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ISSN: 2365-9793

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ABSTRACT

Employment Protection and Firm-Provided Training: Quasi-Experimental Evidence from a Labour Market Reform

In 2012, a labour market reform in Italy known as the Fornero Law substantially reduced firing restrictions for open-ended contracts in the case of firms with more than 15 employees. 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 was introduced, 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 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 via temporary contracts.

JEL Classification: J42, J63, J65, M53

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

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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., by lowering productivity and possibly affecting 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 (Dolado 2016, Boeri and Garibaldi 2019), which may be partly related to political economy considerations (Saint-Paul 1997). Moreover, 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, Cingano et al. 2010; 2016, Hijzen et al. 2017, Bjuggren 2018, Bentolila et al. 2019).[2] However, other scholars have reported that EPL may increase labour productivity, such as through a positive impact on innovation (Koeniger 2005, Acharya et al. 2014, Griffith and Macartney 2014) 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 seek to provide 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 new clean evidence on the causal

[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.

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 using a Difference-in-Regression-Discontinuities design (DRDD).

The Italian legislation included size-contingent firing restrictions for open-ended contracts, and based on these restrictions, the 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 substantially differentiate firing costs according to firm size. 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 (Law n. 92 of 28 June 2012). Combining the different levels of EPL below and above the 15-employee cut-off mandated by the Workers' Statute 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 DRDD (see, for instance, Cingano et al. 2016, Grembi et al. 2016). 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.[3]

The conceptual background for our analysis can be found in three main streams of literature that provide some potential explanations for the link between EPL and training. The first stream relates to firms' incentives to provide general training in the presence of imperfect labour markets. Acemoglu and Pischke argue that when labour market frictions compress the wage structure, firms may pay for investments in general training (Acemoglu and Pischke 1999b;a). A second stream of literature is related to the training gap between permanent and temporary workers (Arulampalam and Booth 1998, Booth et al. 2002, Arulampalam et al. 2004, Albert et al. 2005, Cabrales et al. 2017, Ferreira et al. 2018) and the expansion of temporary contracts in dual labour markets (Schivardi and Torrini 2008, Centeno and Novo 2012, Cahuc et al. 2016; 2019, Hijzen et al. 2017), and it indicates that the greater use of temporary contracts should entail lower training. The third stream of literature is constituted

[3] 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).

by the recent theoretical models that investigate the interplay between firms' training decisions and the use of temporary contracts in dual labour markets, which suggest that a change in EPL may influence training provisions even in the absence of a greater use of temporary contracts. If temporary contracts are a 'port of entry' into more permanent employment positions and temporary workers are trained to increase their productivity as permanent workers, then strict EPL on open-ended contracts may induce lower job efforts by permanent workers, thereby pushing firms to simultaneously reduce temporary-to-permanent conversion rates and training provisions (Dolado et al. 2016). Moreover, in the presence of skill and contract heterogeneity, the low duration of temporary contracts may lead to low training levels, with high-skill positions in permanent contracts investing in specific human capital and low-skill positions investing in general human capital in temporary positions (Wasmer 2006, Choi 2019).

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 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 was approximately 3.1 trained workers 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 and placebo regressions, among others.

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 DRDD in a quasi-experimental setting, namely, leveraging a labour market reform that changed the level of EPL for open ended contracts in the case of larger firms over time. This approach represents an improvement over the existing literature 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 and the existence of short-work programs for employees in firms under severe economic difficulties), 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 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 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.

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 that increases in firing costs should also lead to reduced worker turnover and higher worker tenure (Boeri and Jimeno 2005, Kan and Lin 2011) by 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 (1999b), 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 1999b).

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 (Arulampalam and Booth 1998, Booth et al. 2002, Arulampalam et al. 2004, Albert et al. 2005, Cabrales et al. 2017, Ferreira et al. 2018). In turn, much less empirical evidence exists on the relation between EPL and firms' training investments.

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 investment, 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.[4] Temporary workers cannot be renewed, and at

[4] 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

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*, and temporary workers reduce their effort level based on the credible 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.[5] Using a large firm-level dataset across developing countries, Almeida and Aterido (2011) show that stricter enforcement of labour regulations is significantly associated

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.

[5] 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 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.

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 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 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 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.

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.[6]

Before the legislative changes that occurred in 2012 (Fornero Law) and 2015 (Jobs Act),[7] 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.[8] 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).[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

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

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

[8] 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.

[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.

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 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.

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

[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) for the exact formula.

[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.

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.[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 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 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. Moreover, following the arguments in Dolado et al. (2016), a decrease in the gap in EPL between permanent and temporary employees may increase a firm's incentives to train temporary workers. Thus, we can exploit the Fornero Law as a quasi-experiment to carry out a DRDD: the causal effect of EPL on firm-provided

[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] 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.

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).[15] 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 (Dustmann and Schönberg 2009, Stegmaier 2012), and Italian legislation allows workers the right to form worker councils in firms with more than 15 employees. Another potential confounding factor that may interfere with EPL is the so-called *Cassa Integrazione Guadagni Straordinaria* (CIG), which is a short-term work program that consists of a worker redundancy fund extraordinary scheme. This program 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 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. Hence, neglecting these confounders acting at the cut-off would potentially lead to an overestimate of the effect of EPL when using an RDD.

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

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

where i is the firm subscript, t is the survey wave subscript ($t = 2010, 2015$), and y_{it} is

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

the number of trained workers. Our data refer to two cross-sections that are 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.[16] Finally, ε_{it} is a firm error term.[17]

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 for the polynomial to differ not only above and below the threshold but also before and after the reform, which is clearly a more general and considerably more demanding specification than that in equation (1):

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

While we refer to Section 5.1 for a discussion of the validity of our research design, we anticipate in this section that the equations (1) and (2) identify the causal effect of EPL on firm-provided training as long as one can assume that the Fornero Law did not systematically

[16] 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.

[17] Recall that the dummy $post_t$ captures, among the other things, any type of shock that might have hit firms in 2015 relative to 2010. Moreover, because in some specifications (see below) the effect of the dummy $post_t$ is allowed to change with the firm size variable, this dummy also captures 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.

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).[18]

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-hole type of regression (see above). Moreover, we also check that our results are robust if we cluster standard errors, using the number of employees as the clustering variable, as suggested by Lee and Card (2008) when the researcher uses a forcing variable

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

that 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, and since DRDDs, 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, *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 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 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.[19]

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, 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 2010 and 2015, respectively) represent a repeated cross section. In the econometric analysis, we restrict the sample to firms sized in the 5-26 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, 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 changes of this sorting after the reform.[20] 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 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

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

[20] 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.

out the test by estimating the following equation using a linear probability model (LPM):

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

with

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

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);[21] while 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, then it should remain the same before and after the policy change.[22] 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 it does not show any statistically significant change in sorting after the reform. The result does not change if we consider a cubic polynomial (not shown in the table) or if we allow the polynomial to differ on both sides of the threshold. In columns (3) and (4), we repeat Schivardi and Torrini's test on the panel component of

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

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 results 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.[23]

[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.[24] In the first four columns of Table 3, we report estimates with a polynomial in firm size that is allowed to differ on

[23] 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, Acharya et al. 2014, Griffith and Macartney 2014), use of temporary contracts (Hijzen et al. 2017), wages (Leonardi and Pica 2013) and workers' mismatch (Berton et al. 2017).

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

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 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).[25]

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 larger 6–50 bandwidth for which a linear trend may be insufficient to fit the data (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.[26] However, temporary workers tend to receive less training. The Fornero Law, by reducing the wedge in the degree of EPL enjoyed by perma-

[25] 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.

[26] 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 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.

nent 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).

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]

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.[27] 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.[28] 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.

Another placebo analysis (available from the authors upon request) has been performed to re-run our baseline specification but with data for the two RIL waves of 2005 and 2007:[29]. The idea of this robustness check is that we should not expect any change in the propensity to train workers at the threshold over time, which is exactly what we find since the change in the number of trained workers at the threshold is virtually zero, both economically and statistically.

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. The results in columns (9) and (10) (linear polynomial) 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

[27] 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 (1) 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.

[28] In regressions that are 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.

[29] We do not pool these two waves with the 2010 and 2015 waves because we prefer not to mix pre and post crisis data. Moreover, the sectoral classification changed in 2010, which would complicate the inclusion of sector specific fixed effects in the regressions.

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, 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.[30] 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.

6 Potential mechanism: worker turnover and temporary contracts

As we have mentioned in the literature review sections, 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 Spain. 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 (Arulampalam and Booth 1998, Booth et al. 2002, Arulampalam et al. 2004, Albert et al. 2005), one could argue that stricter EPL might cause lower training by firms, with the mediating factors being 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

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

employees at the cut-off.

To further explore the above conjectures, 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.[31] 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.[32] 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 number of workers with fixed-term contracts is reduced in the case of the specification in 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 about 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.[33]

The empirical results in Table 6 also provide some weak 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

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

[32] 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).

[33] In regressions not reported, 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 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 FR did not significantly change the skill composition of workers, at least at the threshold.

between workers in temporary contracts and workers in open-ended contracts. Thus, we cannot test the mechanisms put forward by Dolado et al. (2016), which may be another explanation for the increase observed in training provision.

[Table 6 about here]

7 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 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 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 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 Fornero reform. In other words, in labour markets that have significant asymmetry in the degree of EPL 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, 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. Unfortunately, because of data limitations, we cannot test whether the reduction of EPL for open-ended contracts increased the training provisions for temporary workers as indicated by the theoretical model in Dolado et al. (2016). A thorough exploration of this mechanism is therefore left for future work.

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.[34] 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 1990s, when a stronger dualism emerged in the labour market.

Acknowledgments. This is a substantially revised version of our IZA discussion paper 11339 (February 2018), titled "Employment Protection, Temporary Contracts and Firm-Provided Training: Evidence from Italy" (Bratti, Conti and Sulis) and it also incorporates results from a mimeo titled "Does Reducing Employment Protection Affect Worker Training? New Firm-Level Evidence from a Labour Market Reform" written by the same authors and circulated at various conferences after July 2018. We thank Andrea Ricci for his valuable help with the data and the *Istituto Nazionale per l'Analisi delle Politiche Pubbliche* (INAPP, formerly ISFOL) for giving us access to them. Comments received by Fabio Berton, Diogo Britto, Pierre Cahuc, Lorenzo Cappellari, Guido De Blasio, Francesco Devicienti, Carlo Devillanova, Juan Dolado, Andrey Launov, Marco Leonardi, Sandra McNally, Lia Pacelli, Giovanni Pica, Matteo Picchio, Matteo Sandi, Vincenzo Scrutinio, Daniela Sonedda, Stefano Staffolani, Konstantinos Tatsiramos, by participants to the workshops 'Rigorous impact evaluation in Europe' (Turin),'The Effects of Employment Protection and Collective Bargaining on Workers and Firms' (Cagliari), the IZA Workshop on Labor Market Institutions (Bonn), in seminars at the Joint Research Centre (Ispra), MILLS (Milan Labor Lunch Seminar, University of Milan), Centre for Vocational Education Research (London School of Economics) and University of Siena, at conferences in Berlin (IZA World Labor Conference and COMPIE), Cologne (EEA), Lyon (EALE), Ancona and Novara (AIEL), Bologna (SIE) and Turin (SIEP) are gratefully acknowledged. Part of this work was carried out while Giovanni Sulis was visiting the University of New South Wales, Sydney: we thank that institution for its hospitality. Giovanni Sulis also acknowledges financial support from the University of Cagliari (Fondazione di Sardegna fundamental research grant L.R. 7/2007, Dynamics of Human Capital Accumulation and Skill Biased Technological Change). The usual disclaimer applies.

[34] 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 for the sample used in the regression reported in column (1) of Table 3. 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. 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 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.

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
Sample	cross-section	cross-section	panel	panel
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, and 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%, respectively).

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%, respectively).

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), and 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 to 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%, respectively).

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); 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. 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 to 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%, respectively).

Table 6: Excess worker turnover and number of temporary workers

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
dependent variable		excess worker turnover				number of temporary contracts		
post	0.347***	0.347***	0.391***	0.486***	2.104***	2.098***	2.518***	3.030***
	(0.0770)	(0.0751)	(0.0924)	(0.0920)	(0.540)	(0.536)	(0.749)	(0.883)
above	0.0687**	-0.0174	0.0983***	0.0249	0.587***	-0.423	0.824***	0.435
	(0.0268)	(0.0375)	(0.0316)	(0.0513)	(0.221)	(0.552)	(0.240)	(0.373)
post×above	-0.0426	-0.0410	-0.104**	-0.135*	-0.005533	-0.0110	-0.485	-1.804***
	(0.0263)	(0.0262)	(0.0486)	(0.0755)	(0.479)	(0.468)	(0.737)	(0.795)
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	0.192	0.196	0.194	0.199

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 the average firm employment. Number of temporary workers represents the number 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 to as "both". We exclude firms at the 5th and 95th percentile of the distribution of growth of employment (below and above 50%, respectively).

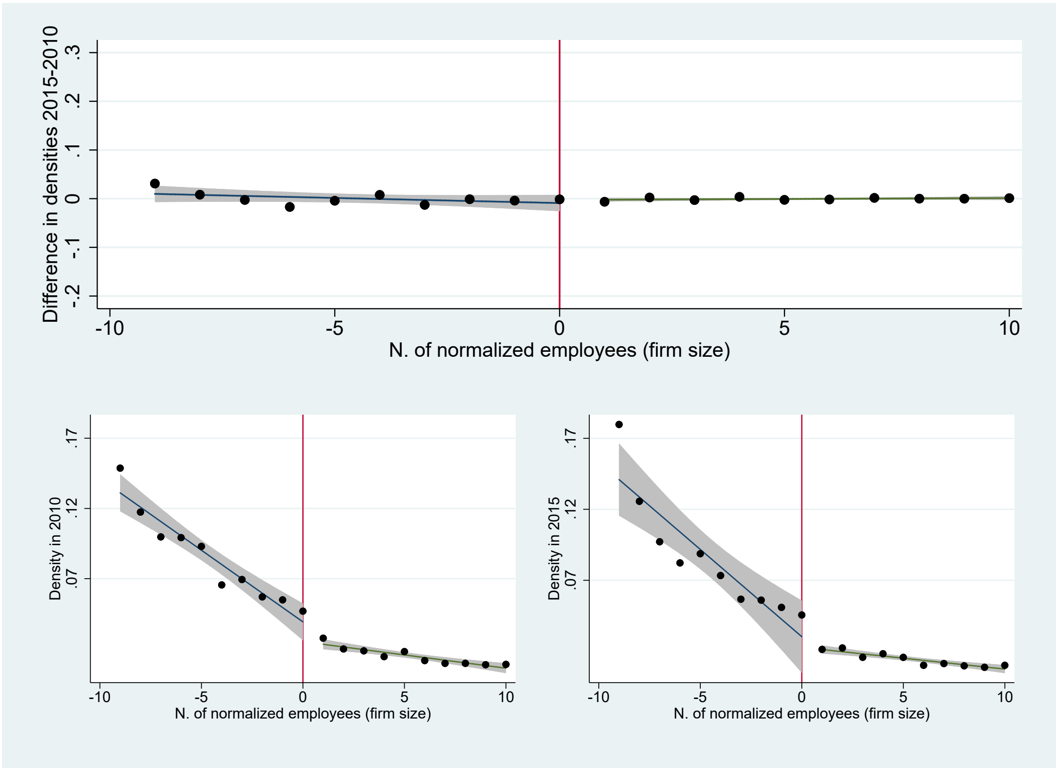
A Additional results

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%, respectively).

Figure 1: Test of differences in densities



Note. The top part of the figure presents 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, with '0' corresponding 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 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 3) 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).

Figure 3: Firm size and *predicted* 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 based on the predicted values of a regression of observed training provision on region and industry dummies as well as the fitted (solid) line of a regression of the predicted number of trained workers on normalized employment 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 of firm size (i.e., normalized size between -10 and 10) is reported for the bandwidth 6–25 employees.