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Employment protection and firm-provided training in dual labour markets

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A B S T R A C T

In this paper we leverage a labour market reform (Fornero Law) which reduced firing restrictions for open-ended contracts in the case of firms with more than 15 employees in Italy. The results from a Difference in Regression Discontinuities design demonstrate that after the reform, the number of trained workers increased in firms just above the threshold by approximately 1.5 additional workers. We show that this effect can be explained by the reduction in worker turnover and a higher use of permanent contracts. Our study highlights the potentially adverse effects of employment protection legislation on training in dual labour markets.

1. Introduction

Employment protection legislation (EPL) has been at the heart of the political and policy debate in many countries for a long time. By imposing constraints on firms' ability to adjust their workforce in reaction to the shocks associated with demand, costs and technology, EPL has often been blamed for negative effects at both the micro and macro level, e.g., lower productivity and possibly lower aggregate employment (Hopenhayn and Rogerson, 1993). Therefore, over the past 20 years, various countries have tried to reduce EPL across the board. However, in countries that have introduced labour market reforms, the EPL rules for open-ended contracts have been barely changed, at least until very recently (Boeri and Garibaldi, 2019; Dolado, 2016), partly owing to political economy considerations (Saint-Paul, 1997). A process of 'labour market flexibilization at the margin' (i.e., for temporary or atypical workers) has been frequently implemented, which has led to dual labour markets.[1]

A better understanding of the effect of EPL in dual labour markets is important because it can provide insights on some unsettled debates in the empirical literature on the impact of EPL on firm performance. Indeed, several scholars have reported robust evidence of the negative impacts of EPL on labour and total factor productivity (Autor et al., 2007; Bassanini et al., 2009; Bentolila et al., 2019; Bjuggren, 2018; Cingano et al., 2010; 2016; Hijzen et al., 2017).[2] However, other scholars have reported that EPL may increase labour productivity, such as through a positive impact on innovation (Acharya et al., 2014; Griffith and Macartney, 2014; Koeniger, 2005) or investments in firm-specific training by employees (Belot et al., 2007). Therefore, as forcefully argued by Boeri et al. (2015), more work is needed because most of the empirical literature tends to look at the overall net effects of EPL on productivity rather than at the exact mechanisms.

In this paper, we provide new insights on the mechanisms linking labour productivity, EPL and temporary contract use by investigating the effect of EPL on firm-provided training in dual labour markets. To this end, we provide clean evidence on the causal effects 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

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[1] Dolado (2016) provides an insightful discussion about the emergence of dual labour markets in some European countries. See also Bentolila et al. (2019).

[2] Consistent with the negative effect on productivity, Leonardi and Pica (2013) report negative effects on individual wages, which is mainly limited to job switchers and low earners, young white-collar workers and workers in low-employment regions.

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using a Difference in Regression Discontinuities (*diff-in-disc*) design (see, for instance, Cingano et al., 2016; Grembi et al., 2016). This identification strategy allows us to overcome limitations of previous studies on the effects of EPL using Regression Discontinuity Design (RDD) strategies (for example, Bolli and Kemper, 2017), i.e., the role of confounding policies operating at the same cut-off as EPL and possible dynamic sorting.

Before the reform, the Italian legislation included size-contingent firing restrictions for open-ended contracts, and based on these restrictions, the firing costs increased sharply above the 15-employee threshold.[3] These restrictions were greatly reduced in 2012, but only for firms above the 15-employee cut-off, by a labour market reform known as the Fornero Law. 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 clear causal evidence on the effect of EPL on training using a *diff-in-disc* design. Providing new evidence is important and timely given the paucity of studies that have empirically investigated the interplay between EPL and firm-provided training (see Section 2) and in light of several reforms that have reduced employment protection, especially at the margin, i.e., for temporary workers, in many countries.[4]

The main results of our paper can be summarised as follows. Our preferred estimates suggest that by reducing EPL for large firms (i.e., firms above the 15-employee cut-off), the Fornero reform increased the average number of trained workers by approximately 1.5 individuals, which corresponds to an approximately 50 per cent increase in the number of trained workers *at the cut-off* firm size (3.1 workers were trained on average prior to 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 cut-off, changes in the bandwidth, changes in the order of the polynomial in firm size, data heaping on firm size, placebo regressions, possible misclassification associated to measurement error in the forcing variable and restricting the analysis to the panel component of the data (and including firm fixed effects), among others.

We further explore the main mechanisms that might be behind the estimated effect of relaxing EPL on training. The first is the *substitution* between temporary and permanent workers (Cahuc et al., 2016). Several scholars have found empirical evidence consistent with this prediction (Cahuc et al., 2019; Centeno and Novo, 2012; Hijzen et al., 2017; Schivardi and Torrini, 2008). Thus, owing to the training gap existing between permanent and temporary workers (Albert et al., 2005; Arulampalam and Booth, 1998; Arulampalam et al., 2004; Booth et al., 2002; Cabrales et al., 2017; Ferreira et al., 2018), the greater use of permanent contracts should also entail higher training. The second mechanism is proposed by Dolado et al. (2016). The authors argue that, under plausible conditions, firms' temporary-to-permanent conversion rates go down when the EPL gap between temporary and permanent workers increases. Temporary workers respond in turn to lower conversion rates by exerting less effort, while firms react by providing less paid-for training to them. Thus, the Fornero reform *by reducing the EPL gap* might have contributed to an increase of firms' training provision to temporary workers.

We provide evidence consistent with the first explanation. Indeed, after the Fornero Law, firms for which EPL was reduced increased the number of permanent workers, which enjoy higher training, by about 1.7 units at the threshold. Under the assumption that training is provided to new permanent hires, for instance, temporary-permanent worker substitution induced by the reform at the cut-off would be able to explain alone the whole effect we find on firm-provided training.

[3] In Section 3, we discuss why these policy-induced differences in employment protection substantially differentiate firing costs according to firm size.

[4] On labour market liberalisation reforms at the margin, see, for example, Boeri and Garibaldi (2007) and Berton and Garibaldi (2012) and the survey paper by Dolado (2016).

We make two main contributions to the existing literature, which is discussed in more detail in Section 2. First, we provide robust evidence on the effects of EPL on training in Italy using a *diff-in-disc* design in a quasi-experimental setting. Our approach represents an improvement over the existing literature since using the *diff-in-disc* design in the Italian context allows us to address some of the weaknesses of RDDs, namely, the existence of other labour market institutions also changing discontinuously at the same margin of firm size as the EPL (i.e. confounding policies). These are the right to create works councils in the firms and the existence of short-work programs for employees in firms under severe economic difficulties, which might affect a firm's provision of training. Accounting for the confounding policies through a *diff-in-disc* design, our analysis leads to different conclusions compared to Bolli and Kemper (2017).[5] Second, we investigate the main mechanisms of the effect of EPL on training, and provide evidence of substitution of temporary with permanent workers around the 15-employee cut-off.

All in all, our analysis shows that in countries characterised by very stringent EPL for permanent workers and persistent dualism in the labour market, 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 provide the theoretical grounds for our empirical analysis and review the empirical literature. In Section 3, we introduce the institutional framework and present our identification strategy. After discussing the data in Section 4, we present our main results in Section 5. In the online Appendix B, we discuss potential threats to identification and test the assumptions of the *diff-in-disc* design. Some robustness checks are conducted in Section 6. Section 7 explores the mechanisms behind the estimated effect. Finally, Section 8 summarises the main findings and draws conclusions.

2. Theoretical underpinnings and empirical evidence: EPL, temporary contracts and training

To understand the potential mechanisms that could link EPL, temporary contracts and firms' training provision, it is useful to briefly review some related literature.

Using the standard human capital model (Becker, 1964), a firm's incentives to invest in its workforce depend on the time the employer expects to reap the benefits of a more trained workforce. Thus, a stronger EPL, e.g. an increase in firing costs, should also lead to reduced worker turnover and higher worker tenure (Boeri and Jimeno, 2005; Kan and Lin, 2011) thereby creating higher incentives to train the employees. By the same token, workers in temporary contracts are expected to receive much less firm-provided training than workers hired with permanent contracts. Another mechanism that can generate a positive association between EPL and training is highlighted by Acemoglu (1997) and Acemoglu and Pischke (1999), who show that when labour market institutions, such as EPL, generate wage compression, firms may have a greater incentive to pay for training 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. A necessary condition for firms to sponsor (general) training is that these rents are increasing in training (Acemoglu and Pischke, 1999).

The above predictions change when permanent and temporary positions in the labour market coexist. Several papers have already tested the relation between temporary contracts and training and found a training gap in favour of permanent workers (Albert et al., 2005; Arulampalam and Booth, 1998; Arulampalam et al., 2004; Booth et al., 2002; Cabrales et al., 2017; Ferreira et al., 2018). In turn, much less empirical evidence exists on the relation between EPL and firms' training investments.

[5] See Section 2 for more details.

The interplay between EPL and temporary contracts in dual labour markets, in which temporary and permanent contracts enjoy different levels of EPL, explains why the expectation of a *positive* association between stronger EPL and firm-provided training may actually break down. The theoretical model in Cahuc et al. (2016) predicts that in labour markets with significant asymmetry in the degree of employment protection enjoyed by permanent and temporary workers, the firms have an incentive to substitute temporary for permanent workers by using a sequence of temporary contracts, thereby creating excess worker turnover. This prediction is indeed supported by the data. 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, which is defined as worker turnover over the absolute value of net employment change, with this value measured as the difference between hiring and separation rates. Interestingly, the authors also find that this effect is entirely explained by the greater use of temporary workers above the threshold. Similar evidence is found by 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. Thus, given that temporary workers receive less training, in dual labour markets, stricter EPL may induce a widespread use of contracts associated with less training investments, thus generating a *negative* association between EPL and training.

However, a change in the level of EPL may affect firms' training decisions independent of an increase in the use of temporary contracts, which is nicely illustrated in the theoretical model in Dolado et al. (2016). In their model, firms do not use temporary contracts as a pure screening device because such contracts ensure an initially higher surplus than permanent contracts, due to their lower firing costs.[6] Temporary workers cannot be renewed, and at their contract's termination, firms have to decide whether to hire them permanently or not. Workers' productivity depends on their effort, although in addition to wages, firms have another instrument available to improve temporary workers' performance when their incentives are not aligned: temporary-to-permanent conversion rates. Moreover, firms provide costly specific human capital to temporary workers through training, which only increases workers' productivity when they become permanent workers. The model's basic insight is that when EPL becomes stricter, i.e., the gap in firing costs between temporary and permanent contracts widens, firms reduce the rate of temporary-to-permanent conversion and *reduce training investments in temporary workers*. As a consequence, temporary workers reduce their effort level based on the likely condition in which permanent workers react to the change in EPL by exerting less or equal effort (which is consistent, for instance, with the positive effect of stricter EPL on absenteeism found by Ichino and Riphahn, 2005).

In brief, although simple economic reasoning indicates that we should expect a positive association between stricter EPL and training, the presence of dual labour markets complicates the overall picture. In particular, stricter EPL may induce a larger use of temporary contracts or reduce the temporary-to-permanent conversion rates, thus reducing training investments in both cases. In this context, Choi (2019) proposes a search and matching model in which the excessive use and persistence of temporary contracts arise endogenously due to selection effects and optimal training decisions. In his framework, less skilled workers self-select into temporary jobs while low levels of training emerge as the result of the short duration of temporary employment relations. Consequently, his framework predicts that, in response to a rise in firing costs, open-ended (permanent) contracts are substituted by temporary ones, which results in a low incidence of training.

Scholars have extensively investigated the effect of EPL on a number of outcomes for both workers and firms; however, few papers have focused on the effect of EPL on firm-provided training.[7] 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 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 study by Messe and Rouland (2014) exploits a reform of EPL in France to identify, using a Difference in Differences (DID) 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 a different source of 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 *diff-in-disc* framework, which enables us to control for other labour market institutions that in Italy change discontinuously at the threshold, such as the *Cassa Integrazione Guadagni Straordinaria* scheme, a short-term work programme featuring a redundancy fund system, or the presence of works councils in the firm. We show that accounting for these confounding policies leads to very different conclusions on the effect of EPL.

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), 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 (Article 18 of the Workers' Statute). Indeed, if a dis-

[6] 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.

[7] The literature on the various effects of EPL is vast and cannot be comprehensively 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 in physical capital, Schivardi and Torrini (2008) for EPL and a firm's' 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.

[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.

missal 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).[9]

The higher *de jure* costs for employers in the case of firms with more than 15 employees were further increased when considering 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.[10] Such a difference in the length of labour trials escalates the 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.[11] Because no forgone wages were due for firms below the threshold, the length of labour trials only matters for firms above the threshold, with firing costs rapidly increasing above the 15-employee threshold if the labour trial lasts longer than 5 months.[12] 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 EPL for temporary and atypical workers does not change discontinuously at the 15-employee threshold; indeed, it does not depend at all on firm size.

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.[13] 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.[14]

As explained in following section, 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 *diff-in-disc* design.

3.2. Identification strategy

Given the sharp discontinuous change in the level of EPL at the 15-employee threshold, a way to estimate the effect of EPL on training is using a RDD such as

$$y_i = \beta_0 + \beta_1 above_i + f(x_i - 15) + g(x_i - 15) \times above_i + \epsilon_i, \tag{1}$$

where i is the firm subscript and y_i is the number of trained workers; $above_i$ is a dichotomous indicator that equals one for the firms above the 15-employee cut-off (i.e. those subjected to stronger EPL of Article 18), and zero otherwise; $f(x_i - 15)$ and $g(x_i - 15)$ are two different polynomials in firm size normalized with respect to the cut-off.[15] Finally ϵ_i is an idiosyncratic error term. Under the usual continuity assumptions (Hahn et al., 2001), the parameter β_1 identifies the effect of EPL on training.

Although RDD has been already used in the literature to identify the effect of EPL on training (Bolli and Kemper, 2017), in the Italian context a potential threat to identification comes from the existence of confounding factors that change discontinuously at the same cut-off as EPL. Among the latter, the two most notable confounders are the *Cassa Integrazione Guadagni Straordinaria* (CIGS) scheme, a short-term work programme featuring a redundancy fund system, and the right to constitute works councils in the firm. CIGS aims to help firms that are either in the process of reorganization and restructuring, facing a severe economic crisis or 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 CIGS.[16] In general, firms with a high share of workers under CIGS schemes are also likely to provide less training since their level of activity is decreasing. Conversely, works councils could positively affect a firm's training provision (Dustmann and Schönberg, 2009; Stegmaier, 2012), and Italian legislation grants workers the right to form works councils in firms with more than 15 employees (*Titolo III* of Workers' Statute).[17]

In this case, using an RDD, it would not be possible to separately identify the effect of EPL from the effect of the confounding policies.

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

[10] 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.

[11] 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).

[12] 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.

[13] 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.

[14] Another important labour market reform, the so called Italian Jobs Act, was introduced in 2015. The reform further lowered EPL by introducing a new open ended contract: "The new open ended contract introduced in March 2015 allowed for employment protection increasing with tenure, confining the possibility of reinstatement of workers to discriminatory dismissals, hence excluding this possibility for dismissals due to economic reasons (*motivo oggettivo*)." (Boeri and Garibaldi, 2019, p. 2). However, the last period considered in the main analysis precedes the Jobs Act.

[15] Thus the polynomial is allowed to be different on each side of the cut-off. At the cut-off $g(.)$ is left continuous with $g(0) = 0$.

[16] In turn, all firms, independently of firm size, may have access to *Cassa Integrazione Guadagni Ordinaria* (CIGO) scheme, aimed to help firms in a temporary financial crisis.

[17] While there are firms with fewer than 15 employees with a worker council, our data show a clear increase at the threshold in the likelihood that there is a worker council within a firm, even controlling for the number of employees. See Cardullo et al. (2020) for more details on this aspect of the institutional background.

However, Grembi et al. (2016) demonstrate that, even in the presence of confounding policies, if there is a *change of the policy of interest* (EPL in our case) over time, the effect can be estimated using a *diff-in-disc* design.[18] Using a parametric specification of *diff-in-disc* (cf. Cingano et al., 2016), the estimated equation reads as follows

$$y_{it} = \alpha_0 + \alpha_1(above_{it} \times post_t) + \alpha_2\ post_t + \alpha_3\ above_{it} + f(x_{it} - 15) + g(x_{it} - 15) \times above_{it} + \epsilon_{it}, \quad (2)$$

where we have added a time t subscript to all variables, with $t = 0$ for the pre-Fornero Reform (wave 2010 of the data described in the next section) and $t = 1$ for the post-Fornero reform (wave 2015). The variable $post_t$ is a dichotomous indicator that equals one in the period after the reform and zero otherwise.[19] All the other variables and polynomials are defined as in equation (1).

Under some identifying assumptions, the coefficient α_1 of the interaction $above_{it} \times post_t$ represents the causal effect of *relaxing EPL* on firm-provided training in the case of firms just above the threshold. In particular, Grembi et al. (2016) show that in order to identify the effect of the policy of interest, in addition to the usual continuity assumption (Assumption 1, in their paper), two other assumptions are needed. The assumption that the effect of the confounding policies in the case of no treatment (i.e. in the absence of the Fornero Law) is constant over time (Assumption 2 in Grembi et al., 2016), allows us to interpret α_1 as the local treatment effect (i.e. at the cut-off) of relaxing EPL on *firms subjected to the confounding policies*. If we introduce the further assumption that the effect of EPL at the threshold does not depend on the confounding policies (Assumption 3 in Grembi et al., 2016), then α_1 measures the causal effect of relaxing EPL in a neighborhood of the cut-off, i.e. the usual RDD estimand.

To test the validity of the *diff-in-disc* design, in online Appendix B we implement some tests suggested by Grembi et al. (2016). We check whether any manipulation of the running variable changes (or arises) over time by testing for the continuity of the *difference* in the densities before and after the Fornero Law (Assumption 1).[20] Assumption 2 is directly tested by running a placebo analysis for the period before the Fornero reform. Under the assumption that the effect of the confounding policies is time invariant, in the absence of treatment the *diff-in-disc* coefficient should be zero. However, with a *diff-in-disc* design and data on the confounding policies (i.e. use of CIGS and presence of works councils in the firm), one might directly test for both Assumption 2 and Assumption 3 of the *diff-in-disc* design. In particular, Assumption 2 can be tested by showing that the effect of the confounding policies on firm-provided training was not changing over time at the cut-off before the implementation of the Fornero Law (i.e. local parallel trends). This can be done by augmenting equation (2) with an interaction term between the confounding policies and the $post_t$ indicator.[21] Assumption 3 can be tested running a similar regression but including both periods before and after the Fornero Law (i.e. the 2010 and 2015 waves) and an interaction between the confounding policies and the $above_{it} \times post_t$ indicator.

Equation (2) 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, and excluding vs. including a vector of control variables, comprising sector-by-year and region-by-year fixed effects.[22] Moreover, as a robustness check, we follow Grembi et al. (2016) and we allow for the polynomial to differ not only above and below the threshold but also before and after the reform and we also include a triple interaction, which is clearly a more general and considerably more demanding specification than that in equation (2):

$$\begin{aligned} y_{it} = {} & \gamma_0 + \gamma_1(above_{it} \times post_t) + \gamma_2\ post_t \\ & + \gamma_3\ above_{it} + f(x_{it} - 15) + g(x_{it} - 15) \times above_{it} \\ & + h(x_{it} - 15) \times post_t + j(x_{it} - 15) \times (above_{it} \times post_t) + \epsilon_{it}. \end{aligned} \quad (3)$$

where $h(x_{it} - 15)$ and $j(x_{it} - 15)$ are two additional polynomials in normalized firm size.[23] Since the *diff-in-disc* design in the presence of heterogeneous effects provides a local average treatment effect (LATE) at the cut-off (Hahn et al., 2001), we are not able to estimate the effects of the Fornero Law on firm-provided training that are mediated by changes (e.g. increases) in firm size, i.e. our estimates apply to firms around the 15-employee cut-off. For the same reason, the estimated effects on the number of trained workers also correspond to the effects on the share of trained workers. Yet, in Section 6, we report panel fixed effects estimates that are similar to a DID design, and that also apply to firm sizes far from the cut-off.

A last comment is in order. In our baseline estimates we use two cross-sections that are representative of Italian firms in 2010 and 2015 (i.e. the years of the survey, but employment and training data used in the empirical analysis refer to the year before), 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. In particular, the identity of the firms above and below the cut-off, and more importantly for identification their characteristics may change. This may affect the population of compliers on which the *diff-in-disc* LATE effect is estimated. For this reason, firm dynamics are investigated in the online Appendix B where we also check for changes in the composition of the two cross sections around the cut-off implementing the test proposed by Carrell et al. (2018).

4. Data

We use two waves (collected in 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, *Istituto per lo sviluppo della formazione professionale dei lavoratori*), 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

[18] The combination of an RDD and a DID identification framework has also been referred to in the statistical literature as Comparative Regression Discontinuity Design (see, for instance Hendren et al., 2019; Kisbu-Sakarya et al., 2018). Unlike in Grembi et al. (2016) — who exploit the DID component in order to isolate the effect of other policies that change discontinuously at the cut-off— Kisbu-Sakarya et al. (2018) argue that the use of pre-treatment data may lead to increased statistical power compared to conventional RDD.

[19] The binary variable $post_t$ captures, among the other things, a common shock that might have hit firms post-reform. Moreover, in some specifications (see equation (3)) the effect of $post_t$ is allowed to change with the firm size variable, so as to capture the effect on training of any temporal shock that may have had a differential effect on firms of different sizes, such as changes in the availability of bank credit.

[20] 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 *diff-in-disc* 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., 2010; 2016), access to credit (Cingano et al., 2016), innovation performance (Acharya et al., 2014; Griffith and Macartney, 2014; Koeniger, 2005), use of temporary contracts (Hijzen et al., 2017), wages (Leonardi and Pica, 2013) and workers' mismatch (Berton et al., 2017).

[21] Although in this case $post_t$ represents a placebo post-reform period.

[22] 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.

[23] $g(.)$ and $j(.)$ are left continuous and $g(0) = j(0) = 0$.

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 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 smaller 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 retrospective information on training activities (in 2009 and 2014), which is usually unavailable in administrative data on firms or workers, and on firm size (along with the number of hirings and firings). Further information is available on the presence of works 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 (but not retrospectively, only for the survey years 2010 and 2015). 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.[24]

Two features of the RIL survey are worth mentioning. First, firm size is provided in discrete units, i.e., head count (excluding collaborators). Unlike employment size, which is available for 2009, 2010, 2014 and 2015, the *composition* of employment, in terms of part-time and full-time workers and type of contracts refer to the year of the survey only (2010 and 2015) while training data, as we said, refers to the year before (2009 and 2014). 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 strictly relevant for the application of Article 18, but we are forced to use employee headcount for the same years.[25] To get a sense of the potential impact of measurement error, we check the sensitivity of our estimates to excluding firms with 16, 16–17 and 16 to 18 employees in a donut-hole type of regression (see Section 6) because, given the incidence of part-time and temporary employment around the cut-off, they are the most likely to 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).

A second data feature deserving attention at this stage is that the forcing variable, i.e., self-reported firm size, is characterised by a higher frequency of firm sizes that are multiple of five. On the one hand, a possible reason is heaping, i.e. rounding by the individual that was interviewed in the firm. On the other hand, multiples of five are also relatively more frequent in the firm size distribution reported in papers using administrative data on National Social Security archives, such as in Cingano et al. (2016).[26] 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'.

Table 1
Descriptive statistics.

Variable	Mean	Std. Dev.
nr. of employees		
wave 2010 (year 2009)	10.98	4.50
wave 2015 (year 2014)	10.80	4.59
nr. of trained workers		
wave 2010 (year 2009)	2.26	4.28
wave 2015 (year 2014)	3.54	5.06
nr. of temporary workers		
wave 2010 (year 2010)	1.23	2.13
wave 2015 (year 2015)	1.12	2.79
nr. of permanent workers		
wave 2010 (year 2010)	9.70	4.64
wave 2015 (year 2015)	9.54	4.95
excess worker turnover		
wave 2010 (year 2009)	0.51	1.22
wave 2015 (year 2014)	0.44	0.78

Note. Descriptive statistics use sample weights and are calculated for the sample used in the regression reported in column (1) of Table 2. See Section 4 for the timing of all variables. Employees represents the total number of employees. Trained workers represents the number of workers trained. We imputed trained workers equal to employees when the number of trained was greater than the number of employees; and we imputed 0 when this information was missing. Excess worker turnover is calculated at the firm level following Hijzen et al. (2017) as follows: $EWT = 2 \cdot \min(H, S)/E$, where H and S are the number of hiring and separations, respectively, and E is average firm employment.

Not being sure on the real amount of measurement error, in our preferred empirical specifications we use the entire available data, since *diff-in-disc* designs, as RDDs, are data-intensive, but in some sensitivity checks we also show that the baseline results are robust to dropping observations with multiples of five in employment size.

In what follows, we describe our sample selection procedure. We begin with 24,459 observations for the 2010 wave and 30,091 for the 2015 wave. We drop firms that have implausible or inconsistent (i.e. negative, of different order of magnitudes across waves or missing values coded as strings) 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 waves, and the whole sample is 54,271 observations. For 10,214 firms, we have two observations (panel), while the remaining firms (14,049 and 19,794 for the 2010 and 2015 waves, respectively) represent a repeated cross section. In the econometric analysis, we restrict the sample to firms sized in the 6–25 employee range (although we present robustness checks using different bandwidths); 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.[27] Finally, 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 2.

5. Main results

This section reports our pooled cross-section baseline estimates of the effect of EPL on firm-provided training using the number of trained workers as the outcome variable. In the first four columns of Table 2, we report estimates with a polynomial in firm size that is allowed to

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

[25] According to Italian legislation, part-time workers count as less than one full-time employee and proportionally to the number of hours worked, when defining the 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 counted as far as the definition of the threshold is concerned, while apprentices should be excluded. Some measurement error is also present in papers using National Social Security archives such as Leonardi and Pica (2013) or Hijzen et al. (2017), which do not have full information on individual working hours and make some simplifying assumptions to compute "legal" firm size. Hijzen et al. (2017), for instance, assume that part-time workers work half time (50%).

[26] Unfortunately, administrative data on firm size is not available in our sample.

[27] This can be computed because each wave reports employment data for the year of the survey and the period before.

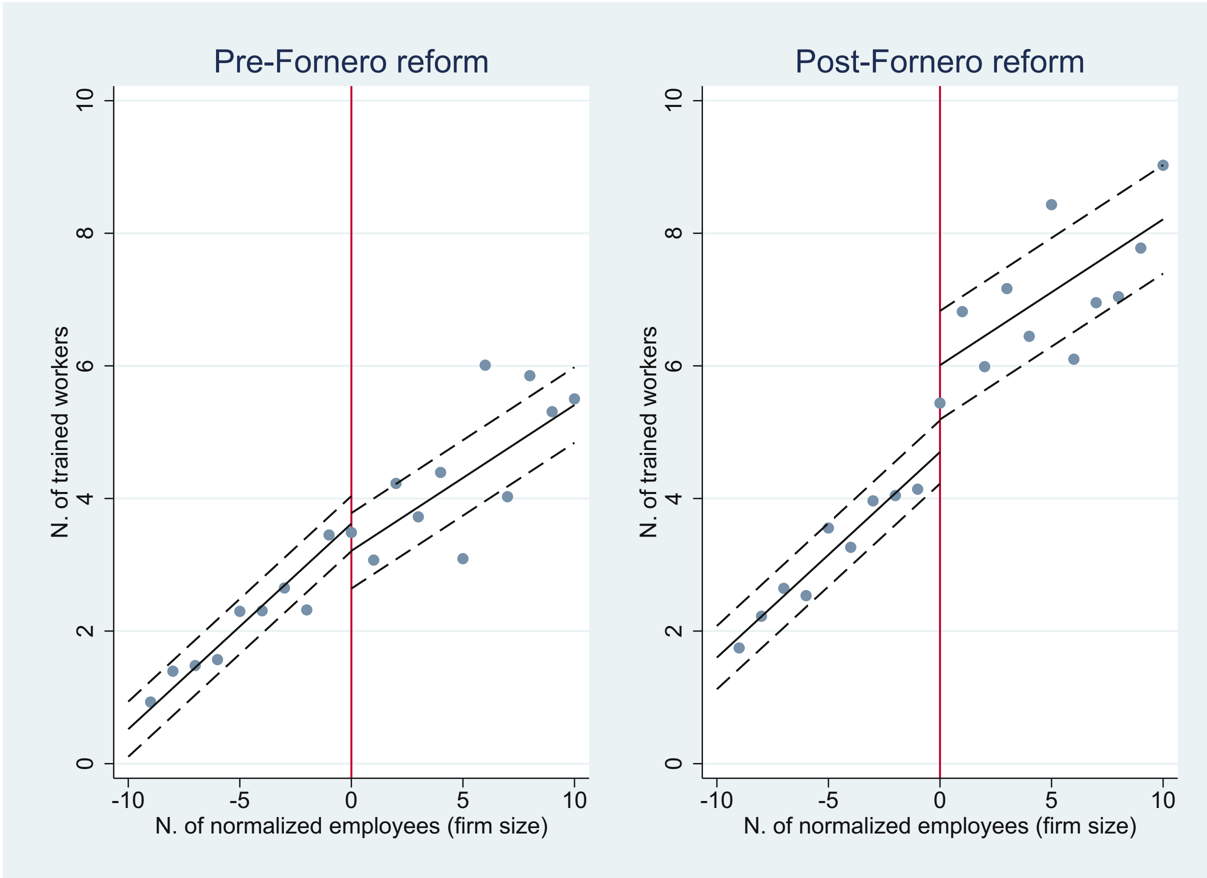

Fig. 1. Firm size and *observed* training provision before and after the Fornero reform Note. The figure presents a scatter plot for the average number of employed workers by one employee-bins of firm size (computed using survey weights) before and after the Fornero reform as well as the fitted (solid) line of a regression of the number of trained workers on normalized employment (see column (1) of Table 2) and the 95% confidence interval (dashed lines). Normalized employment is reported on the horizontal axis, with "0" corresponding to the cut-off (i.e., firm's employment level equal to 15). The scatter plot is presented for the bandwidth 6–25 employees of firm size (i.e., normalized size between - 10 and 10).

differ on each side of the cut-off but that is instead assumed to take on the same coefficient before and after the reform. We also include (exclude) sector and region fixed effects (which sometimes we will refer to as 'firm controls'), 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.[28] The magnitude of the discontinuity can also be appreciated from Fig. 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.[29] The estimates are not sensitive to the inclusion of region and sector fixed effects, as shown in column (2), with a slightly smaller estimated effect of 1.5 (our preferred estimate).[30]

In the remaining columns, we estimate the model in equation (2), 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 larger 6–50 bandwidth for which a linear term in firm size may be insufficient to fit the data (see Table A1 in the online 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 before the reform. 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 more on temporary contracts.[31] How-

[28] All estimates and descriptive statistics are computed using RIL sampling weights.

[29] Fig. 1 shows a slight increase in training provision for firms with 15 employees, which, strictly speaking, should be not affected by the Fornero reform. This is however consistent with some heaping that we observe in the data, and the fact that some firms might have rounded their self-reported size from 16 to 15. This issue is discussed in the next Section. In particular, estimates sensitivity to dropping observations close to the cut-off (including exactly at the cut-off) is investigated through donut-hole regressions.

[30] The number of observations between models omitting and including control variables differs by few units owing to missing values in the controls. We always report in the tables the estimates in the largest possible samples with non-missing values.

[31] 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 that firing decreases (increases) with size for permanent (temporary) workers; moreover, hiring is

Table 2
Baseline pooled-cross section 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	7851	7836	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. The dependent variable is the number of trained workers. The analysis uses the 2010 and 2015 RIL waves. Polynomials in employment have been interacted with the dummy *above* (15-employee threshold). The number of observations between models omitting and including control variables may differ by few observations owing to missing values in the controls.

ever, temporary workers tend to receive less 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. Alternatively, the reduction in the gap between the EPL enjoyed by permanent relative to temporary employees might have increased the incentives for firms to train more temporary workers as argued by Dolado et al. (2016). Mechanisms are investigated in Section 7.

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.

All potential threats to identification are discussed in the online Appendix B.

6. Robustness checks

We conduct several robustness checks in Table 3. First, because there is evidence of heapings in the forcing variable at multiples of five employees, we follow Barreca et al. (2016) and drop firms with 10, 15, 20 and 25 employees from the estimation of equation (2). Reassuringly, the results reported in columns (1) and (2) of Table 3 and those in columns (1) and (2) of Table 2 are very similar.[32] 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.[33] Third, in columns (5) to (8), we 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 *diff-in-disc* 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 (3). The results in columns (9) and (10) (linear polynomial) confirm the magnitude of the effect, which is equal to 1.6 and 1.4, depending on the exclusion 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.

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 we report in the online Appendix B, by running a set of Schivardi and Torrini (2008) tests and the test in the change in densities before vs. after the reform, we do not find clear evidence that the reform increased the propensity for firms to grow in size or cross the 15-employee threshold. In what follows, 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 4, we report estimates of equations (2) and (3) 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 pooled cross-sectional estimates, of approximately 1 additional trained worker. In contrast, in the more general specification reported in columns (7) and (8), where we estimate equation (3), the coefficient

somewhat reduced at the threshold, with the emergence of an asymmetric U-shaped relationship between hiring and firm size.

32 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 the size as if it were discrete, we also re-estimate equation (2) by clustering standard errors by number of employees 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.

33 In regressions that are not reported but available from the authors upon request, we have re-estimated all models in Tables 2, 3 and 4, and the results are generally consistent.

Table 3
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	All	All	All	All
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. The dependent variable is the number of trained workers. The analysis uses the 2010 and 2015 RIL waves. 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); and the fake threshold in columns (5) and (6) is set at 10 employees while that in columns (7) and (8) is set at 20 employees. In columns (9) to (12) polynomials in employment have been interacted with the dummies *above* (15-employee threshold), *post*, and the *above*×*post*; in the table, these interactions are referred to as "All". The number of observations between models omitting and including control variables may differ by few observations owing to missing values in the controls.

Table 4
Panel estimates.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Baseline panel		Heaping		Donut		Interaction post		Non-switchers	
post	1.360***	2.386*	1.217***	2.130	1.231***	2.060	2.250***	3.029**	1.376***	1.428
	(0.125)	(1.245)	(0.135)	(1.396)	(0.126)	(1.386)	(0.363)	(1.261)	(0.126)	(1.052)
above	-0.465	-0.443	-1.301*	-1.131	-1.359	-1.134	-0.916	-0.997		
	(0.692)	(0.688)	(0.774)	(0.757)	(1.177)	(1.174)	(0.827)	(0.823)		
post×above	1.027**	0.838*	1.424**	1.222**	1.163*	0.993	1.858*	1.869*	1.036*	0.832
	(0.500)	(0.495)	(0.587)	(0.579)	(0.615)	(0.610)	(1.002)	(0.988)	(0.556)	(0.550)
Bandwidth	(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
Pol. inter.	Above	Above	Above	Above	Above	Above	All	All	Above	Above
Sec.×year f.e.	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Reg.×year f.e.	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Firm f.e.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	5754	5732	3778	3766	4232	4220	5754	5732	4994	4976
R-squared	0.754	0.760	0.767	0.774	0.760	0.766	0.756	0.762	0.752	0.759

Note. Clustered standard errors at the firm level in parentheses, *** p<0.01, ** p<0.05, * p<0.1. The dependent variable is the number of trained workers. The analysis uses the 2010 and 2015 RIL waves. In columns (3) and (4), we drop multiples of 5 employees (heaping), and in columns (5) and (6), we drop firms with 14, 15, 16 employees (donut). In columns (7) and (8) polynomials in employment have been interacted with the dummies *above* (15-employee threshold), *post*, and the *above*×*post*; in the table, these interactions are referred to as "All". Finally, columns (9) and (10) estimate the baseline models of columns (1) and (2) but restricted to the sample of firms that remained above or below the cut-off in both waves ("non-switchers"). The number of observations between models omitting and including control variables may differ by few observations owing to missing values in the controls.

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 3, 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 columns (9) and (10) we report results for regressions in which we exclude those firms in the panel that have crossed the threshold between the 2010 and 2015 survey waves in either direction, so that we can keep the samples of treated and untreated firms unaltered before and after the reform. This can also be important because the change in the incentives to train workers induced by the Fornero Law could depend on the number of years a firm is actually exposed to these changes, i.e. the number of periods the firm is above the cut-off.[34] When we do that, our fixed-effect estimates suggest that the Fornero reform might have determined an increase of approximately one additional trained worker at the threshold (about 21%), an effect that is almost identical to the panel results reported in columns (1) and (2) of Table 4 on the full sample.[35] It is worth noting that in this case the $above$ dummy does not change over time, the polynomials in firm size drop from the equation (unless they are interacted with $post$), and identification only stems from within-firm pre- vs. post-treatment differences like in a DID design.

A potential threat to our *diff-in-disc* estimates may come from the misclassification of the $above$ indicator, since we are not able to precisely measure the firm legal size (see Section 4). The measurement error is likely to depend on the number of temporary workers (working less than 9 months) and of part-time workers. Computing the average share

[34] Specifically, firms above the cut-off in 2009 and that are above the cut-off in 2014 are more likely to have been above the cut-off in all periods, and to have been "fully" exposed to the EPL reform. Conversely, in the cross-section sample we do not have any measure of the amount of firms' "exposition" to the reform.

[35] We drop 760 firm-year observations, which represents about 13 per cent of our panel sample. This indicates that most firms do not cross the cut-off in either direction between the two periods.

Table 5
Measurement error.

	(1)	(2)	(3)	(4)	(5)	(6)
	Drop 16		Drop 16 and 17		Drop 16, 17 and 18	
post	1.084***	-0.964*	1.084***	-1.039*	1.084***	-0.912
	(0.137)	(0.555)	(0.137)	(0.561)	(0.137)	(0.562)
above	-0.357	-0.344	-0.499	-0.485	-0.699	-0.706
	(0.502)	(0.486)	(0.686)	(0.650)	(1.119)	(1.042)
post×above	1.538***	1.407***	1.717***	1.584***	1.593***	1.454**
	(0.470)	(0.447)	(0.531)	(0.503)	(0.603)	(0.569)
Bandwidth	(6–25)	(6–25)	(6–25)	(6–25)	(6–25)	(6–25)
Polynomial	Linear	Linear	Linear	Linear	Linear	Linear
Pol. inter.	Above	Above	Above	Above	Above	Above
Sec.×year f.e.	No	Yes	No	Yes	No	Yes
Reg.×year f.e.	No	Yes	No	Yes	No	Yes
Observations	15,894	15,875	15,348	15,329	14,840	14,823
R-squared	0.108	0.145	0.107	0.145	0.105	0.143

Note. Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1. The dependent variable is the number of trained workers. The analysis uses the 2010 and 2015 RIL waves. Polynomials in employment have been interacted with the dummy *above* (15-employee threshold). The number of observations between models omitting and including control variables may differ by few observations owing to missing values in the controls.

of workers for each firm size bin just above the cut-off, we find percentages of temporary (part-time) workers of 12.4 (11.5), 12.4 (14.3), 13.1 (11.7), 11.4 (17.7) and 12.0 (14.5) for firms with 16, 17, 18, 19 and 20 employees, respectively. On the basis of these figures, we put forward that firms are more likely to be misclassified in the 16, 17 and 18 size bins. Thus Table 5 reports donut-hole estimates dropping the 16, 16–17 and 16 to 18 firm size groups. Reassuringly, the estimates are very robust and similar to the ones obtained in the full sample in Table 2, suggesting that firm size is unlikely to be affected by substantial measurement error.

7. Potential mechanisms

As we have mentioned in the literature review section, scholars have suggested that in the case 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.

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 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 prediction finds strong support from the empirical studies of Hijzen et al. (2017) for Italy and Centeno and Novo (2012) for Portugal.

If this is a correct representation of what happens in a strongly dual labour market, then in light of the widespread evidence that temporary workers receive less training (Albert et al., 2005; Arulampalam and Booth, 1998; Arulampalam et al., 2004; Booth et al., 2002), one could argue that stricter EPL might cause lower training by firms, with the mediating factors being the excess use of temporary contracts and worker turnover. Clearly, 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 use 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.

Table 6
Excess worker turnover and number of permanent workers.

	Dependent variable			
	(1) excess worker turnover	(2)	(3) number of permanent workers	(4)
post	0.391***	0.486***	-3.013***	-3.557***
	(0.092)	(0.092)	(0.629)	(0.725)
above	0.098***	0.025	-0.656**	-0.484
	(0.032)	(0.051)	(0.265)	(0.433)
post×above	-0.104**	-0.135*	0.504	1.735**
	(0.049)	(0.075)	(0.612)	(0.738)
Bandwidth	(6–25)	(6–25)	(6–25)	(6–25)
Polynomial	Linear	Quadratic	Linear	Quadratic
Pol. inter.	All	All	All	All
Sec.×year f.e.	Yes	Yes	Yes	Yes
Reg.×year f.e.	Yes	Yes	Yes	Yes
Observations	10,724	10,724	16,508	16,508
R-squared	0.197	0.205	0.737	0.738

Note. Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1. The analysis uses the 2010 and 2015 RIL waves. 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 the average firm employment. Number of permanent workers represents the number of permanent contracts. Polynomials in employment have been interacted with the dummies *above* (15-employee threshold), *post*, and *above*×*post*; in the table, these interactions are referred to as "All".

To further explore the above conjectures, in Table 6, we report estimates of equation (3) using as dependent variable excess worker turnover. 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 (in 2009 and 2014 for the 2010 and 2015 waves, provided by RIL), respectively, and E is the average firm employment.[36] The results displayed in columns (1) and (2) point towards a negative ef-

[36] 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).

fect of the reform on excess worker turnover for firms above the threshold.[37]

In order to make a better test for the substitution mechanism between temporary and permanent workers at the cut-off, in columns (3) and (4) we report estimates of the same model but using the number of permanent workers as our dependent variable.[38] Interestingly, our coefficient of interest suggests that the number of permanent workers increased by about 1.7 units at the cut-off (+12.5%).[39] Moreover, this finding also suggests that the increase of 1.5 additional trained workers at the threshold reported in Table 2 could be entirely explained by the substitution of permanent versus temporary employees, if we assume, as the relevant literature surveyed above seems to suggest, that permanent workers receive more training than temporary ones and in particular that new permanent workers are likely to be trained by firms (as new permanent hires or contract conversions). Our results are in line with O'Higgins and Pica (2020), who, using administrative data and a DID design, find that the Fornero reform brought about an increase in permanent contracts of about 5%.[40]

To further investigate whether the increase in training is driven by new hirings, we also estimated equation (2) by interacting the *diff-in-disc* coefficient with the number of new hirings in the year 2014, but the interaction (see Table A3 in the online Appendix A) turns out to be statistically insignificant. So, at least at the cut-off, the increase in training after the Fornero Law does not seem to be larger for firms that hired new workers. However, owing to the lack of separate information on hirings by type of contract we cannot estimate a similar specification including the interaction with the number of hirings of permanent workers or the number of conversions from temporary to permanent contracts. Interestingly, O'Higgins and Pica (2020) do not find any evidence that the Fornero Law induced higher *conversion* rates. Although their DID estimates apply to all firms above 15 employees and not specifically to those around the cut-off, their results suggest that the increase in training that we find is more likely to be explained by *new permanent hires* than by conversions.[41]

The empirical results in Table 6 provide some evidence that before the Fornero Law, both the excess worker turnover and the number 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 (Cahuc et al., 2019). 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., high worker turnover and excess reliance on temporary positions) tend to fall, as the empirical results in Table 6 somehow suggest.

Unfortunately, RIL only provides the number of trained workers without distinguishing between workers in temporary and workers in open-ended contracts. Thus, we cannot test directly the mechanisms put forward by Dolado et al. (2016), i.e. an increase of training for temporary workers after the Fornero Law, which may be another explanation for the rise in firm's training provision.

However, the existence of a heterogeneous treatment effect associated to the share of temporary workers might provide some indirect evidence of Dolado et al. (2016)'s hypothesis: in particular, for the latter to explain our main empirical result, one should find that the increase in the number of trained workers at the threshold after the reform is stronger in those firms that used to employ a larger share of temporary workers. Therefore, in regressions not reported but available from the authors upon request, we have used the panel component of the dataset and estimated an equation otherwise identical to that in column (1) of Table 4, but allowing the effect of the reform to change with the share of temporary workers in the previous wave: however, the interaction between $post \times above$ and the (lagged) share of temporary workers has a small and not statistically significant coefficient. All in all, this result might be seen as indirect evidence that the increase in the number of trained workers at the threshold following the Fornero reform is at least consistent with the mechanism based on the replacement (at the cut-off) of temporary with permanent positions, with the latter that tend to receive more training.

8. Conclusions

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 Difference in Regression Discontinuities design. 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 Article 18 to of the Workers' Statute.

Our preferred 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.

Concerning the mechanisms linking lower EPL to increased firm-provided training, our results 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 excessive worker turnover and greater use of temporary workers, as theoretically predicted by Cahuc et al. (2016) for economies with a two-tier labour market and that this gap decreased after the reform. We show that, firms exposed to an EPL reduction induced by the Fornero Law raised the number (and share) of permanent workers at the cut-off. Moreover, the positive effects on permanent workers and the number of trained workers are very similar in magnitude. Thus, although our data does not enable us to test it explicitly, temporary-permanent worker substitution could explain alone the whole effect on firm-provided training at the cut-off.

Our findings provide an additional explanation for why two-tier reforms could 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.

[37] In this case it is important to allow the polynomial in employment to vary both above-below the threshold and pre-post reform: if we estimate the specification in equation (2) the effect is not statistically significant.

[38] As already mentioned, employment composition is available for 2010 and 2015.

[39] Since with the *diff-in-disc* design we are identifying the effect around the 15-employee cut-off (i.e. keeping the firm size "constant"), we also find a mechanical negative effect of the same magnitude on the number of workers employed with fixed-term contracts (available upon request), thus confirming the results of Centeno and Novo (2012) for Portugal.

[40] In regressions not reported but available upon request, we check if the reduction in EPL entailed by the Fornero Reform had any effect on the skill and age composition of the workforce, and in particular, we look at the share of workers with a university degree or alternatively those younger than 25 years of age. Our results, which are available upon request, show that the interaction dummy is positive but not statistically significant, suggesting that the Fornero reform did not significantly change neither the skill composition nor the age structure of workers, at least at the threshold.

[41] To get a sense of the potential effect of the reform far from the cut-off, we report in Table A4 in the online Appendix A estimates similar to those in columns (9) and (10) of Table 4 (i.e. panel fixed effects estimates on the non-switchers, for which the *above* indicator is *de facto* fixed in 2009) using temporary and permanent employment as the dependent variables, and obtain significant estimates only for the latter. The estimated coefficient suggests that the Fornero Law might have increased the average number of permanent employees above the cut-off (in the 16–25 firm size range) by 0.44 units (about +2.5%).

Our analysis suggests 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 1990s, when a stronger dualism emerged in the labour market.

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Supplementary material

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