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The Perverse Effects of Job-Security Provisions on Job Security in Italy: Results from a Regression Discontinuity Design

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Abstract

This paper analyses the impact of employment protection (EP) on the composition of the workforce and worker turnover using a unique firm-level dataset for Italy. The impact of employment protection is analyzed by means of a regression discontinuity design (RDD) that exploits the variation in EP provisions across firms below and above a size threshold. Using our RDD approach, we show, first, that EP increases worker reallocation, suggesting that EP may tend to reduce rather to increase worker security on average. We further show that this can be entirely explained by the impact of EP on the use of workers on temporary contracts. Our preferred estimates suggest that the discontinuity in EP increases the incidence of temporary work by 2 percentage points. Moreover, EP is also found to reduce labour productivity and this can only to a limited extent be attributed to its impact on the incidence of temporary work.

Keywords: employment protection, worker reallocation, temporary contracts, labour market duality

JEL Codes: J42, J63, J65

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1. Introduction

Over the past two decades, the effects of employment protection (EP) legislation on labour market outcomes have attracted a lot of attention with a rapidly growing number of theoretical and empirical studies and often tense policy debates. EP is generally justified by the need to protect workers from unfair behaviour on the part of their employers and the fact that imperfections in financial markets limit their ability to insure themselves against the risk of dismissal (*e.g.* Pissarides, 2010). But by imposing implicit or explicit costs on the firm's ability to accommodate its workforce to the evolution of demand and technological changes, EP may hinder efficient workforce adjustment, reducing job destruction but also discouraging job creation (*e.g.* Mortensen and Pissarides, 1994) with a potential dampening effect on labour reallocation and economic efficiency (for a review of the empirical evidence, see *e.g.* Martin and Scarpetta, 2012).

In the vast majority of countries, employment protection also varies depending on the type of labour contract, with the main divide applying between the panoply of fixed-term, atypical contracts on the one hand, and open-ended, permanent contracts on the other hand. Asymmetric liberalisation of temporary and other atypical contracts while leaving in place stringent regulations for permanent contracts – as observed in many, mainly European countries as well as Korea and Japan, over the past two decades (see *e.g.* Venn, 2009) – has encouraged firms to substitute temporary for regular workers. The long-run effects of this asymmetric liberalisation on overall employment may be small, (see *e.g.* Boeri and Garibaldi, 2007; Bentolila *et al.*, 2008). But a shift from regular to temporary employment may have a number of side effects. In particular, it may distort the optimal composition of employment and reduce workers' involvement in training and their commitment to work. This, in turn, could have a negative impact on dynamic efficiency and contribute to greater dualism in the labour market between those who are able to maintain a regular contract (often the insiders) and those employed on temporary contracts (often youths and other workers with limited skills or work experience) will bear the brunt of employment adjustment (Saint Paul, 1996). This latter phenomenon has been very marked in the recent great recession, when a number of European countries and Japan, characterized by large shares of temporary workers in total employment, have seen job losses largely concentrated among these workers (see OECD, 2010).

Despite this significant attention, the jury is still out on the effects of EP on labour market outcomes and economic efficiency. One of the problems is that much of the evidence is based on cross-country time series data on the impact of EP on employment and unemployment rates (e.g. Bassanini and Duval, 2006; Blanchard and Wolfers, 2000; Baker *et al*. 2005, Fiori *et al.* 2012). Despite significant efforts, this evidence remains plagued by omitted variable and measurement problems. To overcome these problems, a recent literature has exploited within-country variation, over time, across sectors or types of firms. One

strand of this literature has exploited within-country variations in the expected impact of EP across sectors characterised by differences in the propensity to adjust the workforce, using difference-in-difference econometric techniques (see Micco and Pages, 2007; Haltiwanger *et al.* 2006 and 2010; Bassanini *et al*., 2009; Cingano *et al*., 2010). Another strand exploits differences in regulatory treatment across regions, workers or firms. In particular, Autor *et al.* (2007) analyse the impact of the adoption of wrongful-discharge protection norms by state courts in the United States on different indicators of firms' performance. Boeri and Jimeno (2005), Schivardi and Torrini (2008) and Kugler and Pica (2008) exploit specific clauses exonerating small Italian firms from certain job security provisions and find a significant negative impact of employment protection on job turnover and job destruction, while Venn (2010) finds similar results using a similar strategy to analyse the impact of EP on hiring in Turkey. Gal *et al.* (2012) provide an analysis of the role of firm-size exemptions in the context of EP for the adjustment behaviour of firms across a large number of OECD countries. Kugler *et al.* (2010) look at the effects of a reform in Spain in 1997, which lowered dismissal costs for older and younger workers, and find that it was associated with a relative increase in worker flows for these groups.

The present paper follows this latter line of investigation by analyzing the impact of employment protection (EP) on worker security using a unique firm-level dataset for Italy. The impact of employment protection is analyzed by means of a regression discontinuity design (RDD) that exploits the variation in EP provisions between small and large firms. In particular, the Italian legislation on EP imposes significantly higher costs in case of an unfair dismissal of an individual worker with a permanent contract for firms above a threshold of 15 employees compared with those below the threshold.[1] Considerable effort is made to show that the use of a RDD is appropriate in the present context by showing that the firm-size density is continuous around the threshold; that is to say, firms just below the threshold do not display an unusually low propensity to grow, and the available control variables are balanced around the threshold.

Using our RDD approach, we show, first, that EP increases worker reallocation, suggesting that EP may tend to reduce rather to increase worker security. We further show that this can be entirely explained by the impact of EP on the use of workers on temporary contracts. Our preferred estimates suggest that the discontinuity in EP on the incidence of temporary work is economically large, increasing the incidence of temporary work by 2 percentage points. Moreover, EP is found to reduce labour productivity and this can only to a limited extent be attributed to its impact on the incidence of temporary work.

[1] The previous literature on the effect of the discontinuous change in legislation In Italy focused on the propensity to grow below and above the threshold (see e.g. Garibaldi *et al.* 2004; and Schivardi and Torrini, 2008); that is to say on the potentially negative impact on employment growth in firms around the threshold. This literature generally finds a very small reduction in the propensity to grow for firms below the threshold.

The remainder of this paper is structured as follows. Section 2 provides the institutional background by discussing the history of employment protection, the legislation with respect to permanent and temporary contracts that was in place during the period of interest in the study and the recent reforms that took place in 2012. Section 3 discusses the various data sources used in this study, details the exact way the firm-size threshold is measured, as this is crucial for our identification strategy, and provides basic descriptive statistics on our data. Section 4 explains the regression discontinuity design and shows that this is an appropriate identification strategy in the present context. It also discusses how the regression discontinuity design can be complemented with a difference-in-difference strategy. Section 5 present our econometric results and discusses their robustness using a wide variety of different specification and robustness tests. Based on the micro-econometric estimates, Section 6 draws out the aggregate implications of employment protection on the incidence of temporary work and discusses the role of the recent reforms and the possible need for further reforms. Section 7 concludes.

2. Institutional background

This section provides the institutional background by providing a detailed account of the regulation of permanent and temporary contracts up to 2012 as a brief discussion of the reforms that were implemented in 2012.

2.1 The legislation for permanent contracts until 2012

Employment protection, including dismissal procedures, was first regulated in Italy in 1966 (Law No. 604). The law established that employers could dismiss workers with a permanent (open-ended) contract either for economic reasons (considered as fair "objective" motives, *giustificato motivo oggettivo*) or in case of misconduct (considered as fair "subjective" motives, *giusta causa* or *giustificato motivo soggettivo*). A dismissal is considered unfair in all other cases. However, a worker could take employers to court and have a judge determine if the dismissal was fair or unfair. If the dismissal was judged unfair, employers had the choice to either reinstate the worker or pay severance, which depended on tenure and firm size (more or less than 60 employees).[2] In the case of fair dismissals, including those for economic reasons, workers were not entitled to any form of compensation.

[2] Severance pay for unfair dismissal ranged between 5 and 12 months of the last salary, depending on the size of the firm, the tenure of the worker and the behaviour of the parties during the dispute. The maximum severance pay was reduced to 8 months in case of a worker with tenure of less than 30 months and extended to 14 months for a worker with tenure greater than 20 years. Firms with less than 60 employees had to pay half of the severance paid by firms with more than 60 employees.

In 1970, the *Statuto dei Lavoratori* (Law No. 300) introduced significant changes in dismissal procedures. In particular, the Article 18 of the *Statuto* effectively lowered the firm-size threshold that determines the entitlement of workers in the case of unfair dismissal from 60 to 15 employees and established the so-called "*tutela reale*" for firms with more than 15 employees.[3] In the case of unfair dismissal, the employer either has to reinstate the worker and pay for the foregone wages during the period between the dismissal and the sentence or instead may be required to make a severance payment worth 15 monthly salaries and to compensate the worker for the wages lost during the trial period. Importantly, in contrast to the situation before 1970, the choice between reinstatement and severance payments was moved entirely to the employee.[4] Firms with 15 employees or less the changes imposed by *Article 18* did not apply: the choice between reinstatement and severance pay in the case of unfair dismissals remained with employers and mandated severance pay was much lower. As before the change in the law, workers dismissed for fair reasons were not entitled to any form of compensation.

In 1990, the employer protection regime for firms with fewer than 15 employees was reformed through the introduction of the so-called "*tutela obbligatoria*" (Law No. 108), which is similar in spirit to the previous regime for small firms, but increased the cost of unfair dismissals. As before, the employer decides whether a worker is reinstated or a severance payment is provided in the case of an unfair dismissal. Severance pay ranges from a minimum of 2.5 to a maximum of 14 months of the last salary pay for workers with high seniority. Note that in the case of reinstatement, the worker is not eligible to compensation for wages lost during the period between the dismissal and the court's ruling.

As a result of the threshold of 15 employees, the cost of unfair dismissal differs substantially above and below the threshold. In particular, for firms above the threshold the costs of an unfair dismissal are significantly higher than those of a firm below the threshold: i) they are generally forced to reinstate the dismissed workers and compensate them for the foregone wages over the, often lengthy, trial period;[5] ii) they are also called to pay a high penalty for the omitted social contributions to the Social Security Administration (INPS), which is proportional to the trial's duration; and iii) if workers opt for severance pay, this is up to six times higher than in small firms.

A key factor that increases *de facto* firing costs of dismissals for economic reasons for firms above the threshold is the absence of a stringent definition of fair dismissal and the limited flexibility on the part of judges to adjust the sanction to the severity of the fault. Unfair dismissals generally result in reinstatement

[3] Discriminatory dismissals, such as for ethnic, religious or trade-union membership reasons are never allowed; in this case a worker always has the right to be reinstated in the job irrespective of firm's size.

[4] In practice, this meant that workers had to be reinstated in the case of unfair dismissal.

[5] The average time required for the court ruling in Italy is very long, 23 months. Moreover, almost 60% of the labour cases are appealed, one of the highest in the OECD countries. See Venn, 2009, for further details.

whereas in the case of fair dismissals workers do not receive any form of compensation even though workers may not be at fault (e.g. dismissal for economic reasons). The absence of a stringent dismissal of fair dismissal leaves room for various interpretations and in this sense the judge's discretion assumes a relevant position. In practice, the absence of compensation for workers in the case of fair dismissals has induced judged to adopt a broad interpretation of unfair dismissal, considerably increasing the expected cost of dismissals for economic reasons, particularly in the context of depressed labour markets. Ichino *et al.* (2003) show that local labour market conditions influence the court's decisions. Judges in regions with high unemployment rates are more likely to rule in favour of the workers than judges in regions with low unemployment rates, introducing *de facto* a higher firing cost for firms operating in depressed areas. The high expected cost of firing for economic reasons and the uncertainty of trials' outcomes discourage large firms from initiating dismissal procedures (Ichino, 1996).

It should be noted that other relevant regulations apply to firms above a certain size threshold even if these thresholds are defined using different rules than those applying to the Article 18 discussed above. The threshold of 15 employees is also relevant for the establishment of the so-called "Rappresentanze Sindacali Aziendali" (RSA) a firm-level institution that has the right to call for general meetings, establish referendums, and post union-related posters within the establishment. Firms with more than 15 employees also have the right to a worker representative for safety-related issues. And since 1991, collective dismissals procedures are in place above the 15 employee threshold: for the dismissal of 5 or more workers, the legislation requires proof of the credible risk of bankruptcy[6] and implies additional and often lengthy negotiations with the representative unions even if it does not generate further direct firing costs. Finally, firms employing more than 10 workers are obliged to hire disadvantaged workers, which refer to registered long-term unemployed, and since 1999 firms employing more than 15 workers must employ a quota of disabled workers. These additional constraints applying to firms above the 15 employee threshold could potentially add some noise in our estimates. However, the (limited) empirical evidence on the impact of these other constraints on firms' behaviour does not lend support to the idea that they play a large role.[7]

[6] Firms undergoing temporary crisis or in need of restructuring may access supplementation schemes (Cassa Integrazione Guadagni) instead of firing (part of) their workforce; see below for further details.

[7] In particular, Schivardi and Torrini (2008) using a survey of Metalworking Firm Organisation, show that the share of firms with an RSA and the share of firms with a firm-level contract, whilst generally increasing with firm size, do not show any kink at the 15 threshold. All in all, it seems fair to say that the discontinuity on RSA establishment should not affect in a sensible way the interpretation of our results.

2.2 The legislation for temporary contracts until 2012

Art.1 of Law No.230 of 1962 established that the relationship between the employee and the employer should be on a permanent basis. In this way, the legislation assigned an extraordinary nature to temporary contracts. They were only justified in specific and very restrictive cases. However, the presence of high dismissal costs of permanent workers, the need for adequate flexibility for firms to adjust labour inputs according to production needs and the desire to curtail the development of informal working arrangements have motivated a series of reforms with respect to the regulation of temporary and other atypical labour contracts from the mid-1980s onwards.

Following the prolonged economic crisis in the early 1980s, the government started a reform process which eventually resulted in the adoption of Art.23 of Law No. 28 in 1987, which stipulated that employers could hire, in agreement with labour unions, a certain fraction of their workers on a temporary contract. Regarding the firing cost of a temporary worker, the legislation allows the dismissal only on a just cause basis (*giusta causa* or *giustificato motivo soggettivo*). Employers cannot fire a temporary worker for objective motives (*giustificato motivo oggettivo*). In the case of unfair dismissal, the worker has the right to receive a payment equal to the foregone wages between the firing date and the expected expiration of his temporary contract. Differently from permanent workers, the firing cost for unfair dismissal of temporary employees is the same for firms of all sizes.

Furthermore, the Treu reform in 1997 and the Biagi Law in 2003 promoted further flexibility in the Italian labour market, by liberalizing the use of temporary contracts. Both dealt with the regulation of temporary work agencies (TWAs), while the latter law also introduced new contractual forms of temporary nature (*i.e.* staff leasing, job on call, job sharing). Particularly, the Biagi Law replaced the existing consultant agreements (the so-called "*contratti di collaborazione coordinata e contitnuativa*") with project labour agreements (the so-called "*contratti a progetto*"). These are temporary contracts that can be considered as 'semi-dependent' since they are midway between those of dependent employment and self-employment. Although the two reforms introduced many novel elements to the regulation of the Italian labour market, they did not affect the employment protection level of permanent contracts.

In conclusion, the Italian labour market is characterized by a strong discontinuity in the employment protection of permanent contracts around the threshold of 15 employees, with significantly higher dismissal costs and greater uncertainty in the legal procedures for enterprises above this threshold. Conversely, the regulation for hires and separations of temporary contracts, in their various forms (*i.e.* dependent or semi-independent), is uniform for firms with less or more than 15 employees. Moreover, the strong protection for workers on permanent contracts in large firms and the increased scope for employing

workers on temporary contracts during the past 25 years may have provided incentives to employers to substitute permanent for temporary workers.

2.3 The 2012 Labour Reform

A comprehensive labour market reform was introduced in Italy in June 2012. The reform covers rules for hiring, separation as well as apprenticeships and unemployment benefits.

Concerning the regulation of non-standard contracts, the reform introduced a series of norms aimed at combating abuses in the use of certain forms of atypical contracts and reducing the incentives to hiring workers on non-permanent contracts. In particular, the cooling-off period between two fixed-term contracts has been extended; social security contributions for fixed-term contracts were increased and more strict tests introduced to justify the use of independent contractors.

In addition, the 2012 reform introduced changes in the procedures for the dismissal of a worker with an open-ended contract and modifies the sanctions imposed on employers subject to Article 18, *i.e.* those with more than 15 employees, in case of unfair dismissal. More specifically, the reform made two major changes: i) the judge (and not the worker) decides whether reinstatement should be envisaged once a dismissal is ruled unfair; and ii) the judge is now allowed to graduate the sanction depending on the severity of the fault in the dismissal whereas before the reform the judge did not have any discretion once it was assessed that the dismissal was unfair and the employee had opted for reinstatement. Indeed, reinstatement can be ordered by the judge in the case of most severe violation of the law (*e.g.* in case of discriminatory reasons behind the dismissal) as in a number of other OECD countries. The second level of sanction also includes the reinstatement but limits the pay for foregone wages to a maximum of 12 months. The third and fourth levels relate to dismissals for economic reasons. They only allow for severance pay and not reinstatement. Third level sanctions provide for severance compensation ranging from 12 to 24 months of pay depending on the worker' tenure, firm size, and the behaviour of the parties during the trial, while for fourth level sanctions allow for compensation ranging from 6 to 12 months. Under the new regime, thus, the judge has the possibility of graduating the sanction, with the reinstatement envisaged only when the dismissal was manifestly groundless.[8]

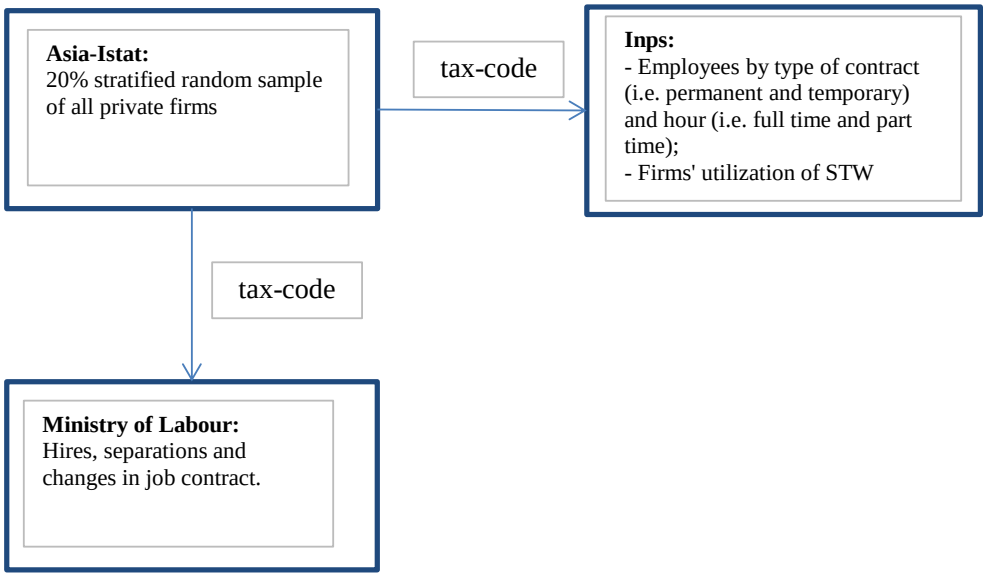
[8] In addition, the reform also requires employers to specify the motivation of dismissal when notifying it to the worker (previously, this was required only after seven days and if requested by a worker in the first 15 days since notification); simplifies the procedures for dispute resolution, through the introduction of a seven days mandatory conciliation process between the parties to assess whether a mutual agreement could be reached; and stipulates that all labour disputes be treated via a fast-track judicial procedure.

All in all, these changes have the potential to reduce significantly the *de facto* dismissal costs for firms above 15 employees, by reducing the uncertainty and time involved in a dismissal procedure and the expected cost in case of unfair dismissals. However, the judges have a higher degree of direction and much will depend on the jurisprudence that will develop over time. In any event, these elements of the reform are likely to imply that the discontinuity at 15 has been significantly reduced.

3. Data description

This section provides a detailed description of the various data sources used in our analysis. More specifically, the core of the dataset used for this paper is based on the collection of three different administrative data sources. The different archives are linked through the use of unique firm tax codes. The resulting dataset is nationally representative of all Italian private firms with at least one employee in 2006. A key feature of the dataset is that it provides information on all hires, separations and contract conversions and allows tracking worker transitions between firms within in our sample. Information about firms' utilization of the Italian short-time working (STW) schemes (the *Cassa Integrazione*) is also available. Figure 1 provides a scheme of the key sources of our dataset.

Figure 1. Data sources



3.1 Data sources

The first dataset consists of the Italian *Statistical Register of Active Enterprises* (ASIA), which is the most reliable source on the universe of the Italian firms. It includes firm-level data obtained by the

integration of administrative sources, coming from public agencies and private companies, and statistical sources managed by the National Institute of Statistics (hereafter, ISTAT).[9] ASIA provides annual information on sales, employment and allows distinguishing between employees and independent-contract workers. See Consalvi *et al*. (2008) for more details.

The firm-level dataset used in this paper represents a 20% stratified random sample of all private firms active in 2006 with at least one employee.[10] These firms are followed during the period 2001-2009. The public sector and agriculture are excluded from the analysis. A stratified sample is used to ensure that the sample is representativeness in terms of firm size, economic activity (2 digits) and region.[11]

The second source comes from the *Italian Social Security Administration* (INPS), from which we obtain quarterly data on the level of employment for permanent and temporary employees as well as full-time and part-time workers. This information is available for the period 2008Q1-2011Q1. Furthermore, it provides information on firms' utilization of *Cassa Integrazione* (in terms of the number of hours subsidized and the number of beneficiaries).

Data on changes in the firm's workforce are collected from the New Informative System of *Compulsory Communications* (CC), managed by the Italian Ministry of Labour. The Ministerial Decree of October 30, 2007 obliges Italian firms to electronically notify all hires and separations, extensions or conversions of job contracts to the Ministry of Labour. Until then the notifications were transmitted on a paper basis. After a transitory phase during which firms could send notifications by paper mail, electronic notification became compulsory from March 2008. From this date, the Informative System records each workforce movement in private and public Italian firms. Moreover, for each worker movement, it provides information on the precise date of the event, the identity of the worker, the identity of the firm and a rich set of worker characteristics: *i.e.* age, gender, nationality, educational level, domicile and for foreigners the reason and the term of residence permission, as well as job characteristics (the type of contract, part-time/full-time, standard weekly hours).[12]

[9] Information in this dataset is drawn from the Italian Social Security Administration, the Italian National Revenue Service (*i.e*. Agenzia delle Entrate) and demographic information comes from the Chamber of Commerce.

[10] The sample size decreases by 15% between 2006 and 2009 due to the role of firm exit . As firm exit is higher among small firms and no new small firms enter our sample, the 2009 sample slightly underrepresents small firms. In principle, this bias could be corrected by reweighting the sample, but this is not done in the present paper.

[11] While the random nature of our sample ensures that the data are representativeness this is not necessarily the case for sub-samples due to sampling variability. Stratification helps to alleviate concerns over representativeness related to sampling variability.

[12] See the Annex for further details.

3.2 Descriptive statistics

The final dataset consists of 122,326 firms with complete information in 2008 and 2009 and at least one permanent employee. Since we focus exclusively on firms with 6 to 25 employees in the econometric part of this paper, it is interesting to explore the implications of this sample restriction for the size of the sample and its composition.

As can be seen in Table 1, 29% of our sample consists of firms with 6 to 25 employees. Micro-firms with less than 6 employees account for 64% of the sample, while firms with more than 25 employees account for just 7%. These data, thus, confirm the relative importance of micro and small firms in the Italian economy. Restricting our focus to firms with 6 to 25 employees results in a large loss of observations due to the exclusion of micro firms, while the resulting loss of observations associated with the exclusion of medium-sized and large firms is modest.

The focus on firms with 6 to 25 employees also has implications for the composition of our sample. In the last two columns of the table, we compare the average values of the main variables in our dataset across two different samples. The first compares small firms defined here as firms with 6-15 employees with larger firms defined as firms with 16-25 employees, while the second compares firms within our estimation sample (*i.e.* firms with 6-25 employees) with all other firms. A t-test for the difference in means is also presented. The descriptive statistics suggest that there are important differences in the characteristics of small and large firms, as well as between firms in our estimation sample and those that are not. Significant differences are also observable in the industry and geographical distribution. However, as expected these differences become less evident in the first case, when we take into account firms just above and below the 15 employee threshold.

Differences in the characteristics of small and large firms may be related due to the differential role of employment protection provisions above and below the 15 employee threshold, but also reflect the independent effect of firmsize or the endogenous of response of firms to employment protection. The main challenge of the econometric analysis is to accurately control for the independent effect of firm size and address the possibility that firms self-select into size groups.

Table 1. Mean values by firm size, 2009

Variable	less 6	6-15	16-25	26 over	Total	t-test groups: (16-25) - (6-15)	t-test groups: (6-25) - (<6 and >25)
employees	2,33	8,66	19,14	129,06	13,18	259,63 ***	-5,64 ***
	(1,18)	(2,75)	(2,86)	(420,32)	(113,52)		
permanent employees	2,22	8,00	17,40	116,34	12,01	224,13 ***	-5,97 ***
	(1,12)	(2,73)	(3,56)	(346,98)	(94,40)		
temporary employees	0,11	0,65	1,74	12,72	1,17	55,82 ***	-1,43
	(0,31)	(1,03)	(2,39)	(206,97)	(53,80)		
independent contractors	0,54	0,57	0,50	0,29	0,53	-4,81 ***	8,02 ***
	(0,81)	(0,90)	(0,94)	(1,00)	(0,86)		
intensity of stw beneficiaries	0,03	0,08	0,12	0,13	0,06	13,6 ***	41,2 ***
	(0,14)	(0,20)	(0,23)	(0,24)	(0,17)		
firms' age	17,93	19,04	20,86	24,56	18,78	10,21 ***	10,31 ***
	(11,06)	(12,05)	(12,93)	(15,37)	(11,86)		
workers' churning rate	0,56	0,54	0,51	0,50	0,55	-2,92 ***	-2,92 ***
	(1,47)	(0,69)	(0,63)	(1,49)	(1,29)		
incidence of temporary employees	0,04	0,08	0,09	0,09	0,05	9,55 ***	55,32 ***
	(0,09)	(0,11)	(0,12)	(0,12)	(0,10)		
incidence of independent contractors	0,15	0,06	0,02	0,01	0,11	-28,31 ***	-75,08 ***
	(0,21)	(0,09)	(0,04)	(0,02)	(0,18)		
temp employees' churning rate (a)	4,04	3,02	2,71	2,89	3,33	-4,71 ***	-14,3 ***
	(5,09)	(3,72)	(3,36)	(7,20)	(4,97)		
perm employees' churning rate	0,13	0,10	0,08	0,07	0,12	-6,67 ***	-11,33 ***
	(0,44)	(0,24)	(0,17)	(0,20)	(0,38)		
ind contractors' churning rate (a)	0,02	0,08	0,24	0,43	0,06	15,51 ***	-2,62 ***
	(0,25)	(0,59)	(1,36)	(2,42)	(0,59)		
permanent hiring rate	0,12	0,09	0,06	0,06	0,11	-9,1 ***	-18,76 ***
	(0,33)	(0,18)	(0,13)	(0,14)	(0,28)		
temporary hiring rate (a)	3,03	2,32	2,03	1,97	2,48	-6,87 ***	-14,06 ***
	(3,09)	(2,39)	(2,24)	(4,01)	(3,00)		
permanent separation rate	0,21	0,15	0,12	0,09	0,19	-9,55 ***	-20,9 ***
	(0,48)	(0,27)	(0,20)	(0,18)	(0,42)		
temporary separation rate (a)	2,72	2,08	1,88	1,91	2,26	-4,62 ***	-13,9 ***
	(3,11)	(2,40)	(2,31)	(3,90)	(2,98)		
log labour productivity	11,58	11,66	11,80	11,89	11,63	10,57 ***	12,34 ***
	(0,93)	(0,89)	(0,93)	(1,18)	(0,94)		
Industry							
Construction	0,18	0,18	0,14	0,08	0,17	-6,9 ***	2,83 ***
Electricity, gas and	0,00	0,00	0,00	0,01	0,00	1,9 *	-0,08
Financial intermediat	0,02	0,01	0,01	0,03	0,02	-0,67	-13,81 ***
Hotels and restaurant	0,07	0,05	0,03	0,02	0,06	-5,83 ***	-10,75 ***
Manufacturing	0,21	0,36	0,47	0,51	0,28	15,1 ***	48,41 ***
Mining and quarrying	0,00	0,01	0,01	0,01	0,00	0,44	9,81 ***
Real estate, renting	0,17	0,11	0,09	0,13	0,15	-2,57 **	-28,67 ***
Transport, storage an	0,04	0,05	0,06	0,07	0,05	1,62	8,87 ***
Wholesale and retail	0,30	0,23	0,18	0,14	0,27	-7,29 ***	-23,57 ***
Geographic Area							
Centre	0,20	0,20	0,19	0,18	0,20	-2,39 ***	0,64
North-East	0,22	0,25	0,28	0,29	0,24	4,72 ***	10,64 ***
North-West	0,30	0,32	0,33	0,36	0,31	0,09	5,27 ***
South	0,27	0,22	0,20	0,16	0,25	-2,78 ***	-16,75 ***
employment share	11,37	16,04	6,63	65,96	100,00		
firm share	64,30	24,40	4,56	6,73	100,00		
Observations	**78.654**	**29.850**	**5.584**	**8.238**	**122.326**		

* 0.1; ** 0.05; *** 0.01

(a) computed on a reduced number of observations

Note: Balanced Panel 2008-2009

Source: Ministry of Labour, INPS, ISTAT

3.3 Measuring the threshold

Since the present paper uses the discontinuity of employment protection by firm size to identify its impact, the accurate measurement of the employment threshold is crucial. In the Labour Code, the threshold measure is defined in terms of full-time equivalent dependent employees. This means that all temporary and permanent employees need to be included in the computation of employment, while independent contractors and apprentices should be ignored. It also implies that all permanent and temporary employees should be counted by taking into account their usual working hours. The second column of Table 2 summarises how employment should be computed across different contract types and working time according to the law, while the last three columns summarise the way employment is measured in the present paper as well as in three previous studies (Leonardi *et al.*2010; Garibaldi *et al.*, 2004; Schivardi *et al.*, 2008).

In order to calculate the number of employees for the EP threshold, we combine the ISTAT and INPS archives. The ISTAT data are used to measure the average number of employees within each firm, while the INPS data are used to obtain the shares of permanent and temporary employees and those of full-time and part-time workers. Since in the case of part-time workers, details about the number of usual hours worked are not available, we assume that they work half time (50%). We do not have any information in our data to determine whether employees are apprentices or not. However, considering the relatively low incidence of apprenticeships in Italy, the resulting bias in our computation of the threshold is likely to be negligible. As shown in Table 2, the availability of detailed information on the composition of the workforce in our employer-employee dataset allows for a more precise definition of firm size than what was possible in previous studies.

Table 2. Job contracts relevant for the 15 employee threshold

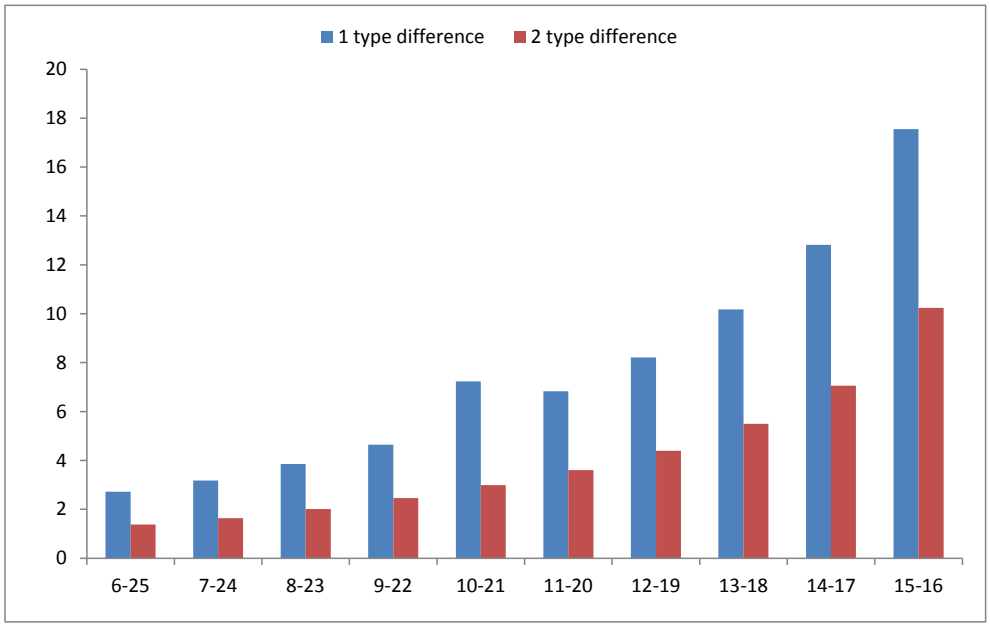
Type of contract	Law	Leonardi et al (2010)	Garibaldi et al (2004) - Schivardi et al (2008)	Hijzen et al (2013)
Permanent full time	Yes	Yes	Yes	Yes
Temporary full time	Yes	No	Yes	Yes
Permanent part time	%	No	As full time	Part time at 50%
Temporary part time	%	No	As full time	Part time at 50%
Apprentices	No	No	Yes	Yes
Independent contractors	No	No	No	No

To shed some light on the importance of these measurement issues for the classification of firms above and below threshold, Figure 2 compares measure of employment used in the present paper, the most refined measure of the threshold in the literature, with the most basic measure in the literature based on the

number of full-time permanent employees. From the left to the right, the figure, respectively, gives the share of firms that are classified as above the threshold according to the basic measure and below the threshold in the present paper ("type I difference") and vice versa ("type 2 difference") for gradually declining bandwidth choices. The overall degree of inconsistency is given by the sum of the two bars. The figure suggests that problems of misclassification are relatively limited when using a large bandwidth (*e.g.* it is just 4% when using the largest bandwidth), while it increases rapidly the smaller the bandwidth (*e.g.* it is about 30% in the smallest bandwidth). The simulation provides a strong argument for using relatively large bandwidths, particularly in cases where doubts over the accurate measurement of the threshold exist.

Figure 2. Classification differences arising from different threshold definitions

by bandwidth size, 2009



Type I difference: share of firms classified above the threshold when the threshold is defined in terms of the number of full-time permanent employees and below the threshold when the definition of the threshold takes account of temporary workers and working time; **Type II difference:** share of firms that is classified as below the threshold when the threshold is defined in terms of the number of full-time permanent employees and above the threshold when the definition of the threshold takes account of temporary workers and working time.

4. Econometric methodology

This section describes the regression discontinuity design used in this paper to assess the causal effects of employment protection on labour market outcomes. We use three formal tests to demonstrate that the regression discontinuity approach is appropriate in the present context. The section puts forward a

more elaborate identification strategy that embeds the regression discontinuity approach in a difference-in-differences framework.

4.1 The regression discontinuity design (RDD)

The econometric analysis in this paper exploits the fact that employment protection provisions in case of individual dismissal of a regular worker vary significantly according to firm size in Italy and thus provide a natural application for a regression discontinuity design (RDD). The main idea of RDD is that individuals (firms in this case) just below the threshold provide a good counterfactual for those just above the threshold (the "treated"). The main advantage of RDD in comparison with other non-experimental approaches is that it relies on relatively weak assumptions (Hahn, Todd and Van der Klaauw, 2001; Lee and Lemieux, 2010) and, consequently, may provide more credible results. Moreover, the assumptions are testable in a similar manner as in randomised experiments.[13]

In order to estimate the causal impact of employment protection, we will present both graphical and regression-based results. The graphical analysis consists of plotting the local averages of the outcome of interest within narrow firm's size intervals ('bins'). Bins in the present context are defined as intervals of 0.1 employees.[14] Covariates can be taken into account by first regressing the outcome variable of interest on the covariates and plotting the local averages of the residual. The regression analysis involves estimating the following general model using ordinary least squares (OLS):

$$Y_i = \sum_{n=0}^{N} \alpha_{0n}(T - F_i)^n + D_i \sum_{n=0}^{N} (\alpha_{1n} - \alpha_{0n})(F_i - T)^n + \sum_{x=1}^{X} \beta_{0x} X_i + \sum_{x=1}^{X} \beta_{1x} D_i X_i + \varepsilon_i \quad (1)$$

$$D_i = 1[F_i > T]$$

$$T - h \leq F_i \leq T + h$$

where *Y* refers to the outcome variable of interest in firm *i*; *F* refers to level of dependent employment and *T* the employment threshold set in the EP legislation (*i.e.* 15); *D* a treatment dummy that equals 1 if dependent employment is larger than the threshold and zero otherwise; *X* represents a vector of predetermined control variables, expressed in terms of the deviation from their sample means among large firms, to reduce the sampling variability of our RDD estimator. The alpha's represent the key parameters to

[13] In particular, the conditional independence assumption is trivially satisfied in an RD design, whereas it is generally considered to be a strong assumption in other non-experimental contexts (Lee and Lemieux, 2010).

[14] It is appropriate to define bins of less than employee in the present case because employment is measured in full-time equivalents and thus represents a continuous variable.

be estimated with the first subscript indicating whether it refers to untreated (0) or treated (1) observations and the second the value of n. ε_i represents a white noise error term.

Equation (1) encompasses a wide variety of different specifications. If N=0, equation (1) reduces to a non-parametric comparison of the means around the threshold: $Y_i = \alpha_{0n} + (\alpha_{1n} - \alpha_{0n})D_i + \varepsilon_i$; if N=1, it reduces to a local linear specification and if N>1 it represents a parametric specification with a polynomial of order N (N_{max}=3). Restricting the slopes to be the same of each side of the threshold is tantamount to equating α_{1n} to α_{0n} for $n \geq 1$ although this is not done in the present paper. *h* refers to the window around the threshold (or bandwidth) and may take the value of 10, 8 or 6.[15]

Equation (1) yields unbiased estimates as long as the behavioural assumption that firms do not "precisely" manipulate the assignment variable around the threshold is valid. Using the definition of "not precise" given in Lee and Lemieux (2010), this is the case when the density of the assignment variable is continuous conditional on all other observable and unobservable characteristics of firms that affect the outcome variable of interest. Importantly, this assumption yields the prediction that treatment is locally randomised. Whether or not this assumption is valid can be empirically verified using a variety of different tests. This is done in the next sub-section.

4.2 Assessing the validity of the RDD in the present context

In order to assess the validity of the RDD approach in the present context, we conducted three different tests. We first performed the standard test in the RDD literature of the assumption that the firm-size density is continuous around the EP threshold, as proposed by McCrary (2008). Since this test has low power if selection takes place on each side of the threshold, we also assess whether firms just below and above the threshold differ in the propensity to grow, as was done in Schivardi and Torrini (2008). Third, we conduct a series of balancing tests to assess to what extent firms just above and below the threshold differ in terms of their observable characteristics.

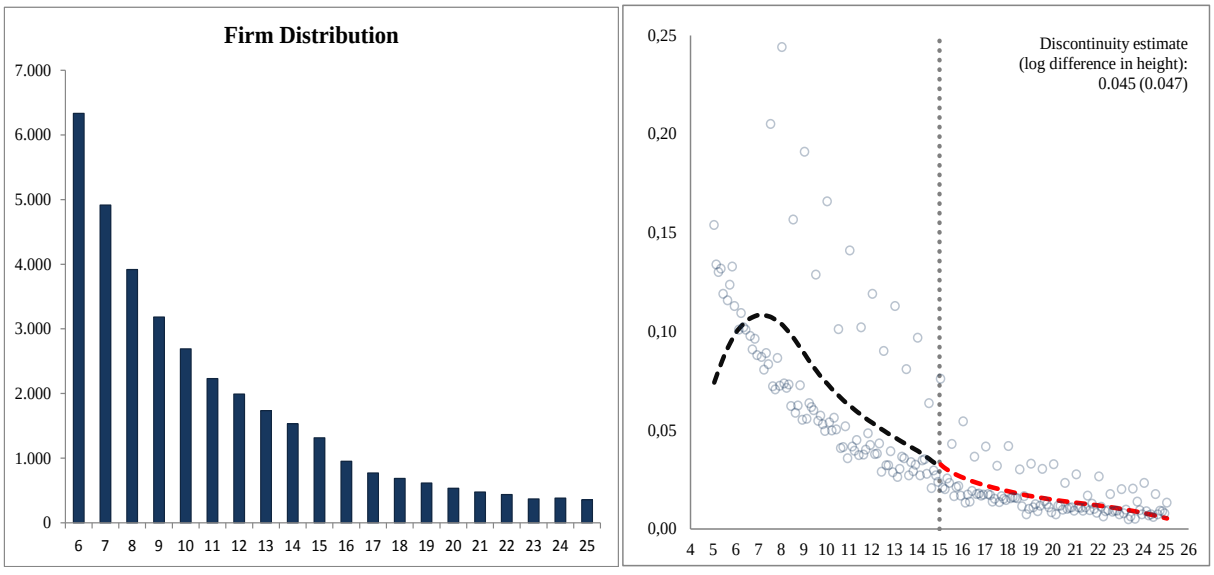
The key behavioural assumption of our RDD is that firms do not manipulate the assignment variable, in our case the number of employees in the firm. This requires the distribution of the assignment variable to be continuous for each firm. Since we only observe a single observation of the assignment variable for each firm at a given point in time, we cannot test this assumption directly. However, we can test whether it holds on average by testing whether the aggregate distribution of the assignment variable is continuous.

[15] This paper does not make use of local linear regression since this method is likely to be very sensitive to the precise definition of the threshold and the spikes in the data at integer values.

McCrary (2008) proposes a two-step procedure to test whether the aggregate distribution of the assignment variable is continuous.The first step involves the discretization of the assignment variable in a certain number of bins of the same width and computing the corresponding frequencies. This allows constructing a histogram of the assignment variable which gives a useful first indication of importance of manipulation. The second step consists of running local linear regressions of the computed frequencies on each side of the threshold. The regressions are weighted, with most weight being given to bins nearer to the threshold. The discontinuity is evaluated on the basis of the implied log difference in frequencies at the threshold (T) from the two regressions. Given the bin size, the optimal bandwidth, which defines the observations included in the regressions, is determined in order to obtain the best possible approximation of the density function. We use a bin size of 0.1 as in the non-parametric analysis.[16] The results are reported in Figure 3. The dots indicate the computed frequencies at the midpoint of each bin, while the tick bold lines correspond to the predictions of the weighted local linear regressions at each side of the threshold. Neither visual inspection, nor the estimated coefficients suggest a significant discontinuity at the threshold of 15 employees. The log difference is 0.045 with a standard error 0.047.[17]

Figure 3. Firm distribution and McCrary test of the continuity of the employment density around threshold

Binsize=0.1 and optimal bandwidth as in McCrary (2008)



16 McCrary recommends using a optimal bin size equal to $\hat{b} = 2\hat{\sigma}n^{-1/2}$, where $\hat{\sigma}$ is the assignment variable standard deviation and n is the sample size.

17 In the context of the differences-in-differences framework discussed in Section 4.3, we repeat the MacCrary test industry-in-industry.

Since the McCrary test is based on the aggregate and not on the individual distribution of the assignment variable, it has low power when selection is not monotonic but occurs in both directions. It is not straightforward why small firms would want to sort above the threshold in response to employment protection rules and, therefore, we do not expect this to be an important issue in the present context.[18] However, in order to show that this is indeed true we conduct two further tests.

Following Schivardi and Torrini (2008), we assess the impact of employment protection provisions on the propensity to grow. This is done by means of a probit model that specifies the probability of growing $P(F_{it} > F_{it-1})$ as a function of a fourth-order polynomial of its initial employment level, F^{j}_{it-1}, and a set of bin dummies with binsize one for firms with employment levels below the threshold, D^{K}, and a set of controls, X.

$$P(F_{it} > F_{it-1}) = \alpha + \sum_{j=1}^{4} \beta_j F^{j}_{it-1} + \sum_{k=1}^{K} \gamma_k (1 - D_{it-1}) D^{K}_{it-1} \quad + \beta_x X_{it} + \epsilon_{it} \quad (2)$$

$$D_{it-1} = 1[F_{it-1} > T]$$

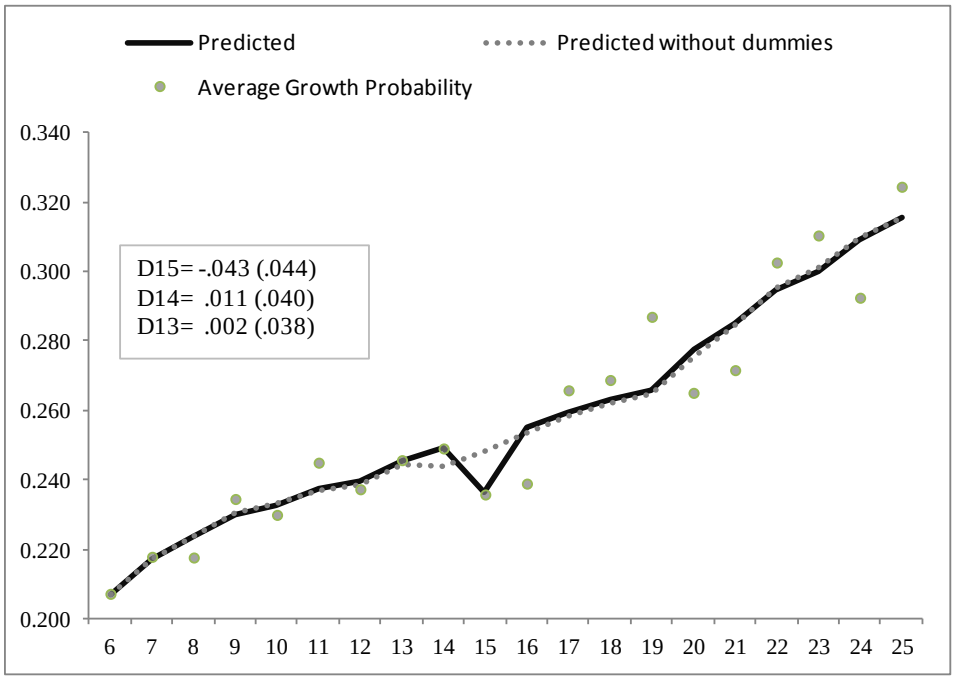
$$D^{K}_{it-1} = 1[K - 0.5 \leq F_{it-1} < K + 0.5] \text{ for } K=5,..,25$$

The fourth-order polynomial in initial employment is assumed to capture the relationship between employment size and the probability to grow if employment protection provisions for large firms were to be extended to small firms. The coefficients on the bin dummies for firms with initial employment levels below the threshold, γ_k, may be interpreted as the threshold effect of employment protection on the probability to grow. The results are reported in Figure 4. Garibaldi and Pacelli (2004), Schivardi and Torrini (2008) and Leonardi and Pica (2013) find that the probability to grow is increasing with respect to the firmsize and slightly reduces the propensity to grow just below the threshold at firmsize 15 (by about 2 percentage points).We also find a lower probability of growth at 15 employees. However, in our case, the difference in the probability is not statistically different from zero. [19]

[18] One possible hypothesis could be that firms self-select above the threshold because they want to signal to workers that open-ended contracts are well protected. This may induce workers to make more important investments in firm-specific human capital.

[19] However, this seems to be due to the different definition of firmsize. Once we define firms in terms of the headcount of dependent employees instead of the full-time equivalent number of dependent employees we find firms at 15 have a two percentage points lower propensity to grow, although the estimate is only weakly significant at 10%.

Figure 4. Probability of growth and predicted probabilities
years 2008-2009



As a final test to assess the validity of the RDD in the present context, we assess whether the baseline covariates are locally balanced on either side of the threshold. This condition should be met if, as assumed in the RDD, the assignment variable can be considered as good as random around the threshold. Indeed, in the context of a valid RDD including any baseline covariates in the regressions should not affect the econometric estimates apart from the standard errors. We consider the following covariates: age of firms, region, industry and the intensity of STW beneficiaries, computed as a percentage of all employees in 2009. We check whether the two groups are balanced by replacing the dependent variable in equation (1) by each of the covariates, whilst using N = 2 and h = 10. The results, reported in the Table 3 below, show that there are no significant discontinuities at the 15 employee threshold.[20]

[20] We use probit for industry and territorial area dummies.

Table 3. Balancing test for covariates

VARIABLES	2 ord specification
intensity of stw beneficiaries	0,00162
	(0.009)
firm's age	-0,570
	(0.515)
Construction	0,0471
	(0.062)
Manufacturing	-0,042
	(0.052)
Real estate, renting and business activities	0,026
	(0.073)
Transport, storage and communication	0,021
	(0.085)
Wholesale, retail trade,etc	-0,006
	(0.060)
Hotels and restaurants	0,101
	(0.101)
Electricity, gas and water supply	-0,011
	(0.286)
Mining and quarrying	-0,281
	(0.212)
Financial intermediation	-0,162
	(0.166)
North-East	0,013
	(0,056)
Nort-West	-0,3083
	(0,053)
Centre	0,029
	(0,059)
South	0,066
	(0,058)

4.3 A difference-in-difference regression discontinuity design

An important feature of RDD is that, as long as the treatment can be considered randomized around the threshold, controlling for any observed or unobserved characteristics does not affect the estimated size of the discontinuity at the threshold. Controlling for any observed or unobserved characteristics in this context may nevertheless be helpful for two reasons. First, to the extent that there are other regulations that make use of a firmsize threshold around 15 could lead to bias in our results. While other existing regulations do not use the same threshold definition as that used in the law for employment protection, there are regulations that make use of similar firm-size thresholds. A difference-in-difference framework can remove the role of such confounding factors by exploiting differences in the *de jure* or *de facto* stringency of employment protection that are unrelated to these other regulations (Grembi *et al.*, 2012). Second, it may help to reduce remaining concerns about the role of manipulation of the assignment

variable for the estimation of treatment effects as long as manipulation depends on being in one employment protection regime or another and does not depend on the *de jure* or *de facto* difference in the stringency of employment protection. However, as was shown in the previous sub-section, manipulation does not appear to be a major concern in the present context. Third, using a DiD set-up can also help to increase the precision of our RDD estimates. This is most relevant when pre-treatment controls and post-treatment outcome variables are highly correlated, for example, due to the role of unobserved fixed effects.

The usual way to implement a difference-in-differences set-up is to focus on reforms that generate changes in the threshold effect before and after reforms. As our data do not allow focusing on reforms in EP, we propose to complement our RDD with a difference-in-differences estimator that exploits the differential role of employment protection provisions across industries characterized by different levels of employment volatility. More specifically, the difference-in-differences approach is motivated by previous work by Rajan and Zingales (1998) which uses the within-country variation to analyze the relationship between financial dependence and growth using a large cross-country dataset. Subsequent studies by Haltiwanger *et al.* (2006, 2010), Bassanini et al. (2009) and Cingano *et al.* (2010) have used similar approaches to analyze the role of employment protection on job and worker flows by exploiting the stylized fact that sectors differ in the needs for adjusting their workforces due to factors unrelated to employment protection such as underlying market and technological characteristics. The present analysis makes use of a similar idea by exploiting cross-sectoral differences in the volatility of employment to proxy for the market- and technologically-driven need to adjust the workforce.[21] Differences in market volatility across sectors may lead to important differences in the impact of employment protection since market volatility provides incentives for firms to adjust employment levels. Indeed, firms that operate in markets characterized by highly volatile output demand are likely to have a greater need to adjust employment levels and consequently are likely to be more strongly impacted by strict and costly EP provisions.

A major challenge is to come up with a measure of market volatility that differs across sectors but is not contaminated by the presence of employment protection. We proceed as follows. We start by measuring *employment* volatility for *each* firm as the standard deviation of log employment over the period 2001-2008. We then conducted a balancing test similar to the ones conducted above to assess whether firms just above and below the threshold differ in their levels of employment volatility using equation (1). The results indicate that employment volatility is slightly lower for firms just above the threshold. In principle, this could reflect the possibility that employment protection negatively influences the volatility

[21] Using administrative data on workers in Italy for the period 1986-1995, Kugler and Pica (2008) use a similar approach to analyse whether the impact of employment protection is stronger in more volatile sectors.

of employment. However, the difference is not statistically significant. In order to make sure that our measure of employment volatility is not affected by the presence of employment-protection provisions, we focus on the *intrinsic* employment volatility by netting out the potential effect of employment protection on employment volatility for firms with employment levels above 15. Finally, we average our measure of intrinsic employment volatility across firms in each two-digit industry to obtain a measure of the intrinsic volatility of the sector in which firms operate.

In order to combine our RDD with a difference-in-differences approach, we extend equation (1) as follows:

$$Y_i = \sum_{n=0}^{N} \alpha_{0n}(T - F_i)^n + \sum_{n=0}^{N} \beta_{0n}(F_i - T)^n V_j + D_i \sum_{n=0}^{N}(\alpha_{1n} - \alpha_{0n})(T - F_i)^n + D_i \sum_{n=0}^{N}(\beta_{1n} - \beta 0n)T - FinVj + \varepsilon i \quad (3)$$

$$D_i = 1[F_i > T]$$

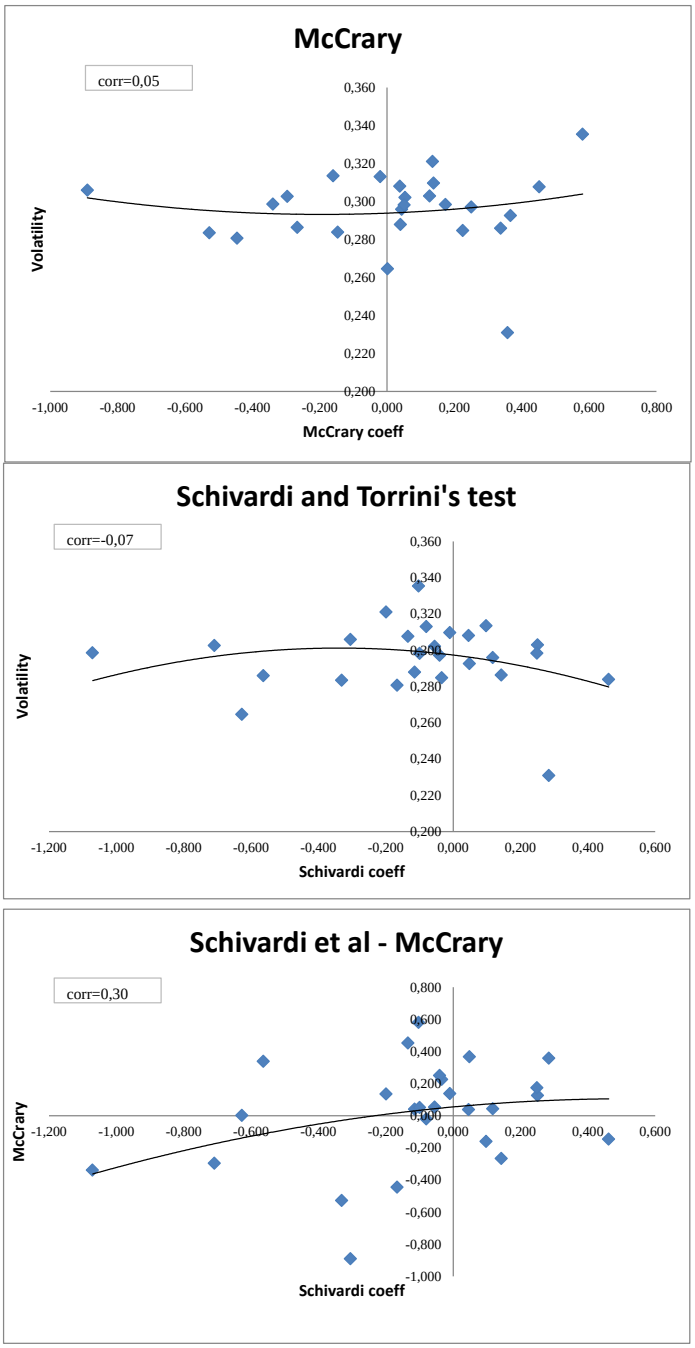
$$T - h \leq F_i \leq T + h$$

where V_j refers to our measure of intrinsic sector volatility.[22] The coefficient β_{00} gives the average change in the outcome variable of interest of small firms that is associated with a 1 percent change in intrinsic sector volatility. The difference $\beta_{10} - \beta_{00}$ gives the difference-in-differences effect of employment protection, that is, it gives the differential response to a one percentage point change in intrinsic market volatility across small and large firms.

A key assumption of our difference-in-differences strategy is that the variation in the impact of employment protection across sectors, and, hence, intrinsic sector volatility is independent of the variation due to self-selection into size groups across sectors. In order to examine the validity of this assumption, we implement the various tests discussed above by two-digit industry. The results are reported in Figure 6. While the results from the McCrary and Schivardi-Torrini tests are positively related across industries (Panel C) neither is significantly related to the degree of intrinsic market volatility (Panels A and B). The pairwise correlation is less than 0.1 in both cases and statistically insignificant. This suggests that complementing the RDD with the difference-in-differences approach described above may indeed be appropriate.

[22] The coefficient α_{00}, represents the constant which either captures the average level of the outcome variable of interest for firms below the threshold when N=0 or the level of the outcome variable evaluated at F=0 when N>0. Similarly, coefficient α_{10} gives the average level of the variable of interest for firms above the threshold for N=0 or its level evaluated at F= when N>0. The difference $\alpha_{10} - \alpha_{00}$ gives the disconuity at the threshold.

Figure 5. McCrary test and Schivardi-Torrini test by industry



5. Econometric results

In the first part of this section, we report the RDD results, using a wide variety of different specifications with varying bandwidths and using alternatively linear, quadratic or third polynomials to control for the independent effects of firm size. In the second part, we conduct a series of robustness checks in order to test the sensitivity of our results.

5.1 Baseline results

Employment protection and worker reallocation

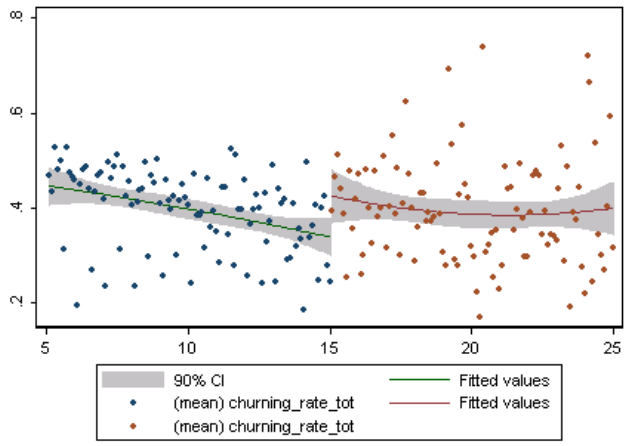
A number of previous studies have suggested that employment protection can have important adverse implications for worker reallocation. For example, Schivardi and Torrini (2008) find that employment protection does not reduce worker reallocation as it intends to do, but instead increases worker reallocation. Schivardi and Torrini (2008) argue that this perverse effect of employment protection (employment protection is supposed to increase job security) is likely to reflect the impact of employment protection on the incentives of firms to employ workers on temporary contracts. However, they or any other studies do not provide direct evidence that this is indeed the case.

In this sub-section, we provide a systematic evaluation of the impact of employment protection on excessive worker reallocation using the RDD approach set out in Section 4. Excessive worker reallocation is defined as twice the minimum of hires (*H*) and separations (*S*) over the average of firm employment:

$$XR = \frac{2\min(H,S)}{E}.$$

The difference between total worker reallocation and excessive worker reallocation represents the net employment change. As it has been demonstrated in the previous section that employment protection does not affect employment growth, we focus directly on excessive worker turnover here. Figure 6 summarizes our RDD results of the impact of employment protection on excessive worker turnover. Consistent with Schivardi and Torrini (2008) the figure shows that excessive worker turnover is substantially higher just above the threshold than in small firms just below the threshold, despite the presence of more stringent employment protection provisions in large firms. The parametric results, reported in Table 4, further show that these results are robust to using a wide variety of different specifications with varying bandwidths and using alternatively linear, quadratic or third polynomials to control for the independent effect of firm size.

Figure 6. The impact of employment protection on excessive worker reallocation



In addition to analyzing the impact of employment protection on worker turnover, we also provide direct evidence on each of its constituents using a shift-share decomposition of the difference in excessive worker turnover between small and large firms. This allows writing the differences in excessive worker turnover in terms of a between effect and a within effect as follows:

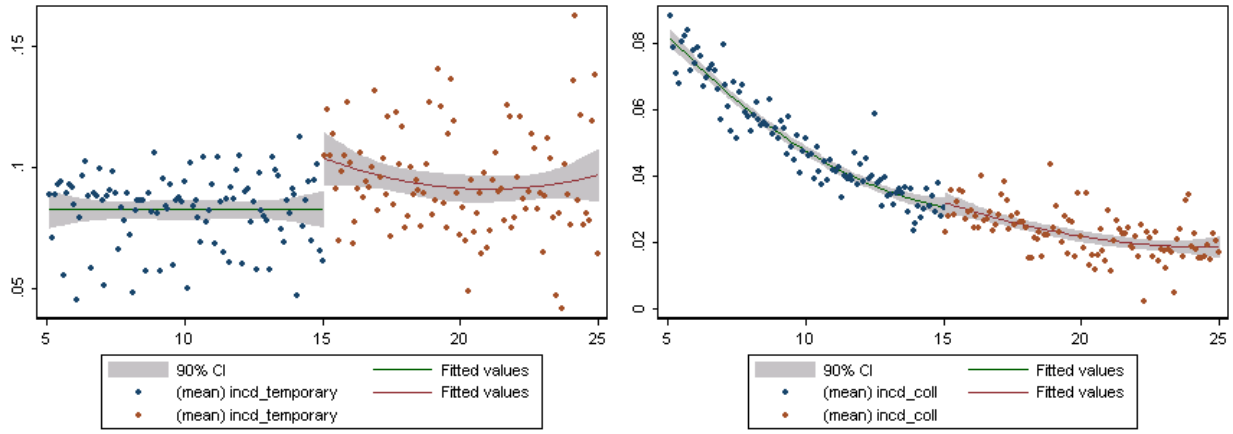
$$\Delta\widehat{XR} = \sum_{i=1}^{N}\sum_{c=1}^{C} \Delta\hat{s}_{ic}\,\overline{XR}_{ic} + \sum_{i=1}^{N}\sum_{c=1}^{C} \bar{s}_{ic}\,\Delta\widehat{XR}_{ic}$$

where *XR* represents worker reallocation, *s* the share in dependent employment of workers with contract type *c*, Δ hat to the estimated difference between large and small firms in the variable of interest that can be attributed to employment protection and bars the average values of large and small firms. The first term on the right-hand side gives the between component of excessive worker reallocation or the composition effect of employment protection. This term captures differences in excessive worker reallocation that can be attributed to differences in the composition of contracts between small and large firms. The second term captures the within component of excessive worker reallocation or the technology effect of employment protection. This represents the differential impact of employment protection on excessive worker turnover by type of contract weighted by the average employment shares of each contract type.

The RDD results for the impact of employment protection on each component of excessive worker turnover are reported in the first panel of Table 4. The RDD results indicate that the impact of employment protection on excessive worker reallocation largely reflects the impact of employment protection on the use of workers on temporary contracts (see also Figure 7, left Panel). This confirms the conjecture put forward

by Schivardi and Torrini (2008) that firms seek to circumvent the impact of employment protection by substituting workers on permanent contracts for workers on temporary contacts. This result is robust to a number of different specifications: i) whether or not the incidence of temporary workers is measured in terms of dependent employment or permanent employment; ii) whether a linear, quadratic or third-order specification is used to control for firmsize; iii) for varying definitions of bandwidth. Our preferred estimates, based on bandwidth 6-25 and the use of a second -order polynomial in firm size, suggest that the discontinuity in employment protection increases the incidence of temporary work by 2 percentage points. There is no evidence that employment protection also increases the use of independent contractors (either as a share of the total workforce or relative to the number of workers on permanent contracts) (Figure 7, right panel).

Figure 7. The impact of employment protection on the incidence of temporary employees and independent contractors



Employment protection does not appear to have any robust effects on excessive worker turnover by type of contract. The results are either statistically insignificant or inconsistent across specifications. While this may not be surprising in the case of temporary and independent contractors, one could advance several arguments of why employment protection might affect the churning rate among permanent workers. Firms may be less inclined to layoff underperforming workers, while workers eligible to severance pay may be less likely to quit. The reason this is not observed in practice in the case of firms may be that unfair dismissals are relatively rare even in small firms. EP may not affect the quit rate of permanent workers since severance pay in Italy is not formally dependent on tenure (although judges tend to take this into account when establishing severance pay levels in their ruling).

Employment protection and labour productivity

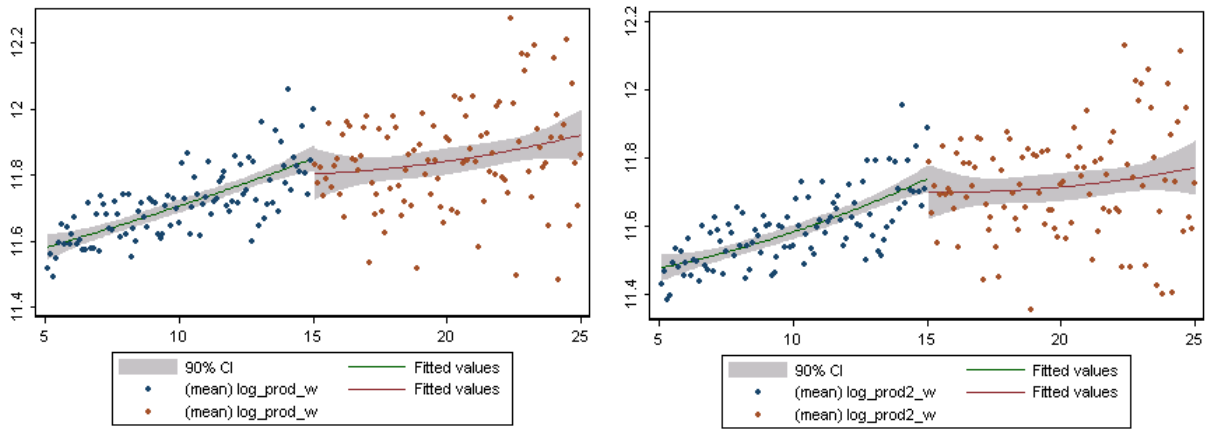
In Section 4 it was already shown that employment protection does not affect employment growth. In the previous section, we have provided evidence that firms tend to circumvent employment protection by substituting permanent for temporary workers.[23] However, this does not necessarily mean that employment protection is costless to firms. While the use of temporary workers may allow firms to effectively circumvent the role of EP, temporary workers may not be as productive as permanent workers, even after controlling for differences in labour costs, because it may reduce incentives to invest in the human capital. Moreover, the greater use of temporary workers may help to circumvent the adverse impact of employment protection on external flexibility, it may not entirely remove it. As a result, employment protection may still hinder the flexibility of firms to respond to shocks and by raising the costs of restructuring or experimenting with new technologies and processes (within firm effect).[24]

We now use our RDD framework to analyse the impact of EP on labour productivity. Moreover, we report both unconditional results and results that condition out the effect of EP on labour productivity through its impact on the incidence of temporary work. The latter is done by including the incidence of temporary employment as an additional covariate. The results, reported in Panel C of Table 4 and Figure 8, show that employment protection has a significantly negative effect on labour productivity and that only a minor part of this can be attributed to its impact on temporary workers. Our estimates indicate that employment protection reduces labour productivity by 5 to 10%. Controlling for the incidence of temporary work tends to reduce the estimated impact of EP on labour productivity. However, comparing the estimated coefficients in the unconditional and conditional regressions suggests that the impact of EP on labour productivity that comes about through its impact on the incidence of temporary work is relatively modest.

[23] The finding not reported that EPL does not affect the volatility of labour productivity (*i.e.* labour hoarding) is also consistent with this

[24] In addition to these partial equilibrium effects, employment protection may also have implications for aggregate productivity by slowing the reallocation of resources from less to more productive firms. However, this latter channel is not captured by the RDD approach used in this paper.

Figure 8. The impact of employment protection on labour productivity



In addition to the various test conducted in Section 4 to assess the appropriateness of RDD in the present context and the numerous alternative specifications of our baseline estimates in Section 5.1, we will now present a number of additional robustness tests.

Table 4. Parametric estimates for different bandwidth choices

Variables	6-25			8-23			12-19		
	1 order	2 order	3 order	1 order	2 order	3 order	1 order	2 order	3 order
Panel A									
workers' churning rate	0.0734***	0.0763***	0.108***	0.0879***	0.0733***	0.107***	0.101***	0.0764**	0.162***
	(0.0153)	(0.0218)	(0.0283)	(0.0167)	(0.0239)	(0.0310)	(0.0225)	(0.0329)	(0.0431)
incidence of temporary employees	0.0179***	0.0204***	0.0268***	0.0196***	0.0211***	0.0297***	0.0245***	0.0238***	0.0400***
	(0.00325)	(0.00477)	(0.00640)	(0.00357)	(0.00532)	(0.00712)	(0.00498)	(0.00758)	(0.0102)
incidence of consultants	0.00726***	0.000238	-0.000225	0.00361**	0.000785	-0.000255	0.00178	0.000860	-0.00390
	(0.00145)	(0.00211)	(0.00276)	(0.00158)	(0.00232)	(0.00305)	(0.00215)	(0.00322)	(0.00419)
temporary employees' churning rate	0.343***	0.284*	0.193	0.401***	0.222	-0.0325	0.280*	-0.0732	0.244
	(0.112)	(0.157)	(0.205)	(0.122)	(0.174)	(0.226)	(0.162)	(0.238)	(0.311)
permanent employees' churning rate	0.00779	0.00217	0.0104	0.0112**	-0.00239	0.0151	0.00762	0.00273	0.0224
	(0.00489)	(0.00702)	(0.00917)	(0.00529)	(0.00775)	(0.0101)	(0.00722)	(0.0105)	(0.0138)
consultants' churning rate	0.0768	0.0256	0.0307	0.0513	0.0381	0.0199	0.0568	-0.0214	0.00473
	(0.0611)	(0.0803)	(0.100)	(0.0686)	(0.0867)	(0.113)	(0.0811)	(0.122)	(0.132)
Panel B									
permanent hiring rate	0.00835**	-0.000890	0.00571	0.00876**	-0.00325	0.00734	0.00155	0.00347	0.0186*
	(0.00372)	(0.00525)	(0.00694)	(0.00401)	(0.00583)	(0.00767)	(0.00548)	(0.00796)	(0.0104)
temporary hiring rate	0.156**	0.0436	-0.0818	0.158*	-0.00991	-0.243*	0.00542	-0.258*	-0.140
	(0.0747)	(0.103)	(0.129)	(0.0817)	(0.112)	(0.142)	(0.107)	(0.149)	(0.193)
permanent separation rate	0.0169***	0.00697	0.0239**	0.0173***	0.00870	0.0208*	0.0192**	0.00728	0.0205
	(0.00584)	(0.00828)	(0.0107)	(0.00645)	(0.00911)	(0.0119)	(0.00854)	(0.0123)	(0.0159)
temporary separation rate	0.122*	0.0148	0.0741	0.123	0.0232	-0.0486	0.0641	-0.116	0.131
	(0.0735)	(0.102)	(0.135)	(0.0790)	(0.114)	(0.149)	(0.108)	(0.156)	(0.208)
temp-perm conversion rate (a)	-0.0112	-0.0180	-0.0410	-0.0114	-0.0270	-0.0520	-0.0490	-0.0384	-0.0310
	(0.0216)	(0.0303)	(0.0380)	(0.0238)	(0.0329)	(0.0413)	(0.0313)	(0.0437)	(0.0612)
incidence of temporary contracts converted in permanent ones	-0.0245**	-0.0151	-0.0227	-0.0216*	-0.0193	-0.0147	-0.0258	-0.00402	0.0129
	(0.0115)	(0.0168)	(0.0231)	(0.0127)	(0.0191)	(0.0243)	(0.0174)	(0.0248)	(0.0287)
Panel C									
log of labor productivity (1)	-0.0655***	-0.0611*	-0.0829*	-0.0753***	-0.0551	-0.104**	-0.0724**	-0.101*	-0.141**
	(0.0239)	(0.0353)	(0.0462)	(0.0265)	(0.0389)	(0.0512)	(0.0366)	(0.0539)	(0.0709)
log of labor productivity (2)	-0.0434*	-0.0524	-0.104**	-0.0545*	-0.0619	-0.122**	-0.0800**	-0.109*	-0.142*
	(0.0256)	(0.0380)	(0.0500)	(0.0284)	(0.0421)	(0.0553)	(0.0392)	(0.0584)	(0.0772)
log of labor productivity (3)	-0.0561**	-0.0505	-0.0690	-0.0642**	-0.0433	-0.0877*	-0.0576	-0.0862	-0.116
	(0.0239)	(0.0352)	(0.0460)	(0.0264)	(0.0388)	(0.0509)	(0.0365)	(0.0535)	(0.0704)
log of labor productivity (4)	-0.0379	-0.0461	-0.0956*	-0.0481*	-0.0549	-0.113**	-0.0701*	-0.0997*	-0.126
	(0.0255)	(0.0380)	(0.0499)	(0.0283)	(0.0420)	(0.0552)	(0.0392)	(0.0581)	(0.0769)

level of significance: *** 0.01; ** 0.05; * 0.1

(a) computed dividing the number of temporary contracts converted in permanent ones over the number of temporary employees

(1) and (3) computed on the average of sales and workers in the period [t-3;t]; (3) controlling for the incidence of temporary employees

(2) and (4) computed on the average of sales and workers in the period [t-1;t]; (4) controlling for the incidence of temporary employees

5.2. Further robustness analysis

In order to check the sensitivity of our results, we conduct a number of further robustness tests. The results are reported in Table 5. In the first column our baseline estimates using a second-order polynomial in firmsize are reported as a benchmark. The results from the robustness checks confirm our baseline estimates and are generally very close quantitatively.

- In the second column of Table 5, we check whether our results hold once we control for firm age, region, industry and the use of short-term working time scheme (Cassa Integrazione Guadagni). The results are quantitatively very similar to those without controls. This is not surprising as it was already shown in Table 3 that there are no significant differences between firms around the threshold along these specific dimensions. In Section 6, we analyse whether the average treatment effect on the treated differs along those dimensions.

- In the third column, we verify whether the results depend on the definition of treatment/assignment variable as a discrete variable instead of as a continuous variable. This is done by assigning each firm for which the yearly average number of employees is $t+\delta$ with $0<\delta\leq 1$ to the $(t+1)$ firm size class. We use in this case clustered standard errors on the distinct values of the firm size as suggested by Lee and Lemieux (2008). As evident, the treatment effect is confirmed for all variables in sign and magnitude, except for the logarithm of productivity and the churning rate of temporary employees. However, the baseline results in these cases are weak (statistically significant only at 10%).

- The final column of the table presents the results that are obtained when the RDD approach is complemented with a difference-in-difference estimator. Also in this case, the threshold effects on worker reallocation and the incidence of temporary employees are confirmed. However, the results with respect to labour productivity discussed in the previous sub-section do not appear to be robust to the use of difference-in-differences in combination with RDD. This implies that the impact of EP on labour productivity does not depend on the intrinsic volatility of the industry.

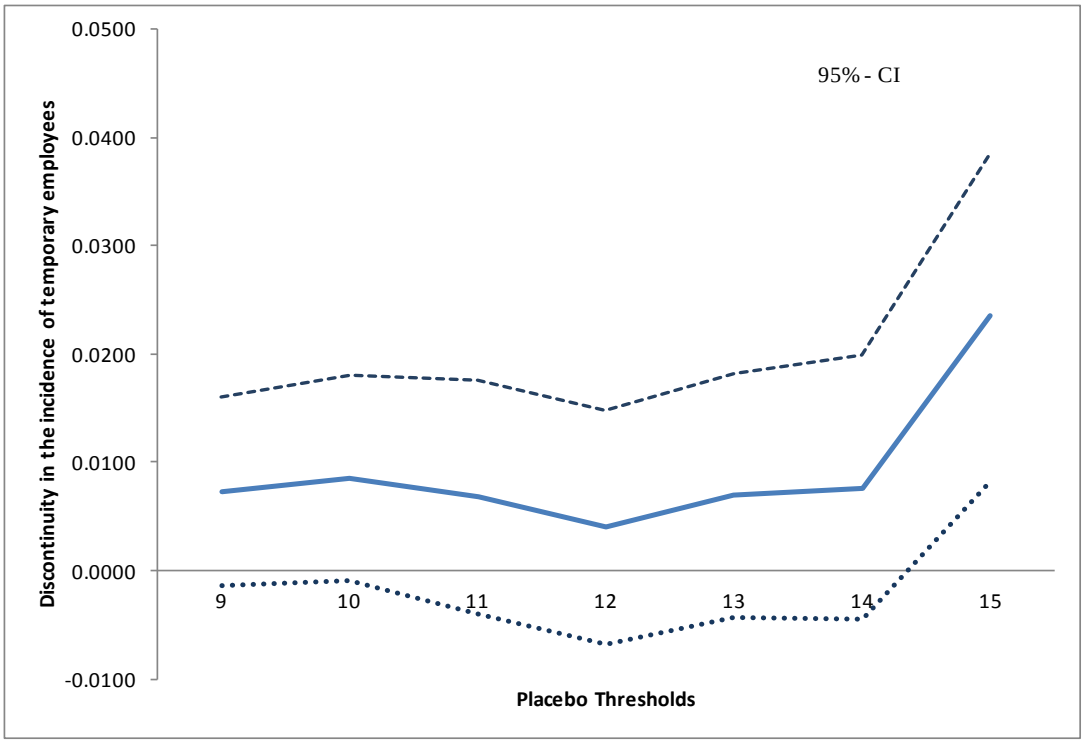
Table 5. Robustness checks
Second-order specification

	RDD	RDD with covariates	RDD discrete case	Diff in Diff approach
Panel A				
workers' churning rate	0.0763***	0.0586***	0.0720***	15.59**
	(0.0218)	(0.0201)	(0.0254)	(6.733)
incidence of temporary employees	0.0204***	0.0179***	0.0215***	3.418**
	(0.00477)	(0.00452)	(0.00549)	(1.482)
incidence of consultants	0.000238	0.00145	0.000166	-0.301
	(0.00211)	(0.00207)	(0.00227)	(0.641)
temporary employees' churning rate	0.284*	0.253*	0.189	-113.4**
	(0.157)	(0.153)	(0.174)	(49.55)
permanent employees' churning rate	0.00217	-0.00242	-0.00286	3.822*
	(0.00702)	(0.00675)	(0.00783)	(1.982)
consultants' churning rate	0.0256	0.0236	0.0458	0.868
	(0.0803)	(0.0780)	(0.102)	(90.73)
Panel B				
permanent hiring rate	-0.000890	-0.00436	-0.00330	2.541*
	(0.00525)	(0.00506)	(0.00591)	(1.474)
temporary hiring rate	0.0436	0.0256	-0.0274	-76.08**
	(0.103)	(0.0102)	(0.112)	(33.16)
permanent separation rate	0.00697	0.00234	0.00634	1.934
	(0.00828)	(0.00804)	(0.00918)	(2.468)
temporary separation rate	0.0148	0.00558	-0.0740	-42.76
	(0.102)	(0.101)	(0.113)	(31.50)
Panel C				
log of labor productivity (1)	-0.0611*	-0.0454	-0.0279	12.27
	(0.0353)	(0.0312)	(0.0406)	(13.29)
log of labor productivity (2)	-0.0524	-0.0417	-0.0178	9.768
	(0.0380)	(0.0339)	(0.0433)	(13.53)

In addition, we implement placebo tests, by estimating average treatment effects on the treated using fake values of the threshold (where there should not be any effect). In particular, regarding the incidence of temporary employees, we look at all t-thresholds, for $9 \leq t \leq 14$. In other words, we focus on firms not affected by the employment quota (Lalive *et al.*, 2009). By using the baseline model, we consider the 95% confidence interval and we do not find a significant discontinuity in any of these points (Figure 9).

Figure 9. Placebo tests

Estimating the treatment effect on the incidence temporary work at fake thresholds



Discontinuities calculated by quadratic model. Bandwidth [T-4; T+4]

6. The aggregate implications of employment protection

The estimates that have been discussed so far relate to the average effect of the discontinuity in employment-protection provisions on firms above the threshold but with less than 25 employees. The estimates do not necessarily say anything about the impact of employment protection on the incidence of temporary work in firms with more than 25 employees. The main reason for this is that the sample of firms used in the analysis differs importantly in its characteristics from the population as a whole (see Table 1). This is an important limitation of the analysis so far since understanding the aggregate effects of employment protection on the economy-wide incidence of temporary work is key from a policy perspective.

In this section, we address this point by departing from the assumption that treatment effects are homogenous, through the adoption of a more flexible specification that allows treatment effects to vary by firm age, region and industry. Using these heterogeneous treatment effects in combination with the characteristics of the population of firms allows us to construct a re-weighted average treatment effect on the treated (ATT) in a way that is more representative for the economy as a whole. In practice, this is done by expressing the vector of X variables in terms of the deviation of the population mean (or more precisely,

the sample mean across firms with at least one employee) instead of the sample mean based on firms with 6-25 employees.

The results are reported in Table 6.[25] It shows that demeaning controls by the population does not affect our results and also that our estimates of the ATT remain unchanged when allowing for interactions between the control variables and the treatment effect (as long as the controls are demeaned using the means of the sample of firms with 6 to 25 employees). However, the ATT slightly increases, from 0.018 to 0.020, when demeaning the vector of controls using population means. The effect of EP increases in this case, as the effect of EP on the incidence of temporary work is stronger in the south of Italy and the south has a greater weight in the overall population of firms than the sample of 6-25 firms due to the large incidence of micro firms. Allowing for the ATT to vary according to age or industry does not appear to be important.

We conclude that our RDD estimates do not just relate to the selective sample of firms with 6 to 25 employees but are also informative for the economy as whole. If anything, the global effects of employment protection on the incidence of temporary work appear to be larger than the local effects although the difference in the estimated effects is small. Taking our heterogeneous ATT at face value, this implies that the incidence of temporary work in firms with 15 or more employees would be 7% instead of the actual value of 9%.[26]

Table 6. Assessing the aggregate effects of employment protection on the incidence of temporary work

based on second-order polynomial of firm size

	Homogeneous ATT	Heterogeneous ATT
without control varibales	0.020***	
	0.005	
with control variables in deviation from 6-25 sample means	0.018***	0.018***
	0.005	0.005
with control variables in deviation from 1+ sample means	0.018***	0.021***
	0.005	0.006

Control variables include age, age squared, the intensity of STW beneficiaries, four region dummies and nine industry dummies.

Source: Author's calculations

[25] As was already shown in Table 5, including covariates slightly reduces our estimate of the impact of employment protection on the incidence of temporary work.

[26] Note that these percentages refer to the firms with at least one permanent employee - the estimation sample.

7. Conclusions

In this paper, we exploit a novel matched employer-employee dataset to investigate the impact of employment protection on firms' workforce behaviour in Italy. We adopt a regression discontinuity design that exploits the variation in employment protection provisions in Italy between firms below 15 employees and those above 15 employees. Before a recent reform (June, 2012) the Italian legislation imposed significantly higher dismissal costs, and greater uncertainly, in case of unfair individual dismissal for firms above the threshold compared with those below. The previous literature on the effect of the discontinuous change in legislation In Italy focused on the *propensity to grow* below and above the threshold and on the *asymmetric expansion and contraction in workforce* just below the threshold. In other words, the focus was on the potentially negative impact on employment growth in firms around the threshold. This literature generally found only a very modest, albeit statistically significant, lower propensity to grow for firms below the threshold.

Our newly established employer-employee dataset allows better identifying the size of firms by considering the different typologies of labour contracts and thus assessing the potential effect of the EP threshold. Using these data, we show the firm-size density is actually continuous around the threshold; that is to say, firms just below the threshold do not display an unusually low propensity to grow, and the available control variables are balanced around the threshold, which in turns justify the use of regression discontinuity design in the empirical analysis.

While not affecting the propensity to grow, employment protection significantly affects the composition of employment and, as a result, tends to dampen worker security in firms above the threshold. Indeed the empirical evidence clearly suggests that firms that decide to go beyond the threshold tend to resort more to temporary employment contracts to circumvent the stricter regulations on permanent contracts. In this way, they exploit the market opportunities and economies of scale offered by the larger size without incurring in extra adjustment costs in case of downsizing. Our preferred estimates suggest that the discontinuity of EP increases the incidence of temporary work by 2 percentage points (more than 22%) in firms above the threshold. We also find that our RDD estimates do not just relate to the selective sample of firms around the threshold (6 to 25 employees) but are also informative for the economy as whole. Our estimates suggest that if anything the overall effects of employment protection on the incidence of temporary work appear is even larger than the local effects.

In the paper we also assess whether the discontinuity of employment composition around the threshold results in a discontinuity in the productivity performance of firms. The evidence clearly indicates

that the greater use of temporary employment in firms above the threshold, to overcome the otherwise much higher workforce adjustments costs, has a negative impact of firm productivity; the effect is again sizeable, employment protection lowers labour productivity by 5-10%.

All in all, our results suggest that employment protection has a quantitatively sizeable impact on the incidence of temporary work and in turns, this tends to reduce rather than to increase job security and hinder labour productivity. While these results deserve further scrutiny, they clearly point to the potential costs of EP not just for the firms concerned but also the workers in these firms and for the overall labour market segmentation in Italy. In this context, the recent labour reforms, if fully implemented, by reducing the stringency and uncertainty of employment protection provisions for firms above the threshold, could contribute to better economic performance and tackle at least in part the large dualism in the Italian labour market.

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Annex

Data on changes in the firm's workforce are collected from the New Informative System of *Compulsory Communications* (CC), managed by the Italian Ministry of Labour. The Ministerial Decree of October 30, 2007 obliges Italian firms to electronically notify all hires and separations, extensions or conversions of job contracts to the Ministry of Labour. Until then the notifications were transmitted on a paper basis. After a transitory phase during which firms could send notifications by paper mail, electronic notification became compulsory from March 2008. From this date, the Informative System records each workforce movement in private and public Italian firms. Moreover, for each worker movement, it provides information on the precise date of the event, the identity of the worker, the identity of the firm and a rich set of worker characteristics: *i.e.* age, gender, nationality, educational level, domicile and for foreigners the reason and the term of residence permission, as well as job characteristics (the type of contract, part-time/full-time, standard weekly hours).

Note that from March 2008 employers are obliged to notify all separations of *permanent* employees, but only those of *temporary* employees that are terminated before the expected expiration date. This implies that the present dataset does not capture separations of temporary employees that were hired before March 2008 and ended at their expected expiration date. Separations of temporary employees who were hired after March 2008 and whose contract ended at the expected expiration date are recorded properly (see Figure 2). Hence, the CC have a tendency to underestimate the overall number of separations of workers on temporary contracts. Since the bias is related to temporary contracts that entered into force before March 2008, it should become gradually less important over time and have completely disappeared by March 2011, since the maximum duration of temporary contracts is three years.

Figure A1. Availability of information in CC related to separations of workers on temporary contracts.