Does Reducing Employment Protection Affect Worker Training? New Firm-Level Evidence from a Labour Market Reform*

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October 2018
(*Preliminary, do not quote*)

Abstract

In this paper we study whether and how employment protection legislation (EPL) affects firm-provided training for workers leveraging a recent labour market reform in Italy, known as the Fornero Law, that reduced the degree of employment protection in firms with more than 15 employees. Results from a difference-in-regression discontinuities design demonstrate that, following the introduction of the Fornero Law, the number of trained workers might have increased in the case of firms just above the threshold, with an order of magnitude of 1-2 additional workers, depending on the specification.

JEL codes: J42, J63, J65, M53.
Keywords: employment protection legislation, firing costs, training, reforms, Italy

*Work in progress, comments are welcome.

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1 Introduction

In this paper we study whether and how employment protection legislation (EPL) affects firm-provided training for workers. As a matter of fact, the relationship between labour market flexibility and the incentives for firms to sponsor training is not clear cut. By way of example, Acemoglu (1997) and Acemoglu and Pischke (1999) show that firms might have stronger incentives to provide training in the presence of labour market institutions that create wage compression. Their argument goes as follows: labour market institutions, such as EPL, create a wedge between a worker's marginal product and her wage, thus generating rents; moreover, EPL might reduce the outside option of workers so that wages increase less than productivity for trained workers. As a result, and as long as the rents generated by EPL increase with training, stricter EPL rules might lead to stronger incentives for firms to sponsor training.

On the other hand, other authors (Hijzen et al. 2017, Bratti et al. 2018) have recently argued that, in dualistic labour markets, stricter EPL on regular contracts might well lead to lower overall firm sponsored training. This is because the co-existence of contracts that entail different levels of protection allow firms that face higher firing costs to make more extensive use of temporary workers, who are less protected by the legislation. However, temporary workers tend to receive less training: in this case, reforms that enhance the flexibility of open-ended contracts might end up producing higher on-the-job firm-sponsored training.[1]

Unsurprisingly, the empirical evidence is mixed: Picchio and van Ours (2011) find that more labour market flexibility is associated to slightly less investment in training using Dutch microdata; similarly, Messe and Rouland (2014) exploit a French reform entailing an increase in firing costs for older workers and find that the latter determined a modest increase in training for workers just below the eligibility threshold. However, Cabrales et al. (2017) use PIAAC data and show that, in dualistic labour markets, temporary workers receive less training so that the effect of EPL on training might depend on the composition of the workforce.[2] Bratti et al. (2018) use a Regression Discontinuity Design (RDD) framework to identify the effect of EPL on training using the discontinuous increase in the degree of EPL for firms above 15 employees in Italy and report a lower number of trained workers at the threshold, which is largely mediated by the relatively greater use of temporary workers at the threshold.

In this study we bring new evidence on the relationship between firms' incentives to sponsor training and EPL using a *difference-in-regression discontinuities design* (DRDD) framework and exploiting a recent reform in Italy, known as the Fornero Law, that reduced the degree of employment protection in firms with more than 15 employees in July 2012.[3] Before the Fornero Law, employees in firms with more than 15 employees had the right, in the case of a dismissal declared unfair by a court of law, to ask for reinstatement (and receive all foregone wages plus health and social security contributions) or receive a monetary compensation. By way of contrast, in firms below the 15-employee threshold, it was (and still is) up to the employer to decide whether to reinstate the worker (without paying foregone wages) or pay a smaller monetary compensation. While we refer to Berton et al. (2017) for an exhaustive explanation of the details of the reform, here we note that the Fornero Law limited the possibility for workers of firms with more than 15 employees to opt between

[1] The impact of EPL on training might also depend on the impact that EPL might have on physical capital accumulation and the degree of complementarity between human and physical capital. See for instance Wasmer (2006).

[2] See also Almeida and Carneiro (2009) and Pierre and Scarpetta (2013) for additional evidence.

[3] Berton et al. (2017) study the impact of the FL on worker mismatch using Italian quarterly LFS data over the period 2011-2014 using DRDD. They report positive effects of the reform both on the probability of a good match and on labour reallocation and labour productivity.

reinstatement and a monetary compensation to a set of well-defined cases; moreover, it reduced the amount of the monetary compensation and eased the uncertainty surrounding the duration and costs of litigation, which used to be very important, especially in certain areas of the country (Gianfreda and Vallanti (2017)).

This study is related to different strands of literature. First, it speaks to the theoretical literature on the relationship between labour market institutions and firm-provided-training, recently summarized by Bratti et al. (2018). Second, it is related to the empirical literature on EPL and training mentioned above. Finally, the paper is also related to the large literature on the economic effects of EPL.

In the empirical application, we use two waves of RIL ('Rilevazione Longitudinale su Imprese e Lavoro'), i.e. firm-level data provided by INAPP (National Institute for the Evaluation of Public Policies) for a representative sample of the population of both the limited liability companies and partnerships in the private (non-agricultural) sectors in Italy for the years 2010 and 2015, respectively. We find that the reduction in EPL has a positive and typically statistically significant effect on the number of trained workers. Moreover, results are broadly confirmed when we exploit the panel dimension of the dataset.

The rest of the paper is organised as follows. In Section 2 we introduce the institutional framework and present our identification strategy. After discussing the main data features in Section 3, we present our main results, some robustness checks and a discussion of potential confounding factors and mechanisms in Section 4. Section 5 summarises the main findings and draws conclusions.

2 Institutional Framework and Identification

2.1 Institutional framework

Since the 1960s, the regulation of unfair dismissals has changed several times in Italy. The most significant reform occurred in 1970 with Law 300-70, also known as *Statuto dei Lavoratori* (Workers' Statute) and, in 1990, with Law 108/1990, which strengthened employees' protection from unfair dismissal only in the case of small firms (i.e. firms below 15 employees).[4]

Before the legislative changes that occurred in 2012 (Fornero Law) and 2015 (Jobs Act), the degree of protection enjoyed by unfairly dismissed workers was considerably greater in the case of employees working in firms with more than 15 employees. Indeed, if a dismissal was declared unfair by a judge, the employee unfairly dismissed from a firm with more than 15 employees could ask to be reinstated and receive the wages forgone and the health and social security contributions (for a minimum of 5 months) related to the period between the dismissal and the sentence. Although reinstatement was the most likely occurrence in practice, the unfairly dismissed employee retained the right to receive, as an alternative, a severance payment amounting to 15 months' salary. In contrast, in the case of firms with fewer than 15 employees, it was up to the employer to choose whether to reinstate the unfairly dismissed worker (without paying any forgone wages) or make a severance payment, which ranged from 2.5 to 14 months in the case of very senior workers (Hijzen et al. 2017).[5]

The higher *de jure* costs for employers in the case of firms with more than 15 employees are further increased if one also takes into consideration the *de facto* costs associated with the very long average duration of labour trials in Italy: Gianfreda and Vallanti (2017) report

[4] See Cingano et al. (2016) and Hijzen et al. (2017) for a brief overview of legislative changes that occurred between 1960 and 2012.

[5] Above the 15-employee threshold, employment protection is also greater in the case of collective dismissals.

average trial decisions of about 850 days over the period 2007-2010, with large variation across regions. Such a difference in the length of labour trials lead to escalating firing costs above the threshold. Indeed, using a formula proposed by Garibaldi and Violante (2005) to compute *ex post* firing costs, Gianfreda and Vallanti (2017) report firing costs equivalent to about 36 months' wages in Trento versus 160 months in Salerno for a blue-collar worker with 8 years of tenure in a firm above the 15-employee threshold.[6] Because below the threshold no forgone wages are due, the length of labour trials matters only above the threshold, with firing costs rapidly increasing above the 15-employee threshold if the labour trial lasts longer than 5 months.[7] Moreover, the lack of a clear definition of unfair dismissal in the Italian legislation (Hijzen et al. 2017) led to some inconsistencies in its implementation, as noted by Ichino et al. (2003), who showed that, in regions with high unemployment rates, judges tended to rule in favour of employees. The variability in decisions therefore led to great uncertainty, which further increased the costs associated with the stricter employment protection for firms above the threshold.

2.2 Identification strategy

The discontinuity in the level of EPL between firms below and above the 15-employee threshold existing before the FL, joint with the change in the level of EPL only above the cut-off induced by the FL, makes this an ideal setting for the application of a difference-in-regression discontinuities design (DRDD). Namely, the causal effect of EPL on firm training is identified by comparing the difference in the number of trained workers below vs. above the threshold, before and after the introduction of the Fornero Law. In our empirical strategy we select firms in the range of 5-15 employees as the control group, and those in the 15-25 employees range as the treated group, respectively.[8]

The main identification assumption is that any unobservable variable impacting on training is either continuous at the threshold (as in RDD) or the discontinuity is constant over time (as in a conventional Difference-in-Differences approach). In this case, the change in training before and after the reform for firms just below the threshold can be considered as a valid counterfactual for the same change for firms just above the threshold in the absence of the FL. One additional advantage of the DRDD approach is that the existence of possible confounding factors that change discontinuously at the threshold, like the rights for unions to form work councils, are controlled for, unlike in a conventional RDD framework. The DRDD approach can be described parametrically trough the following equation:

$$\begin{aligned} y_{it} = \alpha_0 + \alpha_1 post_t + \alpha_2 above_{it} + \alpha_3 above_{it} \times post_t + \alpha_4 f(Size_{it} - 15) \\ + \alpha_5 f(Size_{it} - 15) \times above_{it} + \beta' X_{it} + \varepsilon_{it} \end{aligned} \tag{1}$$

where i is the firm subscript, t the survey wave subscript ($t = 2010, 2015$) and y_{it} the training outcome of interest. Our data refers to two cross sections that should be representative of Italian firms in 2010 and 2015, i.e. the two RIL waves: it is important to note that we pool the two cross sections and that, therefore, the firms in the two waves are generally

[6] If one takes into account the expected probability of a settlement between the parties and the fact that some rulings are decided in favour of the firm, the *ex ante* firing costs fall to about 15 months of wages in Trento, compared with 65 months in Salerno. The formula is based on the time it takes to reach a sentence, the forgone wage, the health and social security contributions, the penalty rate on forgone contributions, the legal fees and the severance payments. See Garibaldi and Violante (2005) for the details.

[7] Indeed, 5 months is the minimum amount of forgone wages and contributions that the unfairly dismissed worker has the right to receive in firms above the threshold.

[8] A similar approach is followed by Cingano et al. (2016) in an analysis of firm investment and access to credit.

not the same, even if he RIL survey has a small panel component, which we will use in some robustness checks. $post_t$ is a dichotomous indicator that equals one in the period after the reform (i.e. in the 2015 wave); $above_{it}$ is a dichotomous indicator that equals one for the firms affected by the Fornero Law, i.e. firms above the 15-employee cut-off; $f(Size_{it}-15)$ is a polynomial in firm size normalised with respect to the cut-off size, which is allowed to differ on each side of the cut-off and which represents the forcing variable; the coefficient of the interaction $above_{it} * post_t$ is the parameter of interest, capturing the causal effect of relaxing EPL on firm-provided training; finally, X_{it} is a vector of controls and ε_{it} a firm error term.

In some specifications, we allow for the polynomial in firm size to be different both before and after the reform, as in Grembi et al. (2016):

$$y_{it} = \alpha_0 + \alpha_1 post_t + \alpha_2 above_{it} + \alpha_3 above_{it} \times post_t + \alpha_4 f(Size_{it}-15) + \alpha_5 f(Size_{it}-15) \times above_{it} + \alpha_6 f(Size_{it}-15) \times post + \alpha_7 f(Size_{it}-15) \times above_{it} \times post_t + \beta' X_{it} + \varepsilon_{it} \quad (2)$$

In equation (2) the effect of firm size on firm-provided training is allowed to have a differential effect not only above and below the threshold, but also before and after the Fornero Law: this is clearly a more general specification than that in equation (1).

We include in all regressions either a first or a second order polynomial in (normalised) firm size. In order to increase the precision of the estimates, in some specifications we also include, as controls, a vector of industry and region fixed effects, which may affect both firing costs and a firm's likelihood to train workers.

Pooling the two cross sections requires the assumption that the population of treated and untreated firms does not change as a result of the reform, e.g. firms in 2015 above the threshold should be representative of firms above the threshold in 2010. We have checked that our results are robust to this assumption in various ways. First, we have run a Schivardi and Torrini (2008) test and we did not find clear evidence of a statistically significant change in the propensity to grow at the threshold after the FL. Second, although this leads to a loss of about two-thirds of the data, we have limited the analysis to the panel component of the survey, which in turn allows us to focus, as in Boeri and Garibaldi (2018), only on firms that have been consistently above or below the threshold in both waves. In the panel analysis we have estimated equation (1) and (2) with both pooled OLS and the within group estimator, i.e. allowing for firm-specific fixed effects, that capture unobserved firm-level heterogeneity.

3 Data

We use two waves (2010 and 2015) of the RIL Survey dataset ('Rilevazione Longitudinale su Imprese e Lavoro') provided by INAPP (National Institute of the Evaluation of Public Policies).[9] From the universe of Italian firms provided by ISTAT (the Italian National Statistical Institute), called ASIA (*Archivio Statistico Imprese Attive*, Statistical Archive of Active Enterprises), the RIL sample is extracted based on firm size and it is representative of the population of both the limited liability companies and partnerships in the private (non-agricultural) sectors at the national level. The survey was conducted in 2005, 2007, 2010 and 2015; and a panel version of the dataset is available for a limited number of firms. However, for our purposes, we limit our analysis to the last two available years.

[9] In the past the survey was run by the *Istituto per lo sviluppo della formazione professionale dei lavoratori* (Institute for development and training of workers), whose acronym was ISFOL. The INAPP institute has been recently created and its main activities are oriented towards research, monitoring and public policy evaluation. It constitutes a building block in supporting policy making by the Ministry of Labour and Social Policies.

Among the others, the dataset provides information on the number of employees, age, gender, types of contract (temporary versus permanent), qualifications in terms of education and training activities for workers; moreover a series of additional variables related to the industrial relations system are available. Contrary to poor availability in administrative datasets, the RIL data offers detailed information on training activities, such as information on the occurrence of worker training activities at the firm level, the number of workers trained, the type of training activities (e.g. coaching, counselling, outdoor training), who provided the training (i.e. external companies, specialised consulting firms or others), who paid for the training (i.e. the firm, the firm with partial coverage from external funding, external contributions), who provided external contributions and the expenditure on training. As far as additional variables related to the industrial relations systems are concerned, it is possible to obtain information on the presence of unions (work councils) in the workplace and the level of bargaining (centralized or based on a two-tiered structure).[10]

Note that, although the reference years of the two waves we use are 2010 and 2015, important variables related to the number of employees and training activities refer to the previous year, i.e. 2009 and 2014, respectively. The whole sample is 54,352 observations, for 10,239 firms we have two observations (panel), while the rest is a repeated cross section. In Table 1, we report some (weighted) descriptive statistics for the full sample for the different years. In the econometric analysis, we restrict the sample to firms with more than 5 and less than 26 employees; moreover, we trim the data by dropping from the analysis those firms that experienced an year-on-year grow rate of employees larger (smaller) than the 95 (5) percentile and we restrict the sample to active firms.

[Table 1 about here]

4 Results

This section reports our baseline estimates of the effect of EPL on firm-provided training as well as a series of robustness checks, using the number of trained workers as outcome variable.[11]

4.1 Main results

In the first four columns of Table 2 we report estimates with a polynomial in firm size that is allowed to differ on each side of the cut-off, but that is instead assumed to take on the same coefficient before and after the reform, i.e. we estimate various versions of equation (1). In column (1) we use a first-order polynomial in firm size and exclude sector and region FEs (to which we will refer as 'firm controls' for brevity). The estimates show that, at the 15 employees threshold, and following the Fornero Reform, there has been an average increase of 1.71 trained workers, significant at the 1 per cent level. Moreover, the estimates are not sensitive to the inclusion of region and sector fixed effects, as shown in column (2). Columns (3) and (4) report the empirical estimates from the estimation of equation (1) using a quadratic polynomial in firm size: the coefficient on $post \times above$, capturing the effect of the Fornero Law, remains highly significant and of magnitude similar to that in columns (1)

[10] For example, Devicienti et al. (2017) use RIL data as a primary source of information to study the relationship between unions and temporary contracts.

[11] As noted by Cingano et al. (2016), it is not correct to use, as dependent variable, a regressor that includes the forcing variable, i.e. the number of employees. For this reason, we focus on the absolute number of trained workers instead of the share of trained workers. Results are however qualitatively quite similar if we consider the share of trained workers.

and (2), namely 1.72 and 1.66 in the specifications excluding and including sector and region FEs, respectively.

In the remaining columns, we repeat the same econometric exercise but considering a more general specification: indeed, we allow the polynomial in firm size to take on different coefficients also before-after the reform and not only above and below the threshold, i.e. we estimate different versions of equation (2) above. In columns 5-6 (linear polynomial), we note a very slight decline in the magnitude of the effect, which falls to 1.61 and 1.48, depending on the inclusion or not of the firm controls. In turn, when we consider a polynomial of second order (columns 7 and 8), the magnitude is sligthly larger than that reported in columns 3 and 4.

All in all, the estimates reported in Table 2 suggest that firms affected by the FL might have increased training by a magnitude of about 1.5-2 additional trained workers, broadly confirming previous results in Bratti et al. (2018) who used a different identification strategy, namely RDD on the 2010 wave of the RIL. Considering that before the FL the average number of trained workers in firms with 15-25 employees was about 1.06, our estimates suggest is a quite large effect.

[Table 2 about here]

4.2 Robustness checks

As we have already mentioned, the use of repeated cross-sections in a DID framework might lead to an estimation bias if the composition of the cross-sections changes significantly before and after the reform, possibly as the result of the very same reform. Indeed, the FL might have altered the incentives for firms to self select above the threshold. However, as mentioned above, by running a set of Schivardi and Torrini (2008)'s tests, we do not find clear evidence that the FL increased the propensity for firms to jump above the 15 employees threshold. However, in Table 3 in Appendix A, we investigate further this potential bias by restricting the estimation sample to the panel component of the dataset: this reduces the sample size and the precision of the estimates; however, because we exclude those firms that have passed the threshold between 2010 and 2015 in either direction, we can keep the sample unaltered before and after the reform.[12]

[Table 3 about here]

In Table 3 we report, in columns 1-4, estimates of equation (1) and (2) with a polynomial of first degree with and without firm controls; moreover, we include a set of firm fixed effects in order to capture possible unobserved firm-level heterogeneity potentially correlated with the included regressors and we cluster standard errors at the firm level. In column 1-2, where we allow the polynomial to take on different values only below and above the 15 employees threshold, we find a positive and statistically significant effect of the $post \times above$ interaction, but with a lower magnitude of about 1 additional trained worker. In turn, in the more general specification reported in columns 3-4, where we estimate equation (2), the coefficient of the $post \times above$ interaction increases to about 2.5, statistically significant at 5 per cent. In columns 5-6 we repeat the same exercise by eliminating the firm fixed effects and results are broadly confirmed, with an estimated effect that varies between about 1.41and 2, statistically significant at the five or ten per cent level of confidence.

We have also carried out additional robustness checks that are available from the authors upon request and that overall tend to confirm our main results. First, because, using the same dataset for the 2010 wave, Bratti et al. (2018) note that there are heapings in the forcing

[12] We drop about 600 observations which amount to about 10 per cent of our sample.

variable at multiple of 5 employees, we follow Barreca et al. (2016) and eliminate firms with 10, 15, 20 and 25 employees and re-estimate the equations reported in Table 2: reassuringly, results are barely altered. Second, we have run a placebo analysis, by assuming that the threshold was at ten, rather than 15, employees: in this case, the estimate of the interaction term is negative, small and largely insignificant, as it should be expected. Third, because we cannot distinguish between part time and full time employees, and therefore we could erroneously classify firms as above the threshold when instead they are just below, we have re-estimated equations reported in Table 2 but eliminating firms with 16 and 17 employees, with very similar effects, although sometimes slightly imprecisely estimated.

4.3 Channels

Our empirical results suggest that, after the liberalization provisions contained in the Fornero Law, there might have been an increase in the number of trained workers at the threshold. Following the line of reasoning of Hijzen et al. (2017) and Bratti et al. (2018) we argue that this result might be explained recalling that the lower degree of employment protection enjoyed by permanent workers after the Fornero Law might have reduced the incentives for firms to rely relatively more on temporary workers: because those receive less training, this might explain our empirical results. Indeed, using a RDD framework, both Hijzen et al. (2017) and Bratti et al. (2018) show that, using pre-Fornero Law data, firms above the 15 employees threshold tended to use more temporary workers. If this intuition is correct, we should observe, after the Fornero Law, an increase in the share of permanent workers above the threshold. This is exactly what we observe in Table 4, where we report estimates of equation (2), but with the share of permanent workers as dependent variable. As we can see, regression results seem to clearly support the idea that, after the Fornero Law, firms above the threshold have increased the share of permanent workers.

[Table 4 about here]

5 Conclusion

In this study we use Italian data and exploit the size-contingent firing restriction occurring at 15 employees, as well as a recent reform that significantly reduced firing costs in the case of firms above the threshold, in order to identify the effects of employment protection legislation on the propensity of firms to train workers. Our DRDD estimation framework consistently points towards a positive effect of the reform, which lessened firing costs for firms above the 15 employees threshold, with a magnitude that varies between 1 and 2 additional trained workers, depending on the model estimated and sample (e.g. repeated cross section versus panel). Considering that, in our data, there are about 5 trained workers in firms with 15-25 employees (our treated group), the magnitude uncovered in this study amounts to a non-negligible effect of the reform. This result is consistent with previous findings reported, in a cross sectional setting and a more conventional RDD setting, by Bratti et al. (2018) who report a negative effect of tighter EPL of about 1.5-2 fewer workers at the threshold. The increase in training after the reform is likely to be explained by the increase in the share of permanent workers above the threshold after the reform together with the fact that permanent workers generally receive more training.

Acknowledgments. We thank Andrea Ricci for his valuable help with the data and the *Istituto Nazionale per l'Analisi delle Politiche Pubbliche* (INAPP, formerly ISFOL) for giving us access to them. Comments received by participants at the Workshop on The Effects of Employment Protection Legislation and Collective Bargaining on Workers and Firms (Cagliari) and at the AIEL conference in Ancona, in particular Fabio Berton, Francesco Devicienti, Juan Dolado, Andrea Garnero, Marco Leonardi and Giovanna Vallanti, are gratefully acknowledged. Giovanni Sulis also acknowledges financial support from the University of Cagliari (Fondazione di Sardegna fundamental research grant L.R. 7/2007, Economic Growth, Cultural Values and Institutional Design: Theory and Empirical Evidence). The usual disclaimers apply.

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Tables and Figures

Table 1: Descriptive statistics

Over	Mean	Std. Err.	[95% Conf.	Interval]
employees				
2010	5.438617	.0862837	5.269497	5.607737
2015	6.103218	.1549052	5.799597	6.406838
trained workers				
2010	1.065782	.0330001	1.0011	1.130463
2015	1.948505	.0796095	1.792467	2.104543
training dummy				
2010	.225235	.0070761	.2113656	.2391045
2015	.3173836	.0111372	.2955541	.339213
permanent (number)				
2010	4.872145	.0746937	4.725742	5.018548
2015	5.586721	.1060385	5.378881	5.794561
union dummy				
2010	.0194709	.0011181	.0172794	.0216625
2015	.03805	.0026378	.0328798	.0432202
cassa integrazione				
2010	.0910274	.0032143	.0847273	.0973275
2015	.0936566	.005415	.0830429	.1042703

Note. We use sample weights for descriptive statistics, the latter are calculated on the sample used in regression in column 1, Table 2. Employees is the total number of employees. Training dummy is equal to 1 if firm has provided training to any worker during the year and 0 otherwise; trained workers is the number of workers trained during the year. We imputed trained workers equal to employees when number of trained was greater than the number of employees; we imputed 0 when this information was missing. Permanent is the number of workers with an open-ended contract. Union dummy is equal to one for firms with a RSA-RSU in place (works council). Cassa integrazione is a dummy for firms with a short-time work arrangement with redundancy fund in place (Cassa Integrazione Guadagni – CIG).

Table 2: Baseline results: Pooled cross-sections

	all firms (5 to 25 employees)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
post	1.083***	1.117***	1.082***	1.116***	1.503***	1.704***	1.510***	1.661***
	(0.137)	(0.121)	(0.136)	(0.121)	(0.390)	(0.331)	(0.548)	(0.447)
above	-0.404	-0.518	-0.192	-0.328	-0.349	-0.424	-0.356	-0.467
	(0.383)	(0.370)	(0.628)	(0.603)	(0.478)	(0.468)	(0.733)	(0.714)
post×above	1.714***	1.663***	1.718***	1.666***	1.615**	1.482**	2.088*	1.991*
	(0.423)	(0.398)	(0.421)	(0.397)	(0.801)	(0.745)	(1.194)	(1.122)
Polynomial	Linear	Linear	Quadratic	Quadratic	Linear	Linear	Quadratic	Quadratic
Pol. interaction	above	above	above	above	above *post*	above *post*	above *post*	above *post*
Sector f.e.	No	Yes	No	Yes	No	Yes	No	Yes
Region f.e.	No	Yes	No	Yes	No	Yes	No	Yes
Observations	16,470	16,449	16,470	16,449	16,470	16,449	16,470	16,449
R-squared	0.110	0.169	0.110	0.169	0.111	0.169	0.111	0.170

Note. Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1. Polynomials in employment have been interacted with the dummy *above* (15-employee threshold) and the dummy *post* (period affected by Fornero reform). The sample includes only firms with growth of employment between plus and minus 50%.

Table 3: Panel evidence

	firms not switching (5-25 employees							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
post	1.376***	1.396***	2.503***	2.516***	1.439***	1.312***	2.352***	2.457***
	(0.126)	(0.128)	(0.374)	(0.375)	(0.148)	(0.134)	(0.434)	(0.407)
above					-0.414	-0.646	-0.677	-0.826
					(0.570)	(0.542)	(0.699)	(0.658)
post×above	1.036*	1.088*	2.548**	2.672**	1.154**	1.157**	2.038*	1.849*
	(0.556)	(0.563)	(1.155)	(1.165)	(0.549)	(0.535)	(1.114)	(1.083)
Polynomial	Linear	Linear	Linear	Linear	Linear	Linear	Linear	Linear
Pol. interaction	above	above	above *post*	above *post*	above	above	above *post*	above *post*
Sector f.e.	No	Yes	No	Yes	No	Yes	No	Yes
Region f.e.	No	Yes	No	Yes	No	Yes	No	Yes
Firm f.e.	Yes	Yes	Yes	Yes	No	No	No	No
Observations	4,986	4,968	4,986	4,968	6,610	6,599	6,610	6,599
R-squared	0.752	0.763	0.756	0.767	0.111	0.177	0.114	0.181

Note. Cluster robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1. Polynomials in employment have been interacted with the dummy *above* (15-employee threshold) and the dummy *post*(period affected by Fornero reform). Firms not switching indicates firms not changing threshold status over time.

Table 4: Share of permanent workers: Pooled cross-sections

	(1)	(2)	(3)	(4)
post	-0.0629	-0.0169	-0.104	-0.0478*
	(0.0462)	(0.0194)	(0.0683)	(0.0289)
above	-0.0608***	-0.0479***	-0.0492**	-0.0245
	(0.0154)	(0.0140)	(0.0245)	(0.0217)
post×above	0.0935*	0.0440	0.169**	0.0963**
	(0.0551)	(0.0344)	(0.0761)	(0.0406)
Polynomial	Linear	Linear	Quadratic	Quadratic
Pol. interaction	above *post*	above *post*	above *post*	above *post*
Sector f.e.	No	Yes	No	Yes
Region f.e.	No	Yes	No	Yes
Observations	16,516	16,495	16,516	16,495
R-squared	0.006	0.207	0.010	0.209

Note. Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1. Polynomials in employment have been interacted with the dummy *above* (15-employee threshold) and the dummy *post* (period affected by Fornero reform).