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

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

Abstract

This paper exploits exceptions in the application of employment protection legislation (EPL) to small firms beneath a particular size threshold to test the theoretical hypothesis that EPL increases the incentives of firms to train their employees in a regression discontinuity setting. Using firm-level data from Finland and Italy provides no empirical evidence for this hypothesis. In fact, the results rather suggest a potentially negative impact, which is unstable across empirical specifications though. We test whether this might be due to a negative selection of employees by comparing firms with low and high shares of old employees. The insignificantly higher effect of EPL for firms with older workers provides at best suggestive evidence that EPL affects training negatively though.

JEL: L5, K31, I21, J21

Keywords: Training, employment protection legislation, regression discontinuity design

1. Introduction

The increased integration the world economy and rapid technological progress has raised the need of firms to adjust their cost structure in response to ever-changing market conditions. In response, more than one-third of OECD countries reformed their employment protection legislation (EPL) in the last decade- especially with regard to temporary employment (OECD, 2013). Reflecting the belief that EPL decreases welfare by increasing the labour adjustment costs of firms, EPL was lowered on average, reducing the average value of the OECD EPL indicator by -0.16 to a value of 2.04 in 2013 (OECD, 2013). This general trend also pertains to the two countries analysed in this paper. Finland reduced EPL by -0.29 to 2.17 in 2002 and Italy reformed EPL in 2013, reducing the OECD EPL indicator by -0.25 to 2.51.

However, while politics appears to believe in the positive welfare, both the theoretical and empirical literature remain agnostic in this respect¹. One reason for the ambiguity lies in the fact that potential welfare losses, e.g. due to decreases in employment might be offset by welfare gains due to an increase in training. By creating wage compression through frictions in the labour market, prolonging employment relations, or by acting as a commitment device of firms, EPL gives firms an incentive to invest in the human capital of their workforce in the form of training (Akerlof, 1984; Acemoglu and Pischke, 1998&1999; Fella, 2005; Wasmer, 2006; Belot et al. 2007, Balmaceda, 2009). However, stricter EPL might decrease the incentives to provide training by increasing wages, or because employers end up with a more heterogeneous workforce or because firms make more intense use of temporary workers (Pierre and Scarpetta, 2004 or Brunello, 2006).

Due to this theoretical ambiguity and the scant empirical evidence regarding this issue, this paper tests whether stricter EPL increases the provision of firm-provided training. Analysing cross-country data, Brunello et al. (2007) find a negative effect of EPL on training, while Almeida and Aterido (2011) and Pierre and Scarpetta (2004, 2013) find the opposite. Piccio and van Ours (2011) also suggest that Dutch firms train more under stricter EPL. Finally, Messe and Rouland (2014) find that an increase in EPL in France increases training. In summary, the existing empirical evidence is rather scant and provides mixed evidence regarding a potential impact of EPL on training.

Hence, this paper mainly contributes to the existing literature in four ways. First, it provides evidence regarding the effect of EPL on training based on a regression discontinuity design that exploits exceptions of small firms from EPL in Italy and Finland. By exploiting within-country variation, the identification strategy circumvents potential endogeneity problems due to general equilibrium effects and correlations of EPL and country-specific labour market conditions that might contaminate cross-country comparisons (see, e.g., Noelke, 2011, or Hijzen et al.2013). The results provide no evidence that EPL increases training. In fact, the evidence points towards a negative effect of EPL on training, though the statistical significance of the effect is cast in doubt by the lack of robustness across empirical specifications.

¹ For theoretical arguments see Bentolila and Bertola (1990), Bertola (1990), Mortensen and Pissarides (1999) and Addison and Teixeira (2003), Noelke (2011) for a review of the empirical literature.

Secondly, the paper tests whether a negative effect might arise due to a negative selection of the workforce. Assuming that an older workforce indicates a stronger selection due to EPL, we test whether the effect of EPL is stronger for firms with an older workforce, for which we find mild evidence too.

Thirdly, most of empirical literature ignores the possibility that a negative effect might arise because EPL decreases training by increasing the use of temporary workers, which have a lower training rate than permanent employees (see, e.g., Pierre and Scarpetta, 2013). This might be problematic as we find some evidence that the effect of EPL differs between firms with and without temporary workers.

And finally, we are the first use within-country variation to analyse the effect of EPL on training for Italy and Finland, both of which have above average EPL (OECD, 2013). This comparison is particularly interesting because the two countries differ substantially in terms of economic development, political system and regulatory framework. Thereby, comparing the results of these countries allows to evaluate the external validity of our results for other countries.

The paper is organised as follows. The first section summarizes the existing theoretical and empirical literature to derive hypotheses. The second section describes the regulatory framework and the corresponding empirical methodology. Section three summarises the data. Section four analyses the validity of the RDD setting and section five presents the estimation results. Section six concludes.

2. Theory and Literature

One effect of employment protection is that it stabilizes employment relationships by reducing labour turnover through an increase in job destruction cost. As a consequence, fewer jobs are destroyed, but also fewer are created by discouraging firms from hiring (Bertola, 1992; Mortensen and Pissarides, 1999). As such, EPL can serve as an insurance against the risk of dismissal (Pissarides, 2010), especially in times of economic downturn, but also discourages firms from hiring during economic upturns. By decreasing the speed with which jobs are created and destroyed, EPL is a source of creating frictions in the labour market (Acemoglu and Pischke, 1998&1999) with an ambiguous overall effect on employment and unemployment (c.f. Bentolila and Bertola, 1990; Bertola, 1990 or Mortensen and Pissarides, 1999).

EPL has a transfer (severance payment) and a tax component (red tape costs) on layoffs (Garibaldi and Violante, 2002). The impact of the transfer component on the outcome variable, i.e. training, is generally neutral, since it can be internalised in wages (Lazear, 1990; Burda, 1992). The tax component of EPL however is vested upon dismissal and thus cannot be easily internalised. Therefore, in line with the existing literature, we focus on the tax component of EPL.

There are three channels through which EPL could increase the incentives of employers to train their workers (thereby reducing potential welfare losses arising from a possible reduction in employment due to EPL. First, the positive effect of EPL on job duration gives employers an incentive to increase

labour productivity by investing into the human capital of their workforce (Akerlof, 1984; Belot et al. 2007; Charlot and Malherbet, 2013). To the extent that the created human capital is firm-specific, the increase in training creates a feedback loop that stabilizes employment further (see, e.g., Wasmer, 2006). Through this mechanism the positive effect of EPL on job duration could be even reinforced and outweigh the usually negative effect on job creation, which is beneficial for job creation and thus welfare-improving (e.g. for the case that human capital is firm-specific, see Wasmer (2006)).

Second, training in response to higher adjustment costs is an especially attractive option for firms if EPL compresses wages (due to labour market frictions), because of a greater wedge between the productivity gains of workers and the actual wage they receive. If their productivity can be further raised through training, the wedge between the productivity gains and the wage could rise even further. Because wages are less responsive to an increase in productivity, the employer can reap a part of the increase in productivity, which would spur his incentives to train the workforce (Acemoglu and Pischke, 1998&1999).

The third argument mentioned in the literature rests on the assumption that the employers lack of commitment to the employment deters employees to invest in firm-specific training. Hence, EPL might serve as a commitment device, thereby stimulating training (Schivardi and Torrini 2008).

However, there are also three theoretical channels that suggest that training of workers decreases in response to an increase in EPL. First, in a labour market with strict employment protection (wages are compressed), once being hired, workers bargaining power increases due to the firing costs the firm would have to pay in case of a dismissal (Lindbeck and Snower, 1988), which raises their incentive to demand a higher wage, labelled “insider wage” (e.g. Mortensen and Pissarides, 1999). By forcing the employer to bear the sum of job creation and job destruction costs, the increased wage reduces firm profits and thus the employers willingness to invest in the human capital of their workers, which is the essence of a hold-up problem.²

Second, facing high firing costs, firms cannot easily dismiss less able workers and thus end up with a more heterogeneous workforce of regular workers than firms which are not subject to EPL. If heterogeneity of the labour force has a negative effect on a worker's productivity or to the extent that training and ability are complements, firms with a more heterogeneous workforce should train less (Pierre and Scarpetta, 2004; Brunello, 2006).

Third, if temporary contracts are allowed alongside regular contracts and thus can act as an alternative mode of employment, the picture of how EPL affects employers decision to train their workforce, might change. The introduction of temporary contracts is likely to create a dual labour market, where one part of the working population is employed on regular contracts, while the others are employed on temporary contracts. The coexistence of relatively strictly regulated regular and relatively less

²This first argument can be studied in a search equilibrium framework by comparing equilibrium conditions without a two-tier wage structure as compared to those with. Under a two-tier wage contract, the first tier wage is usually lower than the second tier. Once being “inside”, the worker has an incentive to renegotiate the two-tier agreement and demand a higher wage, which creates a hold-up problem (Mortensen and Pissarides, 1999).

regulated temporary contracts provides a strong incentive for employers to substitute regular for temporary contracts. Since temporary jobs are more likely to be destroyed and therefore less likely to be converted into permanent contracts, this reduces the incentives for employers to invest in the human capital of their employees, which would be lost in case of a dismissal (see e.g. Charlot and Malherbet, 2013; Pierre and Scarpetta, 2013)³. Consequently, a possibly positive *overall* effect of EPL on training could be shrouded by an increase in the use of temporary work contracts (less regular workers, less training), while regular workers should still receive training.

Hence, the theoretical prediction of how EPL affects training remains ambiguous. Therefore, we propose two alternative hypotheses:

H1a: An increase in EPL increases training of employees

H1b: An increase in EPL decreases training of employees

Furthermore, we test two hypothesis that allow us to analyse the second and third argument why EPL might have a negative effect on training. First, we argue that the negative selection of employees is more pronounce in firms with an older workforce. Therefore, we test whether

H2: The interaction of EPL and the share of workers above 55 training provision negatively

Second, we examine whether an increase in the share temporary workers dampens the effect of EPL on training by testing whether

H3: The interaction of EPL and the use of temporary work contracts affects training provision negatively

Most of the empirical studies analysing the effect of EPL on training have used cross-country data, while we found only two studies exploiting within-country variation to identify the effect of EPL on training. Of the former, there are two studies which differentiate between EPL for regular and temporary workers, and two others which also take the use of temporary employment into account.

Using a panel of 13 European countries⁴ and the OECD Index of Employment Protection for regular and temporary workers, exploiting within country variation over time, Brunello et al. (2007) estimate the effect of EPL on the incidence of workplace training. They find a significant negative effect of stricter EPL on training incidence for both types of workers. However, while the effect is highly significant and large in magnitude for regular workers, it is just slightly significant and low in magnitude for temporary workers. They also account for the share of temporary workers in the total economy receiving training. The effect on training is negative, but insignificant.

³According to Charlot and Malherbet (2013), in a dual labour market, higher firing costs always increase the use of temporary employment. Furthermore, a positive effect of EPL on training and job creation can only dominate in the absence of temporary jobs.

⁴He uses data for 1995-2001 from the European Household Panel (EHP) for Austria, Belgium, Denmark, Finland, France, Ireland, Italy, Greece, the Netherlands, Portugal, Spain, Sweden and the UK.

In a comparable study, Almeida and Aterido (2011) examine the effects of EPL on firm-provided training for a panel of developing countries⁵. They use a difference-in-difference framework that combines variation across EPL and differences in de jure and de facto employment protection accounting for industry and country fixed effects. They find that firms that are subject to stricter labour regulations and enforcement are more likely to train. In addition, they find that stricter hiring regulations increase training, while stricter firing regulations decrease the provision of job training, but just for manufacturing firms.

Pierre and Scarpetta (2004) use a large dataset of developing and emerging economies exploring variation in firm managers' perception on how binding labour laws are to test if firms facing stricter employment protection invest more in training and/or make greater use of temporary employment. Employing a difference-in-difference approach, they find that firms use both strategies to counter high labour adjustment costs; especially when EPL for regular workers is stricter (training is preferred variant). While medium and larger firms are more likely to use both strategies, smaller firms tend to make more use of the temporary contracts. More innovative firms are more likely to use temporary contracts and, especially provide more training to upgrade the skills of their workforce than other firms. Lowering the regulation of temporary contracts, leaving that of regular contracts unchanged, does not significantly change the probability of training provision, while increasing that of the former and having lax regulations on the latter increases training (weak evidence). Facing stricter regulations of both variants, firms discourages firms to lower or keep employment stable. In a follow-up paper, using a comparable data set and method, Pierre and Scarpetta (2013) basically confirm their findings from the previous paper. In addition, they show that these effects are more prevalent in small firms and in sectors with high labour turnover. The drawback of both studies the use of data based on subjective managers' perceptions. Therefore, the findings can be questioned for being harder to interpret and subject to stronger endogeneity concerns.

There are two empirical studies using data at the country-level. Picchio and van Ours (2011) use Dutch worker and firm data to analyse, among others, how labour market imperfections influence firm-sponsored training. Exploiting within-firm variation over time, they find that a reduction in EPL reduces incentives for firms to invest in the training of their workers.

The study of Messe and Rouland (2014) is, from an identification strategy point of view, very close to ours in the sense that it exploits a discontinuity in the applicability of EPL. Employing a difference-in-differences framework, it focusses on the effect of an increase in EPL due to an increase dismissal costs for older workers (>50) on the incentives for French firms to finance training. They show that the increase in EPL significantly raises the amount of training for workers aged 45-49, but not for those aged 50 and above.

Most of the studies, except that of Brunello et al. (2007), confirm a positive channel between some kind of EPL and training (be it on hiring and/or firing regulations etc.), even if these results are not

⁵ Using data from the World Bank *Doing Business* project.

always very robust or small in magnitude. While some differentiate between EPL for regular and temporary workers, strictly speaking, only one accounts for the interdependence between training and the use of temporary contracts. As mentioned in the introduction, a major drawback of these studies remains the use of cross-country data. Which, by potentially being affected by omitted variable and measurement problems, could bias the results. Finally, the two studies using country-level studies are with regard to their content not very relevant for our study. All in all, our study complements the existing studies not only from methodological point of view but also with respect to the content.

3. Empirical Methodology

The literature analysing the effects of EPL on labour market outcomes at the country-level can be separated in two strands. The first strand exploits within-country variation across time and sectors. This approach utilizes the fact that, due to technological characteristics or incidence of aggregate shocks, industries differ with respect to the propensity to adjust their workforce. Hence, sectors differ in how strongly they are affected by EPL, allowing to analyse the effect of EPL allowing to employ a difference-in-difference approach (c.f. Rajan and Zingales, 1998; Haltiwanger et al., 2006; Bassanini and Garnero, 2013).

The second strand exploits exemptions in the applicability of EPL in terms of size (Hijzen et al., 2013), regions or type of workers (e.g. age in Messe and Rouland, 2014). Our main identification strategy belongs into this category. We employ an regression discontinuity design (RDD) that exploits the fact that firms beneath a certain firm-size in terms of number of employees are (partially) exempted from EPL in Italy and Finland. Assuming that firms just above the size threshold are good counterfactuals for firms just below the size threshold, we test the hypothesis that firms just above the threshold, being subject to stricter EPL (treatment group), are more likely to train their employees than those below the threshold (control group). In addition, we follow Hijzen et al. (2013) and combine the two literature strands by providing evidence based on a difference-in-difference approach that tests whether the effect of EPL on training below and above the size threshold is stronger for industries with high propensity to adjust their workforce.

3.1 Firm size exemptions from EPL

The following two paragraphs provide a detailed description of the discontinuity of EPL application in Finland and Italy.

The firm-size threshold in Finland applies for firms with less than 20 employees (<20). There are three major EPL measures from which firms below this threshold are exempted. All involve red-tape costs. The first two concern notification procedures in the case of individual dismissal of a regular worker given a lack of work on the part of the firm. First, in companies with 20 or more employees, a notification must be given to the employment office and trade union representatives. In addition, consultation must be made on reasons and ways to avoid a lay-off. In firms with less than 20 employees, it is sufficient to notify the lay-off to the employment office (OECD EPL Database, 2013).

Second, in firms with less than 20 employees no consultation has to take place, which reduces the time delay involved before notification can take place. And lastly, firms with less than 20 employees are exempted from the definition of collective dismissal (if >9 employees dismissed at a time), which would involve further red tape costs (ibid.). In addition, there is also an age-specific exemption from regular EPL for workers aged 68 or above. They can be dismissed at the end of each month without notice period (Venn, 2009).

Exemptions from EPL are much more encompassing in Italy than in Finland. Exempted are all firms with 15 or less employees (<16). They are not required to pay, back-pay or reinstate workers who are found to be unfairly dismissed (Venn, 2009). In detail, the exemptions concerns two EPL measures, both involving red-tape costs. In case of an unfair or unjustified dismissal (as decided by the labour court), firms with more than 15 employees have to give the employee severance pay, varying by age, tenure, number of employees and size of company. The severance pay can be higher in case of a lack of reasons in the written notice or violation of procedural aspects and highest in case of unfair or unlawful dismissal. In case of discriminatory dismissal or if the reason for dismissal is manifestly false or inapplicable, reinstatement will be ordered instead of monetary compensation. Firms with 15 employees or below have the choice between re-employment (different from reinstatement because it does not give rise to compensation for the period between the date of dismissal and the court decision) and financial compensation of the employee, varying by age, tenure and firm size. Second, firms with less than 15 employees are exempted from the definition of collective dismissal if they have been working over a period of 120 days, not in a single production unit, or several units within one province (OECD EPL Database, 2013).

3.2 Identification Strategy

Since the discontinuity stems from a law, we employ a sharp RD design to identify the weighted average treatment effect of EPL on training by estimating the following OLS regression, including probability weights to account for the stratification of the sample:⁶

$$Y_i = \alpha + D_i\tau + f(F_i) + X_i\beta + \varepsilon_i.$$

Y describes the training activities of firm i . D denotes a dummy variable that takes the value 1 for firms above the size threshold and 0 for firms beneath it. F is the size of firm i , measured as the number of employees. In our RDD, this is the assignment variable. The continuous function $f(\cdot)$ accounts for the different possible functional forms of the assignment variable; in our case, we allow up to third order polynomials of the assignment variable (including interactions). X captures a vector of observed characteristics of firm i . Furthermore, we report a robustness check that includes additional observable firm characteristics such as average wages and industry affiliation. And ε_i denotes the normally distributed error term with mean zero.

⁶ Estimating a probit equation for the extensive margin yields qualitatively the same results, which can be obtained from the authors upon request.

Since our assignment variable, the number of employees, is an integer, the RDD estimation does not allow to compute averages within arbitrarily small neighbourhoods of the threshold. As a consequence, we have to specify a particular functional form of the assignment variable to relate assignment and outcome variable. The idea is to model the deviation between the expected value of the outcome and the predicted value from a given functional form as random specification error, which introduces a common component of variance for all observations at a given value of the assignment variable. Hence, the standard errors will be larger and the OLS standard errors will be too small. Therefore, we cluster standard errors on the firm size to account for the imperfect fit of the parametric function away from the discontinuity point (Lee and Card, 2008).

Our choice of the bandwidth and the functional form of the assignment variable in the RDD, we follow a three step procedure. In the first step we employ the methodology suggested by Imbens and Kalyanaraman (2009) to estimate an optimal bandwidth assuming an interacted linear functional form. Our sample only contains firms with more than ten employees, while the discontinuity starts at firm sizes of 16 and 20 in Italy and Spain, respectively. Therefore, the optimal bandwidths reported in table A2.1 in the appendix can exceed the maximal possible bandwidth allowing for a symmetric window around the threshold. In these cases, we use the maximal symmetric bandwidth in our analysis. Based on the chosen bandwidth⁷, we test different functional forms with up to third order polynomials and choose the best functional form according to the Akaike Information Criterion (AIC) as suggested by Lee and Lemieux (2010). These results appear in tables A2.2.1a, A2.2.1b, A2.2.2a and A2.2.2b in the appendix. Finally, we use these results regarding optimal bandwidth and functional form to run OLS regressions for all training outcome variables.

As mentioned in the beginning of this section, we follow Hijzen et al. (2013) by combine the firm-size difference from the RDD with cross-sector variation regarding employment volatility in a difference-in-difference framework as a robustness check. This methodology allows to remove the impact of confounding factors, remove doubts about a possible manipulation of the assignment variable and helps to increase the precision of estimates if pre-treatment controls and post-treatment outcome variables are highly correlated (Hijzen et al., 2013).

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. Therefore, we use cross-sectoral differences in employment volatility. Sectors with highly volatile output demand or a greater rate of use of new technologies than others, need to adjust their workforce more frequently. Consequently, these sectors are more affected by EPL than others where employment volatility is relatively low. Thus, we use the double difference: small versus large firms in high versus low volatility sectors for our difference-in-difference approach by estimating the following OLS regression, including probability weights to account for the stratification of the sample:

⁷In cases where the optimal bandwidth surpasses the maximal possible bandwidth allowing for a symmetric window around the threshold due to data restrictions, we use the maximal symmetric bandwidth.

$$Y_i = \alpha + D_i\tau + f(F_i) + V_j\gamma + \delta D_i*V_j + X_i\beta + \varepsilon_i.$$

V denotes the employment volatility of sector j to capture the relationship between employment volatility and training activities.⁸ $D*V$ represents the interaction of EPL and employment volatility.

4. Data and Descriptive Statistics

4.1 Data

This paper uses data from the third wave of the Continuing Vocational Training Survey (CVTS3), conducted by Eurostat among firms in 19 EU countries. The survey was carried out in 2006 for CVT activities which took place in the reference year 2005. The sample in each country corresponds to firms with more than 10 employees. The sample is stratified by industry affiliation and firm size. Hence, all empirical results are based on probability weighted methodologies that account for the stratification of the sample.

Training

The definition of training used in this paper refers to pre-planned formal (continuing vocational) training, which is clearly separated from the active work place, i.e. learning takes place off-site. The training must have a programme (e.g. curriculum) and objectives. It is provided by trainers, teachers or lecturers and can be organised by the enterprise or an external organisation. The definition does not comprise on-the-job training or other forms of informal training that are directly connected to the active workplace. It is financed at least partly by the enterprises for their employees, with working contract or unpaid family workers and casual workers. Persons with apprenticeship or training contract are not included.

We consider two outcome variables, corresponding to the extensive and intensive margin of training. Concretely, the extensive margin measures whether a firm provides training to at least one employee (*Train 0/1*). The intensive margin measures the amount of paid working hours spent in training activities (*Train Hours*).⁹

There are two exceptions of labour regulations, both special sorts of labour contracts, which provide for high training rates despite being temporary in nature. Both are independent from the firm-size exemptions. These are training contracts for apprentices on one hand and training contracts for the unemployed on the other. The latter shall just be mentioned, because it enters to some extent in the total amount of training and cannot be separated from it in the case of Italy, though the causality runs through a different channel. The former is more important for one additional aspect of the analysis.

⁸ Including dummy variables for the industry affiliation instead yields qualitatively the same results, which can be obtained from the authors upon request.

⁹ Using the number of trained employees to measure training intensity yields qualitatively the same results, which can be obtained from the authors upon request.

Assignment variable

The assignment variable measures the headcount of all persons employed as in the end of 2005. This is a head-count measure, hence consists of integer values. Centering the assignment variable means that firms which are at the point zero receive treatment.

Covariates

Though not directly included in the main regressions, observed covariates are important to prove the validity of the RDD. These comprise industry affiliation(1-digit NACE Rev 1.1), innovation propensity (Innovation), Average Wages (Average Wage calculated as labour costs divided by number of employees), employee age distribution (Share Young, Share Med Age, Share Old, i.e. the share of workers in the age groups <25, 25-55, >55), whether a firm has temporary workers (Temp Emp), whether a firm has part-time workers (Part-Time Emp) and employment growth between 2004 and 2005 (Growth Propensity)

Market volatility measure

The difference-difference identification strategy requires a sector-specific measure for employment volatility. An adequate measure for employment volatility is the job reallocation rate, which is defined as the sum of the job creation and job destruction rate (Haltiwanger et al., 2006). This measure needs to be uncontaminated by the presence of EPL. Therefore, we use the sector-specific job reallocation rate for the USA provided by (Haltiwanger et al., 2006). The USA is ideal for the measure because its OECD EPL indicator takes the value 0.26, which corresponds to the lowest EPL among all OECD-countries (OECD, 2013). Thereby, our proxy for the intrinsic sector employment volatility stems from a country where EPL is (quasi) non-existent. This “EPL-adjusted” intrinsic sector job reallocation rates has the further advantage that it is exogenous to the political and regulatory framework in Finland and Italy, thereby ensuring that the measure is unaffected by other country-specific policies and regulations as well. In addition, the exogeneity of the measure for intrinsic sector job reallocation to idiosyncratic effects in Finland and Italy in 2005 is reinforced by the fact that we use data prior to the year the survey was conducted (2005), namely for the years 1990, 1991 and 1994-1996. Supposing that general equilibrium effects affecting the intrinsic sector employment volatility in the past are unrelated to that in the present the exogeneity argument regarding the employment volatility assumption holds.

There are two doubts questioning the validity of this approach. First, using the job flows of US-industries as a benchmark requires that these industries are representative for the same industries in other countries, which must not always be the case. Second, it has been questioned that the measure represents long-term industry differences in job flows. If the measure also carries idiosyncratic shocks, the job flow measure could rather reflect short-term effects which are specific to the benchmark country and thus bias the estimates (Cingano et al. 2009).

Another doubt regarding the validity approach arises due to the argument that EPL causes a negative selection of the workforce, thereby reducing training. This selection mechanism is most pronounced if employment volatility is low. Hence, assuming that this mechanism is important suggests that sectors with high employment volatility might actually be less affected by EPL than sectors with low employment volatility. Since this argument opposes the basic idea of the identification strategy, it remains unclear to what extent the identification strategy can be applied in this research setting.

4.2 Descriptive statistics

Tables 1 and 2 report country-specific descriptive statistics of dependent variables and observable covariates of the whole sample and for the firms below and above the threshold. Though the complete dataset for Finland comprises 1240 firms, we finally use up to 353 observations for our RDD, which corresponds to roughly one third of the sample (Table 1). This is due to the maximal possible bandwidth of ten employees, since we want to have the same bandwidth on either side of the cut-off. For Italy we have much more data: 15470 firms in the overall sample and about 7387 in the sample we use for the RDD (about 48% of the sample). Thus, the distribution of enterprises is very favourable for our analysis, since we have a lot of observations despite a very small maximal bandwidth of 6.

Comparing the dependent variables below and above the cut-off, the increase in the mean of the extensive margin of training appears rather small, particularly in comparison with the overall sample, where training is substantially higher. Similarly, the total number of training hours is higher for firm sizes 20 to 29, but only by a relatively small amount. Hence, these simple averages provide little evidence that EPL increases training.

Tables 1 and further provide first information regarding the comparability of firms below and above the threshold. Concretely, comparing the respective means of observable firm characteristics suggest that firms below and above the threshold are relatively similar. An exception is the propensity to grow, which is higher for firms above the threshold.

The difference in the dependent variable suggests that our empirical results might not hold for the full sample, i.e. that external validity in terms of firm size might be questionable. On the other hand, in terms of averages wages, innovation propensity, employee age structure and growth propensity, the full sample displays similar characteristics as the sample employed in this study. However, the use of temporary or part-time workers is more prevalent in the full sample.

Table 1: Descriptive statistics Finland

Finland	Full Sample			Firm Size 10-19		Firm Size 20-29		Firm Size 10-29
Variable	Obs	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.	obs
Train 0/1	1240	0.70	0.46	0.60	0.49	0.66	0.48	353
Train Hours	896	662.3	2178.3	122.8	128.1	328.2	332.5	183
d_ind1	1240	0.01	0.07	0.00	0.07	0.00	0.07	353
d_ind2	1240	0.02	0.15	0.02	0.14	0.02	0.14	353
d_ind3	1240	0.01	0.11	0.01	0.08	0.02	0.13	353
d_ind4	1240	0.03	0.17	0.03	0.16	0.03	0.17	353
d_ind5	1240	0.03	0.17	0.02	0.14	0.02	0.14	353
d_ind6	1240	0.06	0.23	0.06	0.24	0.06	0.24	353
d_ind7	1240	0.06	0.24	0.06	0.23	0.06	0.24	353
d_ind8	1240	0.01	0.10	0.00	0.05	0.01	0.11	353
d_ind9	1240	0.04	0.18	0.04	0.19	0.04	0.19	353
d_ind10	1240	0.01	0.10	0.01	0.10	0.00	0.07	353
d_ind11	1240	0.13	0.33	0.17	0.38	0.14	0.34	353
d_ind12	1240	0.03	0.17	0.04	0.19	0.02	0.13	353
d_ind13	1240	0.08	0.28	0.10	0.30	0.09	0.29	353
d_ind14	1240	0.10	0.29	0.16	0.37	0.09	0.29	353
d_ind15	1240	0.04	0.20	0.06	0.24	0.04	0.19	353
d_ind16	1240	0.08	0.26	0.09	0.29	0.04	0.19	353
d_ind17	1240	0.01	0.09	0.00	0.06	0.01	0.09	353
d_ind18	1240	0.03	0.16	0.03	0.18	0.03	0.18	353
d_ind20	1240	0.24	0.43	0.10	0.30	0.28	0.45	353
Average Wage	1205	34760.1	13490.4	36811.6	12696.2	31945.7	12644.9	353
Innovation	1117	0.28	0.45	0.25	0.43	0.28	0.45	319
Share Young	973	0.17	0.17	0.18	0.18	0.14	0.08	204
Share Med Age	1199	0.76	0.17	0.80	0.17	0.76	0.13	351
Share Old	1060	0.17	0.12	0.19	0.13	0.17	0.13	254
Part-Time Emp	812	0.50	0.50	0.36	0.48	0.41	0.50	158
Temp Emp	813	0.51	0.50	0.23	0.42	0.57	0.50	160
Growth Propensity	991	0.45	0.50	0.25	0.44	0.55	0.50	272

Table 2: Descriptive statistics Italy

Italy	Full Sample			Firm Size 10-15		Firm Size 16-21		Firm Size 10-21
Variable	Obs	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.	obs
Train 0/1	15470	0.27	0.44	0.19	0.40	0.23	0.42	7387
Train Hours	5985	1071.4	9342.7	149.8	276.3	174.4	289.7	1795
d_ind1	15470	0.00	0.07	0.00	0.07	0.00	0.06	7387
d_ind2	15470	0.03	0.18	0.03	0.18	0.02	0.15	7387
d_ind3	15470	0.07	0.26	0.06	0.24	0.10	0.30	7387
d_ind4	15470	0.02	0.15	0.02	0.15	0.03	0.16	7387
d_ind5	15470	0.05	0.23	0.04	0.20	0.05	0.21	7387
d_ind6	15470	0.09	0.29	0.09	0.29	0.11	0.31	7387
d_ind7	15470	0.08	0.27	0.06	0.23	0.11	0.31	7387
d_ind8	15470	0.01	0.10	0.01	0.08	0.01	0.07	7387
d_ind9	15470	0.05	0.21	0.05	0.21	0.06	0.24	7387
d_ind10	15470	0.00	0.05	0.00	0.04	0.00	0.04	7387
d_ind11	15470	0.14	0.35	0.17	0.38	0.15	0.35	7387
d_ind12	15470	0.03	0.18	0.04	0.20	0.02	0.15	7387
d_ind13	15470	0.07	0.26	0.08	0.27	0.09	0.28	7387
d_ind14	15470	0.06	0.24	0.08	0.27	0.05	0.21	7387
d_ind15	15470	0.07	0.25	0.09	0.28	0.07	0.25	7387
d_ind16	15470	0.06	0.23	0.05	0.21	0.06	0.23	7387
d_ind17	15470	0.00	0.03	0.00	0.03	0.00	0.02	7387
d_ind18	15470	0.01	0.11	0.01	0.09	0.01	0.09	7387
d_ind20	15470	0.13	0.34	0.12	0.33	0.09	0.29	7387
Average Wage	15470	28319.0	12607.5	25809.7	11572.1	27769.7	10983.8	7387
Innovation	15470	0.24	0.43	0.21	0.41	0.22	0.42	7387
Share Young	8218	0.13	0.12	0.16	0.12	0.13	0.11	3278
Share Med Age	14493	0.86	0.13	0.86	0.13	0.86	0.12	7378
Share Old	9788	0.12	0.09	0.15	0.09	0.13	0.10	4078
Part-Time Emp	5986	0.62	0.48	0.51	0.50	0.51	0.50	1795
Temp Emp	5986	0.49	0.50	0.31	0.46	0.40	0.49	1795
Growth Propensity	15470	0.55	0.50	0.46	0.50	0.63	0.48	7387

5. 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 only want to isolate differences in the characteristics of small and large firms that are due to the differential role of EPL provisions applying above and below the firm size threshold, and not those reflecting the independent effect of firm size or the endogenous response of firms to EPL. The challenge is to control for the first effect and address the possibility of self-selection. In the present case, we expect that selection would take place below the threshold, i.e. that firms select to employ a lower number of employees in order to avoid being subject to EPL. The only possible reason for the

rather unlikely case that firms select into being subject to EPL could be that firms use the selection into EPL as a device to signal commitment towards their employees (Schivardi and Torrini 2008). Though this would represent a causal effect of EPL on training, it might bias the estimation results due to unobserved heterogeneity between selecting and non-selecting firms.

Technically speaking, the RDD is valid if all other unobservable factors influencing the assignment variable, in our case firm-size, are “continuously” related to it. The validity of this identification assumption cannot be tested directly, since we do not observe all factors that might affect the assignment variable. Nevertheless, the literature proposes three ways to test the random assignment assumption, namely whether the density of firms (McCrary, 2008), values of observable covariates (Lee and Lemieux, 2010) and propensity to grow (Schivardi and Torrini, 2008) are balanced across the threshold. This section describes these three tests in detail and reports the test results.

The first validity test is the McCrary test, which assesses whether the density of firms has a jump at the threshold. In case of a significant discontinuity estimate for a given significance level, rejecting the null hypothesis of continuity of the density gives evidence for sorting around the threshold. If the estimate is negative, this is a sign of selection under the threshold, which corresponds to manipulation of the assignment variable.

The assumption of no manipulation of the assignment variable in our case requires that its distribution is continuous for each firm. However, because we only observe one observation per firm for one point in time, we cannot test this directly. We can just if this assumption holds on average, i.e. by testing if the continuity assumption holds for the aggregate distribution of the assignment variable. Figures 1a and 1b display the results of the McCrary test for Finland and Italy. In order to improve readability, the sample is restricted to the largest bandwidth allowing for a symmetric window across the threshold, i.e. for bandwidths of 10 and 6 for Finland and Italy respectively.

Visual inspection of the graphs suggest that the density of firms indicates no selection of firms at the threshold. This is supported by the corresponding insignificant discontinuity estimates reported in table A1.1 in the appendix. This is in line with In addition, Hijzen et al. (2013) who find no evidence of selection in a more comprehensive dataset for Italy. Doubling the bandwidth to 20 for Finland and 12 for Italy provides some evidence that firms select above the threshold in Italy but not in Finland. As argued above, selection into EPL appears rather odd. Nevertheless, it should be kept in mind that a potential selection above the threshold in Italy would suggest that firms select above the threshold to signal their employees their commitment to the employment, which presumably implies they provide more training. Hence, we expect firms above the threshold to provide more training. This suggests that in the presence of sorting into EPL, our results represent an upper bound of the true effect of EPL on training provision, though the bias in the estimates due to unobserved heterogeneity between selecting and non-selecting firms might offset this interpretation.

Figure 1a: McCrary Test, Finland

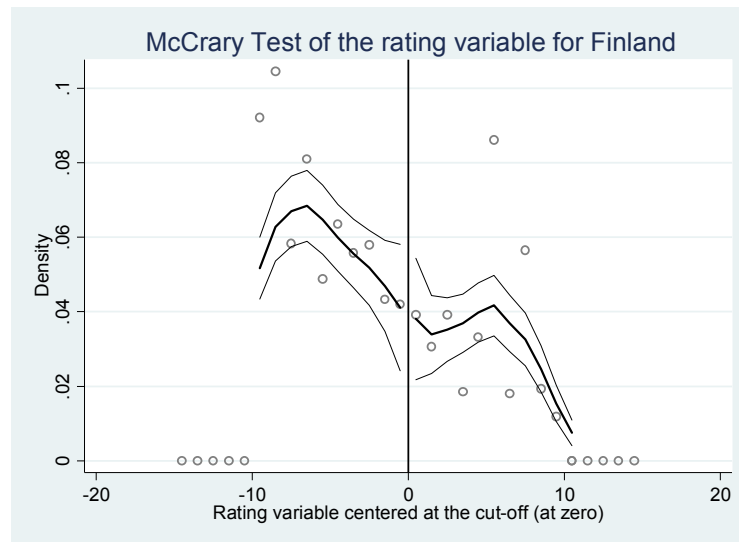
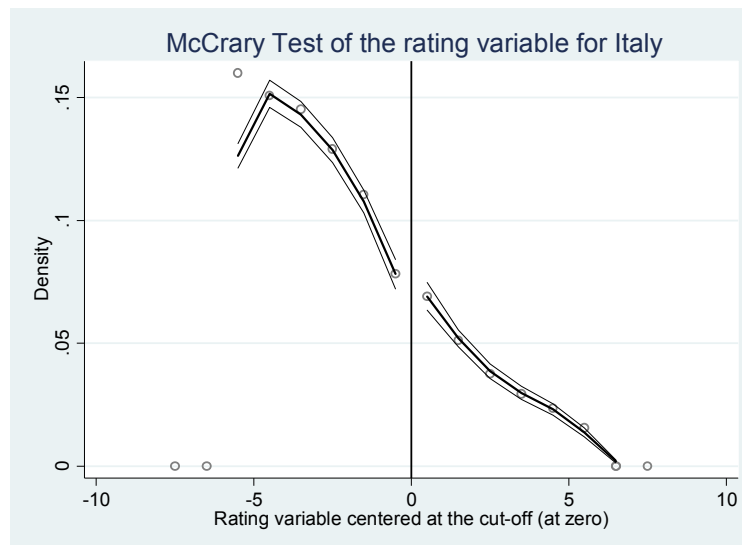


Figure 1b: McCrary Test, Italy



Secondly, we test the validity of the RDD design by examining whether observable firm characteristics, namely industry affiliation, innovation propensity (Innovation), Average Wages (Average Wage), employee age distribution (Share Young, Share Med Age, Share Old), whether a firm has temporary workers (Temp Emp), whether a firm has part-time workers (Part-Time Emp) and employment growth (Growth Propensity) are locally balanced on either side of the threshold. We test this by regressing the observable firm characteristics on the binary cut-off variable and a firm size polynomial of the first, second or third order using either OLS or Probit estimations depending on the nature of the observable firm characteristic. Since the sample consists of firms within the maximal bandwidth and standard errors are clustered at the firm size level, we include no other observable firm characteristic

in the estimations. An insignificant coefficient of the discontinuity estimates displayed in table A1.2 in the appendix provides evidence that the covariates are locally balanced on either side of the cut-off.

Figures A1.1a and A1.1b allow graphical inspection of this relationship by plotting the covariates against the assignment variable, providing no indication of a jump of covariates at the threshold. For Finland, table A1.2 supports this formally by showing that there are significant discontinuity estimates only in two industries and for one covariate when including a third order polynomial, suggesting that the observed firm characteristics are balanced around the threshold. This is different for Italy, where significant discontinuity estimates can be found for some industries, as well as for four of the other covariates. However, this mainly occurs under inclusion of higher order polynomials of the assignment variable. The main exception is the share of firms employing temporary workers, which is lower above the threshold than below. Hence, similar to the McCrary test, we are confident that the RDD is valid for Finland while some doubts regarding Italy remain.

While we have roughly tested whether the propensity to grow differs below and above the threshold, Schivardi and Torrini (2008) suggest a more refined test of the RDD validity based on the propensity to grow. Concretely, they suggest to regress the propensity to grow on a fourth order polynomial of the assignment variable in addition to four dummy variables for firm sizes just below the threshold. Similar to the previous tests, the results displayed in Table A1.2 suggest that firms just below the threshold have the same propensity to grow as firms just above the threshold. However, there is some indication that firms manipulate the assignment variable in Italy as the dummy variables beneath the threshold are significant.

6. Results of the regression discontinuity design

6.1 Graphical analysis of outcome and assignment variable

This section presents the results of the regression discontinuity design, starting with a visual analysis. Concretely, Figures 2a and 2b plot the average outcome variable of each firm against the assignment variable that takes the value 0 at the threshold indicated by a vertical line. According to our hypothesis *H1a (EPL increases training)*, we would expect an upward jump in training at the threshold value of 0. However, the graphical results provide no evidence for this hypothesis but rather in favour of hypothesis *H1b EPL decreases training*). For Finland, there is an increase in both variables, the one measuring the extensive and the intensive margin of training, just below the cut-off. This increase is more pronounced for the extensive margin. For Italy, there is only an increase in the training variable for the extensive margin below the cut-off, while it is the opposite for the intensity variable, where the intensity of training increases in firms just above the cut-off (with 16 employees and above).

Figure 2a: Outcome and rating variables, Finland

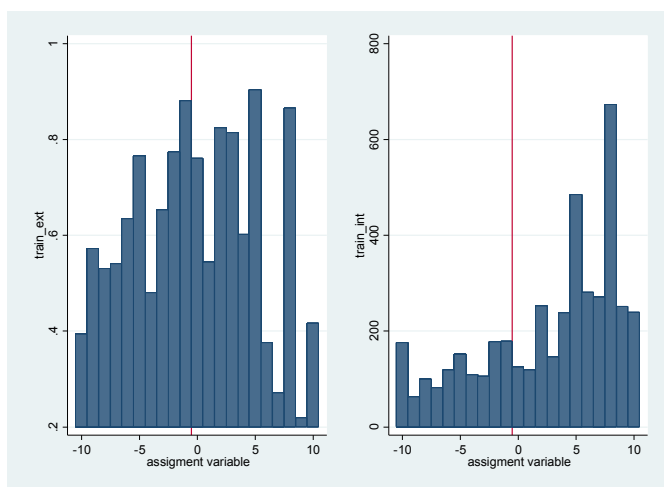
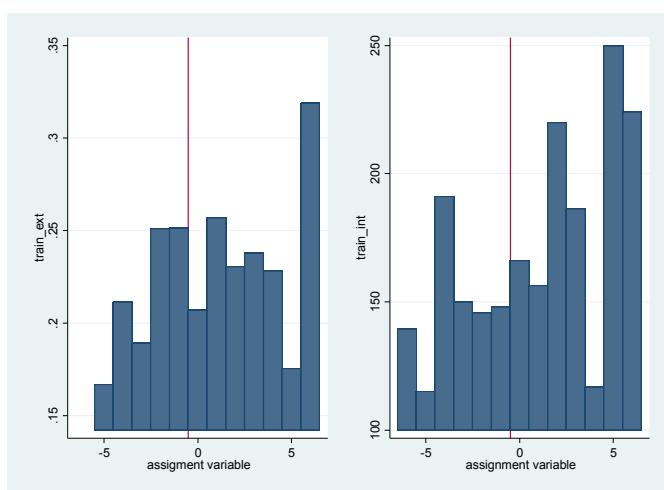


Figure 2b: Outcome and rating variables, Italy



6.2 Baseline Results

Figures 3a and 3b visualize the regression results reported in Tables A3.1a and A3.1b for Finland and Italy, respectively.¹⁰ They plot the estimated discontinuity coefficient (solid line) along with the upper and lower bound of the confidence interval around this coefficient (dashed line) over a specified bandwidth interval. A solid line above/below the zero line represents a positive/negative discontinuity estimate. This estimate is significant as 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 number in the graph shows the discontinuity estimate at the optimal bandwidth (or maximal bandwidth if the optimal surpasses the maximal possible balanced bandwidth).

¹⁰ Note that the discontinuity estimates in figure 3a1 and 3b1 do are not displayed until bandwidth 4 and 3 respectively (vertical line), due to zero coefficients of the discontinuity estimate.

Figure 3a: RDD estimates extensive and intensive margin Finland

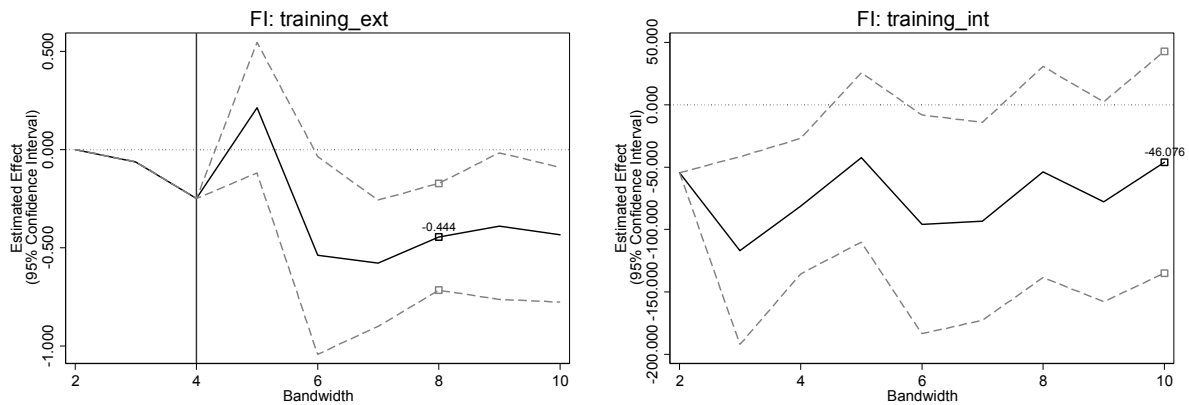
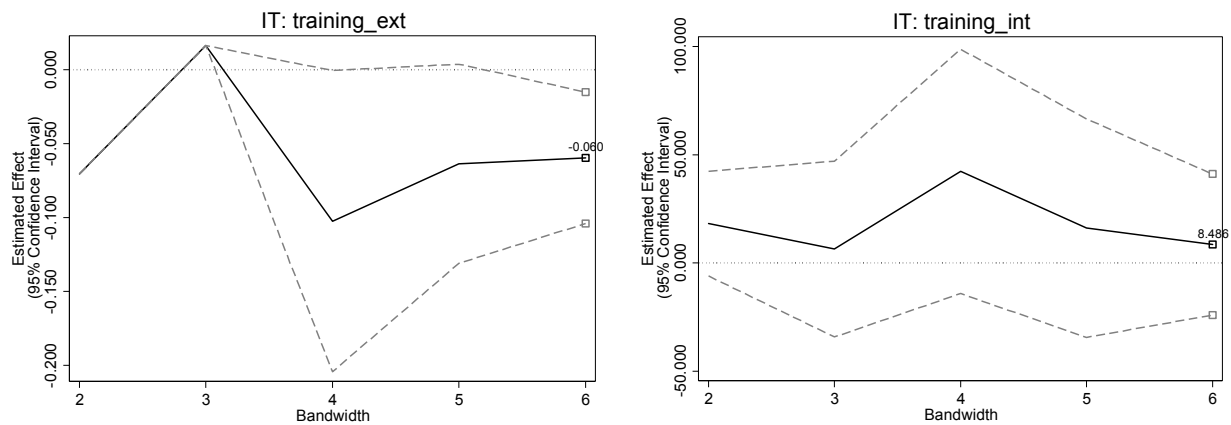


Figure 3b: RDD estimates extensive and intensive margin Italy



Except at bandwidth 5, the discontinuity estimates of the extensive margin of training for Finland are negative and significant along the entire bandwidth interval including the optimal bandwidth. The estimates for the intensive margin are also negative but mostly insignificant (except below bandwidth 5 and at 7), especially towards the optimal bandwidth.

The effect of EPL on the extensive margin of training in Italy is negative and significant at bandwidth 4 and 6, which is the optimal bandwidth. In contrast, the effect of EPL on the intensive margin of training is positive and insignificant along the entire bandwidth interval.

To sum up, the RDD estimates confirm the results from the graphical analysis. There is more evidence in favour the hypothesis that EPL significantly decreases training, at least with respect to the extensive margin. For Finland, this results is likely to hold also for the intensive margin, though less clear-cut. The effect of EPL on the amount of training in Italy, though positive, is not significant at no bandwidth.

6.3 Heterogeneity

This section tests the two hypotheses H2 and H3 which might explain the puzzling finding that EPL might reduce training. First, H2 suggests that one possible explanation is that if, due to stricter EPL, firms “get stuck” with a more heterogeneous workforce, with a higher share of older or less able workers, this decreases their incentive to train relative to firms which are not subject to strict EPL and thus have a more homogenous workforce (see, e.g., Pierre and Scarpetta, 2004; Bertola, 2008). We operationalize the idea by testing whether the interaction between EPL and the share of old workers is negative, i.e. assuming that this negative selection would increase in the share of old workers.

Table 3 allows to assess H2 in two ways. First, it shows the RDD estimates for firms with a share of old workers below or above the country median. Second, this sample split design is complemented by estimates including the interaction of EPL and the share of old workers directly. While the interaction term is negative except for the intensive margin in Italy, the coefficient estimates remain insignificant. Hence, though the coefficients have the expected sign, the data provides only limited support for H2.

Table 2: RDD results, heterogeneous groups

	Finland		Italy	
Margin	extensive	intensive	extensive	intensive
Functional form	cubic interaction	linear interaction	squared interaction	linear
Interaction of Treatment and Share Old (workers aged 55+ and above)				
Treatment	-0.315	101.718		
	[0.202]	[145.684]		
Share Old	-0.345	-19.214	-0.142	-271.658***
	[0.333]	[190.845]	[0.087]	[77.144]
Treatment*Share Old	-0.408	-885.458	-0.106	45.690
	[0.597]	[645.977]	[0.115]	[276.698]
R ²				
N	215	138	4078	1000
Interaction of Treatment and Temporary Worker Use				
Treatment	-	-116.655	-	-17.724
	-	[70.254]	-	[22.409]
Temp Emp	-	9.999	-	-28.106
	-	[33.310]	-	[23.424]
Treatment*Temp Emp	-	157.157*	-	79.135**
	-	[86.431]	-	[35.708]
R ²	-		-	
N	-	160	-	1795

Notes: The table shows OLS coefficients and standard errors clustered at firm size level in parentheses. *, ** and *** denote significance at the 10%, 5% and 1% level, respectively.

Second, we want to test hypothesis H3 suggesting that EPL decreases training by increasing the use of temporary work contracts. Assuming that temporary workers are less likely to receive training, a higher use of temporary contracts would imply less training overall (see, e.g. Pierre and Scarpetta, 2013). Hence, we want to test whether the interaction between EPL and the use of temporary work contracts is negative.

The corresponding results based on a direct interaction appear in table 2, the graphs for the results over different bandwidths can be found in appendix A3. Unfortunately, this test is only possible for the intensive margin because the information concerning temporary work contracts is not available for firms that do not train. The results show that the interaction of EPL and temporary work contracts is positive and significant, suggesting that firms with temporary work contracts train more under EPL than otherwise. This refutes H3.

6.4 Robustness checks

Table 3 displays a number of robustness check to our main estimation strategy, starting with a robustness check that is commonly labelled a “donut regression” (see, e.g., Barreca et al., 2011). In this robustness check, observations of the assignment variable just below and above the cut-off are excluded from the RDD regression, since these are most susceptible to manipulation and thus selection of firms above or below the threshold. Furthermore, we test how our results change if we use robust standard errors instead of clustering standard errors at the firm size level. This also allows us to test whether the inclusion of observable firm characteristics in the estimation affects our results. Furthermore, we report estimation results without using sample weights. Finally, we provide a robustness check that tests whether the estimates of the intensive margin are biased due to a selection bias of training firms by including an inverse Mills ratio in the estimates of the intensive margin.

These robustness checks indicate that the negative effect reported in the baseline results is very sensitive to the choice of the estimation strategy, suggesting that the average effect of EPL is inexistent rather than negative.

Table 3 displays two different donut RDD regressions, first leaving out one observation to the right and left of the cut-off and second leaving out two observations on each side. Dropping one observation of the assignment variable on each side of the cut-off affects the significance level and magnitude of the RDD estimates. Both extensive margin measures for Finland and Italy turn insignificant, while the training intensity variables are just affected in magnitude. Leaving out two observations on each side of the cut-off even reverses the sign of all variables. Also, the increase in magnitude is substantial, while they remain highly insignificant. This finding hints at a potential problem of selection around the cut-off despite the fact that our diagnostic checks did not indicate a selection problem.

Table 3: RDD results

Margins	extensive	intensive	extensive	intensive
Functional form	cubic interaction	linear interaction	squared interaction	linear
Baseline model (clustered standard errors)				
treatment	-0.444**	-46.08	-0.0596*	8.486
	[0.128]	[42.50]	[0.0202]	[14.80]
R ²	0.097	0.273	0.008	0.002
N	290	183	7387	1795
Donut regression				
Leaving out 1 observation of the assignment variable on each side of the cut-off				
treatment	-0.948	-38.7	-0.0346	0.000441
	[0.575]	[72.15]	[0.0535]	[29.55]
R ²	0.09	0.276	0.008	0.002
N	259	162	6099	1431
Leaving out 2 observations of the assignment variable on each side of the cut-off				
treatment	1.344	17.11	0.0686	-19.02
	[2.173]	[94.91]	[0.0984]	[62.04]
R ²	0.1	0.287	0.006	0.003
N	227	145	4799	1083
Robust standard errors				
treatment	-0.444	-46.08	-0.0527	7.279
	[0.303]	[67.22]	[0.0284]	[28.88]
R ²	0.097	0.273	0.008	0.002
N	290	183	7387	1795
Robust standard errors and covariates				
treatment	-0.326	-79.334	-0.050	2.742
	(0.301)	(60.660)	(0.041)	(26.074)
R ²				
N	290	183	7387	1795
Without weights				
treatment	-0.356	-97.81*	-0.0352*	51.63**
	[0.180]	[45.23]	[0.0158]	[12.20]
R ²	0.029	0.172	0.007	0.011
N	290	183	7387	1795
Baseline RDD including inverse Mills ratio (IMR)				
treatment	-	-48.244	-	31.475
	-	(43.561)	-	(18.333)
IMR	-	84.631	-	-13.420**
	-	(82.987)	-	(5.751)
R ²	-		-	
R ² adj	-		-	
N	-	183	-	1795

Notes: Unless noted otherwise, the table shows OLS coefficients and standard errors clustered at firm size level in parentheses. *, ** and ** denote significance at the 10%, 5% and 1% level, respectively.

Using robust standard errors instead of clustering increases the standard errors of all discontinuity estimates. As a consequence, the significance level of the extensive margin estimates turn insignificant. Also, the magnitude of the estimates for Italy changes minimally, while the signs of all variables remain unchanged.

Next, we include some of the observable firm covariates in the regressions of EPL on training, namely average wages and industry dummies. Due to a lack of degrees of freedom, we use robust instead of clustered standard errors. Including covariates in an RDD setting affect standard errors but not coefficient estimates in a valid RDD setting (Lee and Lemieux, 2010). Indeed, the coefficient estimates of the extensive margin remain relatively stable, though increasing standard errors render the estimates insignificant. Furthermore, the estimates of the extensive margin equation are affected in both coefficient magnitude and standard errors.

When we run the RDD regressions without using sample weights but clustered standard errors, the discontinuity estimates maintain the sign of the baseline model, but change in magnitude. The intensity variables for both countries turn significant, i.e. negative in Finland and positive in Italy.

Our measures of training, part-time and temporary employment intensity only exist for firms who do provide training. In order to test whether this causes a selection bias of our estimates, we show the results of estimating a probit equation of the extensive margin of training and include the inverse of the predicted training probability in the estimates of training intensity. The instrument employed in the first stage refers to whether a lack of suitable CVT courses in the market refers to one of the three most important reasons not to provide CVT courses or to increase the scope of CVT courses. For Finland, the inverse Mills ratio is highly insignificant and doesn't affect the estimates. For Italy however, the inverse Mills ratio is significantly negative and the treatment coefficient increases, though remains insignificant.

6.5 Difference-in-difference estimation

Rajan and Zingales (1998) argue that the impact of EPL on training should be larger in high volatility than in low volatility sectors. Hence, we test whether the interaction of the threshold (treatment) variable with the employment volatility variable (intrinsic sector job reallocation rates) has the same sign as the treatment indicator itself.

The key assumption for this strategy is that the variation in the impact of EPL across sectors, thus intrinsic sector volatility, is independent of the variation due to self-selection into size groups across sectors (Hijzen et al., 2013). To test the validity of this assumption, we conduct the McCrary test for each first digit sector of NACE. While each selection test is insignificant for almost all industries, relating sector volatility and McCrary test coefficients suggests a negative albeit small correlation. This suggests that high volatility sectors are more likely to select below the threshold than firms in low volatility sector, thereby casting doubt whether complementing the RDD with the difference-in-difference approach may be appropriate in the present context.

Since the potential presence of a negative selection of the workforce additionally questions the validity of the DiD framework, the detailed results are reported in Tables A3.2a and A3.2b in the appendix.¹¹ The results for Finland suggest that there is no statistical evidence for an impact of EPL on training—neither in high volatility nor in low volatility sectors. The discontinuity estimate as well as the interaction term between the threshold dummy and the volatility variable are both negative and insignificant for both extensive and intensive margin. For Italy, the results indicate that EPL increases the incidence as well as the amount of training in high volatility and decreases both in low volatility sectors. However, only the discontinuity estimate and interaction term of the intensity variable are significant, but not those of the variable measuring the extensive margin.

Hence, the DiD results differ between Finland and Italy. Furthermore, the results do not support the hypothesis that high volatility magnifies the effect of EPL, but rather dampens it. Hence, we conclude that the DiD estimates also provide no evidence that EPL affects training.

6 Conclusion

Neither visual inspection of the relation between assignment and outcome variables nor a series of RDD and DiD estimates provide any support for the hypothesis that EPL increases training. In fact, the presented evidence rather points towards a negative effect of EPL on training. Hypothesizing that this might be due to a negative selection of the workforce, we find some indication that EPL affects firms with an older workforce more negatively, though these results are unstable across empirical specification, e.g. in terms of functional form, bandwidth choice and inclusion of observable firm characteristics. The alternative hypothesis that EPL might decrease training by increasing temporary working rate is not supported by our results.

Hence, contrary to the scant existing empirical evidence (Almeido and Aterido, 2011, Pierre and Scarpetta, 2011, 2013, Piccio and van Ours, 2011, Messe and Rouland, 2014), we conclude that exploiting within-country variation that arises because Finland and Italy exempt small firms from EPL to some extent, yields no evidence that EPL increases Training.

However, this study suffers from a number of drawbacks that should be addressed in future research. First, we only know whether firms have temporary workers or not. Furthermore, we only have this information for firms who train their employees. Hence, we cannot test whether our extensive margin estimations are biased by a shift in temporary worker share due to EPL. Second, our measure of negative selection is very crude. It remains to future studies to examine whether a more refined measure of worker selection provides more convincing results in this regard. Third, relying on within-country variation improves internal validity of our results, but also jeopardizes external validity. This is particularly true for the present analysis that focuses on small firms. Hence, our results might not be applicable for medium-sized and large firms as well as in other countries.

¹¹ Conducting the robustness checks presented for the RDD setting yield qualitatively the same results, which can be obtained from the authors upon request.

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Appendix

Appendix A1: Validity of RDD Design

Table A1.1: McCrary test results for different bandwidths

Finland	Bandwidth	10	20
	Discontinuity estimate	0.057	-0.156
		[0.386]	[0.357]
Italy	Bandwidth	6	12
	Discontinuity estimate	-0.123	0.229***
		[0.109]	[0.085]

Notes: The table shows coefficients and standard errors of a estimation evaluating via local linear regression whether a jump in the density of firms at the threshold occurs. *, ** and *** denote significance at the 10%, 5% and 1% level, respectively.

Figure A1.1a: Graphical Representation of observable firm characteristics around the threshold, Finland

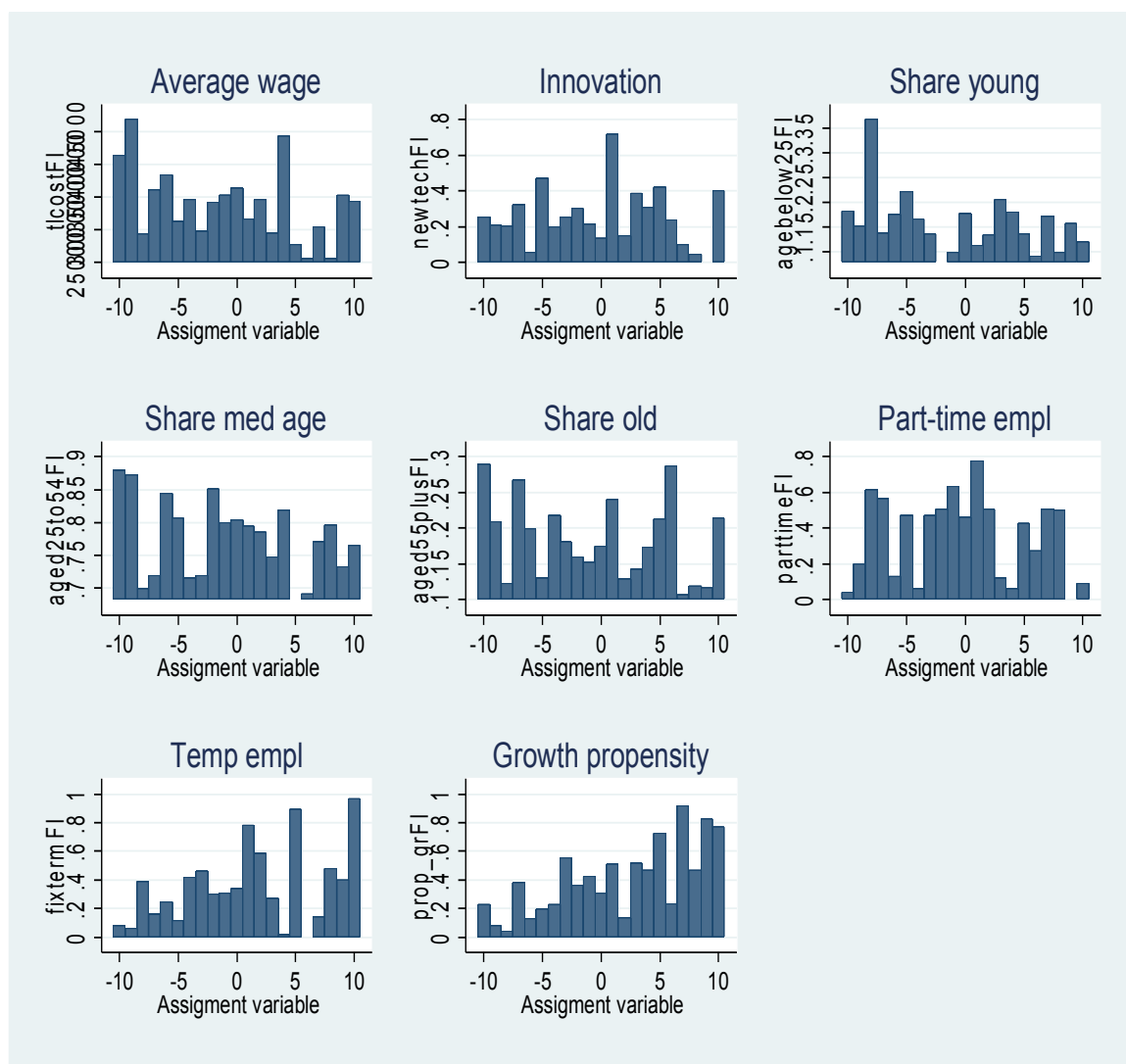


Figure A1.1b: Graphical Representation of observable firm characteristics around the threshold, Italy

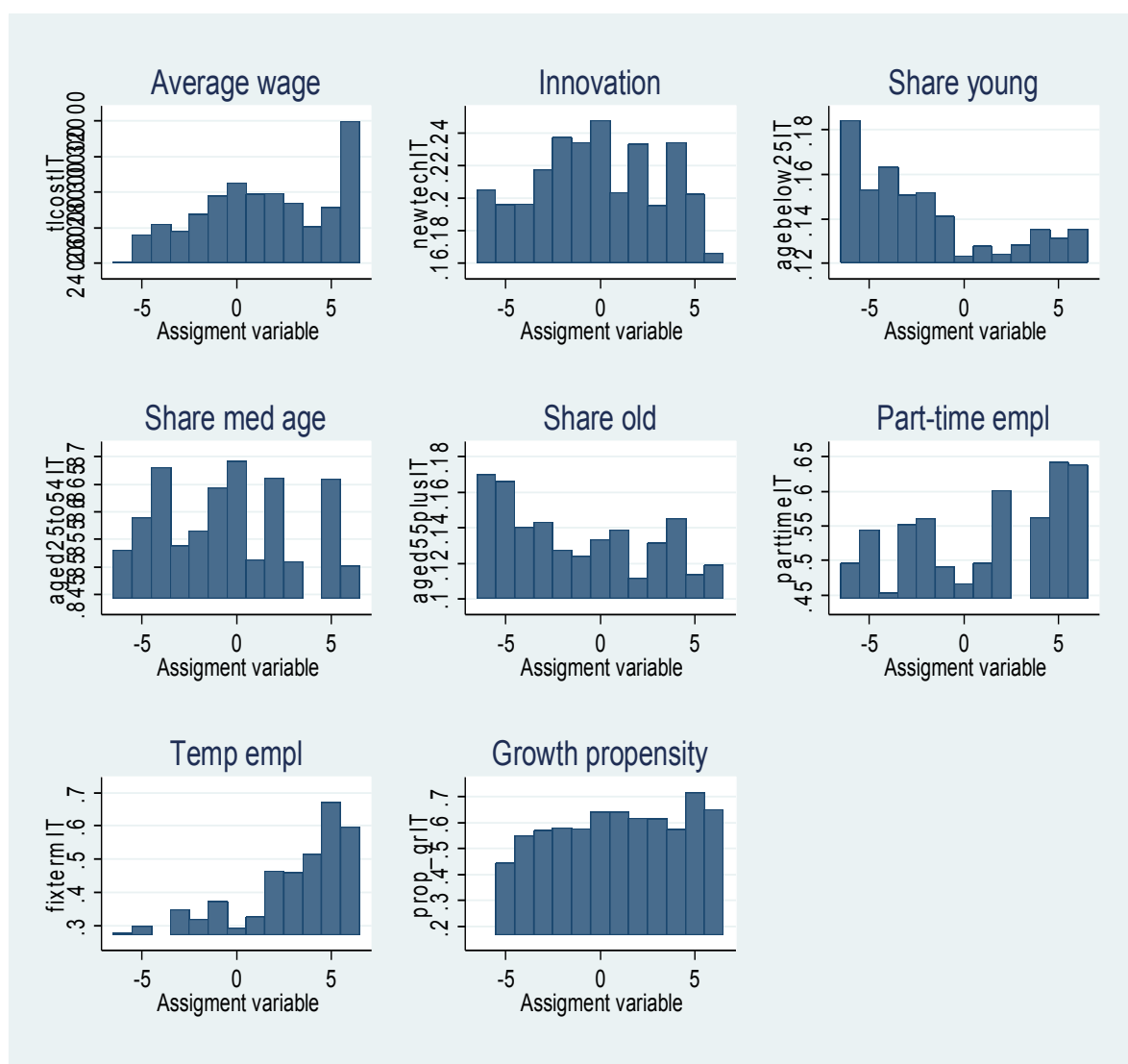


Table A1.2: Balancedness of observable firm characteristics around the threshold

Finland					Italy			
	1st order	2nd order	3rd order	N	1st order	2nd order	3rd order	N
d_ind1	0.0169	-0.108	0.538	353	-0.212	0.0299	0.108	353
	[0.145]	[0.355]	[0.339]		[0.155]	[0.106]	[0.0934]	
d_ind2	-0.0112	-0.223	-0.144		-0.119	0.0279	-0.205	
	[0.233]	[0.373]	[0.541]		[0.198]	[0.167]	[0.154]	
d_ind3	0.381*	0.137	0.664		0.149	0.14	0.164	
	[0.151]	[0.356]	[0.511]		[0.165]	[0.144]	[0.238]	
d_ind4	0.046	0.517	0.0746		0.0639	-0.0755	-0.096	
	[0.221]	[0.312]	[0.298]		[0.144]	[0.115]	[0.154]	
d_ind5	0.046	0.249	0.967*		0.0639	0.273*	0.112	
	[0.221]	[0.490]	[0.472]		[0.144]	[0.130]	[0.110]	
d_ind6	0.000486	0.231	-0.0105		-0.0318	-0.0252	0.095	
	[0.302]	[0.637]	[0.697]		[0.122]	[0.126]	[0.120]	
d_ind7	0.0419	0.484	0.146		0.199*	0.162	0.225	
	[0.192]	[0.252]	[0.226]		[0.0799]	[0.104]	[0.115]	
d_ind8	0.497**	0.156	0.694		-0.0452	0.0818	-0.142	
	[0.193]	[0.560]	[0.862]		[0.172]	[0.156]	[0.108]	
d_ind9	-0.0172	-0.337	-0.26		-0.165	-0.163*	-0.069	
	[0.169]	[0.338]	[0.362]		[0.0853]	[0.0817]	[0.0891]	
d_ind10	-0.276	-0.743	-0.589		-0.0881	-0.108	-0.307	
	[0.234]	[0.399]	[0.483]		[0.134]	[0.159]	[0.191]	
d_ind11	-0.154	-0.382	-0.517		-0.0513	-0.170***	-0.167*	
	[0.200]	[0.462]	[0.527]		[0.0881]	[0.0445]	[0.0675]	
d_ind12	-0.337	0.794*	0.926		-0.248	-0.107	-0.382**	
	[0.317]	[0.389]	[0.510]		[0.170]	[0.196]	[0.132]	
d_ind13	-0.0473	-0.285	-0.334		-0.151	-0.117	-0.035	
	[0.367]	[0.637]	[0.646]		[0.111]	[0.114]	[0.151]	
d_ind14	-0.344	-0.223	1.047		0.129	0.253	-0.041	
	[0.347]	[0.521]	[0.733]		[0.145]	[0.145]	[0.0587]	
d_ind15	-0.235	0.536	0.883		0.151	-0.103	0.008	
	[0.263]	[0.576]	[0.865]		[0.179]	[0.0799]	[0.128]	
d_ind16	-0.442	0.0762	-0.0909		-0.287	-0.193	-0.085	
	[0.361]	[0.422]	[0.469]		[0.190]	[0.205]	[0.259]	
d_ind17	0.31	-0.334	0.532		-0.371	-0.293	-0.145	
	[0.257]	[0.420]	[0.517]		[0.294]	[0.339]	[0.441]	
d_ind18	0.0222	0.166	0.721*		0.276**	0.261**	0.134	
	[0.228]	[0.284]	[0.292]		[0.0952]	[0.0847]	[0.104]	
d_ind20	0.699	2.346**	-1.424		0.0449	0.284*	0.284*	
	[0.543]	[0.850]	[1.088]		[0.118]	[0.136]	[0.143]	
Innovation	0.0919	0.505	0.0105	319	-0.0483	-0.00573	-0.065	7387
	[0.224]	[0.530]	[0.633]		[0.0772]	[0.0673]	[0.0822]	
Average Wage(1)	-4866	5537.1	1928.9	353	-223.8	969	1122	7387
	[2771.7]	[3103.1]	[3688.3]		[1116.5]	[517.0]	[780.3]	
Share Young (1)	-0.0393	0.0412	0.0937	204	-0.00646	-0.0163**	-0.0235*	3278
	[0.0310]	[0.0333]	[0.0494]		[0.0110]	[0.00438]	[0.00892]	
Share Med Age (1)	-0.042	0.0365	-0.02	351	-0.00017	0.00422	0.012	7378
	[0.0325]	[0.0452]	[0.0653]		[0.00828]	[0.00798]	[0.0104]	
Share Old(1)	-0.0247	0.0517	0.00624	254	0.0270*	0.0169*	0.0167*	4078
	[0.0278]	[0.0330]	[0.0449]		[0.00883]	[0.00597]	[0.00592]	
Part-Time Emp	0.126	-0.29	0.0833	158	-0.127	-0.17	-0.14	1795
	[0.225]	[0.302]	[0.424]		[0.115]	[0.0975]	[0.158]	
Temporary Emp	0.909*	0.182	0.00865	160	-0.186	-0.323***	-0.315***	1795
	[0.399]	[0.373]	[0.500]		[0.124]	[0.0520]	[0.0562]	
Growth Propensity	0.774**	-0.547	-0.678	272	-0.456	-0.138	0.461***	7387
	[0.296]	[0.339]	[0.385]		[0.323]	[0.175]	[0.129]	

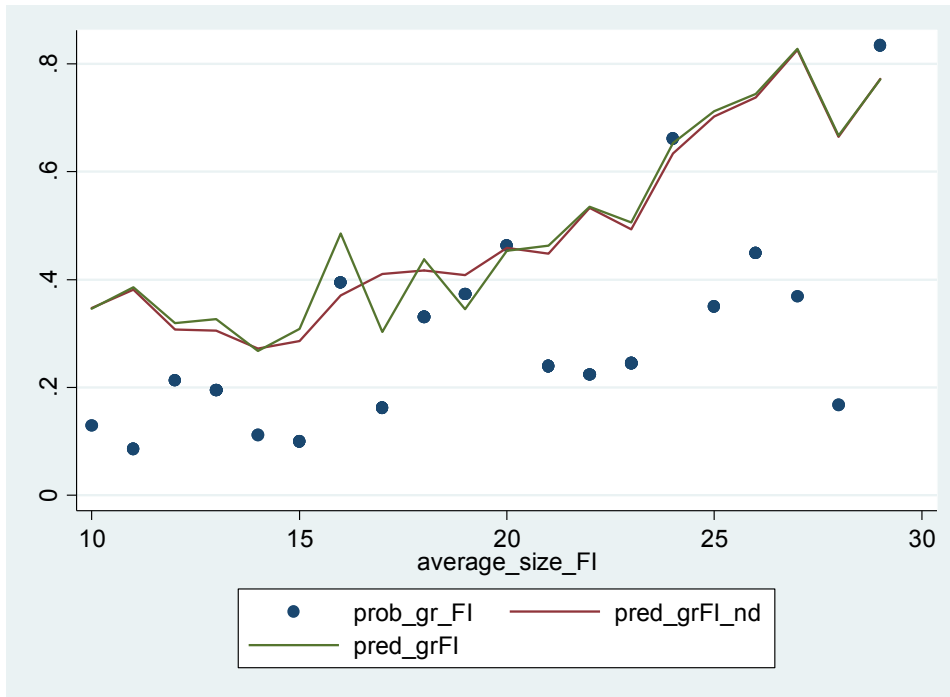
Notes: *, ** and *** denote significance at the 10%, 5% and 1% level, respectively. The table displays marginal effects of a Probit model or, those marked with a (1), with OLS; both with standard errors clustered at firm size level in parentheses. Each entry corresponds to a regression of the observable firm characteristics on the threshold, controlling for a first order, second order or third order polynomial of firm size, respectively.

Table A1.4: Propensity to grow by country

	Finland		Italy	
	No industry dummies		No industry dummies	
Size dummy				
Firm size 12			-0.0688	-0.0689
			[0.635]	[0.635]
Firm size 13			0.112	0.113
			[0.664]	[0.665]
Firm size 14			-0.0645	-0.0649
			[0.645]	[0.645]
Firm size 15			0.00929	0.00859
			[0.555]	[0.555]
Firm size 16	0.484	0.548		
	[0.837]	[0.816]		
Firm size 17	-1.079	-1.059		
	[0.872]	[0.874]		
Firm size 18	-0.0658	-0.161		
	[0.851]	[0.804]		
Firm size 19	-0.228	-0.174		
	[0.684]	[0.699]		
Total firm size _{t-1}	-1.009*	-0.94	-0.82	-0.819
	[0.479]	[0.510]	[0.465]	[0.465]
(Total firm size _{t-1}) ²	0.0598*	0.0564*	0.0464	0.0463
	[0.0253]	[0.0268]	[0.0241]	[0.0241]
(Total firm size _{t-1}) ³	-0.00125*	-0.00118*	-0.000983*	-0.000983*
	[0.000490]	[0.000518]	[0.000479]	[0.000479]
(Total firm size _{t-1}) ⁴	0.00000713**	0.00000679*	0.00000609*	0.00000608*
	[0.00000272]	[0.00000287]	[0.00000289]	[0.00000288]
Average Wage	-0.0000226	-0.0000238	0.0000102***	0.0000102***
	[0.0000166]	[0.0000168]	[0.00000161]	[0.00000164]
Innovation	0.465	0.449	0.0668	0.0692
	[0.315]	[0.308]	[0.0654]	[0.0655]
Share Young	-2.326**	-2.235**	-0.706**	-0.713**
	[0.729]	[0.784]	[0.260]	[0.264]
Industry sector				
Mining and quarrying		0.05		0.0541
		[0.561]		[0.135]
Manufacturing		-0.131		-0.0217
		[0.272]		[0.0496]
Construction		0.335		-0.0115
		[0.568]		[0.0561]
Services		0		0
		[.]		[.]
N	191	191	5278	5278
AIC	24.44	30.43	24.53	30.53
BIC	63.46	79.22	103.4	129.1
chi2	37.53	46.52	96.82	174.4

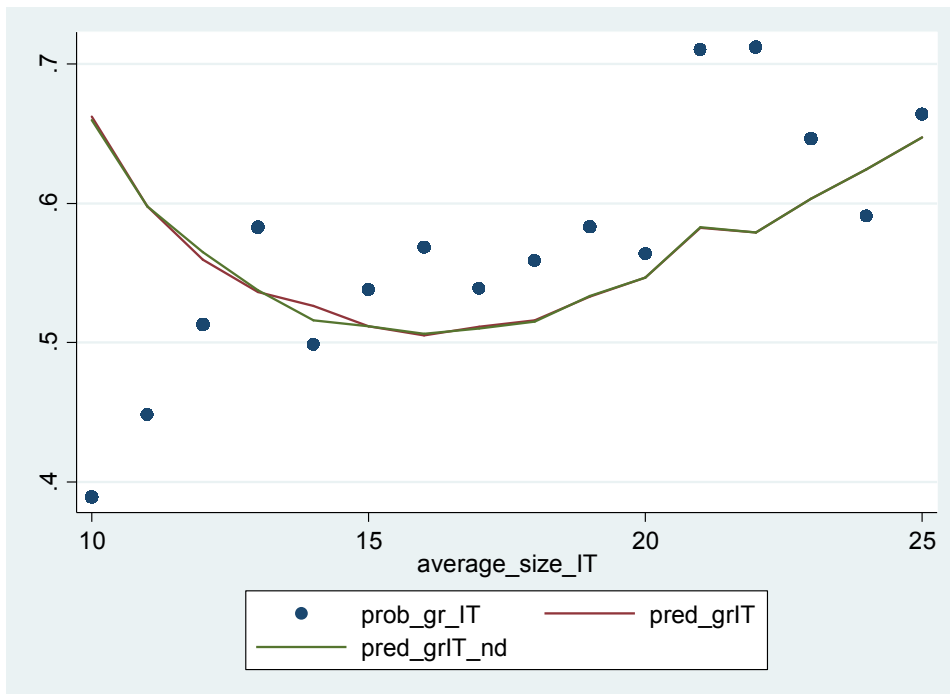
Notes: The table shows the marginal effects of a probit estimation with the propensity to grow as dependent variable and standard errors clustered at firm size level in parentheses. *, ** and *** denote significance at the 10%, 5% and 1% level, respectively.

Figure A1.2a: Graphical Representation of Propensity to grow, Finland



Note: Propensity to grow for Finland (prob_gr_* is the share of firms which grew from 2004 to 2005, pred_gr*_nd is the predicted probability without firm size dummies, pred_gr* with size dummies)

Figure A1.2b: Graphical Representation of Propensity to grow, Italy



Note: Propensity to grow for Italy (prob_gr_* is the share of firms which grew from 2004 to 2005, pred_gr*_nd is the predicted probability without firm size dummies, pred_gr* with size dummies)

Appendix A2: Optimal Bandwidth and Functional Form

The first step in choosing the bandwidth and functional form is to estimate optimal bandwidths based on the Imbens/Kalyanaraman (2009) methodology using the Stata rd-package (Nichols, 2007). The resulting bandwidth choice is shown in Table A2.1 together with the maximum bandwidth that allows to estimate the RDD with a symmetric window.

Table A2.1 Optimal bandwidth

Variable	optimal bandwidth	Maximum Bandwidth with Symmetric Window
Finland		
Train 0/1	8	10
Train Hours	20	10
Italy		
Train 0/1	6	6
Train Hours	23	6

Based on the optimal bandwidth, we run the RDD for six possible ways to model the assignment variable. The corresponding regression results are shown in Tables A2.2.1a,b for Finland and Tables A2.2.2a,b for Italy, respectively. The AIC criterion serves to select the best-fitting model. In the case of the extensive margin estimates for Finland and Italy, the AIC suggests to use cubic and quadratic interactions of the assignment variable in the estimation, respectively. Intensive margin estimates for Italy include the assignment variable linearly in the estimation while in Finland, the linear interaction model is chosen. Additional model selection criteria are the adjusted R², the BIC information criterion and p-values of Wald tests that examine successively whether the model fit improves significantly by adding the more complex polynomial.

Table A2.2a Finland: Extensive margin

training_ext	linear	linear interaction	quadratic	quadratic interaction	cubic	cubic interaction
rvcFI	0.00274 [0.0243]	0.0387*** [0.00873]	-0.0595 [0.0878]	0.0884 [0.0730]	-0.00706 [0.189]	0.351* [0.123]
zFI	-0.000961 [0.174]	0.00319 [0.123]	0.103 [0.175]	-0.298 [0.171]	-0.183 [0.247]	-0.444** [0.128]
rvcFI_zFI		-0.0822* [0.0370]	0.0911 [0.153]	0.0834 [0.122]	0.124 [0.160]	-0.486* [0.171]
rvcFI_sq			-0.0106 [0.00914]	0.00538 [0.00799]	-0.0193 [0.0445]	0.0732* [0.0295]
rvcFI_sq_zFI				-0.0359* [0.0141]	0.00861 [0.0752]	0.00688 [0.0552]
rvcFI_cub					-0.00181 [0.00318]	0.00499* [0.00213]
rvcFI_cub_zFI						-0.0151** [0.00489]
_cons	0.656*** [0.130]	0.829*** [0.0667]	0.655** [0.185]	0.917*** [0.103]	0.822** [0.208]	1.178*** [0.121]
N	290	290	290	290	290	290
R-sq	0.001	0.038	0.049	0.078	0.079	0.097
R-sq_adj	-0.006	0.028	0.035	0.061	0.059	0.075
aic_n	398.675	389.537	388.42	381.456	383.113	379.157
bic_p	11.13	5.663	8.216	4.921	10.248	9.962

Notes: The table shows OLS coefficients and standard errors clustered at firm size level in parentheses. *, ** and *** denote significance at the 10%, 5% and 1% level, respectively.

Table A2.2b Finland: Intensive margin

training_int	linear	linear interaction	quadratic	quadratic interaction	cubic	cubic interaction
rvcFI	21.05*	6.999	16.92	33.68*	-38.59	-20.46
	[8.431]	[5.246]	[22.50]	[15.28]	[56.52]	[43.61]
zFI	3.68	-46.08	-58.22	-121.8*	-24.99	-35.15
	[80.44]	[42.50]	[56.92]	[44.68]	[60.60]	[47.93]
rvcFI_zFI		47.76**	31.02	40.22	75.45	33.77
		[14.29]	[39.15]	[44.75]	[56.98]	[83.34]
rvcFIsq			0.895	2.408	-13.28	-9.344
			[2.187]	[1.638]	[12.02]	[9.346]
rvcFIsq_zFI				-4.919	22.62	26.21
				[6.087]	[20.09]	[26.33]
rvcFIcub					-0.952	-0.713
					[0.718]	[0.561]
rvcFIcub_zFI						-0.844
						[2.141]
_cons	243.3***	162.8***	182.8***	216.6***	135.7*	156.0**
	[45.74]	[24.99]	[44.67]	[24.84]	[62.19]	[43.37]
N	183	183	183	183	183	183
R-sq	0.216	0.273	0.273	0.277	0.281	0.282
R-sq_adj	0.207	0.261	0.257	0.257	0.257	0.253
aic_n	2497.574	2485.729	2487.575	2488.585	2489.635	2491.483
bic_p	-34.039	-42.674	-37.618	-33.4	-29.14	-24.082

Notes: The table shows OLS coefficients and standard errors clustered at firm size level in parentheses. *, ** and *** denote significance at the 10%, 5% and 1% level, respectively.

Table A2.3a Italy: Extensive margin

training_ext	linear	linear interaction	quadratic	quadratic interaction	cubic	cubic interaction
rvclT	0.0173**	0.0226***	0.0042	0.0158	0.0422	0.0386
	[0.00404]	[0.00235]	[0.0139]	[0.0167]	[0.0525]	[0.0625]
zIT	-0.0647*	-0.0527*	-0.0309	-0.0596*	-0.0831	-0.0808
	[0.0253]	[0.0233]	[0.0201]	[0.0202]	[0.0429]	[0.0490]
rvclT_zIT		-0.0243**	0.00505	0.0191	0.00167	0.00976
		[0.00722]	[0.0230]	[0.0190]	[0.0376]	[0.0668]
rvclTsq			-0.00251	-0.000929	0.00751	0.00637
			[0.00204]	[0.00239]	[0.0160]	[0.0192]
rvclTsq_zIT				-0.00746*	-0.0214	-0.023
				[0.00317]	[0.0262]	[0.0230]
rvclTcub					0.000786	0.00068
					[0.00141]	[0.00172]
rvclTcub_zIT						0.000505
						[0.00236]
_cons	0.261***	0.281***	0.255***	0.271***	0.294***	0.291***
	[0.0178]	[0.0137]	[0.0161]	[0.0191]	[0.0412]	[0.0488]
N	7387	7387	7387	7387	7387	7387
R-sq	0.006	0.008	0.008	0.008	0.008	0.008
R-sq_adj	0.006	0.007	0.007	0.007	0.007	0.007
aic_n	7431.141	7420.966	7421.309	7420.862	7422.572	7424.552
bic_p	-26.085	-29.352	-22.102	-15.641	-7.024	1.863

Notes: The table shows OLS coefficients and standard errors clustered at firm size level in parentheses. *, ** and *** denote significance at the 10%, 5% and 1% level, respectively.

Table A2.3b Italy: Intensive margin

training_int	linear	linear interaction	quadratic	quadratic interaction	cubic	cubic interaction
rvclT	3.06	2.496	-21.38	-26.39	10.4	-18
	[3.547]	[3.708]	[21.87]	[24.18]	[64.48]	[74.75]
zIT	8.486	7.279	33.42	45.58	13.2	31.1
	[14.80]	[14.58]	[29.14]	[31.21]	[49.38]	[56.44]
rvclT_zIT		2.461	40.84	35.09	10.91	72.3
		[9.944]	[32.54]	[32.53]	[46.08]	[91.91]
rvclTsqr			-3.384	-4.094	7.895	-1.36
			[2.890]	[3.219]	[23.58]	[27.15]
rvclTsqr_zIT				3.219	-16.57	-27.75
				[6.762]	[40.29]	[41.15]
rvclTcub					1.133	0.259
					[2.379]	[2.726]
rvclTcub_zIT						3.842
						[5.021]
_cons	160.7***	158.7***	125.5***	118.6**	148.9**	125.5*
	[11.43]	[9.505]	[27.40]	[30.53]	[47.58]	[55.54]
N	1795	1795	1795	1795	1795	1795
R-sq	0.002	0.002	0.003	0.003	0.003	0.003
R-sq_adj	0.001	0	0	0	0	-0.001
aic_n	25323.36	25325.3	25325.83	25327.6	25329.3	25330.68
bic_p	11.829	19.259	25.285	32.549	39.736	46.612

Notes: The table shows OLS coefficients and standard errors clustered at firm size level in parentheses. *, ** and *** denote significance at the 10%, 5% and 1% level, respectively.

Appendix A3: Estimation Results of RDD Estimates shown in Figures 3a to 3c and 4a to 4c

The following table report the detailed estimation results displayed in Figures 3a to 3c and 4a to 4c

Table A3.1a: RDD Results for Finland

Specification	Bandwidth								
training_ext	2 (1)	3(1)	4(1)	5	6	7	8	9	10
cubic interaction	0	-0.0620***	-0.249***	0.213	-0.538*	-0.578**	-0.444**	-0.389*	-0.433*
	[.]	[6.53e-15]	[4.50e-13]	[0.147]	[0.229]	[0.149]	[0.128]	[0.177]	[0.164]
R^2	0.07	0.057	0.089	0.07	0.047	0.054	0.097	0.041	0.065
N	[1.55e-13]	96	136	172	206	248	290	323	353
training_int	0.038	3	4	5	6	7	8	9	10
linear interaction	38	-116.8*	-81.21**	-42.25	-95.84*	-93.33*	-53.87	-77.75	-46.08
	0	[29.25]	[22.96]	[30.01]	[39.81]	[36.70]	[39.69]	[37.96]	[42.50]
R^2	[.]	0.058	0.038	0.031	0.234	0.24	0.204	0.291	0.273
N	63	59	83	106	124	140	160	173	183

Notes: The table shows OLS coefficients and standard errors clustered at firm size level in parentheses. *, ** and *** denote significance at the 10%, 5% and 1% level, respectively. (1) indicates insufficient degrees of freedom. Extensive margin estimates for Finland and Italy include cubic and quadratic interactions of the assignment variable in the estimation, respectively. Intensive margin estimates for Finland and Italy include the assignment variable linearly in the estimation.

Table A3.1b: RDD Results for Italy

Specification	Bandwidth				
training_ext	2 (1)	3	4	5	6
quadratic interaction		0.0163***	-0.102*	-0.0636	-
		[8.71e-14]	[0.0431]	[0.0297]	[0.0202]
R^2		0.004	0.002	0.004	0.008
N	2588	3904	5194	6314	7387
training_int	2	3	4	5	6
linear	18.13	6.441	42.3	16.05	8.486
	[7.598]	[15.80]	[23.88]	[22.32]	[14.80]
R^2	0.001	0.003	0.001	0.001	0.002
N	712	1006	1334	1586	1795

Notes: The table shows OLS coefficients and standard errors clustered at firm size level in parentheses. *, ** and *** denote significance at the 10%, 5% and 1% level, respectively. (1) indicates insufficient degrees of freedom. Extensive margin estimates for Finland and Italy include cubic and quadratic interactions of the assignment variable in the estimation, respectively. Intensive margin estimates for Finland and Italy include the assignment variable linearly in the estimation.

Table A3.2a: DiD Results for Finland

Specification									
training_ext	Bandwidth								
cubic interaction	2	3	4	5	6	7	8	9	10
zFI	0	0.312	-0.0244	0.557*	-0.139	-0.163	-0.274	-0.466	-0.597
	[.]	[0.356]	[0.354]	[0.232]	[0.319]	[0.306]	[0.359]	[0.382]	[0.394]
zFI*JR_USA	-3.883*	-1.459	-0.655	-1.415	-1.461	-1.674	-0.605	0.254	0.51
	[1.026]	[1.502]	[1.419]	[1.283]	[1.116]	[1.035]	[1.259]	[1.295]	[1.235]
R^2	0.191	0.077	0.093	0.083	0.062	0.069	0.102	0.042	0.076
N	59	92	126	156	186	222	260	289	316
training_int									
linear interaction	2	3	4	5	6	7	8	9	10
zFI	-42.66	-7.405	-80.5	-115	126.4	105.9	209.7	183.3	179.3
	[314.5]	[195.7]	[188.6]	[161.0]	[179.3]	[175.6]	[153.8]	[156.4]	[159.4]
zFI*JR_USA	10.77	-471.8	-4.828	258.9	-947.2	-831.2	-1178.2	-1185.5	-1045.3
	[1281.6]	[703.7]	[657.9]	[626.7]	[757.6]	[750.2]	[643.3]	[635.3]	[644.4]
R^2	0.06	0.067	0.038	0.042	0.284	0.284	0.283	0.353	0.332
N	36	57	78	97	114	130	149	160	169

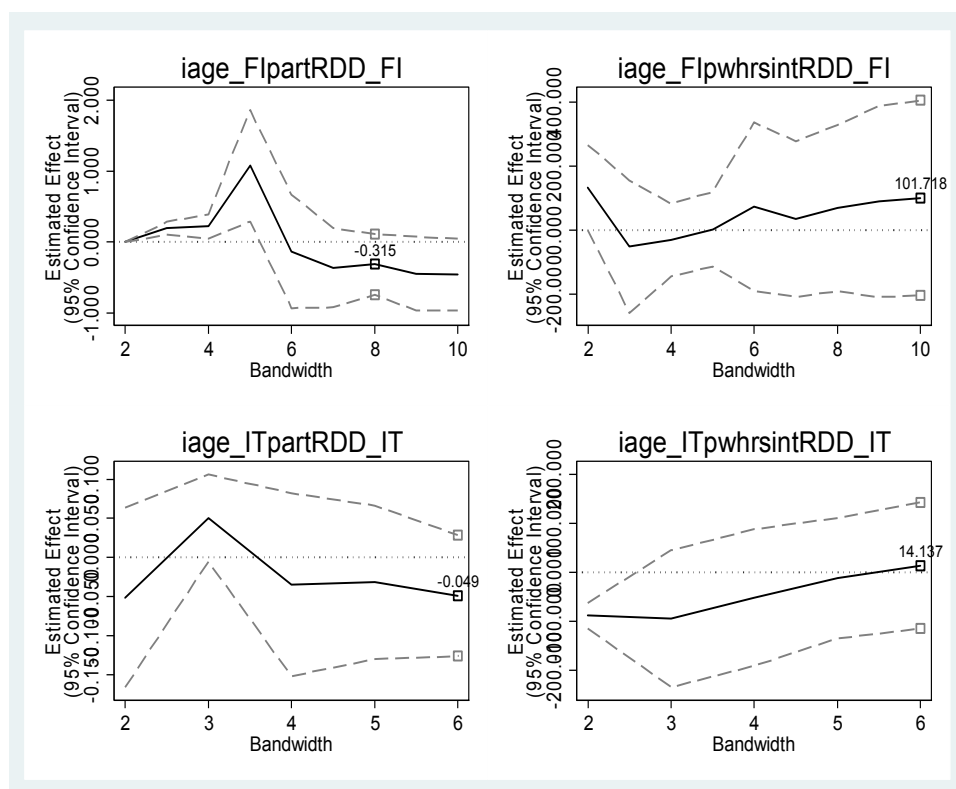
Notes: The table shows OLS coefficients and standard errors clustered at firm size level in parentheses. *, ** and *** denote significance at the 10%, 5% and 1% level, respectively. (1) indicates insufficient degrees of freedom. Extensive margin estimates for Finland and Italy include cubic and quadratic interactions of the assignment variable in the estimation, respectively. Intensive margin estimates for Finland and Italy include the assignment variable linearly in the estimation.

Table A3.2b: DiD Results for Italy

Specification					
Train 0/1	Bandwidth				
squared interaction	2	3	4	5	6
zIT	-0.081	-0.00355	-0.117	-0.0825	-0.103*
	[0.0388]	[0.0312]	[0.0557]	[0.0427]	[0.0416]
zIT*JR_USA	0.081	0.148	0.0929	0.11	0.219
	[0.168]	[0.134]	[0.134]	[0.116]	[0.147]
R^2	0.005	0.008	0.005	0.008	0.012
N	2517	3797	5044	6142	7198
Train Hours					
linear	2	3	4	5	6
zIT	9.934	-49.86	-31.27	-68.79	-75.37*
	[17.85]	[45.97]	[33.29]	[33.84]	[29.62]
zIT*JR_USA	24.07	233.1	303.3	354.5*	356.0*
	[77.39]	[170.8]	[162.1]	[143.7]	[127.1]
R^2	0.004	0.006	0.007	0.008	0.009
N	687	974	1294	1542	1748

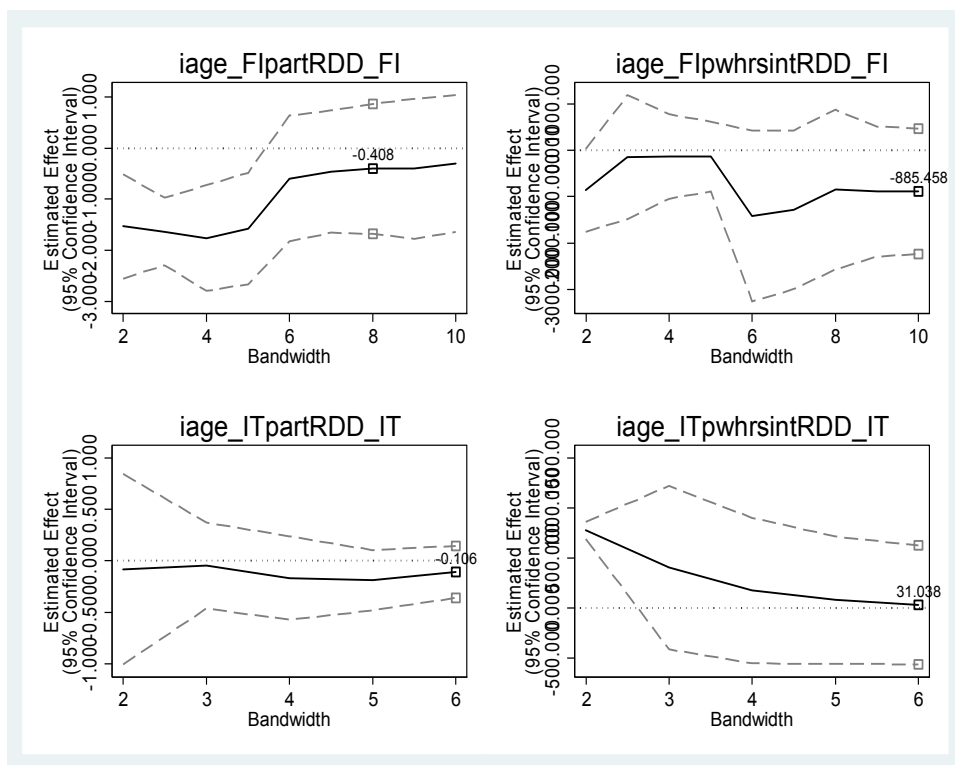
Notes: The table shows OLS coefficients and standard errors clustered at firm size level in parentheses. *, ** and *** denote significance at the 10%, 5% and 1% level, respectively. (1) indicates insufficient degrees of freedom. Extensive margin estimates for Finland and Italy include cubic and quadratic interactions of the assignment variable in the estimation, respectively. Intensive margin estimates for Finland and Italy include the assignment variable linearly in the estimation.

Figure A3.2a: Heterogeneity; Firms with a share of workers aged 55+ above country median, Threshold effect



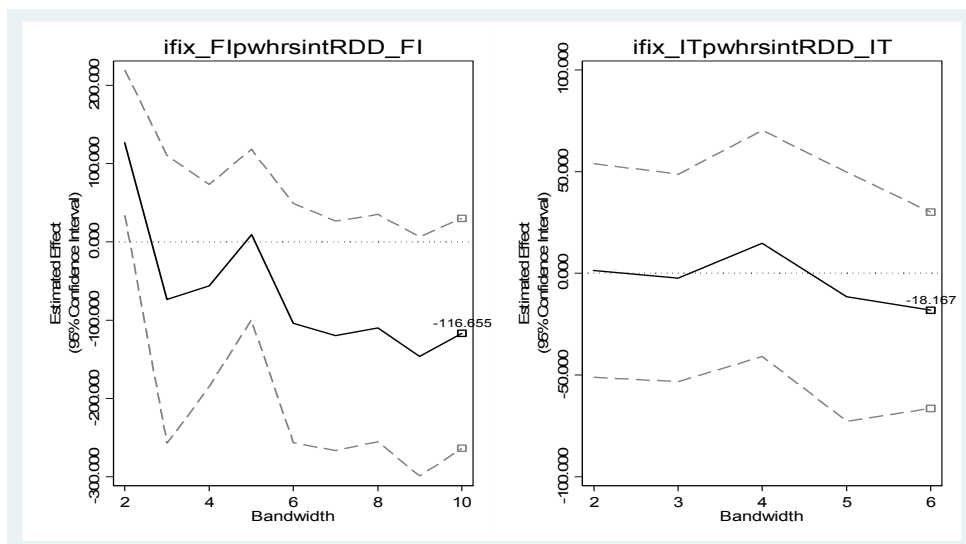
Two graphs above: results for Finland; two graphs below: results for Italy

Figure A3.2b: Heterogeneity; Firms with a share of workers aged 55+ above country median, Interaction effect



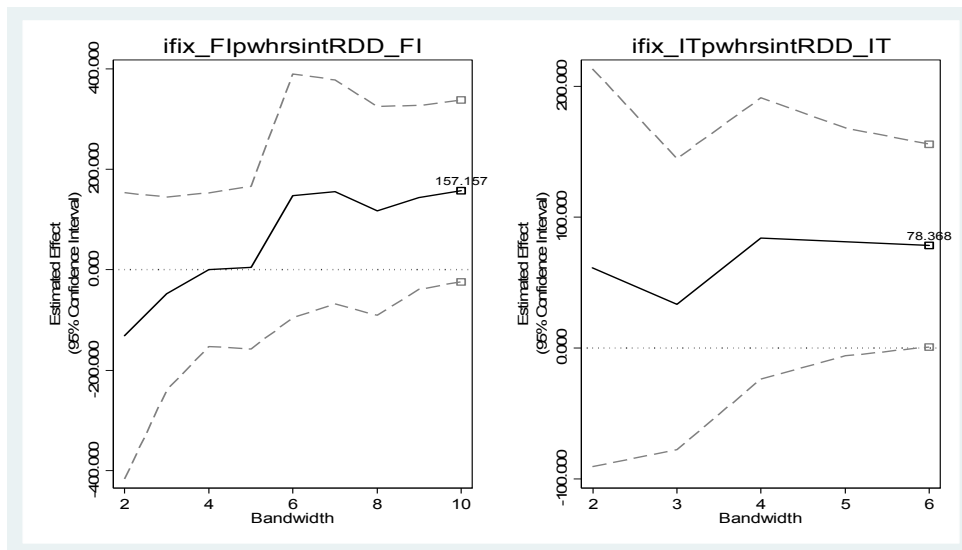
Two graphs above: results for Finland; two graphs below: results for Italy

Figure A3.1a: Heterogeneity; Interaction of Treatment and Temporary Worker Use (for intensive margin only), Threshold effect



Lhs: results for Finland, rhs: results for Italy

Figure A3.1b: Heterogeneity; Interaction of Treatment and Temporary Worker Use (for intensive margin only), Interaction effect



Lhs: results for Finland, rhs: results for Italy

Appendix A4

Variable definitions and construction

Training variables 1. Continuing vocational education and training (CVT): Continuing vocational training measures and activities includes CVT courses and “other” forms of CVT. Courses are <i>financed at least partly by the enterprises for their persons employed</i>. Persons employed holding an apprenticeship or training contract must not be taken into consideration for CVT. The training measures or activities must be planned in advance and must be organised or supported with the special goal of learning. Random learning and initial vocational training (IVT) are explicitly excluded. CVT courses included in the training measure of paper: CVT courses are typically clearly separated from the active work place (learning takes place in locations specially assigned for learning, a class room or training centre). They exhibit a high degree of organisation (time, space and content) by a trainer or a training institution. The content is designed for a group of learners (e.g. a curriculum exists). Two distinct types of CVT courses are identified: internal CVT courses (principally designed and managed by the enterprise itself) and external CVT courses (principally designed and managed by organisations which are not part of the enterprise itself). “Other” forms of CVT courses not included in training measure: Are typically connected to the active work and the active work place (e.g. planned training through: on-the-job-training, job-rotation, exchanges, secondments or study visits, participation in learning or quality circles, self-directed learning, attendance at conferences, workshops, trade fairs and lectures). Variable construction: Variable construction: Extensive margin: If persons employed by the enterprise participate in one or more, either internal or external CVT courses. Binary variable: 1= yes; 0=no. Intensive margin: Total paid working time (in hours) spent on ALL CVT courses broken down by internal or external CVT course; continuous variable. 2. Initial vocational training (IVT): IVT is a work-based training measure or activity for apprentices/trainees. It lead to a formal qualification. The measures are often financed (partly or wholly) by the enterprise although this is not a mandatory condition. Apprentices/trainees often have a special training contract. Variable construction: Total number of IVT participants in the enterprise during 2005. Binary variable: 1= if one or more participant; 0= if none. Assignment variable Firm size (<i>rvc_FI</i>, <i>rvc_IT</i>): Total number of persons employed by firm in 2005 (includes: working proprietors, partners working regularly in the enterprise, unpaid family workers, persons from the enterprise and paid by it who work away from the enterprise (e.g. sales representatives, delivery personnel, repair and maintenance teams, part-time workers and seasonal workers, people absent for a short period (e.g. sick leave, paid leave or special leave, those on strike but not absent for an indefinite period). Variable construction: (Total number of persons employed by firm in 2005)—(firm-size threshold; FI:20; IT:16) = centered assignment variable. Assignment-to-treatment variable (<i>z_FI</i>; <i>z_IT</i>): =1 if treatment (FI: all firms with 20 employees and above; IT: 16 or above) Covariates Industry affiliation (<i>ind 1-ind 20</i>): Industry affiliation of firm according to NACE Rev1.1.; NACE19 classification (19 sectors). Binary variable: <i>ind</i> (industry code)=1 if in industry x; =0 if not. Innovation (<i>Innovation</i>): If firm had SIGNIFICANTLY new technologically improved products or services or methods of producing or delivering products and services during the reference year (2005). Def.: A new or significantly improved good or service is one whose characteristics or intended usage differs significantly from those of previously produced products or has been significantly enhanced or upgraded. A significantly new method of production or delivery of goods and services should represent a significant difference from those of previously utilised. Variable construction: Binary variable; 1=yes technologically improved products during 2005); 0=no.

Labour productivity (Average Wage) Total labour costs of persons employed (excluding persons employed holding an apprentice or training contract) is defined as the sum of the direct and indirect labour costs. The estimate of total labour costs represents all expenditure borne by employers in order to employ workers. It should include: direct labour costs (direct pay, other bonuses and gratuities, payments for days not worked, benefits in kind) and indirect labour costs (statutory social security contributions and family allowances, non-statutory payments, other social expenditure, vocational training costs (gross), taxes, less subsidies on labour). Variable construction: total labour costs divided by number of employees.
Employee age distribution: Number of employed persons: a. under 25 years of age as share of total number of employed persons b. 25 to 54 years of age as share of total number of employed persons c. 55 years and older as share of total number of employed persons
Employment growth (Growth Propensity) Binary variable: =1 if total number of employees in firm 2005 larger than that in 2004; =0 if not.
Part-time workers (Part-Time Emp): If firms had persons with part-time contract persons employed in 2005. Variable construction: =1 if yes;=0 no.
Fix-term workers (Temp Emp): If firms had persons with fix-term (temporary) contract persons employed in 2005. Variable construction: =1 if yes;=0 no.