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

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Abstract

In 2012 a labour market reform, known as Fornero Law, substantially reduced firing restrictions for firms with more than 15 employees in Italy. The results from a difference in regression discontinuities design that compares firms below versus those above the cut-off before and after the reform demonstrate that, after the Fornero Law, the number of trained workers increased in firms just above the threshold, with an order of magnitude of approximately 1.5 additional workers in our preferred empirical specification. We show that this effect might be partly explained by the reduction in worker turnover and a lower use of temporary contracts at the threshold after the reform. Our study highlights the counter-intuitive and potentially adverse effects of employment protection legislation (EPL) on training in dual labour markets due 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 and productivity and, consequently, are paid higher wages, while firms enjoy returns to training in the form of higher worker 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 an increase in the number of empirical studies testing competitive and non-competitive theories of training, including possibly leveraging policy-induced variation in market imperfections. In this paper, we follow their suggestion and shed light on a relatively under-explored source of labour market imperfections, namely employment protection legislation (EPL). Indeed, the latter 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 includes size-contingent firing restrictions, according to which firing costs increase sharply above the 15-employee threshold (article 18 of the Workers' Statute, Law n. 300 of 20 May 1970; *Article 18*, hereafter). In Section 3, we discuss why these policy-induced differences in employment protection ultimately substantially differentiate firing costs according to firm size. These restrictions were greatly reduced in 2012 by a labour market reform known as the Fornero Law (Law n. 92 of 28 June 2012). Combining the different levels of EPL below and above the 15-employee cut-off with the EPL changes introduced in 2012 gives us a unique opportunity to obtain 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 between

[1] Mincer (1962) estimates that approximately 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.

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 suggest that the Fornero reform, by reducing EPL for large firms (i.e., firms above the 15-employee cut-off), increased the average number of trained workers by approximately 1.5 individuals. This is not a negligible effect and corresponds to an approximately 50 per cent increase in the number of trained workers *at the cut-off* firm size, which prior to the reform was approximately 3.1 trained workers. 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 cut-off, changes in the bandwidth, changes in the order of the polynomial in firm size, data heaping on firm size and placebo regressions, among 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 could increase more than productivity for workers hired on temporary contracts, thus reducing the incentive for firms to provide training. This, in turn, may happen because trained workers in temporary contracts could 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 this appeals 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 with which they convert tem-

[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 a greater 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.

porary to permanent jobs. This leads, in turn, to 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).

Consistent with this strand of the literature, we find some mild evidence that the decrease in EPL associated to the Fornero reform reduced excess turnover and the use of temporary contracts within firms above the cut-off.

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 using the DRDD in the Italian context allows us to address some of the weaknesses of the regression discontinuities design (RDD), 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), which 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 remainder 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 conduct some robustness checks in Section 5. Section 6 proposes 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 the literature. First, it is related to theoretical studies dealing with incentives to invest in human capital by firms and workers. Following the 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 a greater incentive to pay for training. This is because labour market imperfections, such as search frictions, information asymmetries and labour market institutions, determine a 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 for 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 by 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 greater use of temporary contracts. They also find that

[6] See Belot et al. (2007), Fella (2005) and Lechthaler (2009) for other papers that examine 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.

EPL 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 score 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 training for 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 (from 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-term 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 for Spain and Italy, analyse the effects of EPL on training by type of contract (temporary vs. permanent) and on the composition of the labour force by type of contract. Cabrales et al. (2017) use the PIAAC survey data and document a large gap in training provisions between 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, and thereby reducing the incentive for firms to invest in training. In contrast, 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 n. 300/70, also known as the 'Statuto dei Lavoratori' (Workers' Statute) and, in 1990, with Law n. 108/90, which strengthened employee protection from unfair dismissal only in the case of small firms.[8]

Before the legislative changes that occurred in 2012 (Fornero Law) and 2015 (Jobs 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, an employee unfairly dismissed from a firm with more than 15 employees could ask to be reinstated and receive forgone wages 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 the legislative changes that occurred between 1960 and 2012.

[9] See Boeri and Garibaldi (2019) for a description of the Jobs Act reform.

[10] It is important to note that according to Italian legislation, part-time workers count as less than one full-time employee when defining firm size, which 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 were 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 approximately 850 days over the period 2007-2010, with large variation across regions.[12] Such a difference in the length of labour trials escalated 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 approximately 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 no forgone wages were due for firms below the threshold, the length of labour trials mattered only to firms 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 show 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.

Thus 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 the 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.

[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 approximately 15 months of wages in Trento (North), compared with 65 months in Salerno (South). 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 for firms above the threshold.

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 using a conventional RDD design, as explained in Section 3.2.

In July 2012, a reform, known as the Fornero Law, significantly reduced firing costs for permanent workers in the case of firms with more than 15 employees. We refer to Berton et al. (2017) for a detailed analysis of the novelties introduced by the 2012 reform, but here, we note that the Fornero Law limited the possibility for permanent workers in firms with more than 15 employees to choose between reinstatement and a monetary compensation in case of unfair dismissal to a set of well-defined cases.[15] Moreover, it substantially reduced the amount of monetary compensation and eased the uncertainty surrounding the duration and costs of litigation, which, as highlighted above, was fairly high, especially in some areas of the country.[16]

As explained in Section 3.2, we use the reduction in firing costs brought about by the Fornero Law in firms above the 15-employee threshold 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 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

[15] For instance, the judge was granted the ability 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.

[16] In 2015, the Jobs Act, introduced by the Renzi government further reduced firing costs for firms above the 15-employee threshold. In particular, it strictly linked monetary compensation to seniority (thus limiting judges' discretion) and de facto eliminated the ability of judges to order a reinstatement; the consequences, however, largely fall outside the sample period considered in this study.

relative to firms below the threshold. Because temporary workers generally receive less training, we expect that following the reform, the propensity to train workers should increase in firms above the 15-employee threshold. In other words, we can exploit the Fornero Law as a quasi-experiment to carry out a DRDD: 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 either that any unobservable variable impacting training is continuous at the threshold (as in RDD) or that its effect at the discontinuity is constant over time (as in a conventional difference-in-differences approach).[17] In this case, the change in training before and after the reform for firms just below the threshold can be considered to be 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 a firm's training provision (Kennedy et al. 1994, Dustmann and Schönberg 2009, Stegmaier 2012), and Italian legislation allows workers the right to form worker councils in firms with more than 15 employees.[18] Hence, neglecting this confounder acting at the cut-off would potentially lead to an overestimate of the effect of EPL when using an RDD.[19]

The DRDD approach can be described parametrically through the following equation, as

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

[18] Another potential confounding factor that may interfere with EPL is the so-called *Cassa Integrazione Guadagni Straordinaria* (CIG), i.e. a short-term work program comprising a worker redundancy fund extraordinary scheme. This institute aims to help firms that are either in a process of reorganization and restructuring, or that have been facing a severe economic crisis or are under an insolvency procedure. The Italian legislation for the period related to this study mandated that only firms above the 15-employee threshold could use CIG. In general, firms with a high share of workers under CIG schemes are also likely to provide less training since their level of activity is decreasing.

[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.

in Cingano et al. (2016):

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

where i is the firm subscript, t is the survey wave subscript ($t = 2010, 2015$), and y_{it} is the number of trained workers. Our data refer to two cross-sections that should be representative 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 use 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 normalised 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 and captures 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, with both a linear and a quadratic polynomial specification. Moreover, as a robustness check, we follow Grembi et al. (2016) and 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):

$$y_{it} = \alpha_0 + \alpha_1 post_t + \alpha_2 above_{it} + \alpha_3 above_{it} \times post_t + \alpha_4 f(E_{it} - 15) + \alpha_5 f(E_{it} - 15) \times above_{it} + \alpha_6 f(E_{it} - 15) \times post_t + \alpha_7 f(E_{it} - 15) \times above_{it} \times post_t + \beta' \mathbf{X}_{it} + \varepsilon_{it}. \tag{2}$$

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

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' propensity to grow above the threshold. We have tested this assumption using a modified version of Schivardi and Torrini's test (Schivardi and Torrini 2008) in Section 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 as 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 cut-off were an impediment to firm growth before the Fornero Law. This is clearly related to Schivardi and Torrini's test, which we have discussed above. 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 approximately 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 cut-off in the two waves, as in Boeri and Garibaldi (2019).[21]

Another econometric issue that is worth mentioning is that, in our survey, firm size is provided in discrete units, i.e., head count. The 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 is done in Leonardi and Pica (2013) or Hijzen et al. (2017). We address this issue in two ways. First, we drop firms with 16 employees (because they could be spuriously considered as above the threshold when they are in fact below it, e.g., if they have at least two part-time employees, which are counted as a fraction of a full-time employee) in a donut-

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

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) for use 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-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 use the total available data, as DRDDs such as RDDs are data-intensive, we also show that the 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). The INAPP institute has been recently created (replacing ISFOL), and its main activities are oriented towards research, monitoring and public policy evaluation. It constitutes a building block in supporting policymaking by the Ministry of Labour and Social Policies. Using the universe of active 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 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 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 firm activities, such as their export, innovation or offshoring activities, only limited information is available on balance sheet data.[22]

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 fewer than zero (or an abnormal number of) employees in 2010 (196 observations) and in 2015 (83 observations). The above selections result in 24,263 and 30,008 observations for the two years, respectively, the whole sample being 54,271 observations. For 10,214 firms, we have two observations (panel), while the remainder (14,049 and 19,794 for 2010 and 2015, respectively) is a repeated cross section. In the econometric analysis, we restrict the sample to firms sized in the 5-26 employee range; moreover, we trim the data by dropping from the analysis firms that experienced a year-on-year growth rate in the number of employees larger (smaller) than the 95th (5th) percentile, and we restrict the sample to still-active firms, resulting in a final sample of 16,532 observations (5,794 for the panel component). In Table 1, we report descriptive statistics for the sample used in the baseline regressions reported 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 the systematic self-sorting of firms at or below the 15-employee threshold before and after the Fornero reform, and of a change of this sorting after the reform.[23]

We do this using a variant of the test proposed by Schivardi and Torrini (2008) and later used in Leonardi and Pica (2013), Hijzen et al. (2017), among others. In practice, the test is

[22] Devicienti et al. (2018) use the RIL data as a primary source of information to study the relationship between unions and temporary contracts.

[23] Previous studies have generally not found clear evidence supporting the self-sorting of firms (Schivardi and Torrini 2008, Leonardi and Pica 2013, Hijzen et al. 2017). However, in their recent evaluation of the Jobs Act (reform) of 2015, Boeri and Garibaldi (2019) report a significant increase in firms' propensity to grow above the 15-employee threshold after the introduction of the Jobs Act.

based on the existence of systematic differentials in the firms' likelihood of growing in size when they are just below the 15-employee threshold. We carry out the test by estimating the following equation using a linear probability model (LPM):

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

with

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

E_{it-1} and E_{it} are firm size in year $t-1$ (2014 and 2009, for the 2015 and 2010 waves, respectively) and t (2015 and 2010, for the 2015 and 2010 waves, respectively);[24] D_{it-1}^{k} is a set of bin dummies, with the bin size equal to 1 (namely, for sizes 13, 14 and 15 employees). A fundamental assumption for the validity of the DRDD is the that if sorting at the threshold is present, it should remain the same before and after the policy change.[25] Indeed, in this case, any confounding policy (or factor) existing exactly at the threshold is removed by the 'difference' part of the estimator. To test this, we allow for the firm size dummies to have differential effects before vs. after the Fornero Law by interacting them with $post_t$; 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 parametrically captures the underlying relationship between firm size and the probability of employment growth in the absence of employment protection, while the three bin dummies can be interpreted as the threshold effect of EPL on firms' employment growth at 13, 14 and 15 employees. In particular, the interaction of the three bin dummies with the $post_t$ dummy allows 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 (approximately 9 percentage points) when at 14 employees. The 14 employees $\times$ $post_t$ interaction term is positive and large in magnitude, but very imprecisely estimated and does not show any statistically significant change in sorting after the reform. The result does not change if we consider a cubic polynomial (not

[24] Indeed, in each survey wave the current employment and the past year employment are available.
[25] See assumption 2 in Grembi et al. (2016).

shown in the Table) or if we allow the polynomial to differ on both sides of the threshold. In columns (3) and (4), we repeat Schivardi and Torrini's test on the panel component of the survey, which allows us to control for firm fixed effects. In this case we find much less clear evidence of firms' self-selection below the threshold. Before the Fornero Law, there is again some evidence of a lower propensity to grow at 14 employees, which is, however, statistically nonsignificant; moreover, there is no evidence that the propensity to grow was altered by the Fornero Law: the coefficients on the interacted 14- and 15-employee dummies are not only statistically nonsignificant but also very close to zero.

[Table 2 about here]

To further test the assumption of no difference in firm sorting below the threshold before vs. after the reform, similar to Grembi et al. (2016), we report in Figure 1 the scatter plot of the difference in the densities of normalised employment by one-employee bins and a linear fit with the 95% confidence interval. The graph clearly shows no sign of a change in the density after the Fornero Law.

Although the analysis reported in this section generally supports the validity of the DRDD as far as change in sorting is concerned, in our main result sections we report the results for both the pooled cross-sections and for the panel specification with firm fixed effects; moreover, we also report the results of a donut-hole specification whereby we drop firms with 14, 15 and 16 employees, where firm sorting is more likely to take place.[26]

[Figure 1 about here]

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 the outcome variable.[27] In the first four columns

[26] Firms with 14 and 15 employees are dropped because of possible manipulation and those with 16 employees because they might actually 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 that 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).

[27] 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

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 (which we will refer to 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-employee threshold and following the Fornero reform, there has been an average increase of 1.72 trained workers, which is significant at the 1 per cent level. The magnitude of the discontinuity can also be appreciated from Figure 2, 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).[28]

In the remaining columns, we estimate the model in equation (1), allowing for a different bandwidth around the 15-employee threshold. The results reported across columns confirm that the $post \times above$ coefficient is always positive and statistically significant at conventional 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 approximately 3 in column (7) for the largest bandwidth (6 to 50 employees). Again, we detect very minor differences depending on whether or not firm controls are included. 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 (see Table A1 in Appendix A).

Interestingly, the *above* dummy is negative in all specifications and statistically significant in the 6–30 and 6–50 bandwidth cases; this means that there were fewer trained workers above the threshold in 2010. It is possible that in a strongly dual labour market, to escape the more stringent firing costs on open-ended contracts above the threshold, firms were relying on a sequence of temporary contracts.[29] However, temporary workers tend to receive less

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, the results are qualitatively similar if we consider the share of trained workers as the outcome variable.

[28]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 a magnitude similar to that reported in Table 3, namely 1.72 and 1.54 in the baseline specifications excluding and including sector and region fixed effects, respectively.

[29] Consistent with this prediction, Boeri and Jimeno (2005) study the variable enforcement of EPL for permanent and temporary workers at the threshold to analyse the dynamics of hiring and firing in Italy. They find

training. 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 fewer than 15 employees.

Returning 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 approximately 1.5 additional trained workers. Considering that before the reform, the average number of trained workers in firms with 15 employees was approximately 3.1, our estimates suggest that the Fornero Law might have increased the number of trained workers by approximately 50% at the threshold.

[Table 3 about here]

[Figure 2 about here]

As is well known, the DID estimator, when applied to repeated cross-sections, may be biased by changes in the composition of the sample over time. The same issue can bias the DRDD estimator. For this reason, we implement a test following Carrell et al. (2018). In particular, rather than testing for lack of balance for each presumably exogenous firm characteristic, namely, region of location and industry (dummies), we regress the outcome (number of trained workers) on these dummies and compute the predicted values from the regression. Then, we plot these predicted values by averaging by one-employee bins, as we did for the observed outcome. This method allows us to assess the influence of the change in firm characteristics on the outcome of interest. Finding a significant discontinuity in the predicted outcomes would imply that the estimated effect could be artificially produced by a change in firms' observable characteristics. As shown in Figure 3, which must be compared with Figure 2 based on the observed outcome, this does not seem to be the case, as no significant jump is evident from the graph.

[Figure 3 about here]

that firing decreases (increases) with size for permanent (temporary) workers; moreover, hiring is somewhat reduced at the threshold, with the emergence of an asymmetric U-shaped relationship between hiring and firm size. See also Cahuc et al. (2016) for a model explaining the spread of temporary jobs in dual labour markets.

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, the results reported in columns (1) and (2) of Table 4 and those in columns (1) and (2) of Table 3 are very similar.[30] Second, in columns (3) and (4), we run a series of donut-hole regressions to address 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 due to the presence of part-time employees: again, the results are broadly unchanged.[31]

Third, in columns (5) to (8), we have run a placebo analysis by assuming that the threshold was at 10 (20), rather than at 15, employees. In these cases, the estimates of the interaction term are still positive but much smaller (in the case of 10 employees) or largely statistically 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 consider a more general specification. Indeed, we allow the polynomial in firm size to take on different coefficients before versus after the reform and not just 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, respectively. When we consider a polynomial of second order (columns 11 and 12), the magnitude is slightly larger than that reported in previous columns.

[Table 4 about here]

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,

[30] 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 if 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* $\times$ *above* coefficient is equal to 0 at the 1% level of confidence.

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

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 tests, we do not find clear evidence that the reform increased the propensity for firms to cross the 15-employee 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.

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 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 approximately 1 additional trained worker. In 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 approximately 1.9, which is statistically significant at the 10 per cent level. 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.

[Table 5 about here]

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.[32] When we do that, our empirical results suggest that the Fornero reform might have determined an increase of approximately one additional trained worker at the threshold.

[32] We drop approximately 600 observations, which represents approximately 10 per cent of our estimation sample.

6 Potential mechanism: worker 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 and increase profits by making greater use of temporary contracts.[33]

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 could, as a result, increase the incentives for firms to rely on a sequence of temporary jobs (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 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 excess of worker turnover over the absolute value of net employment change, the latter in turn being measured as the difference between hiring and separation rates. Interestingly, the authors also found that this effect is entirely explained by the 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, Arulam-

[33] Daruich et al. (2017) exploit an Italian reform that lifted constraints on the employment of temporary contracts while maintaining the level of EPL in permanent contracts unaltered and demonstrate that firms increased the use of temporary contracts and experienced lower labour costs and higher profitability. The authors also report that workers on a temporary contract receive only 66% of the rents shared by firms with workers hired under a permanent contract.

palam et al. 2004, Albert et al. 2005), one could argue that stricter EPL might cause lower training by firms, with the mediating factors being the 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 with 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 cut-off.

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 equations (1) and (2) with polynomials in firm employment of the first and second degrees.[34] 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 hires and separations, respectively, and E is the average firm employment.[35] The results displayed in columns (1) to (4) point 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 fixed-term contracts is reduced in the case of the specification of equation (2), confirming the results of Centeno and Novo (2012) for Portugal. Our results are also in line with O'Higgins and Pica (2019), who, using administrative data, find that the Fornero reform brought an increase in permanent contracts of approximately 5 percentage points. However, no effect is found for conversion from temporary to permanent contracts, at least for younger workers.

The 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 reform: in other words, these results seem to be consistent with the idea that an overly large 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

[34] The inclusion of industry and region fixed effects does not qualitatively change the results.

[35] 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).

Fornero reform, the 'perverse effects' (in terms of training) of a dual labour market (e.g., high worker turnover and excess reliance on temporary positions) tend to disappear, as the empirical results in Table 6 somehow suggest.

[Table 6 about here]

7 Conclusion

In this paper, we provide new clean evidence on the causal effect of EPL on firm-provided training using a labour market reform, the Fornero Law, that 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 DRDD. Indeed, the Law decreased the level of EPL only for firms above the 15-employee cut-off, which before the reform had been subject to substantial firing restrictions due to article 18 of the Workers' Statute.

Our preferred DRDD estimates suggest that the Fornero reform led to an increase in the number of trained workers of approximately 1.5 units at the cut-off, i.e., an approximately 50 percent increase. The 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 changes of bandwidth.

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) from a different dataset, we provide evidence that firms above the threshold were characterised by higher excess worker turnover and greater 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 decreased after the introduction of the Fornero Law. In other words, in labour markets that have 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-employee threshold, might have reduced the incentives for 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 downward-sloping labour demand.[36] Our empirical findings, in turn, suggest that by favouring growth in the number 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.

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[36] See also Cahuc et al. (2016).

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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 statistics use sample weights and are calculated on the sample used in regression reported in column (1) of Table 3. Employees is the total number of employees. 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. Share of temporary workers is the share of fixed term contracts. 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.

Table 2: Probability of growing: Schivardi and Torrini (2008) tests

	(1)	(2)	(3)	(4)
13 employees	-0.000593	-0.0147	-0.0105	0.0103
	(0.0318)	(0.0343)	(0.0636)	(0.0642)
14 employees	-0.0908***	-0.105***	-0.0883	-0.0661
	(0.0275)	(0.0306)	(0.0567)	(0.0567)
15 employees	-0.0425	-0.0561	-0.0653	-0.0368
	(0.0345)	(0.0370)	(0.0606)	(0.0623)
13 employees × post	-0.0242	-0.0250	-0.0638	-0.0582
	(0.0541)	(0.0542)	(0.0792)	(0.0795)
14 employees × post	0.192	0.191	-0.0143	-0.00755
	(0.123)	(0.123)	(0.0741)	(0.0741)
15 employees × post	-0.0268	-0.0276	0.00364	0.00409
	(0.0466)	(0.0466)	(0.0742)	(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 that the size of the firm increased with respect to the previous year. 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***	-2.416***	1.291***	-3.287***	1.084***	-2.611***	1.084***	-2.635***
	(0.137)	(0.611)	(0.303)	(1.107)	(0.137)	(0.642)	(0.137)	(0.690)
above	-0.407	-0.487	-0.501	-0.718	-0.848**	-0.857**	-1.966***	-1.925***
	(0.382)	(0.382)	(0.575)	(0.556)	(0.358)	(0.349)	(0.412)	(0.394)
post×above	1.722***	1.544***	1.946***	1.642***	2.049***	1.887***	3.075***	2.857***
	(0.422)	(0.402)	(0.594)	(0.535)	(0.383)	(0.368)	(0.532)	(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***	-2.646***	1.055***	-1.843***	0.983***	-2.657***	1.302***	-2.316***	1.503***	-1.886***	1.508***	-1.893**
	(0.139)	(0.653)	(0.133)	(0.544)	(0.134)	(0.623)	(0.135)	(0.611)	(0.390)	(0.682)	(0.547)	(0.758)
above	0.0336	-0.101	-0.240	-0.134	-0.702	-0.714*	-0.867	-0.692	-0.356	-0.430	-0.359	-0.657
	(0.421)	(0.411)	(0.529)	(0.514)	(0.493)	(0.395)	(1.722)	(1.698)	(0.478)	(0.491)	(0.732)	(0.767)
post×above	1.384***	1.262***	1.566***	1.351***	0.810***	0.815***	0.668	0.490	1.631**	1.437*	2.096*	2.064*
	(0.474)	(0.450)	(0.469)	(0.446)	(0.280)	(0.248)	(0.629)	(0.611)	(0.801)	(0.764)	(1.193)	(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	Linear	Linear	Linear	Linear	Linear	Linear	Linear	Linear	Quadratic	Quadratic
Pol. inter.	above	above	above	above	above	above	above	above	both	both	both	both
Sec.×year f.e.	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Reg.×year f.e.	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	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***	1.574	1.217***	1.521	1.231***	1.060	2.250***	2.164*
	(0.125)	(1.097)	(0.135)	(3.419)	(0.126)	(1.048)	(0.363)	(1.140)
above	-0.465	-0.398	-1.301*	-1.103	-1.359	-1.044	-0.916	-0.964
	(0.692)	(0.691)	(0.774)	(0.760)	(1.177)	(1.190)	(0.827)	(0.825)
post×above	1.027**	0.829*	1.424**	1.249**	1.163*	0.994	1.858*	1.901*
	(0.500)	(0.494)	(0.587)	(0.583)	(0.615)	(0.609)	(1.002)	(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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
dependent variable	excess worker turnover				share of temporary contracts			
post	0.347***	0.347***	0.391***	0.486***	0.158***	0.158***	0.194***	0.230***
	(0.0770)	(0.0751)	(0.0924)	(0.0920)	(0.0383)	(0.0380)	(0.0490)	(0.0549)
above	0.0687**	-0.0174	0.0983***	0.0249	0.0273**	-0.0230	0.0525***	0.0313
	(0.0268)	(0.0375)	(0.0316)	(0.0513)	(0.0131)	(0.0258)	(0.0147)	(0.0230)
post×above	-0.0426	-0.0410	-0.104**	-0.135*	0.00685	0.00674	-0.0443	-0.107***
	(0.0263)	(0.0262)	(0.0486)	(0.0755)	(0.0200)	(0.0196)	(0.0340)	(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%).

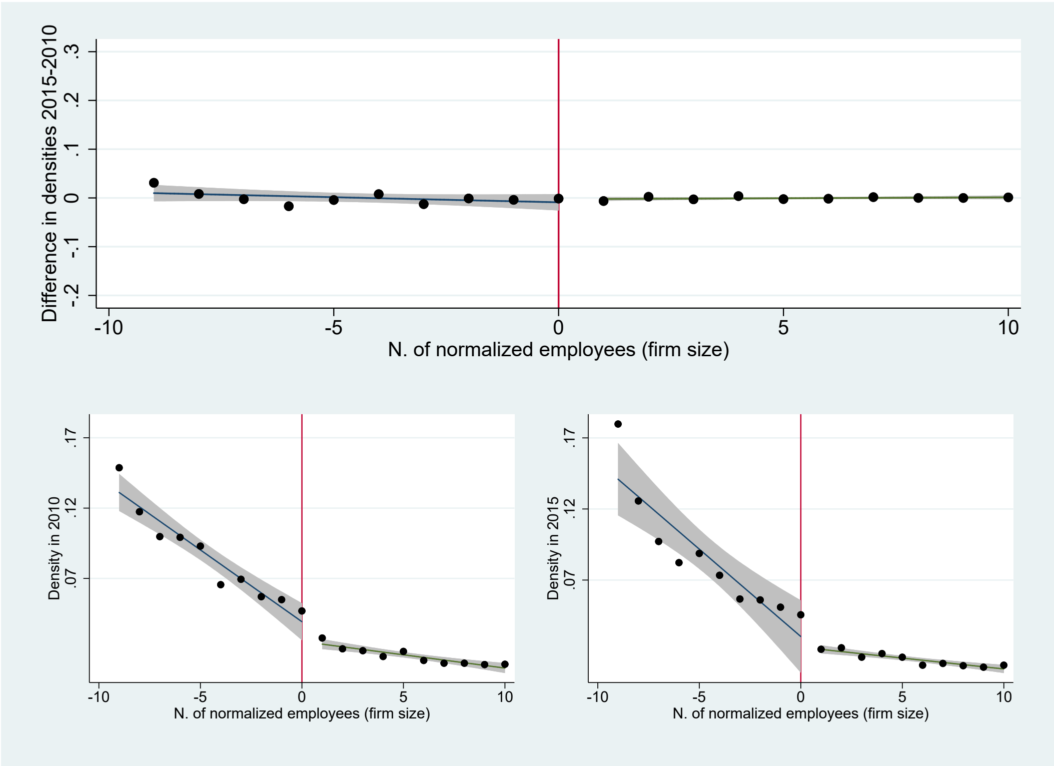
A Additional results

Table A1: Baseline results: quadratic polynomial

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
post	1.083***	-2.419***	1.284***	-3.311***	1.083***	-2.598***	1.083***	-2.607***
	(0.136)	(0.610)	(0.302)	(1.106)	(0.136)	(0.640)	(0.136)	(0.693)
above	-0.196	-0.426	-0.680	-0.928	-0.0720	-0.250	-1.221**	-1.258**
	(0.628)	(0.619)	(1.079)	(1.032)	(0.494)	(0.487)	(0.604)	(0.554)
post×above	1.726***	1.547***	1.952***	1.649***	2.063***	1.900***	3.065***	2.848***
	(0.421)	(0.401)	(0.589)	(0.531)	(0.382)	(0.368)	(0.534)	(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

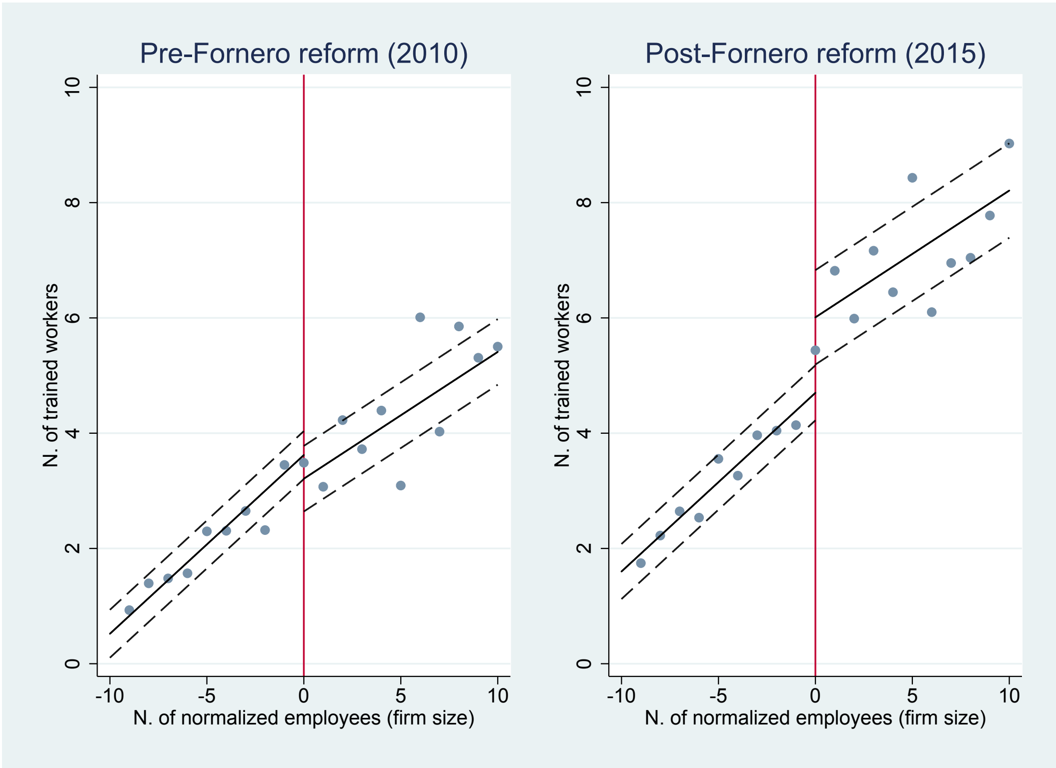
Note. Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1. Polynomials in employment have been interacted with the dummy *above* (15-employee threshold). We exclude firms at the 5th and 95th percentile of the distribution of growth of employment (below and above 50%).

Figure 1: Test of difference in densities



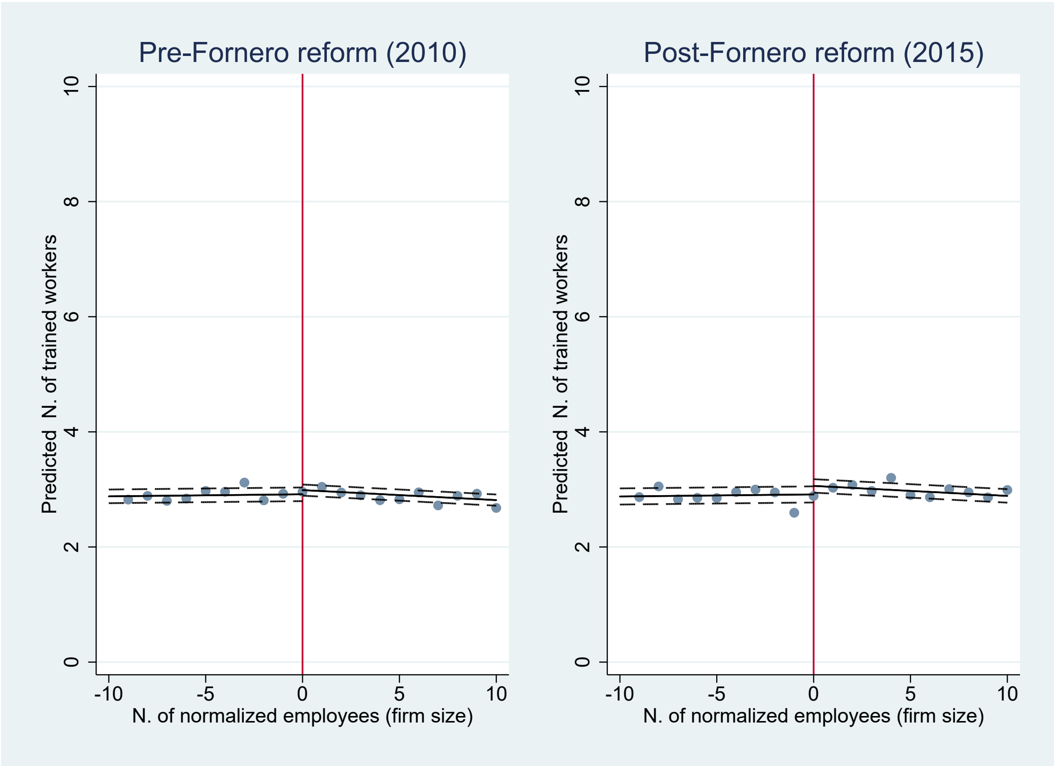
Note. The top part of the Figure reports a plot of the difference in the 2015-2010 densities of normalized employment size by one-employee bins, along with a linear fit and the 95% confidence interval. The bottom part of the figure reports the densities of normalized employment size by one-employee bins for 2010 and 2015, respectively. 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).

Figure 2: Firm size and *observed* 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 the 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.

Figure 3: Firm size and *predicted* 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 based on the predicted values of a regression of observed training provision on region and industry dummies, and the fitted (solid) line of a regression of the predicted number of trained workers on normalized employment. 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.