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Bratti, Massimiliano

Conti, Maurizio

Sulis, Giovanni

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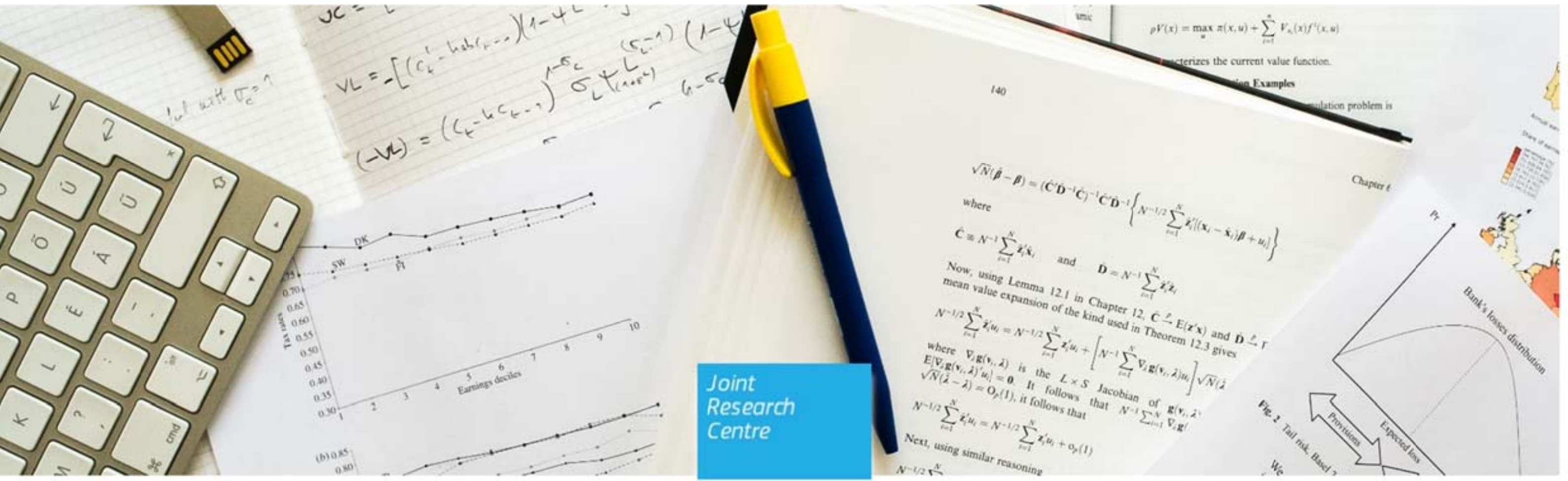

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Contact information
Name: Massimiliano Bratti
Address: Via E. Fermi 2749, TP 361 Ispra (VA), I-21027, Italy
Email: massimiliano.bratti@ec.europa.ec
Tel.: +39 0332 785182

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Employment Protection, Temporary Contracts and Firm-provided Training: Evidence from Italy

Massimiliano Bratti* Maurizio Conti† Giovanni Sulis‡

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Abstract

In this study, we leverage on Italy's size-contingent firing restrictions to identify the causal effect of employment protection legislation (EPL) on firm-provided training using a regression discontinuity design. Our analysis demonstrates that higher levels of EPL reduce incentives for firms to invest in workers' training. The number of trained workers falls by about 1.5-1.9 units at the threshold: this is not a negligible effect, corresponding to a 16-20% reduction in the number of trained workers. The results are robust to several sensitivity checks and controls for potential confounding factors (e.g. work councils). The effect of EPL on training is not mediated by different levels of investment in physical capital or propensities to innovate, while it is mostly accounted for by higher worker turnover and more use of temporary contracts, which entail less training, in firms with higher firing costs. Our study highlights the potential adverse effects of EPL on worker training in dual labour markets, owing to larger firms seeking to avoid the higher costs of EPL by means of temporary contracts.

JEL codes: J42, J63, J65, M53.

Keywords: employment protection legislation, firing costs, training, Italy

*European Commission Joint Research Centre (JRC), Università degli Studi di Milano and IZA, massimiliano.bratti@unimi.it

†European Commission Joint Research Centre (JRC) and Università degli Studi di Genova, maurizio.conti@ec.europa.eu

‡Università degli Studi di Cagliari, CRENoS and IZA, gsulis@unica.it

1 Introduction

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

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

The Italian legislation, which envisages size-contingent firing restrictions — according to which firing costs increase sharply above the 15-employee threshold — gives us a unique opportunity to provide clean causal evidence on the effect of EPL on training using a sharp regression discontinuity design (RDD). In Section 3.1 we discuss why these policy-induced differences in employment protection are able to substantially differentiate firing costs according to firm size. This is a timely moment to add new evidence, given the paucity of studies that have empirically investigated the interplay between EPL and firm-provided training (see Section 2), and in the light of several reforms that have reduced employment protection — especially at the margin, i.e. for temporary workers — in many countries.[3]

Our paper demonstrates that, contrary to what simple economic intuition may suggest, higher EPL — and lower worker turnover — is not necessarily associated with higher firm-provided training in contexts characterised by dual labour markets.[4] The co-existence of contracts that entail different levels of protection induces firms that face higher firing costs

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

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

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

[4] In reality, predictions from economic theory are not clear-cut (Acemoglu and Pischke 1999a).

(i.e. larger firms in the Italian context) to make more extensive use of temporary workers, who are less protected by the legislation (Hijzen et al. 2017, Pierre and Scarpetta 2013, Cabrales et al. 2017). However, temporary workers receive less training.

The main results of our paper can be summarised as follows. The baseline estimate suggests that EPL reduces the number of trained workers by about 1.5-1.9 units at the threshold. This is not a negligible effect and corresponds to a 16-20 per cent reduction in the number of trained workers. The effect of EPL on training does not appear to be mediated by different levels of investment in physical capital or propensities to innovate, while it is mostly accounted for by higher worker turnover and more use of temporary contracts in firms with higher firing costs. Our study therefore points to the potential adverse effects of EPL on worker training in dual labour markets, owing to firms' attempts to reduce firing costs through the use of temporary contracts. It also provides a potential explanation for the fall in productivity that has been observed in some EU economies after the labour market reforms that reduced EPL for temporary workers (Damiani et al. 2016).

These findings can be explained by noting that, first, in dual labour markets, firms tend to avoid the higher firing costs associated with permanent positions by relying more on a sequence of temporary contracts (Cahuc et al. 2016). Second, in dual labour markets, outside employment opportunities for workers hired on temporary contracts could increase more than their productivity after receiving training, thus reducing the incentive for firms to provide training. This, in turn, may happen because trained workers in temporary contracts may easily find a better (e.g. permanent) employment opportunity outside the firm that provided the training (the employee's productivity is higher thanks to training and therefore they appeal to other firms that want to save on training costs).[5] Another mechanism explaining why EPL may reduce training in the presence of temporary contracts is highlighted by Cabrales et al. (2017) and Dolado et al. (2016). Their basic insight is that firms can use the conversion of temporary to permanent contracts to push workers to increase the effort they put into the job. An increase in the differential in EPL between permanent and temporary contracts, under reasonable assumptions, causes permanent workers to reduce the effort they put into the job (e.g. through higher absenteeism, as suggested by Ichino et al. 2003), which leads firms to reduce the rate of converting temporary to permanent jobs. This entails, in turn, a reduction in the effort that temporary workers put into the job (as the likelihood of conver-

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

sion is lower) and a reduction in the level of firm-provided training to temporary workers endogenously chosen by firms.[6] Thus, our study also speaks to the growing literature on the (possibly) perverse economic effects associated with two-tier reforms of employment protection (Boeri and Garibaldi 2007).

We make three 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 different measures of training and data sources compared with previous studies. 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 is one of the main determinants of the incentives for firms to (not) provide training. Third, we employ the non-parametric local polynomial estimation method of Calonico et al. (2014), which uses data-driven non-parametric regressions to select the optimal bandwidth around the threshold and which does not consider the local polynomial regression as correctly specified. In so doing, we avoid *ad-hoc* and subjective choices, which could affect the RDD results.

The rest of the paper is organised as follows. In Section 2 we review the related literature. In Section 3 we introduce the institutional framework and present our identification strategy. After discussing the data in Section 4, we present our main results, some robustness checks and a discussion of potential confounding factors and mechanisms in Section 5. Section 6 summarises the main findings and draws conclusions.

2 Past Literature

This paper is related to different strands of literature. First, it is related to theoretical studies dealing with incentives to invest in human capital by firms and workers. After seminal work by Becker (1964), more recent studies show that, in imperfectly competitive environments where labour market institutions are at work, firms (and workers) may have incentives to invest in general training. Papers by Acemoglu (1997) and Acemoglu and Pischke (1999b) show that, when labour market institutions, such as EPL, generate wage compression, firms may have more incentive to pay for training. This is because labour market imperfections, such as search frictions, information asymmetries and labour market institutions, determine the gap between a worker's marginal product and her wage, thus generating rents to be shared

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

between workers and firms. Moreover, labour market imperfections reduce the outside option of workers so that wages increase less than productivity for trained workers. A necessary condition for firms to sponsor (general) training is that these rents are increasing in training (Acemoglu and Pischke 1999b). In a similar vein, Wasmer (2006) shows that in an environment with search frictions, when EPL is high and turnover is low, workers may have an incentive to invest more in specific skills than in general skills.[7]

Second, this paper is related to more recent empirical contributions on the relationship between employment protection and training.[8] Simple economic reasoning suggests that, by increasing the time horizon in which the firm can reap the economic benefits of worker training, stricter EPL should increase firm-provided training. However, the empirical evidence does not always point in this direction. Using a large firm-level dataset across developing countries, Almeida and Aterido (2011) show that stricter enforcement of labour regulations is strongly associated with higher investments of firms in their employees' human capital, but that the effect is very small. Similarly, Pierre and Scarpetta (2013) use cross-country harmonised survey data and find that higher EPL is associated with higher investment in training and more use of temporary contracts. They also find that EPL has larger effects on small firms and in sectors characterised by greater job reallocation. Furthermore, studies exploiting within-country variation in levels of EPL do not find strong positive effects of EPL on training. For instance, Picchio and van Ours (2011) use Dutch data for manufacturing firms and find that higher labour market flexibility (i.e. lower EPL) marginally reduces firms' investment in training; however, this effect is rather small. A recent study by Messe and Rouland (2014) exploits a reform of EPL in France to identify, using a difference-in-differences approach combined with propensity scores methods, the effect of EPL on the incentive for firms to pay for training. They find that higher EPL (in the form of a tax on firings) had no effect on the training of older eligible workers, while it had a positive effect on workers just below the eligibility threshold. The authors interpret this finding as stressing the complementarity between training and firing decisions.

The paper most closely related to ours is Bolli and Kemper (2015), in which the authors use a regression discontinuity design framework exploiting variation in firing regulations across size thresholds in Italy and Finland to study the relationship between EPL and train-

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

[8] 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) for EPL and productivity, Cingano et al. (2010) and Cingano et al. (2016) for EPL and investment, Schivardi and Torrini (2008) for EPL and firms' propensity to grow, Leonardi and Pica (2013) for EPL and wages, Kugler and Pica (2008) for EPL and worker flows, and Bottasso et al. (2017) for EPL and firm dynamics.

ing provision. Their RDD results for Italy point to a statistically significant negative effect of stricter EPL on the extensive margin of training (i.e. a dichotomous indicator for having provided training).[9] However, there is no effect on the intensive margin (amount of paid training hours). We add to their analysis by using a much richer dataset, which, in addition to providing different indicators for training (e.g. number of trained workers, training expenditures, source of financing of training), also gathers information on several potential confounding factors (e.g. presence of work councils in the firm or the presence of workers under the *Cassa Integrazione Guadagni* scheme, a short-time work programme featuring a redundancy fund system). Moreover, we also have information on firms' physical capital investments, innovation activities and use of temporary contracts, which helps us disentangle the potential sources of the EPL effects on training.[10] Finally, our paper is also different, as we use the non-parametric local polynomial estimation method suggested by Calonico et al. (2014).

Third, our paper is related to two recent studies that, in the cases of Spain and Italy, analyse the effects of EPL on training by type of contract (temporary vs permanent) and on the composition of the labour force. Cabrales et al. (2017) use the PIAAC survey data and document a large gap in training provisions for temporary and permanent workers in Spain, characterised by persistent dualism in the labour market. In this environment, lower levels of EPL for temporary workers reduce expected job duration, increasing turnover for this group of workers, thereby reducing the incentive for firms to invest in training. On the contrary, permanent workers benefit from higher levels of training, as firms find it profitable to invest in their workers' skills. Hence, employment protection is related to training depending on the composition of the labour force. In the case of Italy, Hijzen et al. (2017) exploit variation in EPL across firms of different sizes and find that higher levels of EPL result in excessive worker turnover and that this effect is entirely due to the excessive use of temporary contracts. Moreover, they show that, by increasing excessive worker turnover, stricter EPL also has negative effects on labour productivity.

[9] The estimates are, however, very sensitive to the degree of the polynomial, going from -0.44 to -0.06 for cubic and quadratic polynomials, respectively.

[10] This could not be done by Bolli and Kemper (2015) because, as the authors stress, their data only provide information on whether firms have temporary workers or not and only on firms that train some of their employees.

3 Institutional Framework and Identification

3.1 Institutional framework

Since the 1960s, the regulation of unfair dismissals has changed several times in Italy. The most significant reform occurred in 1970 with Law 300-70, also known as 'Statuto dei Lavoratori' (Workers' Statute) and, in 1990, with Law 108/1990, which strengthened employees' protection from unfair dismissal only in the case of small firms.[11]

Before the legislative changes that occurred in 2012 and 2014, the degree of protection enjoyed by unfairly dismissed workers was considerably greater in the case of employees working in firms with more than 15 employees. Indeed, if a dismissal was declared unfair by a judge, the employee unfairly dismissed from a firm with more than 15 employees could ask to be reinstated and receive the wages forgone and the health and social security contributions (for a minimum of 5 months) related to the period between the dismissal and the sentence. Although reinstatement was the most likely occurrence in practice, the unfairly dismissed employee retained the right to 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).[12]

The higher *de jure* costs for employers in the case of firms with more than 15 employees are further increased if one also takes into consideration the *de facto* costs associated with the very long average duration of labour trials in Italy: Gianfreda and Vallanti (2017) report average trial decisions of about 850 days over the period 2007-2010, with large variation across regions.[13] Such a difference in the length of labour trials lead to escalating firing costs above the threshold. Indeed, using a formula proposed by Garibaldi and Violante (2005) to compute *ex post* firing costs, Gianfreda and Vallanti (2017) report firing costs equivalent to about 36 months' wages in Trento versus 160 months in Salerno for a blue-collar worker with 8 years of tenure in a firm above the 15-employee threshold.[14] Because below the threshold

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

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

[13] 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.

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

no forgone wages are due, the length of labour trials matters only above the threshold, with firing costs rapidly increasing above the 15-employee threshold if the labour trial lasts longer than 5 months.[15] Moreover, the lack of a clear definition of unfair dismissal in Italian legislation (Hijzen et al. 2017) led to some inconsistencies in its implementation, as noted by Ichino et al. (2003), who showed that, in regions with high unemployment rates, judges tended to rule in favour of employees. The variability in decisions therefore led to uncertainty, which further increased the costs associated with the stricter employment protection for firms above the threshold.

So far we have discussed only employment protection for open-ended contracts. However, as in other countries, such as Spain or France, the Italian labour market has in the past 15 years been characterised by a notable increase in the use of temporary and atypical labour contracts, following the liberalisation that started at the end of 1980s (in the case of temporary contracts), and at the end of the 1990s in the case of semi-autonomous atypical workers. It is, however, important to note that the degree of employment protection for temporary and atypical workers does not change discontinuously at the 15-employee threshold: indeed, it does not depend at all on firm size.[16]

In contrast, there are regulations that change discontinuously at the 15-employee threshold, the most important being the right to form a worker council, which is granted to firms with more than 15 employees. Previous empirical evidence discussed in Schivardi and Torrini (2008) suggests, however, that the establishment of worker councils does not seem to change discontinuously at the 15-employee threshold, although the proportion of firms with a worker council does seem to grow with size. As we have information on the existence of a worker council within firms, in the next sections we will show that our results are robust to controlling for its presence in the RDD analysis.

Finally, it is important to note that, according to Italian legislation, part-time workers count as less than one full-time employee in defining the firm size, 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.

fees and the severance payments. See Garibaldi and Violante (2005) for the exact formula.

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

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

3.2 Identification strategy

In this study, we identify the effect of employment protection on firm-provided training by exploiting the discontinuous change in the degree of regulation of unfair dismissals at the 15-employee threshold, using a sharp regression discontinuity design (RDD) framework. In particular, we employ the non-parametric local polynomial estimation method of Calonico et al. (2014), which has recently been gaining popularity in the applied literature.[17]

Relative to the parametric method, which is based on *ad hoc* chosen bandwidths and which assumes away any misspecification bias in both estimation and inference, the approach proposed by Calonico et al. (2014) presents three notable characteristics: first, the bandwidth is chosen using a data-driven procedure on the basis of a non-parametric approximation; second, the resulting point RDD estimator is asymptotically optimal in a mean squared error sense; and third, inference is conducted without the assumption that the local polynomial regression is correctly specified. In practical terms, the researcher needs to specify a given polynomial order and a specific kernel: the bandwidth is chosen in such a way that it trades off the lower variance associated with a larger bandwidth, with the higher bias associated with the increasingly poor parametric polynomial approximation when observations far away from the threshold are included in the analysis. More specifically, the bandwidth is chosen to minimise the mean squared error of the RDD estimator.

After selecting the optimal bandwidth (in a mean squared error sense), Calonico et al. (2014)'s procedure amounts, in our case, to running the following weighted least squares regression:

$$Y_i = a + \tau(D_i) + \beta_1(\tilde{E}_i) + \beta_2(\tilde{E}_i)^2 + \lambda_1(\tilde{E}_i D_i) + \lambda_2(\tilde{E}_i D_i)^2 + u_i \qquad (1)$$

where D_i is a dummy variable equal to 1 for firms above the threshold; Y_i is a variable measuring training, e.g. the number of workers that have received training in the firm i.[18] $\tilde{E}_i$ represents employment recentred so that it equals zero in the case of firms with exactly 15 employees (i.e. $\tilde{E}_i = E_i - 15$) and we allow up to a polynomial of order two,[19] with possibly different coefficients on both sides of the threshold. τ represents the RDD effect of

[17] For recent studies that have used the Calonico et al. (2014)'s approach, see, for example, Stampini et al. (2018), Kantorowicz (2017) and Bhalotra et al. (2017).

[18] Cingano et al. (2016) note that, in an RDD setting, one should not normalise the outcome variable by the forcing variable. In some robustness checks, however, we provide some evidence showing that the main results do not substantially change if we use, as the dependent variable, the proportion of workers who received training.

[19] Following the recommendation of Gelman and Imbens (2017), we have considered only quadratic and linear polynomials.

employment protection on training.

The type of weights depend on the specific kernel considered in the analysis. As suggested by Calonico et al. (2014), we consider the triangular kernel, which gives a weight of zero to observations outside the bandwidth and a positive weight, which declines linearly and symmetrically with distance from the threshold, to observations inside the bandwidth.[20] Calonico et al. (2014) note that the regression function is misspecified by construction near the cut-off point and that the optimal bandwidth (in the mean squared error sense) is too large for the misspecification bias to be of second-order importance for inference purposes. They estimate the bias and modify the confidence interval by recentring and rescaling the point estimator so that inference can be conducted with a robust p-value that accounts for the bias.[21]

In this study, we therefore estimate the RDD effect by considering the robust bias-corrected confidence interval approach of Calonico et al. (2014).[22] In particular, we do not impose a symmetric bandwidth around the threshold and we deal with the discrete nature of our forcing variable[23] by clustering standard errors, using the number of employees as the cluster identifier, as suggested by Lee and Card (2008). Moreover, we show that our key results are not sensitive to an alternative methodology for selecting the bandwidth, namely the coverage error optimal bandwidth proposed by Calonico et al. (2017), or to a different kernel.[24]

Another important point to discuss at this stage is that the forcing variable, i.e. self-reported firm size, is characterised by non-random heaping problems at multiples of 5 (see Section 5.1), 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 important biases and that omitting observations at data heaps should lead to unbiased estimates of the treatment effects for the 'non-heaped types'. Authors also note that keeping data heaps in the analysis that are far away from the threshold

[20] We consider the triangular kernel because Cattaneo et al. (2017) note that, for a bandwidth that is optimally chosen according to a mean square error criterion, the triangular kernel leads to a point estimator with optimal properties in MSE sense. However, in the robustness check, we also control that results do not depend on the specific kernel choice.

[21] See Cattaneo et al. (2017) for an in-depth discussion.

[22] This is implemented using the `rdrobust` routine for STATA described in Calonico et al. (2017).

[23] For training, we have information only for the year 2009, and in that year we do not have information for either part-time or temporary employees (those with a contract of less than 9 months), but we do for the total number of employees. As a robustness check, we show that the results are robust to dropping firms with 16 employees, which might indeed be spuriously considered to be above the threshold if they have two or more part-time employees.

[24] All regressions were run using sample weights to ensure that results are representative of the population of firms in Italy. Baseline results are, however, robust to not weighting.

but still within the bandwidth may, again, be misleading. For these reasons, our main analysis is conducted by dropping firms with a number of employees equal to every multiple of five up to 95, which is in the right tail of the forcing variable distribution, when a second-degree polynomial is used.[25]

The assumption underlying the validity of any RDD method is that firms do not sort below the size threshold, and so their characteristics should not change sharply around the threshold and the density of the forcing variable should not be characterised by any discontinuity around the cut-off. We will discuss the validity of the RDD identification strategy in our data in Section 5.1.

4 Data

We use data from the ISFOL-RIL Survey ('Rilevazione Longitudinale su Imprese e Lavoro').[26] For the year 2010, the sample comprises about 24,000 firms in the national territory, extracted from the universe of Italian firms ASIA (*Archivio Statistico Imprese Attive*), which is made available by ISTAT (the Italian Statistical Institute). The sampling procedure is based on firm size and it is representative of the population of both the limited liability companies and partnerships in the private (non-agricultural) sectors.[27]

The dataset contains information on indicators of firm size, performance, training and additional variables related to the industrial relations system. A peculiar characteristic of the data is that they contain detailed information on training activities, which is usually unavailable in administrative data on firms or workers. In particular, we know if job training activities have been carried out at the firm level, the number of workers trained, the type of training activities (e.g. coaching, counselling, outdoor training), who provided the training (i.e. external companies, specialised consulting firms or others), who paid for the training (i.e. the firm, the firm with partial coverage from external funding, external contributions),

[25] Reassuringly, results are robust even if we drop multiples of five up to 145 employees or in the range of 5-30 only, which is precisely where heaping problems seem to be more severe.

[26] ISFOL is the acronym for the *Istituto per lo sviluppo della formazione professionale dei lavoratori* (Institute for development and training of workers). The institute has been recently named INAPP (*Istituto Nazionale per l'Analisi delle Politiche Pubbliche*) and its main activities are oriented towards research, monitoring and public policy evaluation. It constitutes a building block in supporting policy making by the Ministry of Labour and Social Policies.

[27] The survey was conducted in 2005, 2007 and 2010, and a panel version of the dataset is, in principle, available for a limited number of firms. However, to identify the effects of interest, we decided to use only the 2010 wave. The reasons for this choice are twofold. First, the panel version of the dataset comprises only a very limited number of firms that were surveyed in all 3 years (less than 20% of the total), entailing too big a loss in terms of observations and issues of sample representativeness. Second, in 2005 and 2007, different industry classifications were adopted, leading to an important loss of details with respect to the industry classification used in 2010.

who provided external contributions and the expenditure on training. Further information is available on the presence of unions (work councils) in the workplace and the level of bargaining and contractual labour agreements. The survey also contains information on the composition of the workforce in terms of skills and types of contracts for workers. On the firm side, although the dataset is quite rich in terms of variables related to firms activity, such as their export, innovation or offshoring, only limited information is available concerning balance sheet data. We have, nonetheless, information on the expenditure on physical capital investment and sales.[28]

In what follows, we describe our sample selection procedure. We begin with 24,459 observations for the year 2010. We first drop firms that have below zero (or an abnormal number of) employees in 2010 (no observations) and in 2009 (196 observations).[29] After the above selection we end up with 24,263 observations. In Table 1, in the top panel, we report descriptives for the full sample and for the two samples used in baseline regressions (reported in rows I and II of Table 3).[30]

[Table 1 about here]

5 Results

5.1 Validity of the regression discontinuity design

Although previous studies have already demonstrated the lack of a substantial self-sorting of firms at, or below, the 15-employee threshold (Schivardi and Torrini 2008, Leonardi and Pica 2013, Hijzen et al. 2017), above which the costs of EPL substantially increase in Italy (see Section 3.1), in this section we look for evidence of manipulation of the running variable (i.e. firm employment) in our data. First, we formally test whether the density function of firm size exhibits a discontinuity at 15 employees, using the procedure proposed by McCrary (2008). Second, following both Schivardi and Torrini (2008) and a standard RDD, we also check whether or not firms just below the 15-employee threshold are less likely to raise employment to avoid larger firing costs.

[28] Devicienti et al. (2017) use ISFOL-RIL data as a primary source of information to study the relationship between unions and temporary contracts. Using social security codes, the authors are able to match their data with additional information on balance sheets. Unfortunately, for confidentiality reasons, the current version of the dataset does not contain social security codes and does not allow us to match other sources of data.

[29] Note that, although the reference year in our data is 2010, important variables related to training refer to the previous year, i.e. 2009. We discuss this point in more detail in Section 5.1.

[30] Note that we consider only firms in the endogenously chosen bandwidth interval and that we drop observations related to heaping. See Section 5.2 for more details.

[Figure 1 about here]

In the ISFOL-RIL survey, firm size is provided in discrete units, i.e. head counts. Composition of employment, in terms of part-time and full-time workers and type of contracts is provided only for 2010, while information on training is provided only for 2009, i.e. the year before. For this reason, we cannot build a continuous measure of employment in 2009 using proxy measures of the legal definition of firm size, as did Leonardi and Pica (2013) or Hijzen et al. (2017).[31] Accordingly, we implement the McCrary test using one-employee bins. The histogram of the density of firm size is shown in the left panel of Figure 1 and shows a fall in the frequency from 15 to 16, which is, however, not very different from those observed in other parts of the distribution, e.g. from 5 to 6 or from 10 to 11. Heaping at multiples of five, when self-reporting firm size in the ISFOL-RIL survey, may be partly responsible for these drops (see the discussion in Section 3.2). The McCrary test is visualised in the right panel of Figure 1 with separate weighted kernel estimations and 95% confidence intervals of the logarithm of density of firm size on either side of the cut-off. In spite of the non-negligible heaping at 15 employees, which should make it more likely to under-reject the null of no discontinuity in the density (producing a mass point at 15 employees), the test gives a log difference between the frequencies to the right and the left of 15 employees of -0.054 ($s.e. = 0.074$) statistically insignificant at the 5% level.

The McCrary test is based on aggregated bin data and not on individual firm size distributions, and it has low power when selection around the cut-off is not monotonic but occurs in both directions.[32] To address this issue, in Table 2 we follow Schivardi and Torrini (2008) and report the results of another test of firms' self-sorting below the threshold (i.e. manipulation of firm size), based on individual firms' likelihoods of growing in size.

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

$$Pr(E_{it} > E_{it-1}) = \alpha + \sum_{j=1}^{n} \beta_{jl} E^{j}_{it-1} + \sum_{j=1}^{n} \beta_{jr} (E^{j}_{it-1} \cdot D_{it-1}) + \sum_{k=13}^{15} \gamma_k D^{k}_{it-1} + \beta_x \mathbf{X}_{it} + v_{it} \quad (2)$$

[31] Some studies, for instance, count part-time workers as half a worker when computing firm sizes.

[32] However, Hijzen et al. (2017) observe that there is no particular reason why small firms may want to sort above the 15-employee threshold.

with

$$D_{it-1} = \mathbb{1}[E_{it-1} \leq 15];$$
$$D^k_{it-1} = \mathbb{1}[E_{it-1} = k] \text{ for } k = 13, 14, 15.$$

E_{it-1} and E_{it} is firm size in year $t-1$ (2009) and t (2010), respectively; D^k_{it-1}s are a set of bin dummies, with bin size equal to 1 (namely for sizes 13, 14 and 15 employees); D_{it-1} is a dummy equal to 1 if lagged firm size was not above the 15-employee threshold; $\mathbf{X}_{it}$ is a vector of firm characteristics and v_{it} is a firm-level error term. The polynomial of firm size captures parametrically the underlying relationship between firm size and the probability of employment growth in the absence of employment protection, while the bin dummies can be interpreted as the threshold effect of EPL on firms' employment growth.

Columns (1)-(4) of Table 2 show the results of this test using polynomials of different orders on the sample of firms with $6-25$ employees.[33] Bin dummies for 13, 14 and 15 employees are never statistically significant at conventional levels, except for the 14-employee bin with a linear polynomial.

[Table 2 about here]

We complement the evidence on manipulation by also reporting, in columns (5)-(8) of Table 2, the results of a more standard parametric RDD specification

$$Pr(E_{it} > E_{it-1}) = \alpha + \gamma D_{it-1} + \sum_{j=1}^{n} \beta_{jl} \tilde{E}^j_{it-1} + \sum_{j=1}^{n} \beta_{jr} (\tilde{E}^j_{it-1} \cdot D_{it-1}) + \beta_x \mathbf{X}_{it} + v_{it}. \quad (3)$$

with $\tilde{E}_{it-1} = E_{it-1} - 15$, i.e. firm employment normalised with respect to the cut-off.

The coefficient of the cut-off indicator (γ) is never statistically significant, with a magnitude in absolute value increasing in the degree of the polynomial.

Overall, the results of these checks are consistent with the absence of strong manipulation of firm size found in earlier papers investigating the effects of EPL in Italy using administrative data to define firm employment. It is worth stressing once again that the outcomes of these tests could be partly affected by heaping observed in our self-reported data. In particular, heaping, by creating a mass point at 15 employees, should make it less likely that the null of no manipulation is rejected.

[33] Similar bandwidths are used by Leonardi and Pica (2013) and Hijzen et al. (2017) when running this test. Papers reporting this test generally did it with third- or fourth-degree polynomials.

Heaping at multiples of five could also affect the RDD estimates of the effect of EPL on training, as observations at each multiple of five can be considered as a mixture of observations for which employment has been correctly reported, observations for which employment size is rounded upward and observations for which it is rounded downward. When focusing on firms with 15 employees, heaping means that some observations at 15 are, in reality, below the threshold while others are above. This should produce a misclassification error and reduce the RDD estimates in the presence of a true effect of EPL (Lewbel 2007). To address this concern, we follow the suggestion of Barreca et al. (2016) and focus our baseline estimates on the sample that drops the data at reporting heaps, for which employment size should be correctly (or more precisely) reported, and which yields an unbiased estimate of the treatment effect for 'non-heaped types'. However, in the robustness checks, we also report the estimates in the sample retaining the heaped observations.

To check for manipulation, some papers have reported balancing tests of some firm characteristics around the cut-off. Sorting according to the covariates would bias the RDD estimates only if they are correlated with firm-provided training. 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 the RDD. To take a few examples, firm characteristics affected by EPL and which also interact with worker training may include investments in physical capital (Cingano et al. 2016; 2010), access to credit (Cingano et al. 2016), innovation performance (Koeniger 2005), use of temporary contracts (Hijzen et al. 2017), wages (Leonardi and Pica 2013), and workers' mismatch (Berton et al. 2017). In Section 5.4 we check the sensitivity of our estimates to including in the regressions some of these covariates, which are available in the ISFOL-RIL survey, and further discuss their role in mediating the effect of EPL on firm-provided training.

5.2 Main results

Our baseline estimates are reported in Table 3. For each model, we report the RDD coefficient, the p-value, the order of the polynomial (one or two), the number of observations to the left (N. obs. L.) and to the right (N. obs. R.) of the cut-off, the estimated left (Band L.) and right (Band R.) bandwidths, the use of strategies to address heaping (H) or 'donut-hole' regressions (D), and two columns indicating whether industry and region fixed effects are included in the estimation. For all models, except models III and IV, which use the heteroskedasticity-robust variance estimator, we report the cluster-robust variance estimator and remove heaped observations (i.e. multiples of five), which might bias our estimates (Barreca et al. 2016). The estimated effect of employment protection (i.e. stricter firing restrictions above the 15-employee threshold) is 1.48 fewer trained workers (p-value=0.017) with a first-degree polynomial and -1.94 trained workers (p-value=0.007) with a second-degree polynomial. These decreases correspond to 16% and 20% reductions in the number of trained workers at the cut-off, respectively. We consider the estimates in models I and II as our baselines, and present in the remaining rows some robustness checks.

[Table 3 about here]

The estimates in rows III and IV, which use the heteroskedasticity-robust variance estimator, are practically indistinguishable from those reported in the first two rows. However, because of the discrete nature of our forcing variable, in the remainder of the analysis we stick to the use of standard errors clustered at the employment level, as suggested by Lee and Card (2008).[34]

In rows V and VI, we check the robustness of our results to re-including heaped observations. When using a first-degree polynomial (model V), estimates are not reported by `rdrobust`.[35] On the contrary, using a second-degree polynomial in model VI, the estimated coefficient is -3.6, larger than in our baseline estimates, and is statistically significant at the 1% level.

In models VII-X we check the robustness of our estimates to including industry and region fixed effects. In a RDD design, the main goal of including covariates is just to increase precision; in turn, if the RDD assumptions are valid, the inclusion of covariates should not greatly affect the estimates. Using industry fixed effects, the RDD coefficients in models VII-VIII slightly increase, to -1.68 (p-value=0.005) and -2.33 (p-value=0.001), respectively. The estimates controlling for region fixed effects remain virtually unchanged with respect to the baseline. As expected, including the fixed effects makes the estimates more precise.

As we stressed in Section 5.1, we do not have information about the type of contracts with which employees in 2009 were hired and cannot build the 'legal' size of the firm (which is relevant for EPL). We have to rely only on head counts. This means that some firms above 15 employees can actually be below the threshold (e.g. if firms employ part-time workers).

[34] When the forcing variable is discrete, as in our case, it is not possible to compare units just below and just above the threshold. Lee and Card (2008) suggest modelling the difference between the expected value of the outcome variable and the predicted value from the selected functional form as a random specification error; the latter gives rise to a cluster error structure, where the cluster variable is the forcing variable.

[35] When considering heteroskedasticity robust standard errors, we find a reduction of 1.9 workers, although it is estimated with some noise (the p-value is 0.11).

Thus, in models XI and XII, we report the estimates of a 'donut-hole' regression in which firms with 16 employees are dropped from the sample.[36] The estimated effects with first- and second-degree polynomials are -1.01 and -1.85, respectively, which are statistically significant at the 5% and 1% level, respectively.

5.3 Different dependent variables and further robustness checks

Up until now, we have focused our attention on the number of trained workers. Panel (a) of Table 4 reports the estimates for our baseline specifications (i.e. models I and II of Table 3) using different dependent variables. In particular, we consider the proportion of trained workers, the number of trained workers for firms using prevalently or exclusively their own funds for training, and the training expenditure (in euros). Model I (II) points to a statistically significant reduction of 5.5 points (-9.0 points) in the percentage of trained workers associated with EPL when a first-degree (second-degree) polynomial is used.[37] Focusing only on the firms that exclusively or mostly used their own funds to bear training costs does not change the point estimates of the EPL effect, which are -1.54 (p-value=0.008) and -1.88 (p-value=0.005) with first- and second-degree polynomials, respectively.

[Table 4 about here]

The results are less robust when the expenditure on training is used as the dependent variable, presumably owing to a larger measurement error affecting the variable or the smaller sample size. In this case, the specification using a first-degree polynomial gives an estimate of EUR -233 in the expenditure, which is, however, statistically insignificant. By contrast, the estimate is much larger (-888) and significant at the 5% level when a second-degree polynomial is used.

In panel (b) of Table 4 we show further robustness checks. Models VII and VIII report the results of a placebo analysis in which the cut-off for firing restrictions is set at 12 employees. In this case, we expect a much lower effect of EPL owing to the misclassification of the treatment D_i. Indeed, both with a first-degree (model VII) and with a second-degree polynomial (model VIII) the effect is very small, 0.22 and 0.18, respectively, and statistically insignificant at conventional levels. Models IX and X use coverage error optimal bandwidths, and the estimated effects are -1.64 (p-value=0.017) and -2.15 (p-value=0.028),

[36] Firms with 15 employees are already removed because of heaping.

[37] We are reporting these estimates even though Cingano et al. (2016) suggest not using dependent variables that are normalised by the running variable.

respectively.[38] Finally, the last two rows of the table use a different kernel (Epanechnikov instead of a triangular kernel), which yields larger estimates, -2.31 (p-value=0.029) and -2.12 (p-value=0.013) with first- and second-degree polynomials, respectively.

5.4 Discussion: confounding and mediating factors

In Table 5, we investigate some potential factors that may confound or mediate the effect of EPL on training. We start with potential confounding variables in panel (a). Italian legislation mandates that workers in firms with more than 15 employees have the right to constitute unitary workplace union structures (work councils) called *Rappresentanze Sindacali Aziendali* (RSA) in accordance with Article 19 of the Workers' Statute.[39] Unions may have an interest in promoting worker training to increase their productivity and wages, and also their value to the firm through acquisition of firm-specific human capital. Thus, the presence of work councils in the firm may confound the effect of EPL, making it more positive.[40] Luckily, our dataset provides information about the presence of union structures within the firm, so we can include, in the RDD regressions, a dichotomous indicator for the presence of an RSU or an RSA. The results in models I and II of Table 5 are very robust to the inclusion of the work council indicator, with statistically significant estimates of -1.51 and -1.80 with first- and second-degree polynomials, respectively.

[Table 5 about here]

Another potential confounding factor that may interfere with EPL is the so-called *Cassa Integrazione Guadagni Straordinaria* (CIGS), which is a short-time work programme comprising an extraordinary worker redundancy fund scheme. CIGS aims to help firms that are in the process of reorganisation and restructuring, those that have been facing a severe economic crisis and those that are undergoing an insolvency procedure. The CIGS scheme relieves those firms from the costs of the unused workforce by providing income benefit to the workers affected. The Italian legislation, for the period related to this study, mandated that only firms above the 15-employee threshold could apply for CIGS; by way of contrast,

[38] The coverage error optimal bandwidth is an alternative methodology to choose the bandwidth that seeks to build a confidence interval for the RDD effect with the asymptotic smallest coverage error rate. It generally leads to selecting a smaller bandwidth around the threshold. See Cattaneo et al. (2017).

[39] Another form of workers' representation in the firm are the *Rappresentanze Sindacali Unitarie* (RSU) created by a national agreement between the most representative trade unions and the main employers confederations at the beginning of the 1990s.

[40] Booth et al. (2003) report that, in the UK, union-covered workers were more likely to receive training and also received more training days, relative to non-union workers. Dustmann and Schönberg (2009) use German data to show that union recognition, via the imposition of minimum wages and wage compression, led to more training in the case of an apprenticeship programme.

all firms could apply for another scheme, known as *Cassa Integrazione Guadagni Ordinaria* (CIGO) — ordinary worker redundancy fund scheme — which, in turn, can be requested by firms facing temporary financial difficulties. In general, firms with a high proportion of workers under CIG schemes are also likely to provide less training, as their level of activity is decreasing; as a result, the presence of a CIG scheme appears to be making the negative effect of EPL larger. In models III and IV, we control for a dummy variable equal to one if the firm has made use of either CIGO or CIGS and we show that the estimated EPL effect is virtually unaffected by the inclusion of the CIG indicator.

In panel (b) we discuss instead some potential mediating factors for the effect of EPL. In particular, EPL may affect several firms' characteristics, which in turn interact with training provision. The first of these factors can be a firm's investment in physical capital. It seems fair to say that both theoretical and empirical studies do not agree on the direction of the effect of stricter EPL on physical capital investment. Indeed, in competitive models of the labour market, higher EPL, by increasing the costs of labour, can lead to capital-labour substitution, as is empirically shown by, for instance, Cingano et al. (2016). In turn, in models with frictions, EPL might exacerbate hold-up problems and lead to lower investment per worker, as found by Cingano et al. (2010). Moreover, if physical capital and firm-specific human capital are complementary inputs into a firm's production function, the capital intensity of a firm can increase following a tightening of EPL, as in the model of Wasmer and Janiak (2014), which predicts a U-shaped relationship between physical capital intensity and EPL. Therefore, the possible complementarity between training and physical capital accumulation predicted by some theoretical models might affect the relationship between firm-provided training and EPL, which we uncover in our RDD estimates. However, models V and VI give little support to this hypothesis. Indeed, the estimated effects of EPL remain substantial when firms' investment is controlled for, with point estimates of -1.516 (p-value=0.008) and -2.117 (p-value=0.002) with first- and second-degree polynomials, respectively, which are very close to our baseline specification.

EPL may also interact with firms' innovation. Several papers have investigated, both theoretically and empirically, the effect of employment protection on innovation, generally reporting positive effects. Bastgen and Holzner (2017) formulate an equilibrium matching model, in which firms facing higher employment protection, which negatively affects productivity, invest in R&D or buy new technologies to restore their productivity. They calibrate the model on US data, replicating the association between EPL and innovation. Similar evidence of a positive association between the two variables was found by Griffith and Macartney (2014), who report that multinational enterprises do a larger proportion of innovative activity in countries with high EPL, although the same multinationals perform a higher proportion of radical innovation in countries with low EPL because the latter requires more adjustment of the workforce.[41] Acharya et al. (2014) provide empirical evidence for the positive effect of EPL on innovation using the staggered adoption of wrongful discharge laws across US states, and they explain the effect with a theoretical model in which EPL enhances employees' innovative efforts and encourages firms to invest in risky but potentially mould-breaking projects. If training and innovation are complementary (e.g. adoption of new production processes or production of new goods require more worker training), innovation may account for a big part of the effect of EPL on training. To test this hypothesis, we control in our baseline specification for either a dummy variable equal to one, for those firms that had introduced in the previous 3 years a product or a process innovation (models VII and VIII, respectively), or a dummy equal to one, for firms that have bought/registered patents in the previous 3 years (models IX and X). In both cases, the estimates are only marginally affected.

Finally, some recent literature has suggested that, in the presence of dual labour markets, firms may try to avoid the costs associated with stricter EPL for regular workers by making greater use of temporary contracts. Moreover, when firing costs for regular workers are high and there are rules forbidding temporary contract renewal, firms might be reluctant to convert temporary jobs into permanent ones. This might, as a result, increase the incentives for firms to rely on temporary jobs in sequence (Cahuc and Postel-Vinay 2002), thereby increasing (excessive) worker turnover. Cahuc et al. (2016) present a search and matching model featuring regular jobs (with possibly stricter EPL) and temporary contracts (which can be terminated at zero cost when they expire, but which cannot be terminated before their expiry date): they show that, in their model, stricter EPL for regular workers leads firms to employ the latter only to exploit production opportunities that are expected to last for a very long time. This, in turn, can lead to an important substitution of temporary jobs with permanent ones, leading to a 'strong excess of labour turnover'. This theoretical prediction also seems to be borne out by the data. Indeed, Hijzen et al. (2017) show that, in the case of Italy, the stricter EPL above the 15-employee threshold is associated with higher rates of excessive worker turnover, defined as the excess of worker turnover over the absolute value of net employment change, the latter in turn measured as the difference between hiring and

41 Saint-Paul (2002) builds a theoretical model in which researchers in countries with higher EPL tend to specialise in 'secondary innovation', which improves existing products, rather than 'primary innovation', which introduces new products.

separation rates. Interestingly, the authors also found that this effect is entirely explained by greater use of temporary workers above the threshold. Similar evidence can be found in Centeno and Novo (2012), who report an increase in the proportion of fixed-term contracts following a Portuguese reform that tightened EPL for regular workers in the case of firms with 11 to 20 workers.

Because there is evidence that temporary workers receive less training (Booth et al. 2002), it might well be the case that the negative association between higher EPL and firm-sponsored training in our data is, at least in part, explained by the higher reallocation rates induced by stricter EPL. To test this hypothesis, in models XI and XII we control for excessive worker reallocation rates at the firm level. The latter is defined, following Hijzen et al. (2017), as $EWT = 2 \cdot min(H,S)/E$, where H and S are the number of hirings and separations, respectively, and E is average firm employment.[42] Contrary to the previous regressions, we note that, once we control for excessive worker turnover, the magnitude of the effect of EPL on firm-provided training is about halved, and it is no longer statistically significant at conventional levels. This result suggests that, in a dual labour market, EPL might not affect training directly, but instead via worker turnover. Indeed, as discussed previously, stricter EPL might induce firms to increase worker turnover by using more temporary workers, who, in turn, receive less training.

To give some additional support to this interpretation, in Table 6 we report a RDD exercise in which we explore the effect of EPL on excessive worker turnover, as in Hijzen et al. (2017). The results displayed in models I and II point towards a statistically significant increase in the excessive worker turnover rates above the threshold. Moreover, in model III we also demonstrate that, above the threshold, the proportion of workers with a fixed-term contract is significantly higher.[43]

[Table 6 about here]

These results suggest that, in a two-tier labour market, higher firing costs for regular workers lead firms to increase the excessive turnover of workers by substituting temporary positions for regular ones, as argued by Hijzen et al. (2017); in turn, temporary workers tend to receive less training because of their shorter expected tenure within the firm. As a result, in a dual labour market, stricter EPL might be associated with less firm-provided training.

[42] It can easily be shown that this formula is equivalent to the definition of excessive 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).

[43] We show only the result with a linear polynomial because estimates are not reported by `rdrobust` with a second-order polynomial.

6 Conclusion

In this paper we have exploited Italy's size-contingent firing restrictions for permanent workers in a sharp RDD framework to identify the impact of stricter EPL for regular workers on firm-provided training.

Our baseline results, which are conducted using the non-parametric local polynomial estimation method of Calonico et al. (2014), suggest that the number of trained workers falls by about $1.5 - 1.9$ units at the 15-employees threshold, depending on model specification: this is not a negligible effect because it corresponds to a 16%-20% reduction in the number of trained workers. Results are robust to an extensive set of sensitivity checks, such as placebo analysis, 'donut-hole' RDD analysis and controls for possible confounders (e.g. the presence of certain work councils within the firm that Italian law envisages for firms with more than 15 employees).

However, our results also suggest that the negative effect of stricter EPL above the 15-employee threshold is largely mediated by the higher excessive worker turnover associated with stricter EPL above the threshold. Indeed, and confirming the results of Hijzen et al. (2017) on a different dataset, we show that, at the threshold, firms are characterised by a much higher excessive worker turnover and more use of temporary workers, as theoretically predicted by Cahuc et al. (2016) for economies characterised by a two-tier labour market. In other words, in labour markets characterised by a significant asymmetry in the degree of employment protection enjoyed by permanent and temporary workers, there is an incentive for firms to substitute temporary for permanent workers by using a succession of temporary contracts (Cahuc et al. 2016), thereby creating excessive worker turnover. However, because temporary workers generally receive less training (Booth et al. 2002), stricter employment protection legislation for permanent workers might reduce incentives for firms to provide training.

This finding might provide an additional explanation for why two-tier reforms might be associated with a drop in labour productivity: indeed, Boeri and Garibaldi (2007) explain the reduction in labour productivity following a two-tier labour market liberalisation as the consequence of a transitory increase in temporary employment coupled with the decreasing marginal returns associated with a downward sloping labour demand.[44] Our empirical findings, in turn, suggest that, by favouring the growth of temporary workers, a large gap between the EPL relating to permanent and temporary workers might lead to less firm-provided

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

training and, possibly, to lower labour productivity, as found by Hijzen et al. (2017). This, in turn, may have played a role in explaining the dismal productivity performance of the Italian economy since the second half of the 1990s.

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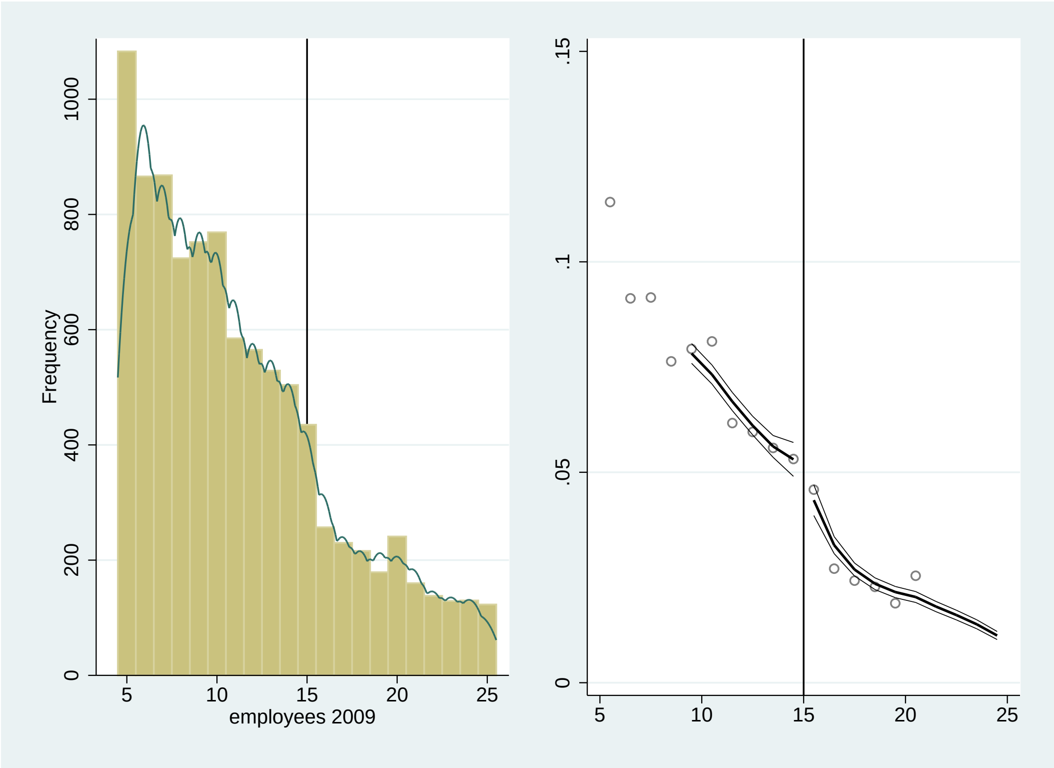
Tables and Figures

Figure 1: Firm size distribution (2009)



Note. The left panel of the figure shows the histogram of the frequency of firm size in 2009 and the right panel the McCrary (2008) test for the presence of discontinuities in the difference in the log density of firm size.

Table 1: Descriptive statistics

Variable	Obs	Mean	Std. Dev.
	full sample		
employees	16,830	64.25	442.15
trained workers	6,377	71.44	481.33
training share	6,377	0.63	0.34
cost of training	5,692	78168	2085484
excess worker reallocation	10,842	0.92	8.81
share of temporary workers	16,481	0.12	0.23
unions	16,665	0.17	0.38
cassa integrazione	16,797	0.18	0.38
total investment	16,282	1830845	69966073
innovation	16,757	0.45	0.50
patents	16,775	0.04	0.19
	first order polynomial		
employees	5,876	26.22	16.82
trained workers	2,659	15.95	14.41
training share	2,659	0.58	0.35
cost of training	2,381	30351	1025431
excess worker reallocation	4,683	0.46	1.68
share of temporary workers	5,846	0.11	0.18
unions	5,738	0.22	0.41
cassa integrazione	5,863	0.24	0.43
total investment	5,656	620136	14176036
innovation	5,847	0.52	0.50
patents	5,860	0.05	0.21
	second order polynomial		
employees	9,751	26.14	28.24
trained workers	4,202	17.25	21.89
training share	4,202	0.59	0.35
cost of training	3,760	23022	816507
excess worker reallocation	7,357	0.52	1.67
share of temporary workers	9,694	0.11	0.19
unions	9,596	0.18	0.39
cassa integrazione	9,730	0.23	0.42
total investment	9,412	759538	16121276
innovation	9,707	0.50	0.50
patents	9,726	0.04	0.20

Note. We eliminate heaps at multiples of 5. Employees is the total number of employees in 2009. Training dummy is equal to 1 if firm has provided training to any worker during the year and 0 otherwise; trained workers is the number of workers trained. We imputed trained workers equal to employees when number of trained was greater than the number of employees; we imputed 0 when this information was missing. Training share is calculated as the share of trained employees in 2009 imposing upper bound to number of employees. Cost of training is the original variable expressed in euros. Excess worker reallocation is calculated at the firm level following Hijzen et al. (2017), as $EWT = 2 \cdot min(H,S)/E$, where H and S are the number of hiring and separations, respectively, and E is average firm employment. Share of temporary workers is the share of fixed term contracts. *Cassa Integrazione Guadagni* (CIG) is a dummy for firms with a short-time work arrangement with redundancy fund in place. Union is a dummy for works councils. Total investment is expressed in euros. Innovation and patents are dummies for product (or process) innovation and patents at the firm level.

Table 2: Firm sorting below the 15-employee threshold

Variables	(1) ST	(2) ST	(3) ST	(4) ST	(5) RDD	(6) RDD	(7) RDD	(8) RDD
13 employees	-0.001	-0.001	-0.010	-0.011				
	(0.040)	(0.064)	(0.106)	(0.106)				
14 employees	-0.091**	-0.092	-0.112	-0.116				
	(0.041)	(0.090)	(0.216)	(0.216)				
15 employees	-0.031	-0.034	-0.070	-0.082				
	(0.050)	(0.128)	(0.389)	(0.389)				
15 or less employees					-0.004	-0.016	-0.042	-0.055
					(0.038)	(0.062)	(0.098)	(0.159)
Observations	8274	8274	8274	8274	8274	8274	8274	8274
R-squared	0.030	0.030	0.031	0.031	0.029	0.029	0.029	0.030
Linear polynomial	Yes	No	No	No	Yes	No	No	No
Quadratic polynomial	No	Yes	No	No	No	Yes	No	No
Cubic polynomial	No	No	Yes	No	No	No	Yes	No
Quartic polynomial	No	No	No	Yes	No	No	No	Yes

Note. Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1. Columns (1)-(4) report the results of a specification similar to Schivardi and Torrini (2008) (ST), including a polynomial in firm size and indicators for 13, 14 and 15 employees, while columns (5)-(6) report the results of a standard RDD specification (RDD). In columns (1)-(4) the polynomial is in firm size and in (5)-(8) in firm size – 15. All polynomials are allowed to be different on each side of the 15-employee threshold. All models also include industry fixed effects. The estimation sample only includes firms with between 6 and 25 employees.

Table 3: Baseline results

Model	Coeff.	*p*-value	P	N. obs L.	N. obs R.	Band L.	Band R.	Heaping / donut-hole	Region f.e.	Industry f.e.
I	-1.481	0.017	1	813	1846	5	64.26	H	no	no
II	-1.945	0.007	2	1300	2314	7.809	127.941	H	no	no
III(a)	-1.521	0.061	1	813	1799	5.477	60.152	H	no	no
IV(a)	-1.841	0.041	2	1874	2292	9.584	123.995	H	no	no
V	n.a.	n.a.	1	n.a.	n.a.	n.a.	n.a.	no	no	no
VI	-3.584	0.004	2	611	2978	3.058	129.244	no	no	no
VII	-1.681	0.005	1	813	1745	7.628	213.374	H	no	yes
VIII	-2.326	0.001	2	1300	2323	7.4	110.978	H	no	yes
IX	-1.392	0.025	1	813	1822	7.858	235.995	H	yes	no
X	-1.877	0.006	2	1300	2299	7.805	124.103	H	yes	no
XI	-1.011	0.048	1	813	1434	5.57	39.817	D	no	no
XII	-1.850	0.009	2	1874	2107	9.795	109.464	D	no	no

Note. P = order of the polynomial; N. obs L. = number of observations on the left of the cut-off; N. obs. R. = number of observations on the right of the cut-off; Band L. = bandwidth on the left of the cut-off; Band R. = bandwidth on the right of the cut-off. H stands for Heaping and D for donut-hole. Estimates and *p*-values are bias-corrected RDD estimates with the cluster robust variance estimator, with the exception of models II(a) and III(a), which report bias-corrected RD estimates with the heteroskedasticity robust variance estimator. All regressions use a triangular kernel and a mean squared error optimal bandwidth. In all specifications we eliminate heaps at multiples of 5 (5, 10,..., 80), with the exception of models V and VI. All regressions are estimated using the Calonico et al. (2017) `rdrobust` routine for STATA.

Table 4: Robustness

Model		Coeff.	p-value	P	N. obs L.	N. obs R.	Band L.	Band R.
		Different depedent variable						
I	Share trained	-0.055	0.01	1	2551	2765	12	271.056
II	Share trained	-0.090	0.001	2	2211	3007	11.014	492.647
III	Own funds	-1.545	0.008	1	736	1583	4.426	59.059
IV	Own funds	-1.879	0.005	2	1409	2095	8.859	140.354
V	Expenditure	-233.890	0.239	1	649	1283	5.158	48.938
VI	Expenditure	-888.530	0.011	2	1404	1705	9.293	110.833
		Robustness checks						
VII	Placebo	0.227	0.195	1	1263	2303	7.235	52.867
VIII	Placebo	0.185	0.194	2	1600	2834	8.686	113.908
IX	Coverage error	-1.641	0.017	1	611	1639	3.599	46.427
X	Coverage error	-2.150	0.028	2	813	2069	5.387	88.248
XI	Different Kernel	-2.315	0.029	1	402	1776	2.441	57.589
XII	Different Kernel	-2.122	0.013	2	1061	2271	6.231	118.287

Note. P = order of the polynomial; N. obs L. = number of observations on the left of the cut-off; N. obs. R. = number of observations on the right of the cut-off; Band L. = bandwidth on the left of the cut-off; Band R. = bandwidth on the right of the cut-off. Estimates and p-values are bias-corrected RD estimates with the cluster robust variance estimator. All regressions use a triangular kernel and a mean squared error optimal bandwidth. In all specifications we eliminate heaps at multiples of 5 (5, 10,..., 80) with the exception of models V and VI. All regressions are estimated using the Calonico et al. (2017) `rdrobust` routine for STATA.

Table 5: Confounding and mediating factors

Model	Control	Coeff.	*p*-value	P	N. obs L.	N. obs R.	Band L.	Band R.
			Confounding factors					
I	Union	-1.511	0.017	1	808	1798	5	66.636
II	Union	-1.803	0.011	2	1869	2252	9.203	130.816
III	CIG	-1.408	0.024	1	812	1844	5	64
IV	CIG	-1.861	0.009	2	1299	2314	8	128
			Mediating factors					
V	Investment	-1.516	0.008	1	783	1629	4.696	51.131
VI	Investment	-2.117	0.002	2	1255	2072	7.561	102.482
VII	Innovation	-1.562	0.011	1	810	1817	5.029	62.726
VIII	Innovation	-2.005	0.005	2	1297	2308	7.846	127.674
IX	Patents	-1.457	0.018	1	810	1844	4.998	64.191
X	Patents	-1.937	0.007	1	1297	2310	7.822	127.299
XI	Worker turnover	-0.739	0.252	1	1761	1713	15	73.759
XII	Worker turnover	-0.764	0.251	2	1761	2216	15	157.867

Note. P = order of the polynomial; N. obs L. = number of observations on the left of the cut-off; N. obs. R. = number of observations on the right of the cut-off; Band L. = bandwidth on the left of the cut-off; Band R. = bandwidth on the right of the cut-off. CIG is a dummy for firms that used the *Cassa Integrazione Guadagni* (short-time work programme with a worker redundancy fund) scheme; investment is the total amount of investment; innovation is a dummy for firms that implemented either a process of a product innovation in the past 3 years; patents is a dummy for firms that have filed or purchased a patent in the past 3 years; worker turnover is the excessive worker turnover described in the data section; and estimates and *p*-values are bias-corrected RD estimates with the cluster robust variance estimator. All regressions use a triangular kernel and a mean squared error optimal bandwidth. In all specifications, we eliminate heaps at multiples of 5 (5, 10,..., 80) with the exception of models V and VI. All regressions are estimated using the Calonico et al. (2017) `rdrobust` routine for STATA.

Table 6: Worker reallocation and temporary workers

	Model	Model	Model
	I	II	III
Coeff.	0.248	0.266	0.054
p-value	0.000	0.072	0.008
P	1	2	1
N. obs L.	717	1105	2153
N. obs R.	2989	4020	3997
Band L.	2.402	3.744	5.296
Band R.	52.765	154.093	84.287

Note. P = order of the polynomial; N. obs L.= number of observations on the left of the cut-off; N. obs R.= number of observations on the right of the cut-off; Band L = bandwidth on the left of the cut-off; Band R.= bandwidth on the right of the cut-off. Estimates and $p-$values are bias-corrected RD estimates with the cluster robust variance estimator. The dependent variable is excessive worker reallocation (models I and II) and the share of temporary workers (model III). All regressions use a triangular kernel and a mean squared error optimal bandwidth. In all specifications, we eliminate heaps at multiples of 5 (5, 10,..., 80). All regressions are estimated using the Calonico et al. (2017) `rdrobust` routine for STATA.

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