

Employment Protection and Firm-provided Training: Quasi-experimental Evidence from a Labour Market Reform

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Abstract

In this paper we study whether and how employment protection legislation (EPL) affects firm-provided training for workers, leveraging a recent Italian labour market reform. In Italy, workers employed in firms with more than 15 employees enjoyed a higher employment protection, thanks to article 18 of the Workers' Statute (Law No. 300 of May 20, 1970) which imposed several firing restrictions to firms, including the obligation to reinstate unfairly dismissed workers if ruled by a court. However, in 2012 a labour market reform, known as the Fornero Law, substantially reduced these restrictions. Results from a Difference in Regression Discontinuities Design, where firms below vs. above the cutoff are compared before and after the reform, demonstrate that, following the introduction of the Fornero Law, the number of trained workers increased in firms just above the threshold, with an order of magnitude of about 1.5 additional workers in our preferred empirical specification. The effect of EPL on training can be partly explained by higher worker turnover and more use of temporary contracts, which generally entail less training, before the reform in firms with higher firing costs. Our study highlights the counter-intuitive and potentially adverse effects of EPL on training in dual labour markets, owing to larger firms seeking to avoid the higher costs of EPL by means of temporary contracts.

JEL codes: J42, J63, J65, M53.

Keywords: employment protection legislation; training; dual labour markets; temporary contracts; Italy

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1 Introduction

On-the-job training is a fundamental source of human capital accumulation and, as such, is very high on the policy agenda ([Brunello et al. 2007](#), [OECD 2014](#)).¹ Both workers and firms benefit from training: workers improve their skills, productivity and, consequently, perceive higher wages, while firms enjoy returns to training in the form of higher productivity.² As workers and firms both benefit from investments in training, in imperfect labour markets they are also likely to share their costs.

In their review article on the effect of imperfect labour markets on firm-sponsored training, [Acemoglu and Pischke \(1999a\)](#) called for the need to increase the number of empirical studies testing competitive and non-competitive theories of training, possibly leveraging policy-induced variation in market imperfections. In this paper, we follow their suggestion and throw light on a relatively underexplored source of labour market imperfections: employment protection legislation (EPL). EPL can be an important determinant of a firm's training supply, [Acemoglu and Pischke \(1999b\)](#), for instance, emphasised that non-competitive labour markets and firing restrictions generate rents that are an increasing function of worker training: stricter levels of EPL might therefore foster incentives for firms to increase training expenditure.

The Italian legislation envisages size-contingent firing restrictions, according to which firing costs increase sharply above the 15-employee threshold (article 18 of the Workers' Statute, Law No. 300 of May 20, 1970; *Article 18*, hereafter). In Section 3 we discuss why these policy-induced differences in employment protection are able to substantially differentiate firing costs according to firm size. These restrictions were greatly reduced in 2012 by a recent labour market reform known as the Fornero Law (Law 28 June 2012, n. 92). Combining the different levels of EPL below and above the 15-employee cutoff with the EPL changes introduced in 2012 gives us a unique opportunity to provide clean causal evidence on the effect of EPL on training using a Difference in Regression Discontinuities design (see, for instance, [Cingano et al. 2016](#), [Grembi et al. 2016](#)). This is a timely moment to add new evidence, given the paucity of studies that have empirically investigated the interplay

¹ [Mincer \(1962\)](#) estimates that around half of human capital accumulation over the life cycle is related to investment in training at the workplace.

² [Haelermans and Borghans \(2012\)](#) conduct a meta-analysis and show that the average reported effect on wages of on-the-job training, corrected for publication bias, is 2.6 per cent per course.

between EPL and firm-provided training (see Section 2), and in the light of several reforms that have reduced employment protection — especially at the margin, i.e. for temporary workers — in many countries.³

The main results of our paper can be summarised as follows. Our preferred estimates demonstrate that the Fornero reform, by reducing EPL for large firms (i.e. firms above the 15-employee cutoff), increased the average number of trained workers by about 1.5 individuals. This is not a negligible effect and corresponds to an about 50 per cent increase in the number of trained workers *at the cutoff* firm size, for which the average number of trained workers was about 3.1 before the reform. The results are not sensitive to an extensive set of robustness checks, including donut-hole regressions to account for potential manipulation of firm size around the cutoff, changes in the bandwidth, changes in the order of the polynomial in firm size, data heaping on firm size and placebo regressions, among the others.

These findings can be explained by noting that, first, in dual labour markets, firms tend to avoid the higher firing costs associated with permanent positions by relying more on a sequence of temporary contracts (Cahuc et al. 2016). Second, in dual labour markets, outside employment opportunities for workers hired on temporary contracts could increase more than their productivity after receiving training, thus reducing the incentive for firms to provide training. This, in turn, may happen because trained workers in temporary contracts may easily find a better (e.g. permanent) employment opportunity outside the firm that provided the training (the employee's productivity is higher thanks to training and therefore they appeal to other firms that want to save on training costs).⁴ Another mechanism explaining why EPL may reduce training in the presence of temporary contracts is highlighted by Cabrales et al. (2017) and Dolado et al. (2016). Their basic insight is that firms can use the conversion of temporary to permanent contracts to push workers to increase their job effort. An increase in the differential in EPL between permanent and temporary contracts, under reasonable assumptions, causes permanent workers to reduce the job effort (e.g. through higher absenteeism, as suggested by Ichino et al. 2003), which leads firms to reduce the rate of converting

³ On labour market liberalisation reforms at the margin, see, for example, Boeri and Garibaldi (2007) and Berton and Garibaldi (2012).

⁴ Using the European Community Household Panel (ECHP) data, Kahn (2012) shows that workers in temporary jobs make more effort to search for a new job than do those in permanent jobs. Moreover, Akgündüz and van Huizen (2015) demonstrate that, in dual labour markets, where the probability of quitting is higher for temporary workers, the incentive for firms to provide training is related to the quality of the job match.

temporary to permanent jobs. This entails, in turn, a reduction in the effort that temporary workers put into the job (as the likelihood of conversion is lower) and a reduction in the level of firm-provided training to temporary workers endogenously chosen by firms.⁵ Thus, our study also speaks to the growing literature on the (possibly) perverse economic effects associated with two-tier reforms of employment protection (Boeri and Garibaldi 2007).

Consistently with this strand of literature, we find some mild evidence that the Fornero reform reduced excess turnover and the use of temporary contracts within the firms above the cutoff.

We make two main contributions to the existing literature, which is discussed in more detail in Section 2. First, we provide new and clean evidence on the effects of EPL on training in Italy using a Difference in Regression Discontinuities Design (DRDD, hereafter) in a quasi-experimental setting, namely leveraging a labour market reform *that changed the level of EPL for larger firms over time* (i.e. the Fornero reform). This is an improvement over the existing literature (for instance, Bolli and Kemper 2017), since, especially in the Italian context, the DRDD allows to address some of the weaknesses of Regression Discontinuities Designs (RDDs), namely the existence of other labour market institutions also operating at the same margin of firm size as the EPL related to *Article 18* (e.g. in Italy, the right to create work councils within firms) and that might affect a firm's provision of training. Second, we explicitly show that for a country characterised by very stringent EPL for permanent workers and persistent dualism in the labour market, such as Italy, the excessive use of temporary contracts and the short duration of employment spells may be one key determinant of the incentives for firms to (not) provide training.

The rest of the paper is organised as follows. In Section 2 we review the related literature. In Section 3 we introduce the institutional framework and present our identification strategy. After discussing the data in Section 4, we comment on the validity of our research design, present our main results and some robustness checks in Section 5. Section 6 put forward a possible interpretation of our results. Finally, Section 7 summarises the main findings and draws conclusions.

⁵ Booth et al. (2002) show that temporary workers increase their effort when career prospects improve, i.e. when conversion rates into permanent positions are higher.

2 Past Literature

This paper is related to different strands of literature. First, it is related to theoretical studies dealing with incentives to invest in human capital by firms and workers. After seminal work by [Becker \(1964\)](#), more recent studies show that, in imperfectly competitive environments where labour market institutions are at work, firms (and workers) may have incentives to invest in general training. Papers by [Acemoglu \(1997\)](#) and [Acemoglu and Pischke \(1999b\)](#) show that, when labour market institutions, such as EPL, generate wage compression, firms may have more incentive to pay for training. This is because labour market imperfections, such as search frictions, information asymmetries and labour market institutions, determine the gap between a worker's marginal product and her wage, thus generating rents to be shared between workers and firms. Moreover, labour market imperfections reduce the outside option of workers so that wages increase less than productivity for trained workers. A necessary condition for firms to sponsor (general) training is that these rents are increasing in training ([Acemoglu and Pischke 1999b](#)). In a similar vein, [Wasmer \(2006\)](#) shows that in an environment with search frictions, when EPL is high and turnover is low, workers may have an incentive to invest more in specific skills than in general skills.⁶

Second, this paper is related to more recent empirical contributions on the relationship between employment protection and training.⁷ Simple economic reasoning suggests that, by increasing the time horizon in which the firm can reap the economic benefits of worker training, stricter EPL should increase firm-provided training. However, the empirical evidence does not always point in this direction. Using a large firm-level dataset across developing countries, [Almeida and Aterido \(2011\)](#) show that stricter enforcement of labour regulations is significantly associated with higher investments of firms in their employees' human capital, but that the magnitude of the association is very small. Similarly, [Pierre and Scarpetta \(2013\)](#) use cross-country harmonised survey data and find that higher EPL is associated with higher investment in training and more use of temporary contracts. They also find that EPL

⁶ See [Belot et al. \(2007\)](#), [Fella \(2005\)](#) and [Lechthaler \(2009\)](#) for other papers that look at the welfare-increasing effects of EPL and training in a search and matching environment.

⁷ The literature on the various effects of EPL is vast and cannot be reviewed here. See [Messina and Vallanti \(2007\)](#) for the effects of EPL on job flows, [Bassanini et al. \(2009\)](#) and [Bjuggren \(2018\)](#) for EPL and productivity, [Cingano et al. \(2010\)](#) and [Cingano et al. \(2016\)](#) for EPL and investment, [Schivardi and Torrini \(2008\)](#) for EPL and firms' propensity to grow, [Leonardi and Pica \(2013\)](#) for EPL and wages, [Kugler and Pica \(2008\)](#) for EPL and worker flows, and [Bottasso et al. \(2017\)](#) for EPL and firm dynamics.

has larger effects on small firms and in sectors characterised by greater job reallocation. Furthermore, studies exploiting within-country variation in levels of EPL do not find strong positive effects of EPL on training. For instance, [Picchio and van Ours \(2011\)](#) use Dutch data for manufacturing firms and find that higher labour market flexibility (i.e. lower EPL) marginally reduces firms' investment in training; however, this effect is rather small. A recent study by [Messe and Rouland \(2014\)](#) exploits a reform of EPL in France to identify, using a difference-in-differences approach combined with propensity scores methods, the effect of EPL on the incentive for firms to pay for training. They find that higher EPL (in the form of a tax on firings) had no effect on the training of older eligible workers, while it had a positive effect on workers just below the eligibility threshold. The authors interpret this finding as stressing the complementarity between training and firing decisions.

The paper most closely related to ours is [Bolli and Kemper \(2017\)](#), in which the authors use an RDD framework exploiting variation in firing regulations across size thresholds in Italy and Finland using data (in 2005 and 2010) to study the relationship between EPL and training provision. Their RDD results do not show any statistically significant effect of EPL on firm-provided training (measured as a dichotomous indicator of a firm's training provision, training hours and number of trained employees). We add to their analysis by leveraging quasi-experimental variation provided by the Fornero reform in a DRDD framework, which allows us to control for other labour market institutions that in Italy change discontinuously at the threshold, such as the *Cassa Integrazione Guadagni* scheme, a short-time work programme featuring a redundancy fund system, or the presence of worker councils in the firm.

Third, our paper is related to two recent studies that, in the cases of Spain and Italy, analyse the effects of EPL on training by type of contract (temporary vs permanent) and on the composition by type of contract of the labour force. [Cabrales et al. \(2017\)](#) use the PIAAC survey data and document a large gap in training provisions for temporary and permanent workers in Spain, which is characterised by persistent dualism in the labour market. In this environment, lower levels of EPL for temporary workers reduce expected job duration, increasing turnover for this group of workers, thereby reducing the incentive for firms to invest in training. On the contrary, permanent workers benefit from higher levels of training, as firms find it profitable to invest in their workers' skills. Hence, employment protection

is related to training depending on the composition of the labour force. In the case of Italy, [Hijzen et al. \(2017\)](#) exploit variation in EPL across firms of different sizes and find that higher levels of EPL result in excess worker turnover and that this effect is entirely due to the excessive use of temporary contracts. Moreover, they show that, by increasing excess worker turnover, stricter EPL also has negative effects on labour productivity.

3 Institutional Framework and Identification

3.1 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.⁸

Before the legislative changes that occurred in 2012 (Fornero Law) and 2014 (Job’s Act),⁹ 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.¹⁰ 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 instead receive 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](#)).¹¹

⁸ See [Cingano et al. \(2016\)](#) and [Hijzen et al. \(2017\)](#) for a brief overview of legislative changes that occurred between 1960 and 2012.

⁹ See ([Boeri and Garibaldi 2018](#)) for a description of the Job’s Act reform.

¹⁰ It is important to note that, according to Italian legislation, part-time workers count as less than one full-time employee in defining the firm size that is relevant for the application of EPL. By way of example, a firm with 16 employees, three of which have a 50% part-time contract, would be equivalent to a firm with 14.5 full time employees, and is therefore *de facto* below the 15-employee threshold. Similarly, only temporary employees with at least a 9-month contract should be considered as far as the definition of the threshold is concerned. This issue is further discussed in Section 5.1.

¹¹ Above the 15-employee threshold, employment protection is also greater in the case of collective dismissals.

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 average trial decisions of about 850 days over the period 2007-2010, with large variation across regions.¹² 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 of 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.¹³ 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.¹⁴ Moreover, the lack of a clear definition of unfair dismissal in 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 uncertainty, which further increased the costs associated with the stricter employment protection for firms above the threshold.

So far we have discussed only employment protection for open-ended contracts. However, as in other countries, such as Spain or France, the Italian labour market has in the past 15 years been characterised by a notable increase in the use of temporary and atypical labour contracts, following the liberalisation that started at the end of 1980s (in the case of temporary contracts), and at the end of the 1990s in the case of semi-autonomous atypical workers. It is, however, important to note that the degree of employment protection for temporary and atypical workers does not change discontinuously at the 15-employee threshold: indeed, it does not depend at all on firm size.¹⁵

¹² For instance, [Gianfreda and Vallanti \(2017\)](#) report an average length of labour trials of 313 days in Trento, in the north of Italy, versus 1397 days in Salerno, in the south of the country.

¹³ 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 exact formula.

¹⁴ 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.

¹⁵ See [Cahuc et al. \(2016\)](#) for a model explaining the spread of temporary jobs in dual labour markets.

Most importantly, there are regulations that change discontinuously at the 15-employee threshold, although they have been somehow neglected in previous studies, the most important being the right to form a worker council, which is granted to firms with more than 15 employees. Although previous empirical evidence discussed in [Schivardi and Torrini \(2008\)](#) suggests that the establishment of worker councils does not seem to change discontinuously at the 15-employee threshold, we believe that this feature might nevertheless constitute a possible threat to identifying the impact of stricter EPL in a conventional RDD design, as explained in Section [3.2](#).

In July 2012 a reform, known as the Fornero Law (Law 92/2012), significantly reduced firing costs for permanent workers in the case of firms with more than 15 employees. While we refer to [Berton et al. \(2017\)](#) for a detailed analysis of the novelties introduced by the 2012 reform, here we note that the Fornero Law limited the possibility for permanent workers in firms with more than 15 employees to opt between reinstatement and a monetary compensation in case of unfair dismissal to a set of well-defined cases;¹⁶ moreover, it substantially reduced the amount of the monetary compensation and eased the uncertainty surrounding the duration and costs of litigation which, as highlighted above, used to be fairly high, especially in some areas of the country.¹⁷ As explained in Section [3.2](#) we use the reduction in firing costs brought about by the Fornero Law in firms above the 15 employees threshold in order to identify the effect of EPL on firms' propensity to train workers in a DRDD framework.

3.2 Identification strategy

In this study we exploit the change in firing costs brought about by the Fornero reform in order to identify the impact of EPL on the firms' propensity to train workers. The idea is that the fall in firing costs of permanent workers experienced after 2012 by firms with more than 15 employees should reduce their propensity to rely on a sequence of temporary contracts, relatively to firms below the threshold. Because temporary workers are generally trained

¹⁶ For instance, the judge was granted the possibility to order a reinstatement only if she believed that the just cause of justified subjective reason invoked by the firm simply did not exist or the collective agreement applied by the firm foresaw a different punishment. Similarly, in the case of an economic lay-off, reinstatement was allowed only as long as no justified objective reasons actually existed.

¹⁷ In 2014 the Job's Act introduced by the Renzi government further reduced firing costs for firms above the 15 employees threshold, it strictly linked monetary compensations to seniority (thus limiting judges' discretion) and de facto eliminated the possibility for judges to order a reinstatement, whose consequences however largely follow outside the sample period considered in this study.

less, we expect that, following the reform, the propensity to train workers should increase in the firms above the 15 employees threshold. In other words, we can exploit the Fornero Law as a quasi-experiment to carry out a Difference in Regression Discontinuities Design: the causal effect of EPL on firm-provided training is identified by comparing the difference in the number of trained workers at the threshold, before and after the introduction of the Fornero Law.

The main identification assumption in a DRDD framework is that any unobservable variable impacting on training is either continuous at the threshold (as in RDD) or the effect at the discontinuity is constant over time (like in a conventional Difference-in-Differences approach).¹⁸ 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 Fornero Law. An important advantage of the DRDD approach over the RDD design used in other papers to study the Italian context (Bolli and Kemper 2017) is that the existence of possible confounding factors that change discontinuously at the threshold are controlled for, unlike in a conventional RDD framework. This is potentially important in our case, because as we explained in the previous section, worker councils could positively affect firm' training provision (Kennedy et al. 1994, Dustmann and Schönberg 2009, Stegmaier 2012) and the Italian legislation allows workers the right to form worker councils in firms with more than 15 employees. Hence, neglecting this confounders acting at the cutoff would potentially lead to an overestimate of the effect of EPL, when using an RDDs.¹⁹

The DRDD approach, can be described parametrically through the following equation, as in Cingano et al. (2016):

$$y_{it} = \alpha_0 + \alpha_1 post_t + \alpha_2 above_{it} + \alpha_3 above_{it} \times post_t + \alpha_4 f(E_{it} - 15) + \alpha_5 f(E_{it} - 15) \times above_{it} + \beta' X_{it} + \varepsilon_{it} \quad (1)$$

where i is the firm subscript, t the survey wave subscript ($t = 2010, 2015$) and y_{it} the number of trained workers. Our data refers to two cross sections that should be representative

¹⁸ See Grembi et al. (2016) for a detailed explanation of the identifying assumption underlying the DRDD

¹⁹ Namely, if the effect of EPL is positive, one would estimate a much higher effect of EPL, while if the effect is negative, one might estimate a smaller (in magnitude) negative or even a null effect of EPL.

of Italian firms in 2010 and 2015, which are described in the next section: it is important to note that we pool the two cross sections and that, therefore, the firms in the two waves are generally not the same, even if the survey we employ has a panel component, which we will be used in some robustness checks. The variable $post_t$ 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; $f(E_{it} - 15)$ is a polynomial in firm size normalized with respect to the cut-off size, whose effect is allowed to differ on each side of the cut-off and which represents the forcing variable; the coefficient of the interaction $above_{it} \times post_t$ is the parameter of interest, capturing the causal effect of relaxing EPL on firm-provided training in the case of firms just above the threshold; X_{it} is a vector of controls, comprising sector-by-year and region-by-year fixed effects.²⁰ Finally, ε_{it} is a firm error term.

Equation (1) is estimated with local linear regression techniques, i.e. we consider a linear polynomial and quite a narrow bandwidth around the threshold, namely 6-25 employees. However, the baseline specification is also estimated with different bandwidths, namely 11-20, 6-30 and 6-50 both with a linear and a quadratic polynomial specification. Moreover, as a robustness check, we follow [Grembi et al. \(2016\)](#) and we allow the polynomial to differ not only above and below the threshold, but also before and after the Fornero Law, which is clearly a more general, and considerably more demanding, specification than that in equation (1):

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

While we refer to Section 5.1 for a discussion of the validity of our research design, we anticipate in this Section that the equations (1) and (2) identify the causal effect of EPL on firm provided training as long as one can assume that the Fornero Law did not systematically change firms' propensities to grow above the threshold. We have tested this assumption using a modified version of the Schivardi and Torrini's test ([Schivardi and Torrini 2008](#)) in Section

²⁰ Industry-by-year fixed effects are included to capture time-varying industry specific differences in training provision, which may change over time. Similarly, including region-by-year fixed effects, we allow for time varying regional differences in training provision.

5.1) but because the empirical results are not always clear cut, namely we find some evidence of self-sorting at the 14-employee size, we also show our baseline regressions using a donut-hole approach, i.e. we drop firms with 14, 15 and 16 employees which may be affected by ‘manipulation’ (of firm size).

More generally, pooling the two cross sections like in a DID design, 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. This may fail if higher EPL above the cutoff were an impediment to firm growth before the Fornero Law. This is clearly related to the Schivardi and Torrini’s test that we have discussed above. In order to conduct additional robustness checks, we also run various regressions for our baseline specification by restricting the analysis to the panel component of the survey (although this leads to a loss of about two-thirds of the observations), and, as an additional check, by dropping those firms that have crossed (from above or from below) the 15-employee cutoff in the two waves, as in [Boeri and Garibaldi \(2018\)](#).²¹

Another econometric issue that is worth mentioning is that, in our survey, firm size is provided in discrete units, i.e. head counts. Composition of employment, in terms of part-time and full-time workers and type of contracts, is provided only for 2010 (2015), while information on training is provided only for 2009 (2014), i.e. the year before. For this reason, we cannot build a continuous measure of employment in 2009 and 2014 using proxy measures of the legal definition of firm size, i.e. the one relevant for the application of *Article 18*, as it is done in [Leonardi and Pica \(2013\)](#) or [Hijzen et al. \(2017\)](#). We deal with this issue in two ways. First, we drop firms with 16 employees (because they could be spuriously considered as above the threshold when in fact they are below it, e.g. if they have at least two part-time employees, which are counted as fractions of a full-time employee) in a donut-hole type of regression (see above). Moreover, we also check that our results are robust if we cluster standard errors, using the number of employees as the clustering variable, as suggested by [Lee and Card \(2008\)](#) when the researcher is forced to assume that the forcing variable is discrete.

A final point that is worth discussing at this stage is that the forcing variable, i.e. self-

²¹ Firm specific fixed effects allow to control for time invariant firm-level unobserved heterogeneity possibly correlated with treatment status.

reported firm size, is characterised by non-random heaping at multiples of 5, perhaps because of rounding by the individual that was interviewed in the firm. [Barreca et al. \(2016\)](#) present and discuss simulation evidence suggesting that neglecting non-random heaping can lead to biases and that omitting observations at data heaps should lead to unbiased estimates of the treatment effects for the ‘non heaped types’. Although in our preferred empirical specifications we prefer to use the whole available data, as DRDDs like RDDs are data-intensive, we also show that baseline results are robust to dropping observations with multiples of five in employment size.

4 Data

We use two waves (2010 and 2015) of the RIL Survey dataset (‘Rilevazione Longitudinale su Imprese e Lavoro’) provided by INAPP (National Institute for the Evaluation of Public Policies).²² Using the universe of Italian active firms provided by ISTAT (the Italian National Statistical Institute), called ASIA (*Archivio Statistico Imprese Attive*, Statistical Archive of Active Enterprises), the RIL sample is based on firm size and the sample is representative of the population of both the limited liability companies and partnerships in the private (non-agricultural) sectors. A panel version of the dataset is available for a limited number of firms.

The dataset contains information on indicators of firm size, performance, training and additional variables related to the system of industrial relations. An important feature of the data is that they contain detailed information on training activities, which is usually unavailable in administrative data on firms or workers. Further information is available on the presence of worker councils in the workplace and the level of bargaining and contractual labour agreements. The survey also contains information on the composition of the workforce in terms of skills and types of contracts for workers. On the firm side, although the dataset is quite rich in terms of variables related to firms activities, such as their export, innovation or offshoring activities, only limited information is available concerning balance sheet data.²³

²² The INAPP (formerly ISFOL) 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.

²³ [Devicienti et al. \(2017\)](#) use ISFOL-RIL data as a primary source of information to study the relationship between unions and temporary contracts.

In what follows, we describe our sample selection procedure. We begin with 24,459 observations for the year 2010 and 30,091 for the year 2015. We drop firms that have below zero (or an abnormal number of) employees in 2010 (196 observations) and in 2015 (83 observations). After the above selections we end up with 24,263 and 30,008 observations for the two years. The whole sample is 54,271 observations, for 10,214 firms we have two observations (panel), while the rest is a repeated cross section. In the econometric analysis, we restrict the sample to firms with size comprised in the 5-26 employees interval; moreover, we trim the data by dropping from the analysis those firms that experienced an year-on-year growth rate in the number of employees larger (smaller) than the 95 (5) percentile, and we restrict the sample to still active firms. In Table 1, we report descriptive statistics for the sample used in the baseline regressions in Table 3.

[Table 1 about here]

5 Results

5.1 Validity of the difference in regression discontinuities design

In this section we investigate the existence in our data of a systematic self-sorting of firms at or below the 15-employee threshold before and after the Fornero reform.²⁴ We do this using the test proposed by [Schivardi and Torrini \(2008\)](#) and later used in [Leonardi and Pica \(2013\)](#), [Hijzen et al. \(2017\)](#), among the others. In practice, the test is based on the existence of systematic differentials in the firms' likelihoods of growing in size just below the 15 employees threshold. We carry out the test by estimating the following equation using a linear probability model (LPM)

$$Pr(E_{it} > E_{it-1}) = \alpha + \sum_{j=1}^n \beta_{jl} E_{it-1}^j + \sum_{k=13}^{15} \gamma_k D_{it-1}^k \times post_t + \beta_x \mathbf{X}_{it} + v_{it} \quad (3)$$

with

$$D_{it-1}^k = \mathbb{1}[E_{it-1} = k] \text{ for } k=13, 14, 15. \quad (4)$$

²⁴ Previous studies have in general not found clear evidence of self-sorting of firms ([Schivardi and Torrini 2008](#), [Leonardi and Pica 2013](#), [Hijzen et al. 2017](#)). However, [Boeri and Garibaldi \(2018\)](#) in their recent evaluation of the Job's Act of 2015 report a significant increase in the firms' propensity to grow above the 15-employee threshold after the introduction of the Job's Act.

E_{it-1} and E_{it} are firm size in year $t - 1$ (2014 and 2009) and t (2010 and 2015), respectively; D_{it-1}^k s are a set of bin dummies, with bin size equal to 1 (namely for sizes 13, 14 and 15 employees) whose effect is allowed to vary after the Fornero Law; E_{it-1}^j are the terms of a polynomial in firm size (first and second order); \mathbf{X}_{it} is a vector of region-by-year and sector-by-year fixed effects and v_{it} is a firm-level error term. The polynomial in firm size captures parametrically the underlying relationship between firm size and the probability of employment growth in the absence of employment protection, while the bin dummies can be interpreted as the threshold effect of EPL on firms' employment growth at 13, 14 and 15 employees, respectively: in particular, the interaction of the three bin dummies with the *post* dummy allow the threshold effect to vary after the Fornero reform.

In columns (1) and (2) of Table 2 we report the OLS estimates of the linear probability model with a linear and a quadratic polynomial, respectively. Empirical results suggest the existence of a lower probability of firms to grow (about 9 percentage points) at 14 employees, which is significantly reduced after the Fornero reform: the 14 employees \times post interaction term is positive and large in magnitude, but very imprecisely estimated. The result does not change if we consider a cubic polynomial (not shown in the table) or if we allow the polynomial to differ on both sides of the threshold. These findings therefore suggest the existence of some self-selection below the threshold, namely at 14 employees, which is reduced by the Fornero Law. In columns (3)-(4) we repeat the Schivardi and Torrini's test on the panel component of the survey which allows us to control for firm fixed effects and we now find much less clear evidence of firms' self selection below the threshold, but also of its reduction after the Fornero reform: indeed, before the Fornero Law there is again some evidence of a lower propensity to grow at 14 employees, which is however statistically insignificant, particularly in column (4) where we report the results with a second degree polynomial in employment; moreover, there is no evidence that the propensity to grow was altered by the Fornero Law, a crucial assumption of the DRDD.

[Table 2 about here]

The Schivardi and Torrini's test therefore are somewhat inconclusive: while in the pooled model we can not rule out the possibility that some manipulation of the running variable occurs in our sample, the fixed effects results in the panel case point towards rejecting a

change in the degree of manipulation after the Fornero reform. In the results section therefore we show our main results also for the panel specification with firm fixed effects; moreover, we report also results of a donut hole specification whereby we drop firms with 14, 15 and 16 employees.²⁵

5.2 Main results

This section reports our baseline estimates of the effect of EPL on firm-provided training, using the number of trained workers as outcome variable.²⁶ In the first four columns of Table 3 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). We also include (exclude) sector and region FEs (to which we will refer as ‘firm controls’ for brevity) whose effect is allowed to vary before and after the Fornero reform. The estimates in column (1) show that, at the 15 employees threshold, and following the Fornero reform, there has been an average increase of 1.72 trained workers, significant at the 1 per cent level. The magnitude of the discontinuity can be also appreciated from Figure 1, which shows no significant jump in the number of trained workers before the reform, although smaller firms seemed to train workers slightly more, and a significant jump in favour of larger firms after the Fornero Law. The estimates are not sensitive to the inclusion of region and sector fixed effects, as shown in column (2).²⁷

In the remaining columns, we estimate the model in equation (1) allowing for a different bandwidth around the 15 employees threshold. The results reported across columns confirm that the $post \times above$ coefficient is always positive and statistically significant at conventional

²⁵ 14 and 15 are dropped because of possible manipulation and 16 since it might be below threshold. As a possible additional check for manipulation, one could report balancing tests of some firm characteristics around the cut-off before and after the Fornero reform. Unfortunately, many of these covariates are not predetermined but may instead act as mediating factors for the effect of EPL. Thus, checking for balancing will not help judge the validity of our DRDD framework. To take a few examples, firm characteristics affected by EPL and which also interact with worker training may include investments in physical capital (Cingano et al. 2016; 2010), access to credit (Cingano et al. 2016), innovation performance (Koeniger 2005), use of temporary contracts (Hijzen et al. 2017), wages (Leonardi and Pica 2013) and workers’ mismatch (Berton et al. 2017).

²⁶ 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. Indeed, the results can be interpreted as a per-worker effect given that we control for a polynomial of firm employment. Nevertheless, results are qualitatively similar if we consider the share of trained workers as the outcome variable.

²⁷ In Table A1 in Appendix A, we report results 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 reported in Table 3, namely 1.72 and 1.54 in the baseline specifications excluding and including sector and region FEs, respectively.

levels, with an order of magnitude that varies across columns, ranging from 1.9 in column (3) for the bandwidth 11 to 20 employees to about 3 in column (7) for the largest bandwidth (6–50 employees). Again, we detect very minor differences depending on the inclusion or not of the firm controls. Empirical results are also broadly confirmed if we consider a quadratic polynomial specification, which is reassuring especially in the case of the 6–50 bandwidth (Table A1).

Interestingly, the *above* dummy is negative in all specifications, and statistically significant in the 6–30 and 6–50 bandwidth cases: this means that the number of trained workers was lower above the threshold in 2010. It is possible that in a strongly dual labour market firms, in order to escape the more stringent firing costs on open-ended contracts above the threshold, were relying on a sequence of temporary contracts. However, temporary workers tend to be trained less. The Fornero Law, by reducing the wedge in the degree of EPL enjoyed by permanent and temporary workers in the case of firms above the threshold, might have induced firms to hire more permanent employees, and therefore to increase training, relative to firms with less than 15 employees.

Going back to the magnitude of the *post* \times *above* coefficient, if we focus on our preferred specification, namely that with a 6–25 bandwidth, a linear polynomial and firm-level controls, our results suggest that firms affected by the Fornero Law might have increased training by a magnitude of about 1.5 additional trained workers. Considering that before the reform the average number of trained workers in firms with 15 employees was about 3.1, our estimates suggest that the Fornero Law might have increase trained workers by about 50% at the threshold.

[Table 3 about here]

[Figure 1 about here]

5.3 Robustness checks

We conduct several robustness checks in Table 4. First, because there is evidence of heapings in the forcing variable at multiples of 5 employees, we follow Barreca et al. (2016) and drop firms with 10, 15, 20 and 25 employees from the estimation of equation (1). Reassuringly, results reported in columns (1) and (2) of Table 4 and those in columns (1) and (2) of Table 3

are very similar. Second, in columns (3) and (4) we run a series of donut-hole regressions²⁸ in order to deal with possible firms' self-sorting just below the threshold and to take into account the possibility that firms with 16 employees are in fact below the threshold because of the presence of part-time employees: again results are broadly unchanged.²⁹

Third, in columns from (5) to (8), we have run a placebo analysis, by assuming that the threshold was at ten (twenty), rather than at 15, employees. In these cases, the estimates of the interaction term are still positive, but much smaller (in the case of the fake 10 employees) or largely insignificant (in the case of 20 employees), as one should expect with an incorrectly specified research design.

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 vs. after the reform and not only above and below the threshold, i.e. we estimate different versions of equation (2) above. In columns (9) and (10) (linear polynomial), we confirm the magnitude of the effect, which is equal to 1.63 and 1.44, depending on the inclusion or not of the firm controls. When we consider a polynomial of second order (columns 11 and 12), the magnitude is slightly larger than that reported in previous columns.

As we have already mentioned, the use of repeated cross-sections in a DID-like 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 Fornero Law might have altered the incentives for firms to self select below the threshold. Although, as mentioned above, by running a set of [Schivardi and Torrini](#)'s tests, we do not find clear evidence that the reform increased the propensity for firms to cross the 15 employees threshold (i.e. to grow in size), especially when we control for firm fixed effects, in [Table 5](#), as a further robustness check (in addition to the donut-hole regressions), we investigate this potential bias by restricting the estimation sample to the panel component of the dataset, even if this reduces the sample size and the precision of the estimates.

[Table 4 about here]

²⁸ In regressions not reported but available from the authors upon request, we have re-estimated all regressions in [Tables 3, 4 and 5](#) and results are generally consistent.

²⁹ Because the forcing variable is potentially continuous (i.e. the legal definition of firm size, for which part-time workers count as fractions of full-time employees) but data limitations force us to treat it as it were discrete, we also re-estimate equation (1) by clustering standard errors, using the number of employees as the clustering variable, as suggested by [Lee and Card \(2008\)](#). Reassuringly, we can reject the null hypothesis that the *post* × *above* coefficient is equal to 0 at the 1% level of confidence.

In Table 5 we report 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 treatment status and we cluster standard errors at the firm level. In columns (1) and (2), where we allow for different polynomials only below and above the 15-employee threshold, we find a positive and statistically significant effect of the $post \times above$ interaction, but with a lower magnitude compared to the cross-sectional sample, of about 1 additional trained worker. By contrast, in the more general specification reported in columns (7) and (8), where we estimate equation (2), the coefficient of the $post \times above$ interaction increases to about 1.9, statistically significant at 10 per cent. Finally, in columns (3) to (6) we conduct similar robustness checks to those conducted in Table 4, i.e., we take into account possible data heapings at multiples of 5 for the forcing variable and we run donut-hole regressions. Again, our main results are confirmed.

In regressions not reported in the text, but available upon request, we exclude those firms that have crossed the threshold between 2010 and 2015 in either direction, so that we can keep the sample unaltered before and after the reform.³⁰ When we do that, our empirical results suggest that the Fornero reform might have determined an increase of about one additional trained worker at the threshold.

[Table 5 about here]

6 Potential mechanism: turnover and temporary contracts

Some recent literature has suggested that, in the presence of dual labour markets, firms may try to avoid the costs associated with stricter EPL for regular workers by making greater use of temporary contracts. Moreover, when firing costs for regular workers are high and there are rules forbidding the renewal of temporary contracts, firms might be reluctant to convert temporary jobs into permanent ones. This might, as a result, increase the incentives for firms to rely on temporary jobs in sequence (Cahuc and Postel-Vinay 2002), thereby increasing (excess) worker turnover. Cahuc et al. (2016) present a search and matching model featuring regular jobs (with possibly stricter EPL) and temporary contracts (which can be terminated

³⁰ We drop about 600 observations which amount to roughly 10 per cent of our estimation sample.

at zero cost when they expire, but which cannot be terminated before their expiry date): they show that, in their model, stricter EPL for regular workers leads firms to employ the latter only to exploit production opportunities that are expected to last for a very long time. This, in turn, can lead to an important substitution of permanent jobs with temporary ones, leading to a ‘strong excess of labour turnover’.

This theoretical prediction also seems to be borne out by the data. Indeed, [Hijzen et al. \(2017\)](#) show that, in the case of Italy, the stricter EPL above the 15-employee threshold is associated with higher rates of excess worker turnover, defined as the excess of worker turnover over the absolute value of net employment change, the latter in turn measured as the difference between hiring and separation rates. Interestingly, the authors also found that this effect is entirely explained by greater use of temporary workers above the threshold. Similar evidence can be found in [Centeno and Novo \(2012\)](#), who report an increase in the proportion of fixed-term contracts following a Portuguese reform that tightened EPL for regular workers in the case of firms with 11 to 20 workers.

If the above evidence is correct, then, in light of the widespread evidence that temporary workers receive less training ([Arulampalam and Booth 1998](#), [Booth et al. 2002](#), [Arulampalam et al. 2004](#), [Albert et al. 2005](#)), one could argue that stricter EPL might cause lower training by firms, with the mediating factors being excess use of temporary contracts and turnover. Moreover, one might also expect that the relaxation of EPL for permanent employees above the threshold by the Fornero Law should be associated to a decrease in excess worker turnover and in the share of temporary workers at the threshold, because of a reduction in the wedge between firing costs for permanent versus temporary employees at the cutoff.

Following [Hijzen et al. \(2017\)](#), we measure excess worker turnover as $EWT = 2 \cdot \min(H, S) / E$, where H and S are the number of hirings and separations, respectively, and E is average firm employment.³¹

In order to explore the effect of EPL on excess worker turnover and on the share of temporary workers, as in [Hijzen et al. \(2017\)](#), in Table 6 we report estimates of equation (1) and (2)

³¹ It can easily be shown that this formula is equivalent to the definition of excess worker reallocation as the difference between worker turnover and the absolute value of net employment change: it therefore represents worker flows in excess of job flows, and it is sometimes referred to as churning ([Burgess et al. 2000](#)).

with polynomials in firm employment of first and second degrees.³² The results displayed in columns (1) to (4) point somehow towards a negative effect of the reform on excess worker turnover for firms above the threshold, even if the effect is statistically significant only in the case of the more general specification of equation (2) allowing for different polynomials above-below and before-after. Similarly, in columns (5) to (8), we also show that, above the threshold, after the Fornero reform, the proportion of workers with a fixed-term contract is reduced in the case of the specification of equation (2), confirming the results of [Centeno and Novo \(2012\)](#) for Portugal.

Empirical results in [Table 6](#) also provide some weak evidence that, before the Fornero Law, both the excess worker turnover and the share of temporary workers were higher above the threshold, as reported in [Hijzen et al. \(2017\)](#) for Italy before the Fornero reform: in other words, these results seem to be consistent with the idea that too large a gap between the firing costs of permanent versus temporary employees might lead firms to substitute temporary for permanent employees. However, when this gap is reduced, as in the case of Italy after the Fornero reform the ‘perverse effects’ (in terms of training) of a dual labour market (e.g. large worker turnover and excess reliance on temporary positions) tend to disappear, as empirical results in [Table 6](#) somehow suggests.

[[Table 6](#) about here]

7 Conclusion

In this paper we provide new clean evidence on the causal effect of EPL on firm-provided training from a labour market reform, the Fornero Law, which was introduced in Italy in 2012. Using two waves of a representative survey of Italian firms, we leverage quasi-experimental variation in EPL using a Difference in Regression Discontinuities Design. Indeed, the Fornero Law decreased the level of EPL only for firms above the 15-employee cutoff, which before the reform were subjected to substantial firing restrictions because of article 18 of the Workers’ Statute (Law No. 300 of May 20, 1970).

Our preferred DRDD estimates suggest that the Fornero reform led to an increase in the number of trained workers of about 1.5 units at the cutoff, i.e. an approximately 50 per-

³² The inclusion of industry and region fixed effects does not qualitatively change the results.

cent increase. Results are robust to an extensive set of sensitivity checks, including placebo analyses, donut-hole regressions, changes in the degree of the polynomial of firm size and bandwidths.

Our results also suggest that the negative effect of stricter EPL above the 15-employee threshold before the reform may be partly mediated by the higher excess worker turnover. Indeed, and confirming the results of [Hijzen et al. \(2017\)](#) on a different dataset, we provide evidence that firms above the threshold were characterised by a higher excess worker turnover and more use of temporary workers before the reform, as theoretically predicted by [Cahuc et al. \(2016\)](#) for economies with a two-tier labour market, and that this gap reduced after the introduction of the Fornero Law. In other words, in labour markets that have a significant asymmetry in the degree of employment protection enjoyed by permanent and temporary workers, there is an incentive for firms to substitute temporary for permanent workers by using a sequence of temporary contracts ([Cahuc et al. 2016](#)), thereby creating excess worker turnover. However, because temporary workers generally receive less training, stricter EPL for permanent workers might reduce incentives for firms to provide training. The Fornero reform, by reducing EPL for permanent employees above the 15 employees threshold, might have reduced the incentives for large firms above the threshold to rely on temporary workers, indirectly increasing the propensity to train workers.

This finding could provide an additional explanation for why two-tier reforms can be associated with a drop in labour productivity: indeed, [Boeri and Garibaldi \(2007\)](#) explain the reduction in labour productivity following a two-tier labour market liberalisation as the consequence of a transitory increase in temporary employment coupled with the decreasing marginal returns associated with a downward-sloping labour demand.³³ Our empirical findings, in turn, suggest that, by favouring the growth of temporary workers, a large gap in EPL between permanent and temporary workers might lead to less firm-provided training and, possibly, to lower labour productivity, as found by [Hijzen et al. \(2017\)](#). This, in turn, may have played a role in explaining the dismal productivity performance of the Italian economy since the second half of the 1990s.

Acknowledgments. This is a substantially revised version of our IZA discussion pa-

³³ See also [Cahuc et al. \(2016\)](#).

per 11339 (February 2018), titled “Employment Protection, Temporary Contracts and Firm-Provided Training: Evidence from Italy” and it also incorporates results from a mimeo titled “Does Reducing Employment Protection Affect Worker Training? New Firm-Level Evidence from a Labour Market Reform” circulated at various conferences after July 2018. 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 Fabio Berton, Diogo Britto, Lorenzo Cappellari, Guido De Blasio, Francesco Devicienti, Carlo Devillanova, Juan Dolado, Marco Leonardi, Sandra McNally, Lia Pacelli, Matteo Sandi, Vincenzo Scrutinio, Daniela Sonedda, by participants to the workshops ‘Rigorous impact evaluation in Europe’ (Turin) and ‘The Effects of Employment Protection and Collective Bargaining on Workers and Firms’ (Cagliari), in seminars at the Joint Research Centre (Ispra), MILLS (Milan Labor Lunch Seminar, University of Milan), Centre for Vocational Education Research (London School of Economics) and University of Siena, at conferences in Berlin (IZA World Labor Conference and COMPIE), Cologne (EEA), Lyon (EALE), Ancona (AIEL), Bologna (SIE) are gratefully acknowledged. Part of this work was carried out while Giovanni Sulis was visiting the University of New South Wales, Sydney: we thank that institution for its hospitality. 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 information and views set out in this paper are those of the authors and do not reflect the official opinion of the European Union. Neither the European Union institutions and bodies nor any person acting on their behalf may be held responsible for the use which may be made of the information contained herein. The usual disclaimer applies.

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Tables and Figures

Table 1: Descriptive statistics

Over	Mean	Std. Err.	Min.	Max.
employees				
2010	10.99	4.51	6	25
2015	10.80	4.59	6	25
trained workers				
2010	2.26	4.28	0	25
2015	3.54	5.06	0	25
share temporary workers				
2010	0.11	0.18	0	1
2015	0.09	0.20	0	1
excess worker turnover				
2010	0.51	1.22	0.07	87.17
2015	0.44	0.78	0.06	46.67

Note. Descriptive use sample weights and are calculated on the sample used in regression in column 1, Table 3. Employees is the total number of employees in 2009. 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. 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. Excess worker reallocation is calculated at the firm level following [Hijzen et al. \(2017\)](#), as $EWT = 2 \cdot \min(H, S) / E$, where H and S are the number of hiring and separations, respectively, and E is average firm employment. Share of temporary workers is the share of fixed term contracts.

Table 2: Probability of growing: [Schivardi and Torrini \(2008\)](#) tests

	(1)	(2)	(3)	(4)
13 employees	-0.000593 (0.0318)	-0.0147 (0.0343)	-0.0105 (0.0636)	0.0103 (0.0642)
14 employees	-0.0908*** (0.0275)	-0.105*** (0.0306)	-0.0883 (0.0567)	-0.0661 (0.0567)
15 employees	-0.0425 (0.0345)	-0.0561 (0.0370)	-0.0653 (0.0606)	-0.0368 (0.0623)
13 employees × post	-0.0242 (0.0541)	-0.0250 (0.0542)	-0.0638 (0.0792)	-0.0582 (0.0795)
14 employees × post	0.192 (0.123)	0.191 (0.123)	-0.0143 (0.0741)	-0.00755 (0.0741)
15 employees × post	-0.0268 (0.0466)	-0.0276 (0.0466)	0.00364 (0.0742)	0.00409 (0.0745)
Bandwidth	(6-25)	(6-25)	(6-25)	(6-25)
Polynomial	Linear	Quadratic	Linear	Quadratic
Sec. × year f.e.	No	No	No	No
Reg. × year f.e.	No	No	No	No
Firm f.e.	No	No	Yes	Yes
Observations	16,532	16,532	5,794	5,794
R-squared	0.010	0.011	0.658	0.659

Note. Robust standard errors in parentheses, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Columns (1)-(2) report the results of a specification similar to [Schivardi and Torrini \(2008\)](#) where the dependent variable is the probability of year to year employment growth. The models include a polynomial in firm size and indicators for 13, 14 and 15 employees, columns (3)-(4) report the results using the panel component of the data. The estimation sample only includes firms between 6 and 25 employees. We exclude firms at the 5th and 95th percentile of the distribution of growth of employment (below and above 50%).

Table 3: Baseline results

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
post	1.084*** (0.137)	-2.416*** (0.611)	1.291*** (0.303)	-3.287*** (1.107)	1.084*** (0.137)	-2.611*** (0.642)	1.084*** (0.137)	-2.635*** (0.690)
above	-0.407 (0.382)	-0.487 (0.382)	-0.501 (0.575)	-0.718 (0.556)	-0.848** (0.358)	-0.857** (0.349)	-1.966*** (0.412)	-1.925*** (0.394)
post × above	1.722*** (0.422)	1.544*** (0.402)	1.946*** (0.594)	1.642*** (0.535)	2.049*** (0.383)	1.887*** (0.368)	3.075*** (0.532)	2.857*** (0.495)
Bandwidth	(6-25)	(6-25)	(11-20)	(11-20)	(6-30)	(6-30)	(6-50)	(6-50)
Polynomial	Linear	Linear	Linear	Linear	Linear	Linear	Linear	Linear
Pol. inter.	above	above	above	above	above	above	above	above
Sec. × year f.e.	No	Yes	No	Yes	No	Yes	No	Yes
Reg. × year f.e.	No	Yes	No	Yes	No	Yes	No	Yes
Observations	16,486	16,462	7,851	7,836	17,826	17,797	21,266	21,229
R-squared	0.110	0.154	0.058	0.119	0.132	0.171	0.235	0.265

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). We exclude firms at the 5th and 95th percentile of the distribution of growth of employment (below and above 50%).

Table 4: Robustness: heaping, donut, fake thresholds, different interactions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Heaping		Donut		Fake 10		Fake 20		Interaction post		Interaction post	
post	1.004*** (0.139)	-2.646*** (0.653)	1.055*** (0.133)	-1.843*** (0.544)	0.983*** (0.134)	-2.657*** (0.623)	1.302*** (0.135)	-2.316*** (0.611)	1.503*** (0.390)	-1.886*** (0.682)	1.508*** (0.547)	-1.893*** (0.758)
above	0.0336 (0.421)	-0.101 (0.411)	-0.240 (0.529)	-0.134 (0.514)	-0.702 (0.493)	-0.714* (0.395)	-0.867 (1.722)	-0.692 (1.698)	-0.356 (0.478)	-0.430 (0.491)	-0.359 (0.732)	-0.657 (0.767)
post×above	1.384*** (0.474)	1.262*** (0.450)	1.566*** (0.469)	1.351*** (0.446)	0.810*** (0.280)	0.815*** (0.248)	0.668 (0.629)	0.490 (0.611)	1.631*** (0.801)	1.437* (0.764)	2.096* (1.193)	2.064* (1.143)
Bandwidth	(6-25)	(6-25)	(6-25)	(6-25)	(6-25)	(6-25)	(6-25)	(6-25)	(6-25)	(6-25)	(6-25)	(6-25)
Polynomial	Linear	Linear	Quadratic	Quadratic								
Pol. inter.	above	above	above	above	above	above	above	above	both	both	both	both
Sec.×year f.e.	No	Yes										
Reg.×year f.e.	No	Yes										
Observations	13,113	13,095	13,761	13,746	16,486	16,462	16,486	16,462	16,486	16,462	16,486	16,462
R-squared	0.109	0.151	0.116	0.159	0.108	0.153	0.106	0.151	0.111	0.155	0.111	0.155

Note. Robust standard errors in parentheses, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. In columns (1) and (2) we drop multiples of 5 employees (heaping), in columns (3) and (4) we drop firms with 14, 15, 16 employees (donut), fake threshold in columns (5) and (6) is set at 10 employees, in columns (7) and (8) is set at 20 employees. Polynomials in employment have been interacted with the dummy *above* (15-employee threshold) and the dummy *post* (period affected by Fornero reform), in the Table these interactions are referred as “both”, see columns (9) to (12). We exclude firms at the 5th and 95th percentile of the distribution of growth of employment (below and above 50%).

Table 5: Panel evidence

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Baseline panel		Heaping		Donut		Interaction post	
post	1.360*** (0.125)	1.574 (1.097)	1.217*** (0.135)	1.521 (3.419)	1.231*** (0.126)	1.060 (1.048)	2.250*** (0.363)	2.164* (1.140)
above	-0.465 (0.692)	-0.398 (0.691)	-1.301* (0.774)	-1.103 (0.760)	-1.359 (1.177)	-1.044 (1.190)	-0.916 (0.827)	-0.964 (0.825)
post×above	1.027** (0.500)	0.829* (0.494)	1.424** (0.587)	1.249** (0.583)	1.163* (0.615)	0.994 (0.609)	1.858* (1.002)	1.901* (0.986)
Bandwidth	(6-25)	(6-25)	(6-25)	(6-25)	(6-25)	(6-25)	(6-25)	(6-25)
Polynomial	Linear	Linear	Linear	Linear	Linear	Linear	Linear	Linear
Pol. inter.	above	above	above	above	above	above	both	both
Sec.×year f.e.	No	Yes	No	Yes	No	Yes	No	Yes
Reg.×year f.e.	No	Yes	No	Yes	No	Yes	No	Yes
Firm f.e.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	5,754	5,732	3,778	3,766	4,232	4,220	5,754	5,732
R-squared	0.754	0.764	0.767	0.777	0.760	0.771	0.756	0.766

Note. Clustered standard errors at the firm level. In columns (3) and (4) we drop multiples of 5 employees (heaping), in columns (5) and (6) we drop firms with 14, 15, 16 employees (donut). Polynomials in employment have been interacted with the dummy *above* (15-employee threshold) and the dummy *post* (period affected by Fornero reform), in the Table these interactions are referred as “both”, see columns (7) to (8). We exclude firms at the 5th and 95th percentile of the distribution of growth of employment (below and above 50%).

Table 6: Excess worker turnover and share of temporary workers

dependent variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	excess worker turnover				share of temporary contracts			
post	0.347*** (0.0770)	0.347*** (0.0751)	0.391*** (0.0924)	0.486*** (0.0920)	0.158*** (0.0383)	0.158*** (0.0380)	0.194*** (0.0490)	0.230*** (0.0549)
above	0.0687** (0.0268)	-0.0174 (0.0375)	0.0983*** (0.0316)	0.0249 (0.0513)	0.0273** (0.0131)	-0.0230 (0.0258)	0.0525*** (0.0147)	0.0313 (0.0230)
post×above	-0.0426 (0.0263)	-0.0410 (0.0262)	-0.104** (0.0486)	-0.135* (0.0755)	0.00685 (0.0200)	0.00674 (0.0196)	-0.0443 (0.0340)	-0.107*** (0.0410)
Bandwidth	(6-25)	(6-25)	(6-25)	(6-25)	(6-25)	(6-25)	(6-25)	(6-25)
Polynomial	Linear	Quadratic	Linear	Quadratic	Linear	Quadratic	Linear	Quadratic
Pol. inter.	above	above	both	both	above	above	both	both
Sec.×year f.e.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Reg.×year f.e.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	10,724	10,724	10,724	10,724	16,508	16,508	16,508	16,508
R-squared	0.197	0.202	0.197	0.205	0.166	0.168	0.168	0.171

Note. Robust standard errors in parentheses, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Excess worker turnover is calculated at the firm level following [Hijzen et al. \(2017\)](#), as $EWT = 2 \cdot \min(H, S) / E$, where H and S are the number of hiring and separations, respectively, and E is average firm employment. Share of temporary workers is the share of fixed term contracts. Polynomials in employment have been interacted with the dummy *above* (15-employee threshold) and the dummy *post* (period affected by Fornero reform), in the Table these interactions are referred as “both”. We exclude firms at the 5th and 95th percentile of the distribution of growth of employment (below and above 50%).

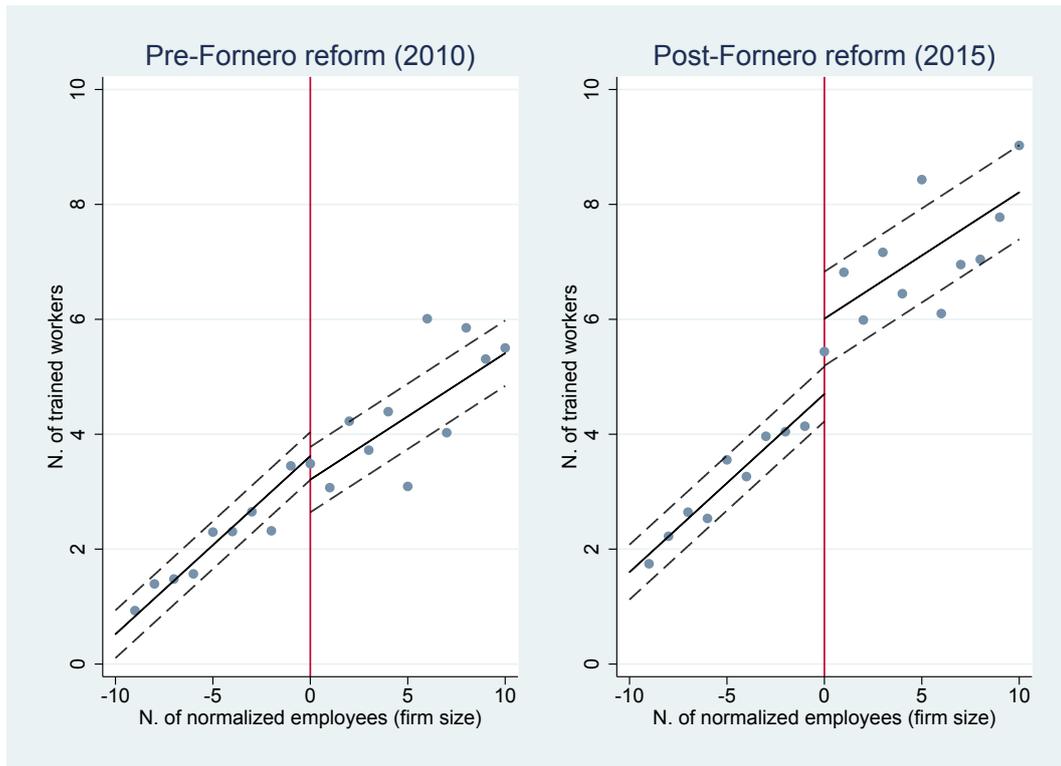
A Additional results

Table A1: Baseline results: quadratic polynomial

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
post	1.083*** (0.136)	-2.419*** (0.610)	1.284*** (0.302)	-3.311*** (1.106)	1.083*** (0.136)	-2.598*** (0.640)	1.083*** (0.136)	-2.607*** (0.693)
above	-0.196 (0.628)	-0.426 (0.619)	-0.680 (1.079)	-0.928 (1.032)	-0.0720 (0.494)	-0.250 (0.487)	-1.221** (0.604)	-1.258** (0.554)
post×above	1.726*** (0.421)	1.547*** (0.401)	1.952*** (0.589)	1.649*** (0.531)	2.063*** (0.382)	1.900*** (0.368)	3.065*** (0.534)	2.848*** (0.499)
Bandwidth	(6-25)	(6-25)	(11-20)	(11-20)	(6-30)	(6-30)	(6-50)	(6-50)
Polynomial	Quadratic	Quadratic	Quadratic	Quadratic	Quadratic	Quadratic	Quadratic	Quadratic
Pol. inter.	above	above	above	above	above	above	above	above
Sec.×year f.e.	No	Yes	No	Yes	No	Yes	No	Yes
Reg.×year f.e.	No	Yes	No	Yes	No	Yes	No	Yes
Observations	16,486	16,462	7,851	7,836	17,826	17,797	21,266	21,229
R-squared	0.110	0.154	0.058	0.119	0.133	0.171	0.236	0.266

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). We exclude firms at the 5th and 95th percentile of the distribution of growth of employment (below and above 50%).

Figure 1: Firm size and training provision before and after the Fornero reform



Note. The figure reports a scatter plot for the average number of employed workers by one employee-bins of firm size (computed using survey weights) before and after the Fornero reform and the fitted (solid) line of a regression of number of trained workers on normalized employment (see column (1) of Table 3). Normalized employment is reported on the horizontal axis so as '0' corresponds to the cut-off (i.e. firm's employment level equal to 15). The scatter plot is reported for the bandwidth 6–25 employees of firm size (i.e. normalized size between –10 and 10). Dashed lines are 95% confidence intervals.