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Evaluating the Impact of Employment Protection on Firm-Provided Training in a RDD Framework*

Thomas Bolli[†] and Johanna Kemper[‡]

Abstract

This paper tests the hypothesis that employment protection legislation (EPL) increases the incentives of firms to train their employees. The identification strategy uses a regression discontinuity design (RDD) that exploits exemptions of small firms from EPL. Using firm-level data from Finland and Italy in 2005 and 2010, we do not find empirical evidence that EPL increases the propensity to train or the intensity of firm-provided training. The estimates remain insignificant throughout and mostly have a negative sign. This result is supported in a heterogeneous treatment setting that exploits variation in sector-specific employment volatility. Hence, though the upper bounds of the estimates include economically significant effects, we conclude that EPL has no effect on training of small firms in Finland and Italy.

Keywords: Employment Protection, Training, Regression Discontinuity, Difference-in-Discontinuity

JEL-Code: J24, K31

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1 Introduction

Following a broad consensus that employment protection legislation (EPL) lowers welfare by restricting the extent to which workers can be hired and fired and thereby discouraging hirings and job creation (see, e.g., OECD, 1994; IMF, 2003), many countries have relaxed EPL in recent decades (OECD, 2016). However, whether or not EPL induces more welfare costs than benefits is an ongoing debate in the theoretical literature. While most research has analyzed the negative welfare effects of EPL, not much empirical work has focused on its positive aspects. One of these is the impact of EPL on training. By increasing labor-market frictions, EPL can provide incentives for firms to invest in training their workers. This investment has several welfare-improving implications, such as a better skilled and more productive workforce.

In these times of increased competitive pressure through globalization and rapid technological change, upgrading the skill level of the workforce has become an especially important topic on the agenda of many policy makers, as for example is highlighted by the implementation of the lifelong-learning program of the European Union (see, e.g., Pepin, 2007). Thereby, the impact of EPL on training is particularly relevant for many European countries with high levels of EPL.

However, the empirical literature has not yet conclusively determined whether or not EPL increases training. Therefore, this paper analyzes whether stricter EPL leads to higher investments in firm-provided training. We do this by looking at the propensity to train and the hours of training during paid working time. The identification strategy consists of a regression discontinuity design (RDD) that exploits variation in EPL from firm-size exemptions in Italy and Finland.

From a theoretical perspective, the effect of EPL on training is not clear-cut. The literature suggests two channels that support the hypothesis that EPL increases the incentives for firms to train their workers. On the one hand, EPL makes firms reluctant to hire workers or create jobs, which leads to fewer job vacancies on the labor market and thereby lowers outside options of skilled workers. Firms can exploit the deterioration of the external wage structure to compress the internal wage structure and pay their skilled workers a wage that is below their marginal product. This compressed wage structure provides firms with an incentive to increase the productivity of their workers through training, which allows them to recoup (a part of) their training investments (Acemoglu and Pischke, 1999). On the other hand, another consequence of EPL is that employment relations last longer. By investing in training, firms can decrease the probability that they have to pay firing costs and can recoup their training investments for a longer period of time (Jansen, 1998; Fella, 2005; Belot et al.,

2007; Lechthaler, 2009). Hence, in contrast to a situation without EPL, the introduction of firing costs may lead to more training provision and thus higher welfare (Jansen, 1998).

However, the literature also discusses three theoretical channels which suggest that stricter EPL decreases the incentives for firms to invest in training. First, since dismissals are costly, EPL increases the bargaining position of workers who are protected by EPL (so-called "insiders"). In this way, these workers can claim a larger share of the productivity gains from training through higher wages, thereby reducing the incentive of firms to invest in training. This is a variant of "hold-up" problem (Jansen, 1998; Charlot and Malherbet, 2013). Second, by lowering the productivity threshold beyond which a firm dismisses workers, EPL may lead to a situation where firms end up with a larger pool of low-productivity workers (Mortensen and Pissarides, 1999). This may create disincentives for firms to train if training and workers' abilities are not complementary (Almeida and Aterido, 2011). Third, firms may respond to EPL by increasing the use of temporary workers. Since employment turnover is higher by the nature of these employment forms, they create disincentives for firms to invest in training, since this investment would be vested upon dismissal (Booth et al., 2002; Pierre and Scarpetta, 2013; Cabrales et al., 2014).

Hence, theoretical considerations regarding the impact of EPL on training remain ambiguous. Despite this theoretical ambiguity, only a few studies have empirically tested the effect of EPL on training provision so far. Using cross-country data, Brunello (2004) finds a negative effect of EPL on training. Exploiting variation in *de jure* EPL across countries and in managers' perceptions of *de facto* EPL within countries by means of a difference-in-differences framework, Pierre and Scarpetta (2004, 2013) find a positive effect of EPL on training. However, in a similar difference-in-differences (DiD) approach that uses objective information of *de facto* EPL, Almeida and Aterido (2011) find no effect of EPL on training. Using firm-level data for Dutch firms, Picchio and van Ours (2011) find that search frictions in the labor market increase training. Finally, Messe and Rouland (2014) exploit a reform in France that increased the firing costs of employees aged above 50 in large firms. Their DiD estimates suggest that EPL increases training for workers aged 45 to 49, but not for those aged above 50. In summary, the existing empirical evidence is rather scant, but it tends to support the hypothesis that EPL increases training.

This paper contributes to the literature by estimating a RDD that identifies the impact of EPL on the extensive and intensive margin of training by exploiting the exemption of small firms from EPL. Analyzing the impact of EPL in Italy and Finland makes it possible to test the external validity of the estimations for other countries to some extent, since these two countries differ substantially with respect to their economic development, political

system, and regulatory framework (The Global Innovation Index, 2016). Furthermore, both countries have stricter employment protection regimes than many other OECD countries. In 2013, the index-value of the OECD Index of Employment Protection was 2.17 for Finland and 2.68 for Italy, while that for the OECD average was 2.04 (OECD, 2016). Hence, we analyze two countries with a relatively high level of EPL. We are the first who analyze the impact of EPL on training in these two countries.

Our results provide no evidence that EPL increases training. In fact, most of the RDD estimates yield a negative though insignificant estimate for the effect of EPL on training. While these estimates remain relatively imprecise for Finland and for the intensive margin of training for Italy, the baseline estimates for the extensive margin in Italy suggest an upper bound of the effect of only 1.4% compared to a sample average of 38%. This suggests that the effect of EPL on training is both statistically and economically insignificant. Similarly, the estimates from a heterogeneous discontinuity framework, which tests whether the effect of EPL increases with employment volatility, yields no evidence of a positive effect on training. Furthermore, it improves the precision of the estimates for Finland, reducing the upper bound of the effect from 18% to 6% compared to a sample average of 57% of firms that train. Hence, exploiting variation in EPL due to exemptions of small firms in Finland and Italy yields no evidence for the hypothesis that EPL increases training.

The paper is organized as follows. The second section summarizes existing theory and empirical literature to derive hypotheses. The third section describes the country-specific regulations regarding EPL and the empirical methodology used in this paper. The fourth section analyzes the validity of the RDD setting and presents the estimation results in the fifth section. Section six concludes.

2 Literature Review

In the first part of this section, we summarize the theoretical, in the second part the empirical literature that analyzes the effect of EPL on training.

2.1 Summary of the Theoretical Literature

In this paper, we focus on the tax component of EPL, which involves red tape and legal costs, which are vested upon dismissal. This component of EPL cannot be internalized in a wage contract and therefore affects the hiring decisions of firms.¹

Theoretical predictions about the effect of EPL on training are not clear-cut. The literature suggests two channels through which EPL could increase the incentives of firms to train their workers. First, Acemoglu and Pischke (1999) argue that frictions in the labor market may lead to a situation where the wages of skilled workers are compressed, i.e. where skilled workers are paid below their marginal product. EPL is one source of frictions in the labor market, as it makes firms more hesitant to hire workers or create jobs. This mechanism leads to fewer job vacancies on the labor market, which deteriorates workers' outside options. Firms can exploit the deterioration of the external wage structure to compress the internal wage structure by paying their skilled workers a wage that is below their marginal product. The wedge between the skilled workers' productivity and his wage gives firms an incentive to increase the skilled workers' productivity through investments in training. As long as the training costs are lower than the gain from increasing the worker's skills, firms will find it profitable to invest in training their workers (Acemoglu and Pischke, 1999).

Second, as EPL increases the cost of dismissing workers, it lowers separations and job destruction, which prolongs existing employment relations (Mortensen and Pissarides, 1999). Because EPL increases the tenure of workers, it gives firms an incentive to increase the productivity of their workforce through investments in training. The increase in tenure acts as a kind of security that the firms could recoup their investments in training. The increase in productivity through training lowers the probability that firms have to dismiss workers and pay firing costs (Jansen, 1998; Fella, 2005; Belot et al., 2007; Lechthaler, 2009). Jansen (1998) shows that, in contrast to a situation without EPL, the introduction of firing costs may lead to the provision of more training and thus to higher welfare, only if job separation rates are lower for skilled relative to unskilled workers and if the positive effect on training outweighs the negative effect of EPL on job creation.²

However, there are three reasons why firms may not want to train their workforce. First, if workers who enjoy legal protection against dismissal ("insiders") exploit their bargaining

¹EPL has a transfer and a tax component (Garibaldi and Violante, 2002). The transfer component of EPL, e.g. severance payment, should not affect the hiring decisions of firms, as it can be undone by a wage contract which lowers the initial wage by the amount of the severance payment (Lazear, 1990; Burda, 1992).

²Usually, job separation rates are lower for skilled relative to unskilled workers, as unskilled workers reach the productivity threshold, beyond which the match does not produce a positive match surplus anymore, relatively faster.

position to extract higher share of the surplus generated by the firm-worker pair through higher wages, this leads to a potential hold-up problem on the part of the workers. The stronger bargaining position of insiders may discourage firm investments in training, since the insiders obtain a higher share of the match surplus without bearing any costs for the training (Charlot and Malherbet, 2013).

Second, by imposing firing costs, EPL decreases the productivity threshold beyond which the cost of dismissing a worker is lower than keeping him (Mortensen and Pissarides, 1999). The firms that are subject to EPL may end up with a selection of low-skilled workers for whom it may not pay to invest in training, as the productivity gain from training would be lower than its cost (Almeida and Aterido, 2011).

Third, if firms in a labor market with a dual type of employment protection—where regular contracts are regulated more strictly than temporary contacts or temporary agency work—respond to stricter EPL by employing more temporary workers, this could also lower their willingness to train, since turnover in these jobs is typically higher. Consequently, training investments may not pay-off, as firms may not have the opportunity to recoup their investments (Booth et al., 2002; Pierre and Scarpetta, 2013; Charlot and Malherbet, 2013; Cabrales et al., 2014).

So far, we have neglected the question whether we speak of firm-specific or general training, which is an important aspect from a theoretical perspective. The key difference between both is that firm-specific training fosters firm-specific skills that increase workers' productivity only within one particular firm, while general training improves general skills that increase the workers' productivity with a range of firms (Becker, 1964). Consequently, since general skills increase workers' outside value and make them mobile, firm-specific skills give firms monopsony power over these skills and thereby over their workers (Acemoglu and Autor, 2011). In a perfectly competitive labor market, firms are always willing to pay for training that fosters firm-specific but are not willing to pay for training that enhances general skills, since the workers' outside value increases along with the risk that he will be poached by another firm. In this case, the training investments of the firm would be vested. On the contrary, workers prefer to pay for general training (assuming no credit constraints), as this increases their outside value and as the firm potentially reaps all of the gains of firm-specific training.

However, the training decision in our paper is made under the assumption of an imperfectly competitive labor market, as EPL is one source of matching frictions in the labor market (Mortensen and Pissarides, 1999). Also under the assumption of an imperfectly competitive labor market, firms are always willing to pay for firm-specific training, while

the workers' decision depends on several factors that require a more detailed analysis, as described in Wasmer (2006).

Since we analyze the effect of EPL on *firm-provided training*, which implies that it is at least partly financed by the firm, most of the attention is given to the investment decisions of the firms. Since firms are always willing to pay for specific training, independent of EPL, the theoretical focus of our paper is by definition on general training. However, the training we focus on in the empirical part of the paper is not purely general. We look at "continuing education and training". This includes education and training that has more of a formal character, i.e., that is clearly separated from the active work place, designed and managed either by the firm or by an external institution and with a fixed curriculum. This can include general education (e.g., courses at universities or schools) or more job-specific courses (e.g., provided by chambers of commerce and adult education centers). It does not include on-the-job-training or other sorts of more informal training (Eurostat, 2005).

2.2 Summary of the Empirical Literature

Only a few studies have analyzed the effect of EPL on training. Most of these have used cross-country data. Using a panel of 13 European countries and the OECD Index of Employment Protection for regular and temporary workers, Brunello (2004) exploits within-country variation over time to estimate the effect of EPL on the incidence of workplace training. Hey finds a large, significantly negative effect of stricter EPL on training incidence for regular workers. The impact on temporary workers is just marginally significant and low in magnitude.

Pierre and Scarpetta (2004) combine cross-country variation in EPL with within-firm variation in managers' perceptions of the bindingness of labor laws in a difference-in-differences (DiD) framework, thereby exploiting variation in *de jure* and *de facto* EPL across countries and firms. They find that EPL increases training. In a follow-up paper, Pierre and Scarpetta (2013) use a comparable data set and method and confirm this finding. In addition, they show that these effects are more prevalent in small firms and in sectors with a high labor turnover. These two studies have the drawback that they measure *de facto* EPL based on subjective managers' perceptions. Using a similar DiD approach, Almeida and Aterido (2011) use objective measures for law enforcement, instead of subjective measures as managers' perceptions, to examine the effect of labor regulations on firm-provided training in developing countries. They find that firms that are subject to stricter labor regulations are more likely to train. However, the estimated effect size is small. Furthermore, they show that this effect is driven through legal restrictions on hiring—for example, by stricter regulation

of temporary work. On the other hand, legal restrictions on firing have a small negative effect that remains statistically insignificant.

Messe and Rouland (2014) exploit a reform of EPL in France that increased firm costs for employees aged above 50 and above in firms with more than 50 employees. However, the relatively small number of observations prevents them from employing a narrow bandwidth around the firm size discontinuity. Their DiD across time and firm size suggests that EPL increases training for employees aged 45-49, but not for those aged 50 and above.

Though they do not directly analyze EPL, Picchio and van Ours (2011) look at the impact of search frictions on training. EPL could be one reason for search frictions in the labor market. By exploiting within-firm variation of Dutch firms over time, they find that higher search frictions increase the incentives for firms to invest in the training of their workers.

Hence, the scant empirical evidence yields mixed results but suggests that EPL increases training. Therefore, we hypothesize that EPL increases training propensity and intensity.

2.3 The Institutional Background

This section considers the firm-size exemptions from employment protection legislation in Finland and Italy.

2.3.1 Firm-size exemptions in Finland

In Finland, the firm size relevant for the application of EPL is calculated based on head-count. The unit for the calculation of the size-threshold is the firm.³ Before July 1, 2007, firms with fewer than 30 employees were exempted from EPL. On July 1, 2007, this threshold was decreased to firms with less than 20 employees. All workers who are directly employed by a firm, including all non-permanent and part-time workers, regardless of their working hours, have to be counted. However, temporary workers who work only for a short period of time—seasonally or in any other way only exceptionally—are not counted for the calculation of the threshold. If an employee has a substitute (e.g., non-permanent), either the employee or the substitute are counted when calculating the threshold. Thereby, all different substitutes of one "normal" employee are regarded together as one employee. The CEO is not

³According to the Finish "Act on Cooperation Within Undertakings (ACU)", the legal unit defined by the ACU is an "undertaking". An undertaking can be a corporation, foundation or natural person engaged in financial operation regardless of whether the operation is intended profitable or non-profitable. In contrast to a "natural person" as the owner of a business, a corporation is often treated as a fictitious person by law. In this sense, the Finish definition of a business for which the ACU applies is rather flexible. Comparing this definition with that used in the CVTS (see section 3.3.1), we have no doubt that both coincide.

included in the calculation. Other persons who are not directly employed by the undertaking are not counted (e.g., temporary agency workers and employees of the subcontractor). Also, workers in public institutions are not included.

There are two ways in which a worker can be dismissed: on "common grounds" or for "personal reasons". Both will be explained in the following.

The case in which a firm above the size-threshold wants to dismiss or lay-off workers for lack of work or for other reasons based on so-called "common grounds" —e.g., a shut-down or re-allocation of the firms' premises⁴-it has to go through a rigorous consultation procedure, provided that the reduction is substantial and permanent(ILO, 2016a).⁵ Besides the firm, this procedure involves the workers, workers' representatives, and local labor authorities (e.g., public employment offices). Before consultation starts, the firm has to inform the involved parties about the intention of the dismissal. Further, it has to provide the workers and their representatives with detailed information about the reasons for planned measures, estimate how many employees will be involved, etc. During the consultation meeting, redundancies, reinstatement and re-training options have to be discussed. If 10 or more workers are involved in a dismissal, the firm has to prepare an action plan for the re-employment of the workers. If not otherwise agreed upon by all parties, the consultation lasts at least 14 days (for measures that concern fewer than 10 workers or for lay-offs that last 90 days or less) and a maximum of 6 weeks (for measures that concern more than 10 workers and lay-offs that last more than 90 days). In case of non-compliance with the regulations regarding dismissals on common grounds, workers are entitled to receive a compensation of at most 30,000 euros as indemnification from the employer (Nordman-Rajaharju et al., 2012).

On the contrary, if a firm below the size threshold wants to dismiss or lay-off workers for a reason based on "common grounds", it must only notify the worker and local labor authorities about its plans (OECD, 2016). Hence, the costs of dismissing workers are substantially higher for firms with more than 20 workers, because the rules and procedures when dismissing a worker are much more complex (or before the size threshold was decreased on July 1st 2007: firms with 30 or more workers).

In case a firm wants to dismiss an employee for individual or so-called "personal reasons"

⁴This includes: any major changes in work duties, working methods, working hours or organization of work; any major acquisitions of machinery and equipment; any major rearrangement of the working premises and changes in the range of goods and services offered; shutdown of the employer's operations or a part thereof; transfer of the operations to another location or any major expansion or reduction of the employer's activities; transfers of business, demergers or mergers; changing of full-time employment into part-time employment, and lay-offs or terminations of employment contracts for economic, production-related or re-organizational reasons.

⁵Thereby "permanent" means that the reduction of work has to continue for at least a period of 90 days.

- such as serious violation or omission of the employee's essential duties or incapacity to work due to a change of conditions that are in the responsibility of the employee (e.g., imprisonment), the same rules apply for all firms regardless of size. According to these rules, the employer must inform the employee about the reasons for dismissal and give him the opportunity to be heard regarding these reasons. The employee can request an advance discussion with a worker representative (OECD, 2016).

In general, the reason for the termination of an employment relation must be objective and weighty. Termination due to illness without permanent disability to work or discriminatory reasons (race, gender, etc.) is prohibited. In case the employer fails to provide valid reasons for a dismissal for individual reasons, he has to pay the worker a minimum of three months' or a maximum of 24 months' salary (ILO, 2016a).

Despite the change in the firm-size exemption on July 1, 2007, the definition of collective dismissal has always been based on firms with more than 20 employees: it includes dismissals of more than nine workers in firms with more than 20 employees at a time for financial or production-related reasons (OECD, 2016). Since this exemption remained the same, we argue that the resulting bias has remained constant over time.

2.3.2 Firm-size exemptions in Italy

In Italy, firms with more than 15 full-time equivalent workers in the same establishment, office or autonomous department located in the same city (5 workers in the agricultural sector) or more than 60 workers in total (summing together all locations) are subject to stricter EPL than firms below this size threshold.⁶ Not included in the threshold are apprentices, workers who are not directly employed by the firm (e.g., temporary agency workers and the employees of a subcontractor), and workers with contracts valid for less than 9 months. Part-time workers are counted according to their working time. Sector-specific collective agreements define the full-time equivalent number of working hours. These are constrained by legislation: the upper bound of a regular work week is fixed at 40 hours, the minimum holiday is four weeks paid leave, and the maximum number of over-time hours in a year is 250 (Toffoletto et al., 2016).

In general, a dismissal is considered unfair unless it is for a just cause, such as a serious breach of a worker's legal or contractual duties or a justified motive, such as reasons related to the economic situation of the firm (e.g., a reorganization or shut-down). A notice of termination is required only in the latter case. In case of a dismissal for a just cause or justified motive, no costs incur for the dismissal (Toffoletto et al., 2016).

⁶An establishment is a smaller unit as a firm, which can consist of several establishments.

However, if a just cause or justified motive is not given, the dismissal is considered unfair. If the concerned worker considers his dismissal unfair and submits his case to a court, differences in the applicability of EPL with respect to establishment-size come into play. If the court judges the dismissal unfair, a firm larger than the threshold must reinstate the worker and re-pay him from the day of dismissal until reinstatement; in any case, a minimum of 5 months' salary. In addition, firms must pay a penalty for the omitted social-security contributions for the time between dismissal and reinstatement of the worker. If the worker refuses the reinstatement, the firm must pay him an indemnity amounting to a 15-24 months' salary, varying by age, tenure and firm size (OECD, 2016). Regarding collective dismissals, if a firm with more than 15 workers in a single production unit or in several units within one province dismisses more than 5 workers in a period of 120 days, it is obliged to negotiate with unions and the government in order to reach an agreement on the dismissals (OECD, 2016).

In firms smaller than the size threshold, a worker who was dismissed unfairly has no right to be reinstated but is entitled to a compensation ranging from 2.5 to 6 months' salary. If the employee is reinstated, he or she has no right to claim the wage loss between the dismissal and reinstatement (ILO, 2016a). Firms with fewer than 15 workers are completely exempted from the definition of collective dismissals (OECD, 2016).

As Hijzen et al. (2013) point out, the cost of an unfair dismissal is substantially higher for firms above the size-threshold, as these firms are obliged to reinstate workers and to pay them their foregone wages during the often rather long trial period. Further, Hijzen et al. (2013) state that, in practice, judges tend to have a broad definition of unfair dismissals and that court decisions tend to be more pro-worker if local labor market conditions deteriorate. Both of these facts increase the uncertainty about the outcome of a dismissal, even if it was for a "just cause" and thereby the cost of dismissals.⁷ Finally, dismissals that are considered unfair often result in the reinstatement of workers.

2.3.3 Other Firm-size Exemptions at the Same Size Threshold

The size threshold in Finland and Italy analyzed in this paper not only matters for the applicability of EPL, but also for other firm regulations, which are discussed briefly in the following, while Appendix A discusses the exceptions in detail. Concretely, in both Finland and Italy, the same size threshold applies to the need to organize employee representation, for example in the form of a works council. Furthermore, in Finland, the law requires firms above the same threshold to fix annual training objectives, though this is not binding. Also

⁷As shown by a study of Ichino et al. (2003).

note that the Finnish regulation of collective dismissal applies only to firms with more than 19 employees and that this size threshold did not change between 2005 and 2010. Finally, the same firm size threshold also regulates two minor issues that are unlikely to affect our identification strategy. Concretely, in both Italy and Finland, firms above the same size threshold need to appoint a health and safety board, and Italian firms employing 16-35 workers must hire at least one disabled worker, firms with 36-50 workers 2 disabled workers and firms with 51 and more workers must reassure that 7% of their workforce consist of workers with disabilities (Eurofund, 2016).

3 Data and Empirical Methodology

Our strategy to identify the impact of EPL on training consists of two steps. In a first step, we exploit variation in EPL that arises due to the exemption of small firms below a certain firm size threshold from substantial parts of EPL in a regression discontinuity design (RDD) framework. In a second step, we use a heterogeneous treatment framework to determine whether the effect of EPL is higher in sectors with higher employment volatility (see, e.g., Raghuram G. Rajan, 1998; Haltiwanger et al., 2008; Bassanini and Garnero, 2013; Hijzen et al., 2013).

3.1 Data

This paper uses data from the third and fourth wave of the Continuing Vocational Training Survey (CVTS3 and CVTS4), which was conducted by Eurostat among firms with more than 10 employees in 19 EU countries. The survey was carried out in 2006 and 2011 for the reference years 2005 and 2010, respectively. The representative firm samples are stratified by industry affiliation and firm size. Our analysis uses data for Finland and Italy.

The unit of observation used in the CVTS is an "enterprise", which is defined as "the smallest combination of legal units that form an organizational unit or a sole legal unit, producing goods or services with a certain degree of autonomy in decision-making, carrying out one or more activities at one or more locations" (Eurostat, 2005). For both Finland and Italy, this definition is in line with the legal definition of the firm.⁸

⁸In the case of Italy, the congruency between the legal definition and that used in the CVTS could be diverging for firms with more than 60 workers, since the legal definition changes for firms above this size-threshold.

3.2 Regression Discontinuity Design

The main variation used to identify the impact of EPL consists of exemptions for small firms from EPL that exist in both Finland and Italy. As stated in the previous section, before July 1, 2007, EPL in Finland fully applied only to firms with 30 or more workers. Afterwards, it applied to firms with 20 or more workers. In Italy, EPL fully applies to firms with more than 15 full-time equivalent employees.

Since the discontinuity stems from a law, we employ a sharp RDD to identify the local average treatment effect of EPL on training by fitting the following empirical specification⁹:

$$Y_{it} = \alpha_t + f_t(F_{it}) + \gamma_t D_{it} + \epsilon_{it} \quad (1)$$

The dependent variable Y_{it} captures the training activities of firm i in time t . Training refers to pre-planned training with the aim to increase skills that is financed at least partly by the employer.¹⁰ Persons with apprenticeship or training contracts are not included. We use two measures of training activities. The first measure refers to the probability of firms to train their employees (*Probability to train*). The second measure captures the number of training hours during payed working hours (*Training hours*).¹¹ The error term ϵ_{it} differs between the estimates for *Probability to train* and *Training hours*. We use a Logit model for *Probability to train*. In order to account for the large number of firms with no training, we use a Tobit model with robust standard errors for *Training hours*. D_{it} denotes a dummy variable that takes the value 1 for firms above the size threshold and 0 for firms below it. Hence, our main coefficient of interest is γ_t that captures the year-specific treatment effect of being subject to EPL on training. In addition to this version of the model, we also estimate a model that restricts γ_t to be equal in both years, thereby improving estimation efficiency.

In order to estimate equation (1), we need to define the bandwidth of observations used in the estimation. We chose the optimal bandwidth of the assignment variable by employing the methodology of Calonico et al. (2014). Thereby, the optimal bandwidth is estimated non-parametrically for each year and each country separately. Table 1 displays the results.

D_{it} is the assignment variable, i.e., the relevant firm size. As discussed above, firm size refers to the number of employees in Finland. Since the threshold refers to full-time equiva-

⁹According to Lee and Lemieux (2010), the RDD estimate corresponds to the "weighted" average treatment effect, where the weights restrict the validity of the estimate to the "neighborhood" around the threshold, i.e. the local average treatment effect.

¹⁰Focusing on general training that takes place public and private (lower and upper) secondary schools, tertiary non-university institutions and polytechnics, universities and other higher education institutions yields qualitatively the same results.

¹¹Using the number of trained employees to measure training intensity yields qualitatively the same results.

lent employers in Italy, we calculate firm size based on annual paid working hours normalized by the average collectively agreed upon annual working hours in 2005 and 2010 (Eurofund, 2006a,b), assuming that each employee has the legally specified 4 weeks of holidays in addition to the nation-wide public holidays. In line with the legal definition of the threshold in both countries, the CVTS excludes employed persons who hold an apprentice or training contract.

The function $f_t(\cdot)$ accounts for the different possible functional forms of the assignment variable. In order to optimize $f_t(\cdot)$, Tables 3.5 and 3.6 provided in Appendix 3B display six estimations for each year, country and dependent variable. The six variations include a linear, quadratic or cubic polynomial of the assignment variable, as well as the corresponding interactions on each side of the threshold. Using the Bayesian Information Criterion (BIC) to select the adequate functional form of the running variable, suggests the use of a linear specification of the assignment as summarized in Table 1. Note that the Akaike Information Criterion (AIC) supports substantially more complex models in some cases. However, these models are clearly over-specified as indicated by the unrealistically large effect sizes. Hence, we include a linear running variable for each year and each country in the estimation. Table 1 summarizes how we estimate equation (1).

Table 1: Summary of RDD

	Finland		Italy	
	2005	2010	2005	2010
Exemption Threshold				
Firm Size	29	19	15	15
Optimal Bandwidth				
Probability to train	17	9	5.91	3.1
Training hours	20	8	4.84	3.95
N at Optimal Bandwidth				
Probability to train	383	529	6515	4148
Training hours	470	467	5563	5283
Functional Form				
Probability to train	Linear	Linear	Linear	Linear
Training hours	Linear	Linear	Linear	Linear

Notes: To estimate the optimal bandwidth, the method suggested by Calonico et al. (2014) was employed. The bandwidth for Finland was rounded since the firm-size threshold is calculated based on a head-count. The functional form results from minimizing the BIC in Tables 3.5 and 3.6 provided in Appendix 3B.

3.3 Low and High Employment Volatility Sectors

We complement the classical RDD with a heterogeneous discontinuity framework that tests the hypothesis that the effect of EPL is stronger in sectors characterized by high employment volatility. This approach exploits the fact that sectors differ in their need to adjust their workforce due to factors unrelated to EPL in reaction to changes in market conditions or technologies. Consequently, sectors with high employment volatility are more affected by EPL than sectors in which employment volatility is relatively low.

We estimate the following model:

$$Y_{it} = \alpha_t + f_t(F_{it}) + \beta_t V_{it} + \gamma_{1t} D_{it} + \gamma_{2t} D_{it} * V_{it} + \epsilon_{it} \quad (2)$$

V_{it} denotes a dummy variable that indicates whether firm i at time t operates in a sector with an employment volatility that is above the median. Hence, $D_{it} * V_{it}$ represents the interaction of EPL and employment volatility. We expect γ_{1t} and γ_{2t} to have a positive sign, thus implying that high-volatility sectors that are more affected by stricter EPL are expected to train more frequently. The estimation methodology, bandwidth selection and choice of $f_t(F_{it})$ remains the same in this estimation as in equation (1). As in model 3.1, we also estimate models that restrict γ_{1t} and γ_{2t} to be the same in both years to increase estimation efficiency.

Following Haltiwanger et al. (2008), we measure employment volatility by the job reallocation rate, which is defined as the sum of the job creation and job destruction rate. In order to ensure the exogeneity of our employment volatility measure, we use the sector-specific job reallocation rate for the USA provided by Haltiwanger et al. (2008). The USA is an ideal candidate for this measure, because it has the least strict EPL among all OECD-countries (OECD, 2016). This ensures that our employment volatility measure reflects characteristics of the market and technology rather than being the result of EPL. We further increase the exogeneity of the measure by using information from the past, i.e., for the years 1990, 1991 and 1994-1996. Note that this approach to strictly ensuring exogeneity of the measure has the drawback that the predictive power of our measure for Finnish and Italian firms in 2005 and 2010 might be relatively low.

Using this heterogeneous RDD approach has two advantages. First, it helps to increase the precision of estimates if pre-treatment controls and post-treatment outcome variables are correlated, i.e., if unobserved heterogeneity across firms exists (Hijzen et al., 2013). Second, since the size threshold in Finland and Italy analyzed in this paper not only matters for the applicability of EPL, but also for other regulations concerning firms, this approach helps to disentangle the effect of EPL from that of other policies applying at the same size threshold.

In order to assess the relevance of these potential confounding factors, it is important to note that we expect these threshold effects to increase training. This is obvious in the case of the training plan in Finland. Furthermore, the literature argues that work councils increase training (see, e.g., Stegmaier, 2012; Addison et al., 2004). Hence, these confounding factors suggest that our RDD estimates represent an upper bound for the true effect of EPL on training. Nevertheless, we further analyze the results from the heterogeneous RDD that account for these potentially confounding factors.

3.4 Descriptive Statistics

Tables 7 and 8, provided in Appendix 3B, display the summary statistics of the dependent variables, differentiated by country, year and firm size. In addition, they display summary statistics of three control variables that are used to test the validity of the RDD. They also present the summary statistics of employment volatility used in the heterogeneous RDD. The control variables comprise industry affiliation (1-digit NACE Rev 1.1 for the CVTS3 and Rev 2.1 for the CVTS4)¹², the total labor costs (*Labor Costs*), the share of female workers in the firm (*Share females*) and whether the firm has introduced an innovation (*Innovation*).

Tables 7 and 8 reveal that 54% and 59% of Finnish firms with fewer than 40 employees trained their employees in 2005 and 2010, respectively. The corresponding values for Italian firms with less than 40 FTE employees was substantially lower in 2005 with only 28%, though the value increased to 45% in 2010. Regarding training hours, both countries experienced a slight increase between 2005 and 2010. In Finland, the average increased from 128 hours to 138 hours, while the corresponding increase in Italy changed from 67 hours in 2005 to 72 hours in 2010. These descriptive statistics suggest that training during paid working hours plays a larger role in Finland than in Italy.

4 Validity of the RDD Framework

The identification strategy of the RDD relies on the assumption that firms are "as good as" randomly assigned around the cut-off, i.e. that firms do not self-select into or out of treatment. This means that we want to isolate the different responses of firms with respect to our outcome variables that are due to the differential role of EPL provisions that apply to firms above and below the size threshold, and not those that are unrelated to EPL or the endogenous response of firms to EPL.

¹²We have aggregated some of the service industries to account for the fact that our employment volatility data refers to this aggregation level.

Technically speaking, the identification assumption rests on two assumptions. First, the RDD is valid only if the probability to receive treatment is continuous except at the threshold. Since the discontinuity stems from a law, we assume perfect compliance of the firms treated in this setup. Second, the RDD is only valid if the local continuity restriction holds, which reassures the comparability of "individuals" – in our case firms – in a small interval around the threshold. This implies that all other observable and unobservable firm characteristics that influence the assignment variable – in our case firm-size – are "continuously" related to the assignment variable (random assignment assumption).

This section presents the results of two tests we used to check the validity of the second assumption. The first test, the McCrary test (McCrary, 2008), analyzes whether the distribution of the assignment variable displays a jump at the threshold, which would imply a selection problem. The second tests the validity of the random assignment assumption by checking if observable characteristics of firms just above and below the size-threshold differ substantially or not. A continuous relationship of the firm characteristics imply similarity and constitute a proof of the validity of the RDD.

4.1 McCrary Test

The McCrary test looks at the smoothness of the distribution of the assignment variable at the threshold and it assesses whether the density function of the assignment variable, firm-size, has a jump at the threshold. In case of a statistically significant discontinuity estimate at a relevant significance level, rejection of the null hypothesis of continuity constitutes evidence for selection around the threshold. To ensure the validity of the McCrary test, we must assure that the possible self-selection occurs only in one direction, above or below the threshold. In the present case, we expect that, if at all, selection would take place below the threshold, i.e., that firms employ a lower number of workers in order to avoid being subject to EPL. This would correspond to a negative sign of the discontinuity estimate.

Figures 1, 2, 3 and 4 display the results of the McCrary test for Finland and Italy in the years 2005 and 2010, respectively. In order to improve the readability of the graph, the sample is restricted to a bandwidth of 20. Visual inspection of the graphs indicates no self-selection of firms under the threshold for Finland in both years, 2005 and 2010, or Italy in 2005. The statistically insignificant results of the McCrary test further support these results.

Rather surprisingly however, the results do provide some concern for Italy in 2010, where the visual inspection does suggest a potential selection below the threshold, though we also see jumps of similar size for other values of the firm size. The McCrary test confirms these

Figure 1: Histogram and McCrary test: Finland 2005

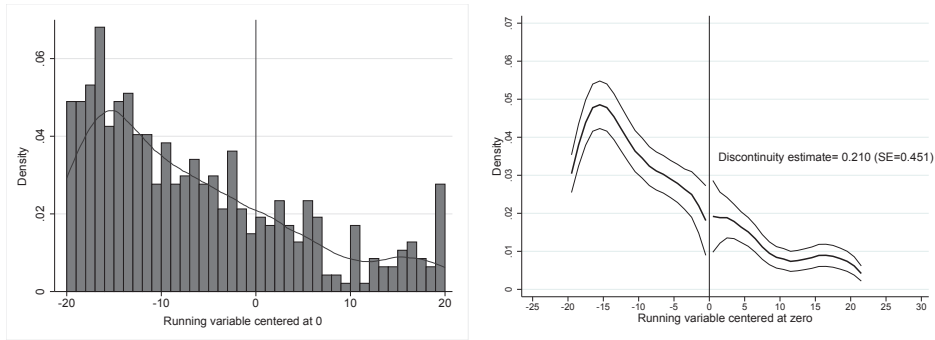


Figure 2: Histogram and McCrary test: Finland 2010

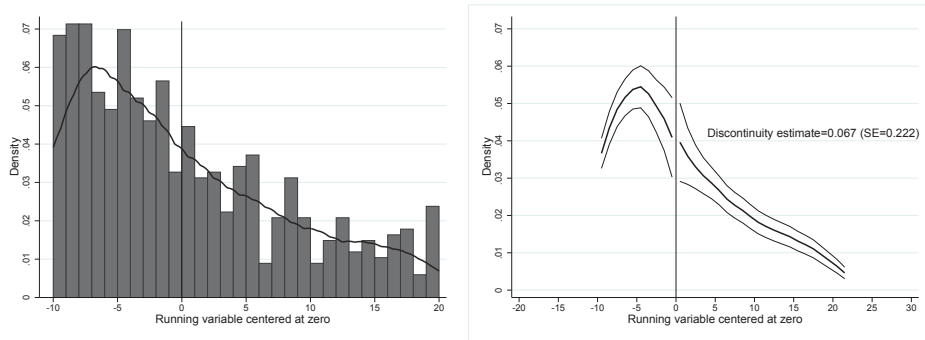


Figure 3: Histogram and McCrary test: Italy 2005

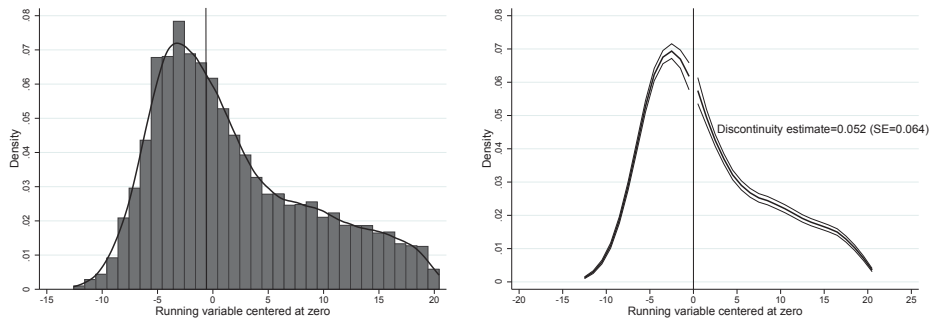
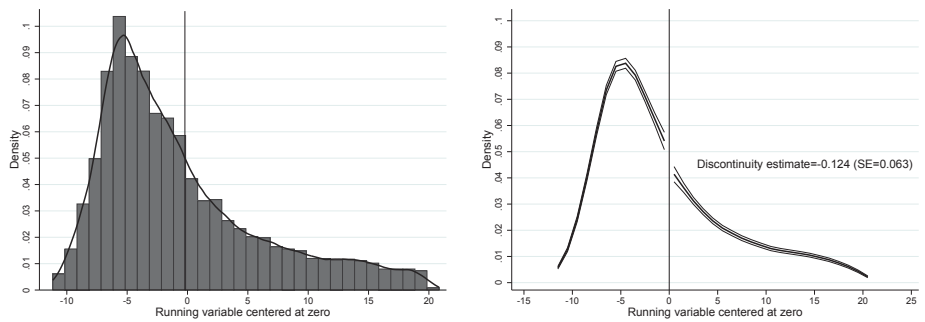


Figure 4: Histogram and McCrary test: Italy 2010



concerns, as the discontinuity estimate is statistically significant at the 5%-level (t-statistic of 1.97) and negative, suggesting that firms select below the size threshold to escape EPL. This results must be kept in mind for the empirical part of the paper, since it invalidates the RDD estimates for Italy in 2010.

4.2 Continuity of Observable Firm Characteristics

Secondly, we test the validity of the RDD design by examining whether observable firm characteristics, namely industry affiliation, innovation propensity (*Innovation*), share of female workers (*Share females*) and labor costs (*Labor costs*), are locally balanced on either side of the threshold, as suggested by Lee and Lemieux (2010). We test this by regressing the observable firm characteristics on the binary threshold variable and the linear running variable for each country and each year. We use either OLS or a logistic regression model, depending on the nature of the observable firm characteristic (continuous or binary). If there is no selection problem (i.e. if the continuity assumption holds), we expect the discontinuity estimate to be insignificant, thereby implying that the covariates are locally balanced on either side of the size threshold.

The results of the covariate discontinuity estimates are displayed in Table 2. Only eight of the 76, or about 10% of the industry estimates are statistically significant. These are distributed across country-year estimations. This suggests that the potential selection of firms remains unrelated to the industry. The results for labor costs are statistically significant in three of the four estimations, though only at the 10%-significance level. Furthermore, the direction of the effect differs between Finland and Italy and the corresponding estimates regarding innovation remain insignificant, though the innovation estimate becomes significant for Italy in 2005. Finally, the estimates display one significant negative sign for the share of females above the threshold.

In summary, we conclude that the tests for the smoothness of the observation distribution and covariates at the size threshold support the validity of the RDD- except for Italy in 2010.

Table 2: Continuity of Covariates

	Finland		Italy	
	2005	2010	2005	2010
Mining&quarrying	-0.332 (0.877)	-0.124 (0.989)	-0.016 (0.388)	-0.136 (0.493)
Food&beverages	0.006 (0.725)	-0.915 (0.844)	0.318 (0.335)	-0.089 (0.289)
Textiles&leather	-1.715** (0.826)	-0.058 (0.734)	-0.480* (0.271)	-0.284 (0.218)
Pulp, Paper&printing	-0.250 (0.843)	0.980 (0.951)	-0.046 (0.285)	-0.150 (0.309)
Coke, petrol; chem.&fuel	0.353 (0.728)	-2.535*** (0.972)	-0.021 (0.267)	0.320 (0.302)
Basic&fabric. metals	-0.460 (0.857)	0.705 (0.813)	0.207 (0.244)	0.206 (0.243)
Machinery, electr.&optical	0.361 (0.786)	0.374 (0.879)	-0.051 (0.241)	0.214 (0.267)
Transport equipment	0.870 (0.818)	-1.114 (1.169)	-0.017 (0.538)	0.974* (0.555)
Wood&wood products	-0.179 (0.723)	-0.495 (0.827)	-0.558** (0.253)	0.373 (0.258)
Electricity, gas&water	0.453 (0.938)	1.238 (1.237)	-0.330 (0.583)	-0.455* (0.268)
Construction	0.549 (0.848)	0.102 (0.816)	0.086 (0.112)	0.028 (0.135)
Repair/Wholesale/Retail/Hotels	0.525 (0.583)	0.658 (0.445)	0.196 (0.144)	0.140 (0.130)
Transport/storage/information/comm.	0.059 (0.822)	0.184 (0.597)	0.387 (0.318)	-0.752*** (0.247)
Financial intermed./bus. serv.	-2.118** (1.013)	0.028 (0.741)	-0.181 (0.283)	0.363 (0.314)
Real estate & other serv.	1.155 (1.127)	-0.769 (0.787)	-0.170 (0.161)	-0.336 (0.260)
Labor costs	4047* (2423)	5438* (2902)	-65 (623)	-1235* (683)
Share females	-11.495** (5.479)	-4.885 (6.166)	-0.011 (0.014)	-0.444 (1.424)
N	470	467	5563	5283
Innovation	-0.049 (0.461)	0.020 (0.559)	-0.261** (0.131)	0.101 (0.117)
N	422	434	5563	5283

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$; The table shows local average marginal effects and standard errors of Logit estimates testing whether the variable differs below and above the cut-off. Estimates for average wages and female share refer to OLS with robust standard errors. N refers to the number of observations at the optimal bandwidth. All estimates further control for the running variable.

5 Results

In the following, we first present the results of a graphical analysis of the effect of EPL on training, then we present the regression results of the empirical analysis.

5.1 Graphical Analysis of Outcome and Assignment Variable

This section presents a visual analysis of the regression discontinuity design. Figures 5 and 6 plot the average training propensity and training hours against the assignment variable, i.e. firm-size, in addition to the density of the respective outcome variable. The vertical line indicates the EPL firm-size threshold. Firm size is measured in Finland by number of employees and in Italy by the rounded number of FTE employees, respectively.

The results provide little evidence for the hypothesis of an increase in training in response to stricter EPL, thus corresponding to an upward jump in the outcome variable at the threshold. In Finland for 2005, both the propensity to train and the average number of training hours actually decrease for firms just above the size threshold. In 2010, we see an increase in training propensity and hours that reflects a slump of training activity just left of the threshold. In Italy, the larger number of observations yield a smoother distribution of the average outcomes, providing no evidence of an increasing jump at the threshold for either training measure or year.

Figure 5: Relationship between the running variable and training: Finland

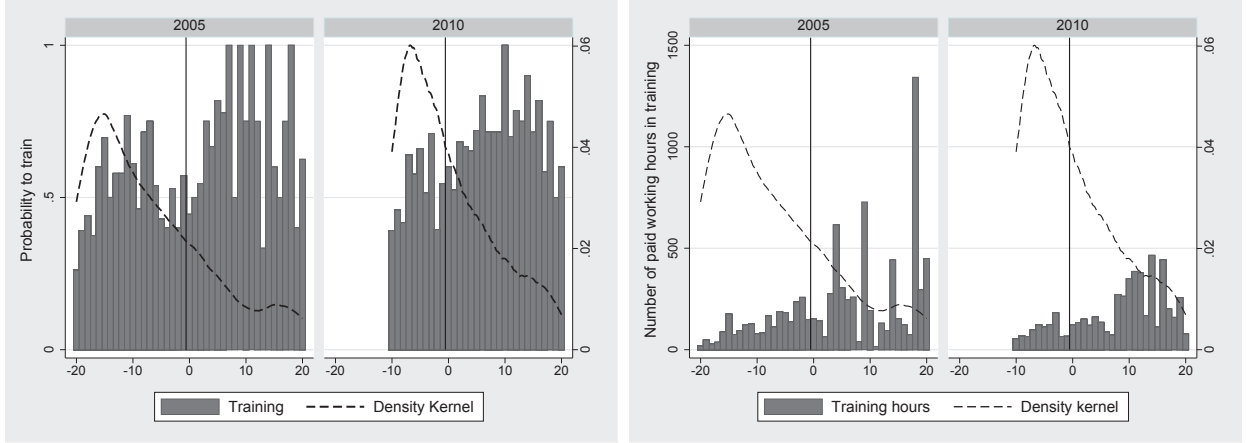
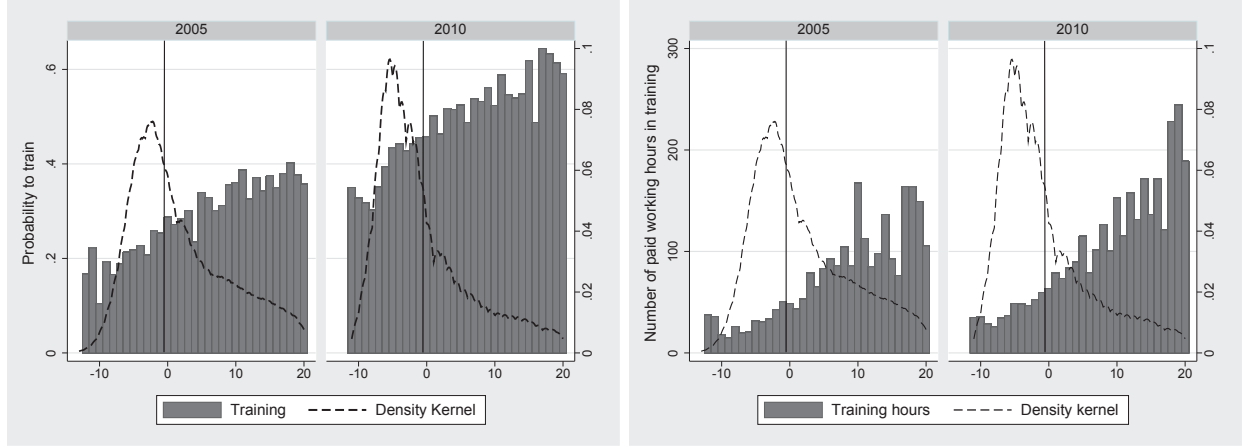


Figure 6: Relationship between the running variable and training: Italy



5.2 RDD Regression Results

The upper and lower panels of Table 3 test the results displayed in Figures 5 and 6 formally either by including a single treatment dummy or by including treatment dummies for 2005 and 2010 separately. For each outcome, we report three types of estimates. The first estimates refer to the baseline estimates based on equation (1). The second estimation type represents a robustness check that is commonly labeled a “Donut regression” (see, e.g., Barreca et al., 2011). In this robustness check, observations of the assignment variable just below and above the size threshold are excluded from the RDD regression, since these are most susceptible to manipulation, thus selection of firms above or below the threshold. This way, we further reduce the problem of measurement error in the assignment variable. The third estimation type includes observable firm characteristics as covariates in the regressions, namely labor costs, female share, innovation and fixed effects for the 1-digit industry. In-

cluding covariates in the estimation affects the standard errors but not coefficient estimates in a valid RDD setting (Lee and Lemieux, 2010). Hence, this test resembles closely the test whether observables are continuous at the size threshold shown in Table 2.

In line with the graphical analysis, the regression results of the restricted estimates with a single coefficient for both periods and countries provide no evidence of an increase in training due to EPL. Furthermore, while most of the estimates for the probability to train show a positive, those for the training hours have a negative sign, though they are not statistically significant.

Since the estimated effects are not statistically significant, it is very important to discuss the precision of the estimates. Generally speaking, standard errors remain relatively high in Finland. In the baseline model, the estimated effect on the probability to train is 3.6% and the upper confidence band with a probability of 5% amounts to 16.7%. This effects is economically significant compared to an average of 57% of firms that train their employees. Similarly, though the estimated effect indicates a decrease of 28.2 hours, the upper bound of the effect is an increase of 76.3 training hours, which amounts to roughly half of the sample average of 134 hours.

The larger sample size makes it possible to estimate the effects more precisely for Italy. However, since selection of firms is a problem in the year 2010 for Italy, the results of the overall treatment effect for both years together, as well as the treatment effect for the year 2010 are invalid. Still, we shortly refer to the magnitude of the overall treatment effect for Italy. The baseline estimate for the overall treatment effect for Italy suggests that EPL increases the training propensity by 0.5%. The upper confidence band with a probability of 5% amounts to 4%. Compared to a sample average of 38% of firms that train their employees, this effect would be economically significant, if selection would not be a problem. Regarding training hours, the baseline estimate suggests that EPL decreases training by 4.2 hours, with an upper bound of the effect of 26 training hours. Again, if selection was not problem, this effect would be economically significant, compared to a sample average of 69 training hours.

Comparing the models with a single restricted coefficient to the models with a separate estimate for each year supports the above results, as none of the estimated treatment effects takes a positive, significant sign. Furthermore, while many of the estimates for the probability to train are positive, most estimates for the number of training hours have a negative sign, though they are not statistically significant. While the point estimates differ between 2005 and 2010, the differences are not systematic across countries, outcomes, and models.

Table 3: RDD estimates

	Probability to train			Training hours		
	Baseline	Donut	Covariates	Baseline	Donut	Covariates
Finland						
Treat	0.036 (0.067)	0.059 (0.078)	-0.014 (0.066)	-28.225 (53.308)	-47.854 (66.329)	-44.128 (54.039)
N	912	844	834	937	869	856
Treat 2005	0.075 (0.102)	0.124 (0.106)	0.041 (0.101)	-67.366 (86.699)	-66.176 (97.113)	-47.093 (87.137)
Treat 2010	0.009 (0.083)	-0.001 (0.103)	-0.050 (0.080)	7.912 (63.693)	-24.093 (85.559)	-41.389 (65.851)
N	912	844	834	937	869	856
Italy						
Treatment	0.005 (0.018)	-0.024 (0.028)	0.008 (0.017)	-4.196 (15.463)	-12.250 (25.586)	1.442 (14.924)
N	10663	8177	10663	10846	8360	10846
Treat 2005	0.001 (0.021)	-0.009 (0.030)	0.004 (0.020)	-27.649 (24.768)	-38.844 (38.236)	-17.207 (24.024)
Treat 2010	0.011 (0.031)	-0.068 (0.060)	0.015 (0.030)	16.813 (19.234)	15.718 (33.573)	18.093 (18.493)
N	10663	8177	10663	10846	8360	10846

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$; The table displays local average marginal effects and standard errors of Logit estimates for the probability to train as well as local average marginal effects and robust standard errors of Tobit estimates for the number of training hours in working time. Donut estimates exclude observations within a bandwidth of one around the threshold. All estimates include separate variables for the running variable in 2005 and 2010 as well as a dummy variable for 2010. Covariate estimates further include controls for labor costs, female share, innovation and fixed effects for the 1-digit industry.

The models that allow for different effects of EPL in 2005 and 2010 further display substantially higher standard errors than the restricted model for the overall treatment effect. Generally speaking, the standard errors remain relatively large in Finland. In the baseline model, the estimate of EPL on the probability to train is 7.5% in 2005 and 0.9% in 2010. The upper bound of the 95% confidence band is 27% and 17%, respectively. Since this effect is compared to an average of 57%, it is economically significant. Similarly, the estimated effects indicate a decrease of 66 and an increase of 8 hours in 2005 and 2010, respectively. However, the upper bound of the effect is an increase of 103 and 133 training hours in 2005 and 2010, respectively. This is almost the sample average of 134 hours.

In the baseline model for Italy, EPL increases training propensity by 0.1% and 1.1% in 2005 and 2010, respectively. Again, it has to be emphasized that, due to a selection problem, the treatment effect for 2010 is invalid in the case of Italy. We only report the estimates for this year for completeness. The corresponding upper bounds of the 95% confidence interval amounts to (plus) 4.7% and 7.1% in 2005 and 2010, respectively. Though the average training propensity of 38% among all firms remains lower in Italy than in Finland, this represents a relatively small effect. The estimated effect for training hours amounts to a decrease of 28 hours in 2005 and an increase of 17 hours in 2010. The corresponding upper bounds are an increase of 21 and 55 hours, respectively. Hence, the precision of the intensive margin remains smaller, as this represents a substantial portion of the average of 69 training hours.

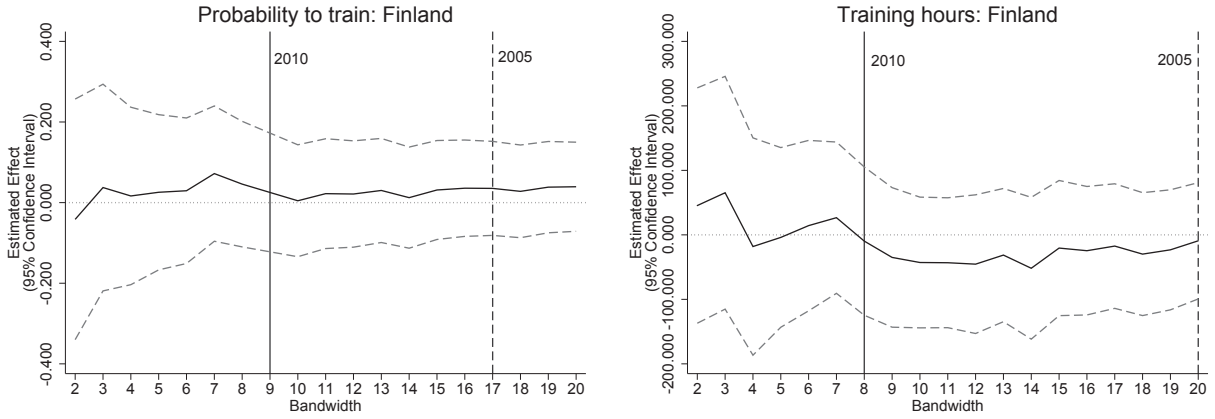
Regarding the stability of estimates across estimation type, Table 3 suggests that omitting observations around the threshold has little effect on the estimates in Finland. In Italy, the Donut estimates turn even more negative than the baseline estimates, though the loss of a fifth of observations slightly reduces estimation precision. This implies that a potential selection bias actually tends to overestimate the true effect, adding further evidence to the case that EPL has no effect on training. The inclusion of additional covariates affects the estimates only slightly. The exception is the probability to train in Finland, for which the estimated effect decreases to 0.003. Hence, the estimates including covariates support the validity of the RDD.

These results also hold in the unrestricted model, which allows for a different effect in 2005 and 2010. With the exception of the probability to train in Finland for 2005, Donut estimations suggest a lower effect of EPL than the corresponding baseline estimates. Furthermore, including covariates decreases the estimates in Finland, while the estimates for Italy remain similar after increasing only slightly.

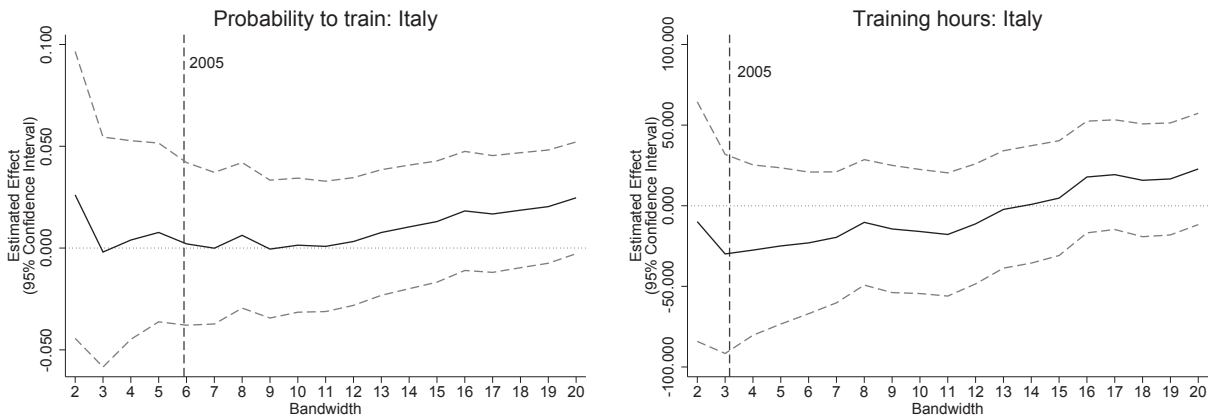
For Finland, Figure 7 visualizes the robustness of the baseline results of the restricted model with a single coefficient for both years, i.e. the overall treatment effect, to the choice of the bandwidth. For Italy, Figure 7 visualizes the robustness of the baseline results of the model for the year 2005 to the choice of the bandwidth. We only show the results for 2005, since a selection problem was detected in Italy in 2010. Similarly, Figure 9 in the Appendix tests the robustness of the Donut estimates for both years for Finland and for 2005 for Italy. These figures plot the estimated discontinuity effect (solid line) along with the upper and lower bound of the 95% confidence interval around this coefficient (dashed lines) for bandwidths between 2 and 20. A solid line above/below the zero line represents a positive/negative discontinuity estimate. This estimate is significant so long as both confidence bands are below the zero line when the coefficient is negative or above the zero line

when it is positive. The results suggest that the choice of the bandwidth has little effect on the estimates.

Figure 7: RDD baseline estimates



(a) Finland: Overall treatment effect for both years (2005 & 2010)



(b) Italy: Year-specific treatment effect for 2005 only

Notes: The figures displays local average marginal effects and 95% confidence intervals of Logit estimates (Probability to train) and Tobit estimates with robust standard errors (Training hours) of the effect of EPL on training for different bandwidths around the threshold. N for Finland is 178, 249, 323, 420, 481, 549, 634, 710, 788, 812, 849, 879, 916, 951, 988, 1036, 1068, 1102, 1143. N for Italy is 2404, 3583, 4680, 5712, 6594, 7248, 7721, 8162, 8469, 8738, 8962, 9154 9323, 9503, 9679, 9831, 9967, 10093, 10209.

5.3 Low and High Employment Volatility Sectors

This section discusses the results of the heterogeneous RDD approach, which compares the impact of EPL in low and high volatility sectors (Raghuram G. Rajan, 1998; Haltiwanger et al., 2008; Bassanini and Garnero, 2013; Hijzen et al., 2013). This approach has two benefits: improving estimation efficiency in the presence of unobserved heterogeneity, and

helping to disentangle the effect of EPL from that of other policies that have the same size threshold. The key assumption for this strategy is that the variation in the impact of EPL across sectors, which depends on intrinsic sector volatility, is independent of the variation due to self-selection of firms into size groups across sectors (Hijzen et al., 2013). In other words, that firms in sectors that have a relative higher employment volatility do not try to escape from EPL in a systematical way. As shown in the previous section, we do not find sizable effects of firms below and above the size threshold when analyzed by industry (see Table 2) and that controlling for industry has little effect on the RDD estimates. In addition, Table 9 in the Appendix displays the results of sector-specific McCrary tests. In Finland, none of the selection estimations suggests a significant jump, while in Italy five of the 20 estimates are significant. The correlation of the t-statistics with employment volatility suggests an insignificant negative correlation in Italy. In Finland the correlation in 2005 is significantly negative, but it turns insignificantly positive in 2010. Note that a negative correlation suggests that firms in high volatility sectors are more likely to select below the threshold than firms in low volatility sectors. Hence, though these correlations remain insignificant, estimating Donut estimations represents an important robustness check.

Table 4 displays the results of the heterogeneous RDD approach for low and high volatility sectors. As before, the upper and lower panel show the results for Finland and Italy, respectively. The first estimates for each country show the results from the restricted model with a single coefficient for both years, while the second estimates for each country display the results of models allowing for different coefficients in 2005 and 2010. Columns refer to the outcome variable and the estimation type. The table displays the local average treatment effect for firms in low (*Treat Low Vola*) and high (*Treat High Vola*) employment volatility sectors in addition to the heterogeneous RDD estimate that contrasts these two local average marginal effects (*Treat*High Vola*).¹³

The results reveal that the average marginal effects remain insignificant in both the low and high volatility sectors. Importantly, difference between these marginal effects remains insignificant in all estimations with the only exception of a marginally significant interaction effect in the Donut estimation of training hours in Italy for 2010.

¹³Figure 10 in the appendix displays the heterogeneity of the treatment effect for a model that includes sector-specific employment volatility as a demeaned linear variable rather than as a dummy. Since testing the slope in the applied non-linear models is problematic, the following discussion focuses on the model with binary employment volatility in the sector.

Table 4: Heterogeneous RDD estimates for sectors with low and high employment volatility

	Probability to train			Training hours		
	Baseline	Donut	Covariates	Baseline	Donut	Covariates
Finland						
Treatment Low Vola	0.064 (0.075)	0.089 (0.085)	0.024 (0.077)	-58.835 (59.886)	-73.096 (73.742)	-70.528 (59.552)
Treatment High Vola	-0.018 (0.079)	0.031 (0.089)	-0.054 (0.072)	-57.952 (61.506)	-52.458 (73.840)	-70.690 (62.263)
Treatment*High Vola	-0.081 (0.071)	-0.058 (0.075)	-0.078 (0.070)	0.883 (54.953)	20.638 (59.505)	-0.163 (54.497)
N	870	808	794	891	829	812
Treat 2005						
Treat 2005 Low Vola	0.066 (0.113)	0.112 (0.111)	0.045 (0.130)	-84.268 (107.057)	-83.533 (117.207)	-83.602 (109.274)
Treat 2005 High Vola	0.030 (0.122)	0.108 (0.129)	-0.007 (0.115)	-106.279 (97.995)	-94.044 (109.770)	-71.129 (100.390)
Treat 2005*High Vola	-0.036 (0.115)	-0.004 (0.116)	-0.052 (0.122)	-22.011 (101.401)	-10.511 (106.769)	12.473 (107.426)
Treat 2010						
Treat 2010 Low Vola	0.068 (0.094)	0.068 (0.115)	0.024 (0.094)	-23.646 (67.793)	-44.767 (89.310)	-49.489 (67.971)
Treat 2010 High Vola	-0.068 (0.096)	-0.047 (0.113)	-0.100 (0.088)	-24.531 (74.554)	-21.947 (94.865)	-91.741 (73.935)
Treat 2010*High Vola	-0.136 (0.087)	-0.115 (0.093)	-0.124 (0.086)	-0.885 (63.493)	22.820 (68.436)	-42.252 (62.493)
N	870	808	794	891	829	812
Italy						
Treatment Low Vola	0.001 (0.020)	-0.035 (0.029)	0.006 (0.017)	-7.408 (18.324)	-20.746 (28.167)	3.692 (17.634)
Treatment High Vola	0.008 (0.020)	-0.016 (0.031)	0.008 (0.020)	0.838 (17.243)	-1.756 (27.325)	-0.513 (16.646)
Treatment*High Vola	0.007 (0.019)	0.019 (0.021)	0.002 (0.018)	8.245 (16.751)	18.990 (19.547)	-4.205 (16.073)
N	10481	8034	10481	10676	8229	10676
Treat 2005						
Treat 2005 Low Vola	-0.015 (0.024)	-0.021 (0.032)	-0.009 (0.019)	-36.016 (28.436)	-41.450 (41.746)	-20.218 (27.533)
Treat 2005 High Vola	0.010 (0.024)	-0.004 (0.033)	0.013 (0.026)	-23.831 (28.458)	-35.539 (41.382)	-19.278 (27.676)
Treat 2005*High Vola	0.025 (0.022)	0.018 (0.025)	0.022 (0.022)	12.185 (26.810)	5.911 (30.957)	0.939 (26.028)
Treat 2010						
Treat 2010 Low Vola	0.025 (0.035)	-0.075 (0.061)	0.030 (0.032)	14.604 (23.457)	1.104 (37.033)	21.449 (22.595)
Treat 2010 High Vola	0.010 (0.035)	-0.041 (0.066)	0.005 (0.032)	28.993 (20.725)	46.358 (35.035)	19.834 (19.802)
Treat 2010*High Vola	-0.015 (0.032)	0.035 (0.039)	-0.025 (0.029)	14.389 (20.703)	45.254* (24.044)	-1.615 (19.683)
N	10481	8034	10481	10676	8229	10676

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$; *Treat Low Vola* and *Treat High Vola* refer to the local average marginal effect of treatment in low and high volatility sectors, respectively. *Treat*High Vola* refers to the difference between the local average marginal effects in sectors with low and high employment volatility. The table displays Logit estimates and standard errors for the probability to train as well as Tobit estimates and robust standard errors for the number of training hours in working time. Donut estimates exclude observations immediately around the threshold. All estimates include separate variables for the running variable in 2005 and 2010 as well as a dummy variable for 2010. Covariate estimates further include controls for labor costs, female share, innovation and fixed effects for the 1-digit industry.

However, since 2010 is the year for which selection was a problem in Italy, though statistically significant, the estimate is invalid. With the exception of the estimates for the probability to train in Finland, the baseline and the Donut estimates depict a positive value in line with the hypothesis that EPL increases training. Note that the Donut estimates are more positive than the baseline estimates, which might suggest that selection plays a larger role in high-volatility sectors. The estimates including the covariates yield relatively similar estimates and indicate a more negative effect than the baseline estimates. Hence, we conclude that the heterogeneous RDD estimates provide no evidence that EPL increases training.

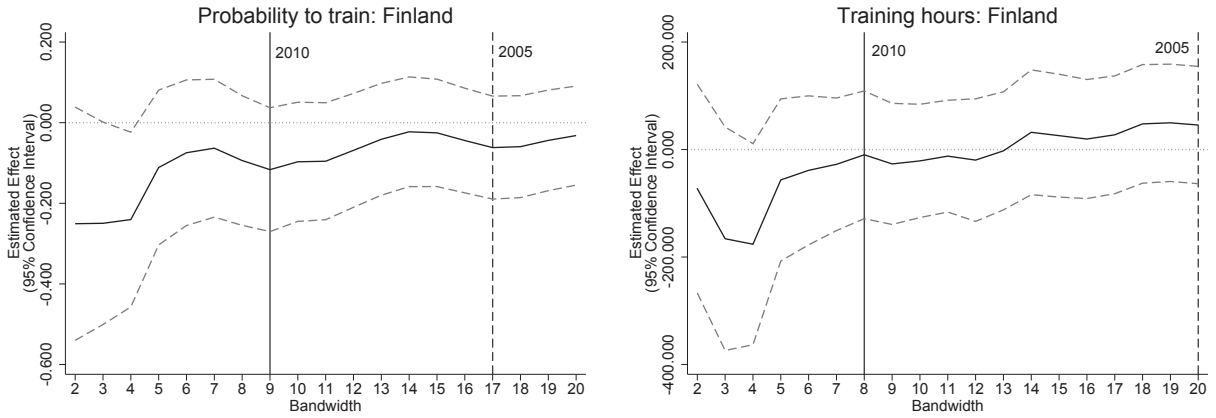
Rather surprisingly, comparing the efficiency of the heterogeneous RDD to the RDD yields mixed results. Using the heterogeneous RDD substantially reduces the upper bound of the effect on the probability to train in Finland from 16.7% to 6% compared to the baseline RDD results, while the upper bound remains about the same for Italy. In the case of the intensive margin, however, the upper bound increases compared to the RDD estimates.

Analyzing the results of the unrestricted model that allows for separate coefficients for 2005 and 2010 yields similar results as almost none of the estimates is statistically significant positive, with the exception of the estimate for the intensive margin in the Donut regression for Italy in 2010, which is statistically significant at the 10%-level. As mentioned before, this result is invalid, as selection is a problem in Italy in 2010. Furthermore, the difference between the estimates for 2005 and 2010 remains statistically indistinguishable. The contrast of margins of treatment for low and high volatility sectors is lower for the intensity to train in Finland, while the differences between 2005 and 2010 remain unsystematic otherwise. However, unlike in the baseline models, the standard errors remain similar in the unrestricted model of the heterogeneous RDD estimates. Hence, the upper bounds of the estimates are similar in the restricted and unrestricted model. An exception are the estimates for Finland in 2005, for which upper bounds are substantially higher compared to those in the unrestricted model.

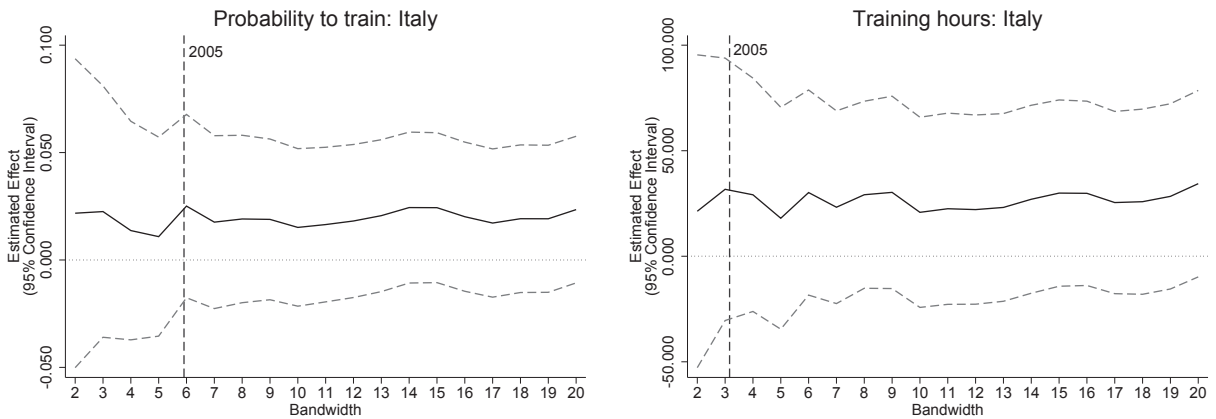
Figure 8 tests the robustness of the estimations regarding the bandwidth choice. It shows the results for the overall treatment effect for Finland for both years (2005 and 2010) and, as a result of the selection problem in Italy in 2010, the year-specific treatment effect for Italy for the year 2005. The results suggest that other bandwidth choices provide even less evidence for an increase in training due to EPL than the presented estimates. Concretely, in Finland, where the optimal bandwidth choices are relatively broad, we see that the estimates increase with the bandwidth choice and nearly turn significantly negative for very low bandwidth

choices. Conversely, the optimal bandwidth is relatively small in Italy, where the estimates decrease as the bandwidth increases.

Figure 8: Heterogeneous RDD estimates for low and high employment volatility sectors



(a) Finland: Overall treatment effect for both years (2005 & 2010)



(b) Italy: Year-specific treatment effect for 2005 only

Notes: The figures are based on Logit estimates (Probability to train) and Tobit estimates with robust standard errors (Training hours) for different bandwidths around the threshold. The figures show the difference between the average marginal effect of EPL in sectors with low and high employment volatility and the corresponding 95% confidence intervals. N for Finland is 110, 181, 255, 352, 413, 481, 566, 642, 720, 744, 781, 811, 848, 883, 920, 968, 1000, 1034, 1075. N for Italy is 2361, 3512, 4589, 5602, 6468, 7114, 7577, 8012, 8309, 8576, 8798, 8986, 9152, 9328, 9497, 9646, 9782, 9906, 10021.

6 Conclusion

This paper tests the hypothesis that EPL increases firm-provided training in terms of propensity to train and the number of training hours during paid working time. The identification strategy exploits a discontinuity in the law that exempts small firms from EPL. Using data from two cross-sections of firm-level data from the CVTS, we estimate a regression-

discontinuity design (RDD) for Italy and Finland in 2005 and 2010. The results provide no evidence for our hypothesis as the estimated effects are insignificant and in most cases even negative. This also holds for Donut estimations that exclude observations in the immediate vicinity of the threshold and estimations that control for observable firm characteristics.

While testing the continuity of firm size density and observable firm characteristics supports the validity of the RDD in Finland in both years and Italy for 2005, selection of firms below the size threshold is a problem in Italy in 2010. Consequently, the pooled estimates for both years, as well as those for 2010 for Italy are invalid and cannot be interpreted.

We complement the classical RDD with a heterogeneous discontinuity framework. The heterogeneous RDD tests whether the impact of EPL is stronger in sectors with high employment volatility. This approach has two advantages. First, the exemption thresholds relevant for EPL also matter for the applicability of other policies, for example the representation of workers in a work council and a mandatory training plan. Though these potential confounding factors suggest that our estimations represent an upper bound of the true effect of EPL, we address this issue via the heterogeneous RDD. The second benefit of the heterogeneous RDD consists of increasing estimation efficiency. However, this identification strategy also provides no evidence in support of our hypothesis suggesting that EPL increases training.

While our empirical analysis suggests that EPL has no impact on firm-provided training, this study faces a number of limitations that should be addressed in future research. First, while an RDD provides an advantage in terms of internal validity, it estimates a local treatment effect, thereby suggesting that the external validity might be questionable. Concretely, while our estimations suggest that the results are similar in Finland and Italy, other institutional arrangements might affect the impact of EPL. Furthermore, though Pierre and Scarpetta (2013) suggest that the impact of EPL is particularly large for small firms, our findings might be invalid for large firms.

A second limitation of the study consists of potential measurement error in the assignment variable, though the Donut estimations suggest that this represents a minor issue. This might arise for several reasons. First, the data stems from firm surveys rather than from administrative data. Second, the calculation of the firm size threshold includes exceptions the data fails to capture such as the exclusion of workers from a temporary work agency in Finland. Third, the calculation of full-time equivalent workers in Italy relies on the accuracy of the average collectively agreed annual working hours. Furthermore, the CVTS refers to the total number of hours actually worked by all persons employed, including overtime, time spent at the place of work standing by or during which no work is done (but for which payment is made) and time corresponding to short rest periods. Similarly, the assumption

that each employer might have four weeks of holidays might be inaccurate in some cases.

More generally, the main limitation of the study consists of uncertainty regarding the question whether estimation efficiency suffices to conclude that EPL has an economically negligible effect. This is particularly true for Finland, where the number of observations is relatively low. Hence, future research should test whether this conclusion is justified.

A third limitation arises because this study focuses on estimating the impact of EPL rather than investigating on the channel through which the impact occurs. Hence, future research should build on the theoretical literature that suggests that EPL affects firm-provided training through a number of channels, such as the use of temporary workers or employment through temporary working agencies.

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A Appendix: Regulatory Framework

A.1 Finland

A.1.1 Firm-size Exemptions from EPL in Finland

According to the ILO (ILO, 2016b). EPL in Finland is summarized by two acts. The Employment Contract Act, which is referred to as the ECA in what follows, and the Act on Cooperation within Undertakings, which is referred to as ACU in what follows. While the ACU only applies to firms above a specified size threshold, the ECA is applicable regardless of the size of the firm. The ACU establishes cooperation procedures between employers and workers (representatives) especially with regard to economic dismissals. It is important to note that it does not apply to firms in the public sector (ILO, 2016b). Therefore, we focus mainly on the ACU in order to trace out the firm-size differences in EPL, which is the source of variation for our identification strategy.

The ACU specifies the obligations of employers towards their workers and under which circumstances employers must inform their workers about these obligations. The act applies to all firms above the firm-size threshold. The co-operation obligations stated in the act oblige firms to inform their workers/ worker representatives about the firm's annual accounts and the firm's financial situation (ILO, 2016a).¹⁴

Besides these information obligations, the ACU obliges firms to undertake consultations with the worker representatives regarding each and every matter that potentially affects the employment situation of the workers. This comprises: any major changes in work duties, working methods, working hours or organization of work; any major acquisitions of machinery and equipment; any major rearrangement of the working premises and changes in the range of goods and services offered; shutdown of the employer's operations or a part thereof; transfer of the operations to another location or any major expansion or reduction of the employer's activities; transfers of business, demergers or mergers; changing of full-time employment into part-time employment, lay-offs or terminations of employment contracts due to economic, production-related or re-organizational reasons (Nordman-Rajaharju et al., 2012).

In case a firm above the size-threshold, for which the regulations specified by the ACU apply, wants to terminate a contract, reduce it into a part-time contract, or wants to lay off

¹⁴This includes a comprehensive report on the organizations financial position, including its prospects, employment, profitability and cost structure. In addition, the employer should also give employee representatives statistical data on pay, including information broken down by occupational groups, and information on the numbers employed on a part-time and fixed-term basis, if requested (ETUI, 2016).

workers is described in section 3.2.3 of this paper. Firms for which the ACU does not apply, that is, firms below the firm-size threshold, only have to comply to the rules set out in the ECA when dismissing workers on collective grounds. Also, if an employer wants to dismiss an employee for individual or personal reasons, such as serious violation or omission of the employee's essential duties and incapacity to work due to a change of conditions that are in the responsibility of the employee (e.g. imprisonment), the regulations of the ECA apply, regardless of the firm size. In case the employer fails to provide valid reasons for a dismissal for individual reasons, he has to pay the worker a minimum of three months' or a maximum of 24 months' salary (ILO, 2016a).

In general, termination due to illness without permanent disability to work or discriminatory reasons (race, gender, etc.) are prohibited. In case of an unjust dismissal, an employee is entitled to a compensation that may amount up to 24 months' salary (ILO, 2016a).

A.1.2 Firm Size exemptions at the Same Threshold as for EPL

In the following, we refer to three regulations stated in the ACU that provide for exemptions at the same firm-size threshold as EPL. The first concerns the rights and duties of worker representatives in the form of works councils or union members. The second the obligation of firms to set up training plans. The third the duty to set up a health and safety board. While the first and second measure potentially confound our identification strategy, the last should not introduce too much noise in the empirical analysis.

A.1.3 Worker Representatives

Worker representatives can be union members who represent all workers in a firm for whom a collective bargaining agreement applies (e.g. shop stewards) or works councils either appointed by all workers of a firm or a group of workers who are not covered by the collective agreement. At the same size threshold as for EPL, the ACU gives workers the right to appoint worker representatives and gives them the right to be involved in so-called "cooperation negotiations". As described above, worker representatives have the right to be informed and to be involved in the cooperation negotiations, for example in case of a dismissal. In some limited cases, they can even take the decisions in such negotiations (ETUI, 2016). Often, worker representatives are union members: in 2013, 49.7% of all worker representatives in Finish firms were union members (Kaarinen and Loennroos, 2016).

A.1.4 Training Plans

In firms above the same size threshold that applies to EPL (firms ≥ 30 before 1st of January 2008 and ≥ 20 thereafter), every year, the employer has to prepare an annual plan covering personnel and training objectives. This plan is part of the cooperation procedure between him and the worker representatives. Besides matters that are relevant for the composition, number or quality of the workforce, or changes to these in the future, the plan has to contain annual training objectives by staff category, the planned budget for training and an annual action plan for training.¹⁵ In particular, it shall respect the training needs of aging wage-earners and salaried employees (ACU, 2005).

The worker representatives have a say regarding the content of the training plans. However, their decision-making power is limited due to the fact that the employer decides about the amount of training by the amount of money he allocates to the areas concerned. One important side-note: in 2013 an amendment to the ACU provided much more detailed regulations and obligations for employers regarding training provision. In turn, they are allowed to deduct the training costs from the taxes (ETUI, 2016). Since this amendment was made after the second reference year of the data used in the empirical part, it does not contaminate our identification strategy.

A.1.5 Health and Safety Board

In firms above the size threshold, a separate health and safety board must be appointed for consultation and supervision purposes on various key health and safety issues (ILO, 2016a).

A.1.6 Firm Size Exemptions at Other Thresholds as for EPL

For firms employing at least 30 workers, agreement should be reached between the employer and the employee representatives regarding the: work rules (e.g. informing about absence) and the disciplinary code, rules on suggestion schemes and the allocation of company accommodation.

In firms with 30 or more workers, also the following topics are subject to cooperation negotiations: methods to be used for internal communications, equality plans, drug and alcohol policies, the use of surveillance equipment, rules on the use of e-mail, and setting up and ending profit bonus systems and personnel funds (ETUI, 2016).

¹⁵For example, plans to downsize or to change the composition of the workforce by employing more temporary workers.

A.2 Italy

Details regarding the differences in EPL for firms below the size threshold, that is firms with 15 or less workers, compared to those above can be found in section 3.2.3.

A.2.1 Firm Size Exemptions at the Same Threshold as for EPL

There are three other exemptions that apply at the same threshold as EPL. Firms above the threshold: i) works councils can be set up, ii) must employ disabled workers and iii) must appoint a worker representative for safety related issues.

A.2.2 Work Councils

In Italy, the size threshold of 15 workers also applies to regulations for workplace representation in the form of work councils. In firms with more than 15 workers, work councils (so-called *Rappresentanze sindacali aziendali*, *RSA*, if appointed uniquely by the unions and *Rappresentanze Sindacali Unitarie*, *RSU* if at least one third of the seats (and since 2013, even all seats) is elected or appointed directly by the workers)¹⁶ can be set up through elections¹⁷ (Eurofund, 2016; ETUI, 2016). RSUs consist of union members. Hence, they are the workplace representatives of the trade unions. As such, their main task is to negotiate with the employer at the workplace level (e.g. binding agreements as part of the bargaining structure). The employer is obliged to inform the RSUs about: the economic and financial situation of the company, investment, the numbers employed, changes in working methods, the introduction of new technology, large scale redundancies, and business transfers, gender equality, training, health and safety related issues, the use of public funds for industrial restructuring (ETUI, 2016).

A.2.3 Duty to Employ Disabled Workers

Firms employing 16-35 workers must hire at least one disabled worker, firms with 36-50 workers 2 disabled workers and firms with 51 and more workers must reassure that 7% of their workforce consist of workers with disabilities (Eurofund, 2016).

¹⁶Before 1993, work councils were always elected by the unions (Eurofund, 2016).

¹⁷In firms with 16-200 workers, these can have 3 members, in firms with 201-500 workers, 6 members. The number can be increased in industry and company agreements (ETUI, 2016).

B Appendix: Supplementary Tables and Figures

See next page.

Table 5: Functional form, Finland

	Probability to train						Training hours					
	(1)	(2)	(3)	(4)	(5)	(6)	(1)	(2)	(3)	(4)	(5)	(6)
2005												
Treatment	0.075 (0.102)	0.061 (0.104)	0.078 (0.106)	0.109 (0.152)	0.157 (0.130)	-0.091 (0.219)	-68.698 (91.087)	-62.392 (94.314)	-53.550 (97.939)	67.155 (138.964)	73.450 (123.182)	-221.694 (184.824)
Linear	0.002 (0.005)	-0.000 (0.006)	0.002 (0.006)	-0.058** (0.027)	-0.009 (0.014)	0.016 (0.078)	14.416*** (3.967)	15.868*** (4.093)	13.284*** (4.910)	-16.409 (18.639)	-0.868 (8.975)	38.063 (49.732)
Interaction	0.008 (0.011)	0.008 (0.011)	0.139*** (0.044)	0.119 (0.110)	0.119 (0.110)	0.119 (0.110)	-4.176 (9.208)	-4.176 (9.208)	29.338 (30.786)	29.338 (30.786)	94.090 (77.375)	94.090 (77.375)
Squared			-0.000 (0.000)	-0.003** (0.001)	0.000 (0.000)	0.006 (0.009)	-0.152 (0.220)	-0.152 (0.220)	-1.469* (0.780)	-1.469* (0.780)	-0.083 (0.242)	4.446 (4.998)
Int. squ.			-0.001 (0.003)	-0.001 (0.003)	-0.020 (0.015)	-0.020 (0.015)	1.406 (1.620)	1.406 (1.620)	1.406 (1.620)	1.406 (1.620)	-20.365** (9.011)	-20.365** (9.011)
Cubic			0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.037 (0.024)	0.037 (0.024)	0.037 (0.024)	0.037 (0.024)	0.181 (0.149)	0.181 (0.149)
Int. cub.					0.000 (0.001)	0.000 (0.001)					0.355 (0.296)	0.355 (0.296)
N	383	383	383	383	383	383	470	470	470	470	470	470
AIC	522.2	523.7	524.2	518.6	525.4	520.9	4124.3	4125.9	4125.4	4126.9	4124.5	4122.6
BIC	534.0	539.5	540.0	542.3	545.1	552.5	4140.9	4146.7	4146.2	4155.9	4149.4	4160.0
2010												
Treatment	0.009 (0.087)	0.008 (0.086)	0.007 (0.087)	0.194 (0.123)	0.105 (0.116)	0.180 (0.199)	6.648 (59.171)	11.496 (58.703)	6.259 (59.418)	220.269** (88.858)	133.532* (74.635)	165.491 (152.291)
Linear	0.013 (0.008)	0.008 (0.010)	0.013 (0.008)	-0.099** (0.048)	-0.010 (0.021)	-0.074 (0.154)	8.464 (6.480)	3.400 (7.567)	8.848 (6.738)	-99.593*** (36.953)	-24.188* (14.346)	-74.172 (129.404)
Interaction	0.011 (0.016)	0.011 (0.016)	0.139** (0.064)	0.082 (0.182)	0.082 (0.182)	0.082 (0.182)	10.443 (12.814)	10.443 (12.814)	93.680** (47.244)	93.680** (47.244)	145.169 (145.758)	145.169 (145.758)
Squared			0.000 (0.001)	-0.010** (0.004)	0.001 (0.001)	-0.005 (0.034)	0.364 (0.768)	0.364 (0.768)	-10.969*** (3.960)	-10.969*** (3.960)	0.797 (0.827)	-4.602 (31.400)
Int. squ.			0.008 (0.007)	0.008 (0.007)	0.013 (0.044)	0.013 (0.044)	13.495** (5.836)	13.495** (5.836)	13.495** (5.836)	13.495** (5.836)	-19.534 (38.219)	-19.534 (38.219)
Cubic			0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.002)	0.500** (0.227)	0.500** (0.227)	0.500** (0.227)	0.500** (0.227)	0.458 (2.225)	0.458 (2.225)
Int. cub.					-0.001 (0.003)	-0.001 (0.003)					1.786 (2.874)	1.786 (2.874)
N	529	529	529	529	529	529	467	467	467	467	467	467
AIC	713.7	715.2	715.6	713.9	716.1	717.8	4207.3	4208.6	4209.0	4205.1	4205.3	4207.5
BIC	726.5	732.3	732.7	739.6	737.5	751.9	4223.9	4229.3	4229.8	4234.1	4230.2	4244.8

Note: The optimal functional form is chosen based on the Bayesian Information Criterion (BIC). ** $p < 0.01$, *** $p < 0.05$, * $p < 0.1$; The table displays marginal effects of Logit and Tobit estimates with normal and robust standard errors in parenthesis, respectively. The sample refers to the optimal bandwidth.

Table 6: Functional form, Italy

	Probability to train						Training hours					
	(1)	(2)	(3)	(4)	(5)	(6)	(1)	(2)	(3)	(4)	(5)	(6)
2005												
Treatment	0.001 (0.021)	0.001 (0.021)	-0.000 (0.021)	0.012 (0.031)	0.007 (0.028)	0.001 (0.041)	-26.762 (29.184)	-27.730 (29.389)	-29.292 (29.637)	-25.096 (43.480)	-30.869 (39.302)	-22.125 (58.040)
Linear	0.010*** (0.003)	0.010*** (0.004)	0.010*** (0.003)	0.022 (0.017)	0.007 (0.008)	-0.016 (0.042)	21.066*** (5.831)	19.549*** (7.168)	21.860*** (6.084)	47.465* (28.563)	22.619* (13.624)	-40.969 (72.599)
Interaction	0.000 (0.007)	0.000 (0.007)	-0.039 (0.025)	0.002 (0.025)	0.066 (0.062)	0.000 (0.014)	3.703 (11.608)	0.804 (1.189)	5.728 (5.480)	0.794 (1.232)	119.060 (104.702)	-39.398 (33.737)
Squared			0.000 (0.001)	0.002 (0.003)	0.000 (0.001)	-0.014 (0.017)			1.905 (9.104)			-1.490 (51.388)
Int. squ.			0.003 (0.004)		-0.011 (0.025)							-0.037 (4.478)
Cubic					0.000 (0.000)	-0.002 (0.002)						-6.169 (0.619)
Int. cub.					0.005* (0.003)							13.033* (7.252)
N	6515	6515	6515	6515	6515	6515	5563	5563	5563	5563	5563	5563
AIC	7346.7	7348.7	7348.5	7350.0	7350.4	7350.5	24540.2	24542.1	24541.7	24543.8	24543.7	24544.4
BIC	7367.1	7375.8	7375.7	7390.7	7384.3	7404.8	24566.7	24575.2	24574.8	24590.1	24583.4	24604.0
2010												
	Probability to train						Training hours					
	(1)	(2)	(3)	(4)	(5)	(6)	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	0.011 (0.031)	0.010 (0.032)	0.011 (0.032)	0.035 (0.048)	0.034 (0.042)	0.022 (0.066)	15.146 (17.112)	14.858 (17.620)	15.670 (17.731)	12.629 (24.460)	13.667 (21.709)	30.085 (31.785)
Linear	0.010 (0.009)	0.008 (0.011)	0.010 (0.009)	-0.027 (0.044)	-0.006 (0.021)	-0.037 (0.113)	7.741** (3.643)	7.504* (4.352)	7.566* (3.962)	-3.230 (16.624)	8.709 (8.690)	-83.237* (43.848)
Interaction	0.003 (0.018)	0.003 (0.018)	0.003 (0.018)	0.029 (0.072)	0.102 (0.181)	0.029 (0.181)	0.672 (7.811)	0.672 (7.811)	27.149 (29.539)	139.350* (77.124)	139.350* (77.124)	-51.839** (25.714)
Squared			0.000 (0.003)	-0.011 (0.013)	0.001 (0.003)	-0.019 (0.081)			-0.135 (0.966)	-2.634 (3.972)	-0.160 (0.997)	-51.839** (25.714)
Int. squ.				0.014 (0.022)	-0.029 (0.134)				-1.471 (7.465)			27.037 (46.439)
Cubic					0.002 (0.002)	-0.002 (0.017)						-8.152* (4.208)
Int. cub.					0.013 (0.028)							11.683 (7.837)
N	4148	4148	4148	4148	4148	4148	5283	5283	5283	5283	5283	5283
AIC	5722.1	5724.0	5724.1	5727.3	5725.4	5731.1	37215.9	37217.9	37217.8	37221.1	37219.8	37221.4
BIC	5741.1	5749.4	5749.4	5765.3	5757.1	5781.7	37242.2	37250.7	37250.7	37267.1	37259.3	37280.6

Note: The optimal functional form is chosen based on the Bayesian Information Criterion (BIC). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$; The table displays marginal effects of Logit and Tobit estimates with normal and robust standard errors in parenthesis. The samples are based on the optimal bandwidth.

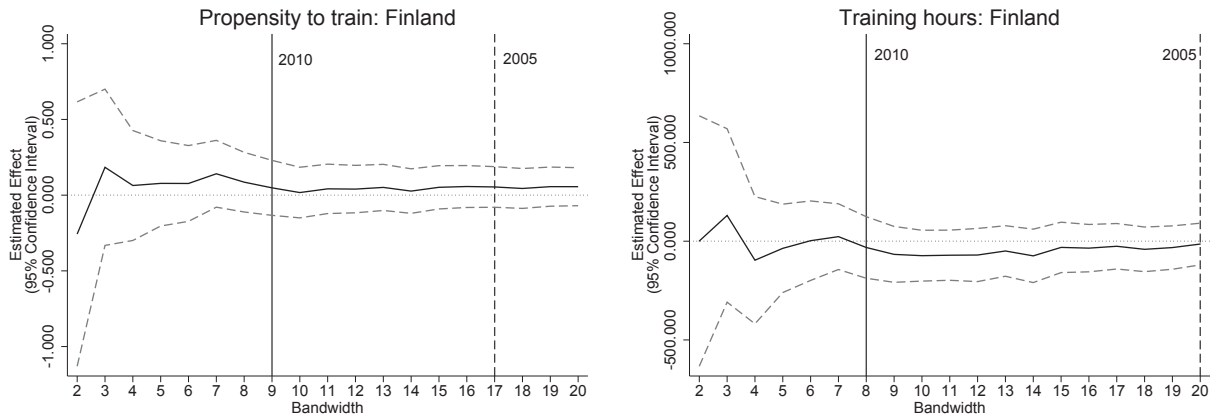
Table 7: Summary statistics Finland

Sample	2005				2010			
	Total Sample	Untreated	Untreated	Treated	Total Sample	Untreated	Treated	Treated
Employees	10-40	10-19	20-29	30-40	10-40	10-19	20-29	30-40
Probability to train	0.544 (0.024)	0.498 (0.034)	0.553 (0.043)	0.667 (0.055)	0.593 (0.019)	0.521 (0.026)	0.665 (0.034)	0.735 (0.045)
Training hours	127.696 (11.452)	75.217 (10.065)	155.333 (22.324)	233.693 (39.981)	137.976 (10.502)	91.578 (8.683)	154.314 (16.540)	287.939 (52.075)
Mining & quarrying	0.058 (0.011)	0.063 (0.016)	0.061 (0.021)	0.040 (0.023)	0.042 (0.008)	0.047 (0.011)	0.026 (0.012)	0.051 (0.022)
Food & beverages	0.072 (0.013)	0.077 (0.018)	0.068 (0.022)	0.067 (0.029)	0.055 (0.009)	0.052 (0.011)	0.063 (0.018)	0.051 (0.022)
Textiles & Leather	0.051 (0.011)	0.036 (0.013)	0.091 (0.025)	0.027 (0.019)	0.061 (0.009)	0.060 (0.012)	0.063 (0.018)	0.061 (0.024)
Pulp, paper & printing	0.058 (0.011)	0.059 (0.016)	0.068 (0.022)	0.040 (0.023)	0.045 (0.008)	0.031 (0.009)	0.058 (0.017)	0.071 (0.026)
Coke, petrol, chem. & fuel	0.065 (0.012)	0.054 (0.015)	0.061 (0.021)	0.107 (0.036)	0.055 (0.009)	0.055 (0.012)	0.037 (0.014)	0.092 (0.029)
Basic & fabric. metals	0.063 (0.012)	0.068 (0.017)	0.068 (0.022)	0.040 (0.023)	0.061 (0.009)	0.052 (0.011)	0.068 (0.018)	0.082 (0.028)
Machinery, electr. & optical	0.075 (0.013)	0.077 (0.018)	0.076 (0.023)	0.067 (0.029)	0.048 (0.008)	0.055 (0.012)	0.037 (0.014)	0.041 (0.020)
Transport equipment	0.056 (0.011)	0.018 (0.009)	0.068 (0.022)	0.147 (0.041)	0.031 (0.007)	0.023 (0.008)	0.031 (0.013)	0.061 (0.024)
Wood and wood products	0.086 (0.014)	0.095 (0.020)	0.083 (0.024)	0.067 (0.029)	0.055 (0.009)	0.063 (0.012)	0.058 (0.017)	0.020 (0.014)
Electricity, gas & water	0.044 (0.010)	0.054 (0.015)	0.023 (0.013)	0.053 (0.026)	0.027 (0.006)	0.021 (0.007)	0.026 (0.012)	0.051 (0.022)
Construction	0.058 (0.011)	0.063 (0.016)	0.045 (0.018)	0.067 (0.029)	0.046 (0.008)	0.049 (0.011)	0.052 (0.016)	0.020 (0.014)
Repair/Wholesale/Retail/Hotels	0.154 (0.017)	0.190 (0.026)	0.114 (0.028)	0.120 (0.038)	0.229 (0.016)	0.229 (0.021)	0.257 (0.032)	0.173 (0.038)
Transport/storage/information/comm.	0.058 (0.011)	0.054 (0.015)	0.053 (0.020)	0.080 (0.032)	0.104 (0.012)	0.115 (0.016)	0.094 (0.021)	0.082 (0.028)
Financial intermed./bus. serv.	0.070 (0.012)	0.077 (0.018)	0.091 (0.025)	0.013 (0.013)	0.074 (0.010)	0.076 (0.014)	0.084 (0.020)	0.051 (0.022)
Real estate & other serv.	0.028 (0.008)	0.014 (0.008)	0.030 (0.015)	0.067 (0.029)	0.068 (0.010)	0.073 (0.013)	0.047 (0.015)	0.092 (0.029)
Average wage	35721 (608)	35802 (850)	34594 (1118)	37463 (1368)	42930 (980)	42733 (968)	41722 (1174)	46060 (5083)
Share females	32.947 (1.398)	33.634 (1.958)	35.430 (2.645)	26.551 (2.871)	38.371 (1.187)	38.333 (1.629)	38.192 (2.124)	38.869 (2.967)
N	428	221	132	75	673	384	191	98
Employment Volatility	0.344 (0.002)	0.340 (0.003)	0.349 (0.005)	0.344 (0.006)	0.342 (0.002)	0.343 (0.003)	0.344 (0.004)	0.336 (0.005)
N	403	207	124	72	645	366	186	93

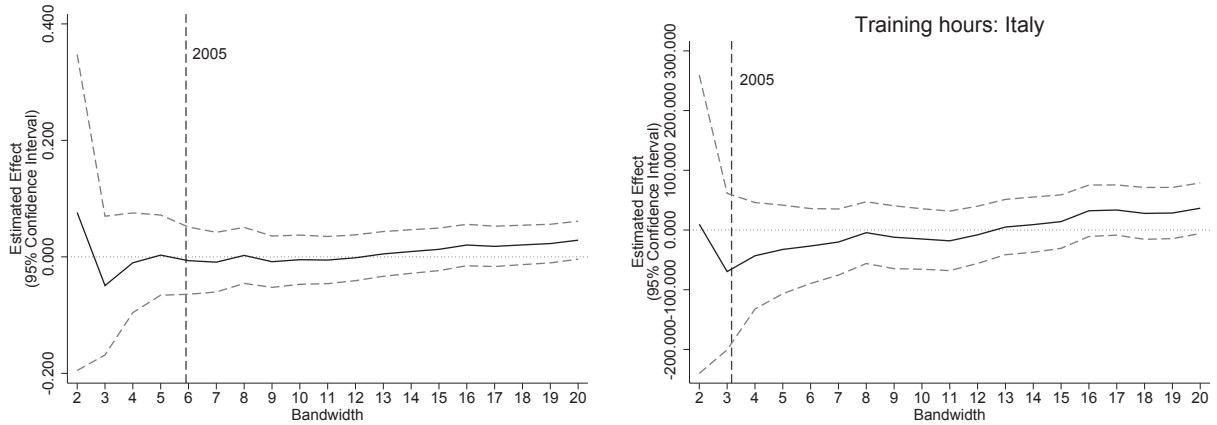
Table 8: Summary statistics Italy

Sample	2005			2010		
	Total Sample	Untreated	Treated	Total Sample	Untreated	Treated
FTE Employees	0-40	0-15	15.01-40	0-40	0-15	15.01-40
Probability to train	0.280 (0.004)	0.223 (0.006)	0.330 (0.006)	0.452 (0.004)	0.408 (0.005)	0.530 (0.007)
Training hours	66.554 (2.344)	34.353 (2.011)	95.448 (4.026)	71.738 (1.814)	44.184 (1.468)	120.492 (4.214)
Mining & quarrying	0.018 (0.001)	0.017 (0.002)	0.018 (0.002)	0.013 (0.001)	0.013 (0.001)	0.012 (0.002)
Food & beverages	0.026 (0.002)	0.026 (0.002)	0.027 (0.002)	0.039 (0.002)	0.040 (0.002)	0.036 (0.003)
Textiles & Leather	0.042 (0.002)	0.043 (0.003)	0.041 (0.003)	0.068 (0.002)	0.064 (0.003)	0.074 (0.004)
Pulp, paper & printing	0.038 (0.002)	0.036 (0.003)	0.040 (0.003)	0.034 (0.002)	0.033 (0.002)	0.037 (0.003)
Coke, petrol, chem. & fuel	0.045 (0.002)	0.038 (0.003)	0.051 (0.003)	0.034 (0.002)	0.030 (0.002)	0.041 (0.003)
Basic & fabric. metals	0.049 (0.002)	0.042 (0.003)	0.054 (0.003)	0.049 (0.002)	0.048 (0.002)	0.051 (0.003)
Machinery, electr. & optical	0.048 (0.002)	0.041 (0.003)	0.054 (0.003)	0.042 (0.002)	0.039 (0.002)	0.048 (0.003)
Transport equipment	0.012 (0.001)	0.009 (0.001)	0.014 (0.002)	0.012 (0.001)	0.009 (0.001)	0.016 (0.002)
Wood and wood products	0.044 (0.002)	0.048 (0.003)	0.040 (0.003)	0.051 (0.002)	0.051 (0.002)	0.050 (0.003)
Electricity, gas & water	0.009 (0.001)	0.008 (0.001)	0.010 (0.001)	0.039 (0.002)	0.033 (0.002)	0.050 (0.003)
Construction	0.316 (0.004)	0.345 (0.007)	0.290 (0.006)	0.214 (0.003)	0.224 (0.004)	0.197 (0.006)
Repair/Wholesale/Retail/Hotels	0.158 (0.004)	0.156 (0.005)	0.161 (0.005)	0.259 (0.004)	0.275 (0.005)	0.229 (0.006)
Transport/storage/information/comm.	0.027 (0.002)	0.021 (0.002)	0.033 (0.002)	0.051 (0.002)	0.046 (0.002)	0.060 (0.003)
Financial intermed./bus. serv.	0.035 (0.002)	0.036 (0.003)	0.035 (0.002)	0.042 (0.002)	0.038 (0.002)	0.048 (0.003)
Real estate & other serv.	0.133 (0.003)	0.134 (0.005)	0.131 (0.005)	0.053 (0.002)	0.055 (0.002)	0.050 (0.003)
Average wage	28406 (122.185)	25361 (161)	31138 (173)	31720 (100)	29773 (109)	35166 (188)
Share females	0.281 (0.003)	0.295 (0.004)	0.269 (0.003)	31.745 (0.215)	32.502 (0.272)	30.404 (0.351)
N	10691	5056	5635	13858	8854	5004
Employment Volatility	0.358 (0.001)	0.362 (0.001)	0.355 (0.001)	0.357 (0.000)	0.357 (0.001)	0.357 (0.001)
N	10499	4968	5531	13684	8741	4943

Figure 9: RDD Donut estimates



(a) Finland: Overall treatment effect for both years (2005 & 2010)



(b) Italy: Year-specific treatment effect for 2005 only

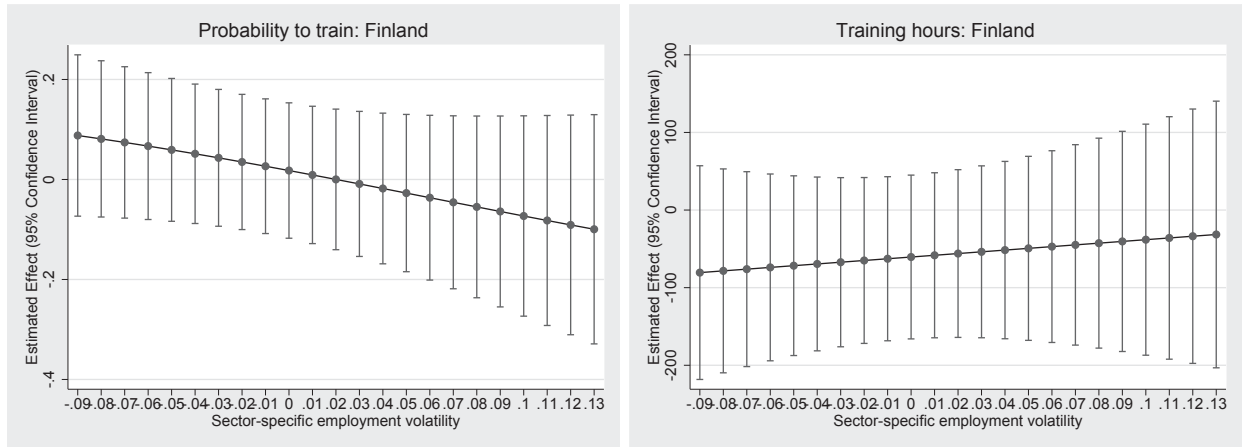
Notes: The figures displays local average marginal effects and 95% confidence intervals of Logit estimates (Probability to train) and Tobit estimates with robust standard errors (Training hours) of the effect of EPL on training for different bandwidths around the threshold. N for Finland is 167, 237, 307, 400, 461, 522, 605, 679, 753, 777, 814, 844, 878, 910, 944, 989, 1019, 1052, 1088. N for Italy is 1101, 2370, 3467, 4499, 5381, 6035, 6508, 6949, 7256, 7525, 7749, 7941, 8110, 8290, 8466, 8618, 8754, 8880, 8996.

Table 9: Relationship of Sector-Specific Employment Volatility and Selection

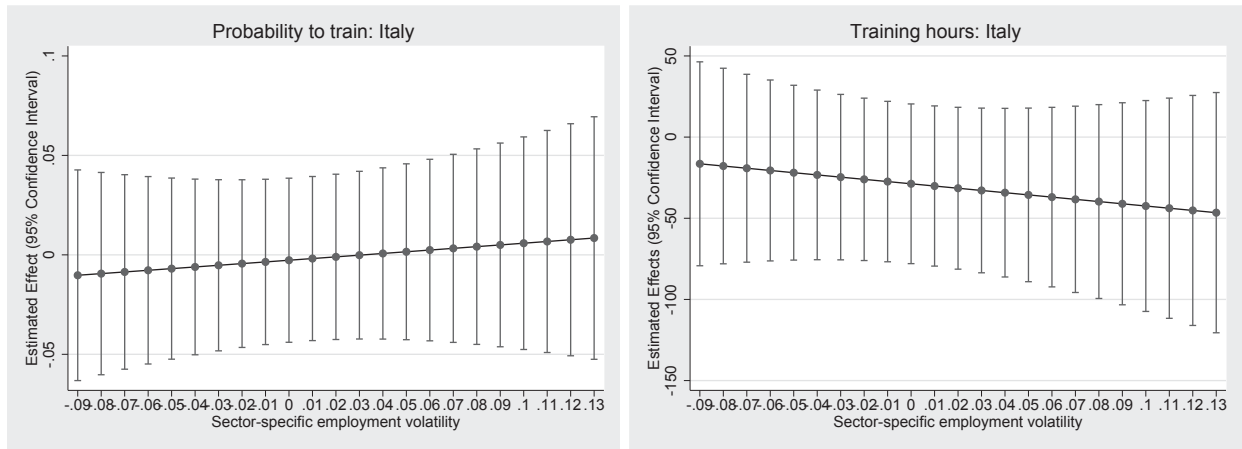
Industry	Employment volatility	T-statistics McCrary test			
		Finland		Italy	
		2005	2010	2005	2010
Mining & quarrying		-0.788	-0.286	-0.246	-0.492
Food & beverages	0.343		-1.155	1.095	-0.640
Textiles & Leather	0.475	-0.813	0.381	-2.243	-1.576
Pulp, paper & printing	0.272	0.492		-1.165	-0.645
Coke, petrol, chem. & fuel	0.311	-0.659		0.458	0.665
Basic & fabric. metals	0.302	0.700	0.317	1.188	0.119
Machinery, electr. & optical	0.304	0.428	1.082	0.293	0.395
Transport equipment	0.389	-0.452		-0.157	1.233
Wood and wood products	0.348		-0.420	-1.962	1.555
Electricity, gas & water	0.336	0.546		0.009	-2.855
Construction	0.428	0.082	0.753	0.054	-1.553
Repair/Wholesale/Retail/Hotels	0.321	-0.689	0.977	1.320	-0.311
Transport/storage/information/comm.	0.369	-0.564	0.513	0.795	-3.141
Financial intermed./bus. serv.	0.366		0.879	-0.620	1.889
Real estate & other serv.	0.283	0.233	-0.428	-0.622	-2.881
Correlation with employment volatility		-4.838**	2.021	-6.778	-2.058
		(1.791)	(2.596)	(5.907)	(6.165)

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The table displays industry-specific employment volatility, industry-specific t-statistics of the McCrary test, and the correlation thereof.

Figure 10: RDD estimates by sector-specific employment volatility



(a) Finland: Overall treatment effect for both years (2005 & 2010)



(b) Italy: Year-specific treatment effect for 2005 only

Notes: The figures displays marginal effects and 95% confidence intervals of Logit estimates (Probability to train) and Tobit estimates with robust standard errors (Training hours) of the effect of EPL on training for different values of sector-specific employment volatility.