	310	307	312
	(0.29)	(85.2)	(78.1)	(79.4)	(77.4)
1993	0.0934	325	310	305	312
	(0.291)	(88.2)	(78.9)	(74.3)	(80.9)
Observations	120652	84962	35690	10414	25276

Notes: Movers are defined as workers who change firm at least once over the period 1989-1993. The sample period is 1989-1993, including year 1990. Real wages expressed in 1995 Euro. Standard deviations in parentheses.

Table B.12. Distribution of wage drift by type of contract

National contract for employees of	*N*	Q05	Q10	Q25	Q50	Q75	Q90	Q95
Non-specified manufacturing firms	276	0.106	0.140	0.203	0.324	0.561	0.815	1.102
Small-size manufacturing firms	180	0.116	0.146	0.194	0.290	0.509	0.715	0.833
Textile artisanal firms	972	0.090	0.112	0.168	0.252	0.427	0.649	0.805
Firms of the food industry	876	0.143	0.217	0.307	0.407	0.545	0.741	1.012
Firms of the shoe industry	333	0.101	0.149	0.214	0.351	0.565	0.801	1.082
Firms of the service sector	6470	0.228	0.281	0.369	0.503	0.705	0.964	1.156
Cooperative firms of the service sector	57	0.232	0.308	0.422	0.480	0.531	0.751	0.841
Firms of the leather industry	1857	0.131	0.186	0.274	0.415	0.645	0.963	1.205
Firms of the construction sector	3111	0.005	0.114	0.223	0.315	0.422	0.579	0.740
Small firms of the construction sector	706	0.061	0.130	0.262	0.381	0.490	0.603	0.706
Construction-related artisanal firms	2184	0.036	0.116	0.216	0.291	0.364	0.478	0.565
Firms of the toys and personal articles sector	144	0.124	0.144	0.217	0.319	0.431	0.699	0.868
Firms of the wood and furniture sector	1595	0.113	0.151	0.201	0.267	0.362	0.519	0.673
Metal-manufacturing and installation firms	6311	0.175	0.232	0.327	0.463	0.663	0.929	1.212
Small metal-manufacturing and installation firms	3191	0.183	0.231	0.332	0.461	0.622	0.868	1.055
Artisanal metal-manufacturing and installation firms	8871	0.116	0.161	0.239	0.365	0.530	0.726	0.879
Firms providing environmental health services	68	0.198	0.218	0.350	0.591	0.852	1.021	1.196
Firms of the transportation sector	649	0.196	0.303	0.431	0.576	0.757	0.951	1.097
Firms providing professional services	160	0.226	0.342	0.544	0.918	1.216	1.647	2.180
Firms of the textile sector	322	0.136	0.159	0.218	0.330	0.463	0.796	1.058
Firms of the tourism sector	562	0.172	0.217	0.284	0.392	0.545	0.772	1.168
Total	38895	0.125	0.179	0.271	0.397	0.579	0.822	1.028

Notes: the wage drift is defined as (wage-contractual wage)/contractual wage. Contractual wages are bargained at the national level by sector and occupation category (typically 5 or more categories according to tasks performed and tenure).

Table B.13. VWH and (random 10%) Italian Social Security Administration (INPS) archives

	Veneto Workers History (VWH)	Italian Sample	Veneto in the Italian Sample
Real weekly wages	314.248	315.37	306.97
	(82.199)	(121.40)	(98.76)
Small firm dummy	0.642	0.69	0.66
	(0.480)	(0.46)	(0.47)
Age	36.904	35.99	34.86
	(8.722)	(9.50)	(9.52)
Firm size	13.133	12.76	13.09
	(5.602)	(5.70)	(5.62)
White collar dummy	0.144	0.20	0.14
	(0.352)	(0.40)	(0.35)
N	96333	7323	907
		ISTAT	
Regional employment rate (males 25-64)		80.22	
		(2.88)	

Notes: The Italian sample is a random 10% sample of a data set drawn from the Italian Social Security Administration (INPS) archives. Employment rate is the average regional rate of employment of males aged 25-64 across 20 Italian regions over the period 1989-1993 (ISTAT Regional accounts). Real wages expressed in 1995 Euro. Standard deviations in parentheses.

Table B.14. IV First-Stage statistics

	Table 2 Panel B	Table 3	Table 4	Table 5	Table 6 Panel A	Table 6 Panel B
Hansen J test	4.14	1.18	0.04	10.27	7.35	6.74
	(0.13)	(0.88)	(0.98)	(0.11)	(0.12)	(0.15)
F-tests of excluded instruments of the first stage equations						
Small firm	6.75	5.13	4.72	1.71	4.06	3.02
	(0.00)	(0.00)	(0.00)	(0.06)	(0.00)	(0.00)
Small firm × Post 1990	9650.85	5612.73	7487.26	563.70	5743.78	1989.45
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Small firm × var. empl. growth		23.15				
		(0.00)				
Small × Post 1990 × var. empl. growth		445.37				
		(0.00)				
Small firm × mover			335.60			
			(0.00)			
Small firm × Post 1990 × mover			830.15			
			(0.00)			
Small firm × Entry dummy				269.54		
				(0.00)		
Small firm × Post 1990 × Entry dummy				230.25		
				(0.00)		
Small firm × Tenure					4326.39	1647.80
					(0.00)	(0.00)
Small firm × Post 1990 × Tenure					3203.36	948.91
					(0.00)	(0.00)

Notes: Each column shows the first-stage statistics of the IV models presented in Table 2 (Panel B) and Tables 3-6. First-stage results of the IV model presented in Panel A of Table 2 are displayed in Table B.5. The first row displays the Hansen J statistic (p-value in the second row). Subsequent rows present the F-test of excluded instruments for each first stage equation (p-values in parentheses).

Table B.15. IV First-Stage statistics *(continued)*

	Table 7		
	Panel A	Panel B	Panel C
Hansen J test	0.91	5.95	4.12
	(0.92)	(0.20)	(0.39)
F-tests of excluded instruments of the first stage equations			
Small firm	3.10	2.26	2.18
	(0.00)	(0.02)	(0.03)
Small firm × Post 1990	5548.92	3017.63	3021.61
	(0.00)	(0.00)	(0.00)
Small firm × blue collar	4865.72		
	(0.00)		
Small firm × Post 1990 × blue collar	445.37		
	(0.00)		
Small firm × young (below 30 above 40)		135.12	
		(0.00)	
Small firm × Post 1990 × young		1198.53	
		(0.00)	
Small firm × young blue collar			151.77
			(0.00)
Small firm × Post 1990 × young blue collar			1034.74
			(0.00)

Notes: Each column shows the first-stage statistics of the IV models presented in Table 7. The first row displays the Hansen J statistic (p-value in the second row). Subsequent rows present the F-test of excluded instruments for each first stage equation (p-values in parentheses).