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The impact of employment protection on temporary employment: Evidence 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 in Italy across firms below and above a size threshold. We present three main findings. First, EP increases worker turnover, defined as the sum of hires and separations, thereby reducing rather than increasing worker security on average. Second, this can be entirely explained by the fact that firms facing more stringent EP make a greater use of workers on temporary contracts. Our preferred estimates suggest that the discontinuity in EP increases the incidence of temporary work by 2–2.5 percentage points around the threshold. Moreover, the effect of employment protection persists well beyond the threshold and may account for about 12% of the overall incidence of temporary work. Third, EP tends to reduce labour productivity. This is partly due to the impact of EP on worker turnover and the incidence of temporary work.

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, the fact that imperfections in financial markets limit workers' ability to insure themselves against the risk of dismissal and by the need to preserve firm-specific human capital by preventing the destruction of jobs that are viable in the longer-term (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, by not only reducing job destruction but also discouraging job creation with a potential adverse effect on economic efficiency (for a review of the empirical evidence, see Skedinger (2011), Martin and Scarpetta (2012)).

Despite 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, 2009; Blanchard and Wolfers, 2000; Baker et al. 2005, Fiori et al., 2012). While such studies have played an important role in the policy debate on employment protection, this evidence remains plagued by omitted variable and measurement problems. To overcome these problems, a recent literature has exploited within-country variation across firms/sectors and over time using difference-in-difference techniques. For example, some studies have focused on differences in the expected impact of EP across sectors based on their propensity to adjust the workforce to market and technological changes (Micco and Pages, 2006; Haltiwanger et al., 2014; Bassanini et al., 2009; Cingano et al., 2010). Others have exploited differences in regulatory treatment across regions (Autor et al., 2007 for the US), workers of different age (Kugler et al., 2003, for Spain) or firms of different size (Boeri and Jimeno, 2005; Kugler and Pica, 2008; Schivardi and Torrini, 2008, for Italy; Centeno and Novo, 2012, for Portugal; and Gal et al., 2013, for 10 OECD countries).

The present paper exploits differences in EP provisions between small and large firms to analyse the impact of EP on worker turnover, the incidence of temporary work and labour productivity in Italy. The Italian legislation on EP – until the reforms of 2012 and 2015 - imposed significantly higher costs in the case of an unfair dismissal of an individual worker with a permanent contract to firms above a

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threshold of 15 employees compared with those below this threshold. Using a unique nationally-representative firm-level dataset for the period 2008–2009,[1] 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. We demonstrate that the use of a RDD is appropriate in the present context by showing that the firm-size density is continuous around the threshold, firms just below the threshold do not display an unusually low propensity to grow, and the available control variables are balanced around the threshold.[2]

Blanchard and Landier (2002) provide a stylized model of the labour market with fixed-term and open-ended contracts. In their model, fixed-term and open-ended contract differ in their termination costs in the form of a layoff tax. All workers initially start with a temporary contract after which they may be converted and obtain an open-ended contract or they are laid off, become unemployed and search for a new temporary contract. They show that partial reforms that increase the gap in protection between workers on fixed-term and open-ended contracts, unambiguously increase worker turnover and can also lower welfare in specific cases. Cahuc and Postel-Vinay (2002) analyse the co-existence of fixed-term and open-ended contracts in the context of a search and matching model. In their model, laying off workers on open-ended contracts is subject to a layoff tax, while those on fixed-term contracts are not. However, the use of fixed-term contracts is restricted by law to specific circumstances, which is captured by means of an exogenous probability that an employer can recruit a worker on a temporary contract. More stringent dismissal rules for workers on open-ended contracts create incentives for employers to use a sequence of fixed-term contracts instead of converting them into open-ended ones, thereby increasing worker turnover as well as the incidence of temporary employment.

Empirical evidence on the role of EP on the incidence of temporary work is scarce. Schivardi and Torrini (2008) show that in Italy worker turnover is higher among large firms just above the threshold than in small firms, but do not analyse to what extent this is due to substituting permanent for temporary workers to circumvent EP. Kahn (2010) uses country-level variation in EP across nine European countries and finds that reforms that made it easier to create temporary jobs raise the incidence of temporary work with no impact on overall employment. A recent study for Portugal by Centeno and Novo (2012) exploits the reduction in the firm-size threshold for the regulation of open-ended contracts and find an increase in the share of fixed-term contracts in firms affected by the reform.

To anticipate our empirical findings, we show that EP tends to increase worker turnover, defined as the sum of hires and separations, suggesting that EP may reduce rather than increase worker security. We also show that this can be entirely explained by the impact of EP on the use of workers on temporary contracts. The discontinuity in EP on the incidence of temporary work is economically large, increasing the incidence of temporary work by 2–2.5 percentage points for the firms around the threshold. Moreover, the effect of employment protection is likely to persist well beyond the threshold and may account for about 12% of the overall incidence of temporary work. There is also evidence that EP reduces labour productivity and that this effect is to a significant extent related to its impact on the incidence of temporary work and worker turnover.

The paper is organised as follows. Section 2 provides an overview of the EP in Italy with respect to permanent and temporary contracts. Section 3 discusses the various data sources used in this study, details the way the firm-size threshold is measured, as this is crucial for our identification strategy, and provides basic descriptive statistics of our data. Section 4 presents the regression discontinuity design and discusses the different tests used to assess its validity for our identification strategy. Section 5 presents our main econometric results, while Section 6 discusses their robustness. Based on the micro-econometric estimates, Section 7 sheds some light on the aggregate implications of EP on the incidence of temporary work. Section 8 concludes.

2. Employment protection legislation in Italy

In Italy, prior to the 2012 labour market reform,[3] employers could dismiss workers with a permanent (open-ended) contract either for economic reasons (fair "objective" motives or *giustificato motivo oggettivo*) or in case of misconduct (fair "subjective" motives or *giustificato motivo soggettivo*). A dismissed worker could take his or her employer to court and have a judge determine if the dismissal was indeed fair or unfair. The consequences of unfair dismissal were regulated differently for small and large firms. Workers dismissed for fair reasons were not entitled to any form of compensation.

For firms with more than 15 employees, the *Statuto dei Lavoratori* of 1970 (Law No. 300) established the so-called "*tutela reale*" via its Article 18. In the case of unfair dismissal, the employer either had to reinstate the worker and pay for the foregone wages during the period between the dismissal and the sentence or instead might 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, the choice between reinstatement and severance payments resided with the employee.[4] Moreover, for dismissals of 5 or more workers, collective dismissals procedures applied, involving the proof of the credible risk of bankruptcy and additional, and often lengthy, negotiations with the representative unions.

The employment-protection regime for firms with 15 employees or less was established through the introduction of the so-called "*tutela obbligatoria*" (Law No. 108), which placed the choice between reinstatement and severance pay in the case of unfair dismissal with the employer and mandated severance pay was much lower, ranging 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 was not eligible to compensation for wages lost during the period between the dismissal and the court's ruling.

For firms above the threshold the *de jure* costs of an unfair dismissal were significantly higher than those of a firm below the threshold[5]: (i) they were generally forced to reinstate the dismissed workers and compensate them for foregone wages during the often lengthy trial period[6]; (ii) if the worker opted for severance pay, this was up to six times higher than in small firms.[7] Moreover, the difference in *de facto* firing cost was likely to be even larger due to the tendency of judges to

[1] Consequently, the present data do not allow analysing the role of specific reforms that took place either before or after this period.

[2] Moreover, previous studies for Italy have generally focused on the impact of EP firm-size exemptions on the distribution of firms around the threshold and their growth and provide little evidence to the idea that firms concentrate just below the threshold have lower propensity to expand (Garibaldi et al., 2004; Schivardi and Torrini, 2008).

[3] Further details on the regulation of permanent and temporary contracts, including the recent reforms can be found in Fornero (2014).

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

[5] It should be noted that other relevant regulations apply to firms above a certain size threshold, even if these thresholds are defined using somewhat different rules than those applying to the Article 18. These additional constraints applying to firms above the 15 employee threshold could potentially add some noise to our estimates. However, the 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. See Hijzen et al. (2013) for further details. Nevertheless, in an effort to control for the role of any rules or regulations other than employment protection that make use of a similar firm-size threshold the RDD is complemented with a difference-in-differences approach.

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

[7] In addition, large firms are also called to pay a penalty for the omitted social contributions to the Social Security Administration (INPS) during the trial, which is proportional to its duration.

adopt a broad interpretation of unfair dismissal due to: (i) the limited flexibility on the part of judges to adjust the sanction to the severity of the fault; (ii) the absence of any form of compensation in the case of fair dismissals, and (iii) the lack of a stringent definition of fair dismissal.[8]

Following the prolonged economic crisis in the early 1980s, the Italian government started a reform process which eventually resulted in a reform in 1987 that 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 changed 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 before the reform of 2012 was characterized by a strong disparity in the degree of EP for 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.[9]

3. Data description

The data used in this paper are drawn from three different administrative data sources linked through the use of unique firm tax identification 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 in our sample.

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 and is managed by the National Institute of Statistics (ISTAT).[10] ASIA provides annual information on sales, employment, labour productivity (defined by sales per employee) and allows distinguishing between employees and independent-contract workers (for more details, see Consalvi et al. (2008)). 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. These firms are followed during the period 2001–2009.[11] The public sector and agriculture are excluded from the analysis.

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.[12]

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 (2012). Since March 2008 Italian firms are obliged to report electronically 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. 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).[13]

Our final dataset consists of 122,326 firms with complete information in 2008 and 2009 and at least one permanent employee.

3.2. Measuring the threshold

Since our analysis uses the discontinuity of EP by firm size to identify its impact, the accurate measurement of firm size is crucial. In the Labour Code, firm size is defined in terms of the average number of full-time equivalent dependent employees over the year. In particular, temporary employees with at least nine month contract and permanent employees need to be included in the computation of employment, while independent contractors and apprentices should be excluded. Moreover, all permanent and temporary employees should be counted by taking into account their usual working hours.

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 over the year, 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 information in our data to determine whether employees are apprentices or not. However, considering the relatively low incidence of apprenticeships in Italy, the resulting effect in the computation of the threshold is negligible.[14]

[8] Indeed, Ichino et al. (2003) showed that local labour market conditions influenced court's decisions. Judges in regions with high unemployment rates were 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 economically-depressed areas.

[9] The reform of 2012 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 addition, it modified the procedures for the dismissal of a worker with an open-ended contract and the sanctions imposed on employers subject to Article 18, i.e. those with more than 15 employees, in case of unfair dismissal. See Hijzen et al. (2013) for further details.

[10] It includes firm-level data obtained by the integration of administrative sources, coming from the Italian Social Security Administration, the Italian National Revenue Service (*i.e. Agenzia delle Entrate*) and the Chamber of Commerce.

[11] A stratified sample is used to ensure its representativeness in terms of firm size, economic activity (2 digits) and region. This allows, amongst others, taking account of non-random attrition.

[12] Furthermore, it provides information on firms' utilization of the Italian short-time working scheme, the *Cassa Integrazione Guadagni* (in terms of the number of hours subsidized and the number of beneficiaries).

[13] Unfortunately, we do not have this type of information for the *stock* of workers but only for those who change their job status (hires, separations and contract conversions). This precludes conducting the present analysis at the worker level or analysing the role of worker characteristics.

[14] The availability of detailed information on the composition of the workforce in our employer-employee dataset allows for an arguably more precise definition of firm size than was possible in previous studies.

Given the importance of the measurement of employment, preference is given in the econometric analysis to specifications that make use of relatively wide bandwidths rather than distance-weighted local linear regression techniques. Simple simulations in Hijzen et al. (2013) to assess the role of alternative firm size measures used in the literature for the classification of firms around the threshold suggests that problems of misclassification can increase quickly the smaller the bandwidth.[15] Another reason for making use of relatively wide bandwidths is the nature of our firm-size variable which is continuous with spikes at integer and half-integer values due to the prevalence of full and part-time workers that have been continuously employed during the year (see Fig. 1, Panel B).[16]

3.3. Descriptive statistics

Since we focus our analysis on firms with 6 to 25 employees, it is interesting to explore the implications of this sample restriction for the size of the sample and its composition. Table 1 represents descriptive statistics on the variables used in the analysis by firm size.

Restricting the analysis to firms with 6 to 25 employees means focusing on slightly less than one third (29%) of all Italian firms with at least one permanent employee. Micro-firms with less than 6 employees account for 64% of the sample, while firms with more than 25 employees account for just 7%.

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. T-tests for differences in the means across different firm-size groups are also presented. These tests show that there are systematic differences in the characteristics of small and large firms, as well as between firms in our estimation sample and those that are excluded. Significant differences are also observable in the industry and geographical distribution.

Differences in the characteristics of small and large firms may be related to the differential role of EP protection provisions above and below the 15 employee threshold, but may also reflect the independent effect of firm size or the endogenous response of firms to EP. 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.

4. The empirical strategy

4.1. The regression discontinuity design (RDD)

The fact that in Italy EP provisions for the individual dismissal of a regular worker vary significantly according to firm size provides 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 et al., 2001; Lee and Lemieux, 2010) and, consequently, provides more credible results. Moreover, the assumptions are testable in a similar manner as in randomised experiments.[17]

To estimate the causal impact of EP we rely on 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'). In the present context, bins are defined as intervals of 0.1 employees.[18] We complement the non-parametric analysis with ordinary least squares estimates based on the following general parametric model:

$$Y_i = \sum_{n=0}^{N} \alpha_{0n}(T - F_i)^n + D_i \sum_{n=0}^{N} (\alpha_{1n} - \alpha_{0n})(F_i - T)^n + \beta_x X_i + \varepsilon_i;$$
$$D_i = 1[F_i > T];$$
$$T - h \le F_i \le T + h \quad (1)$$

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 to reduce the sampling variability of our RDD estimator. The α's represent the key parameters to be estimated with the first subscript indicating whether it refers to untreated (0) or treated (1) observations and the second to n, which indicates the order of the polynomial in firm size. The effect of employment protection on the outcome of interest is given by: $\alpha_{10} - \alpha_{00}$. ε_i represents a white-noise error term. The calculation of the standard errors takes account of the stratified nature of our data.

Eq. (1) encompasses a wide variety of different specifications. If N=0, Eq. (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 on each side of the threshold is tantamount to equating α_{1n} to α_{0n} for $n \ge 1$. h refers to the window on each side of the threshold (or bandwidth) and may take the value of 10, 8 or 6 in our analysis.

Eq. (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 the treatment is locally randomised.

4.2. Assessing the validity of the RDD

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.

McCrary (2007) 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

[15] Nevertheless, we also assess the sensitivity of our results to the use of alternative firm size measures that have been used in the literature.

[16] Moreover, cross-validation tests such as those suggested by Imbens and Lemieux (2008) based on the mean squared error provide little guidance to determining the optimal bandwidth in such a context. We, therefore, provide results based on alternative bandwidth choices.

[17] In particular, the validity of RDD does not hinge on conditional independence, which is often a strong assumption and cannot be verified, but instead on the continuity of the assignment variable, which can be tested (Lee and Lemieux, 2010).

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

Table 1
Summary statistics by firm size, 2009.

Variable names	Mean value (standard deviation)					T-tests			
	≤ 5	< 5, 15]	< 15, 25]	> 25	All	H_0: E[y] $_{<15,25]}$=E[y] $_{<5,15]}$		H_0: E[y] $_{<5,25]}$=E[y] $_{\leq 5 \ \& \ >25}$	
total employment	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 employment	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 employment	0.11	0.65	1.74	12.72	1.17	55.82	***	−1.43	
	(0,31)	(1,03)	(2,39)	(206,97)	(53,80)				
semi-independent employment	0.54	0.57	0.50	0.29	0.53	−4.81	***	8.02	***
	(0,81)	(0,90)	(0,94)	(1,00)	(0,86)				
total excess worker reallocation 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)				
share of temporary employees in dependent employment	0.04	0.08	0.09	0.09	0.05	9.55	***	55.32	***
	(0,09)	(0,11)	(0,12)	(0,12)	(0,10)				
share of independent employees in total employment	0.15	0.06	0.02	0.01	0.11	−28.31	***	−75.08	***
	(0,21)	(0,09)	(0,04)	(0,02)	(0,18)				
temporary excess worker reallocation rate	4.04	3.02	2.71	2.89	3.33	−4.71	***	−14.3	***
	(5,09)	(3,72)	(3,36)	(7,20)	(4,97)				
permanent excess worker reallocation 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)				
semi-independent employee excess reallocation rate	0.02	0.08	0.24	0.43	0.06	15.51	***	−2.62	***
	(0,25)	(0,59)	(1,36)	(2,42)	(0,59)				
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)				
share of STW beneficiaries in dependent employment	0.03	0.08	0.12	0.13	0.06	13.6	***	41.2	***
	(0,14)	(0,20)	(0,23)	(0,24)	(0,17)				
age of the firm (in years)	17.93	19.04	20.86	24.56	18.78	10.21	***	10.31	***
	(11,06)	(12,05)	(12,93)	(15,37)	(11,86)				
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	***
share of employment in sample	0.11	0.16	0.07	0.66					
share of firms in sample	0.64	0.24	0.05	0.07					
Observations (N)	**78,654**	**29,850**	**5,584**	**8,238**	**122,326**				

Note: Balanced Panel 2008–2009.
*, **, *** indicates statistically significant at 10%, 5%, 1% confidence interval, respectively.

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. This is represented in Panel A of Fig. 1. A visual inspection does not suggest any significant discontinuity in the firm-size distribution around the threshold. 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. The results are reported in Panel B of Fig. 1. The dots indicate the computed frequencies at the midpoint of each bin, while the dashed 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.

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 EP rules and, therefore, we do not expect this to be an important issue in the present context.[19]

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

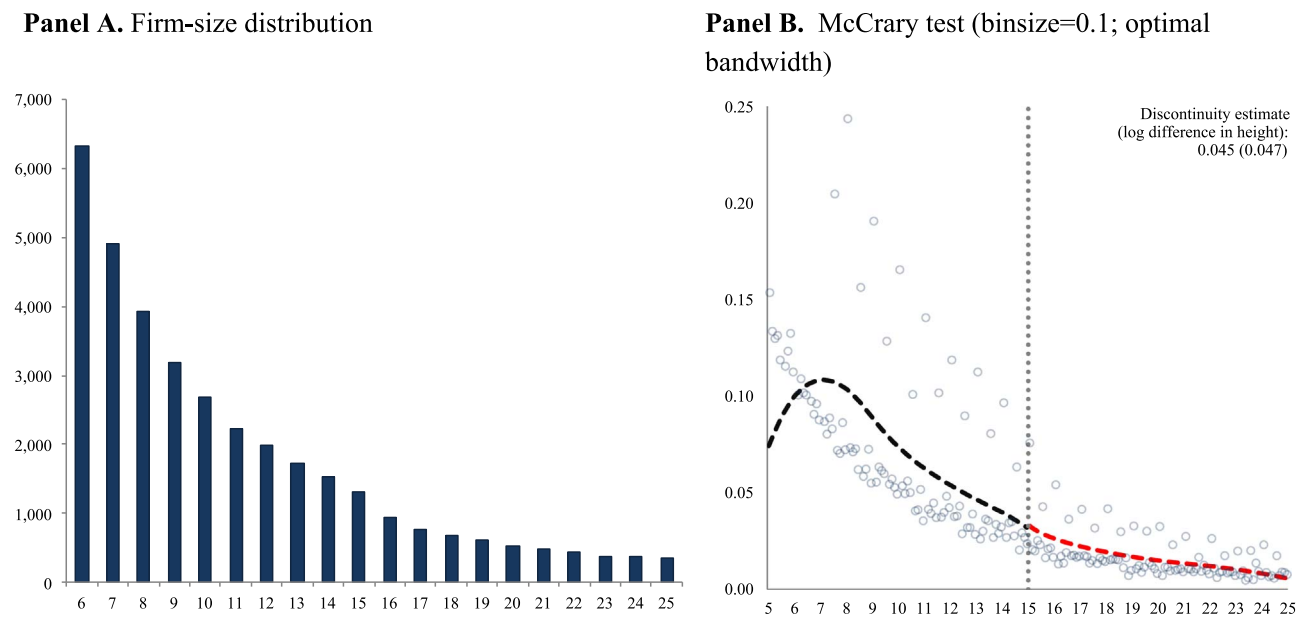

Fig. 1. McCrary test of the continuity of the employment density around the threshold. **Panel A**. Firm-size distribution. **Panel B**. McCrary test (binsize=0.1; optimal bandwidth).

Following Schivardi and Torrini (2008), we therefore also assess the impact of EP 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 just below the threshold, D^{K}, and a set of controls, X.

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

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

$$D^{K}_{it-1} = 1[K - 0.5 \leq F_{it-1} < K + 0.5] \text{ for } K = 5, \ldots, 25 \qquad (2)$$

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 EP on the probability to grow. The results are reported in Fig. 2. Consistently with Schivardi and Torrini (2008), Leonardi and Pica (2013), Garibaldi et al. (2004), we find that the probability to grow is increasing with respect to the firm size. We also find a lower probability of growth at 15 employees. However, the difference in the probability is not statistically different from zero.[20]

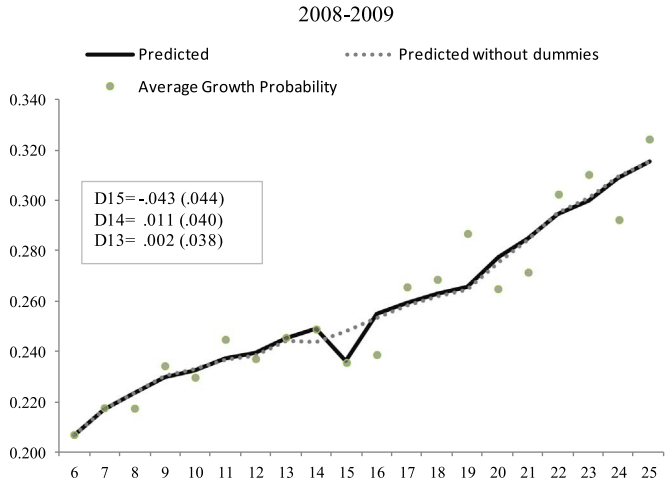

Fig. 2. Actual and predicted growth probabilities by firm size 2008–2009.

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 number of workers involved in the short-time working scheme (CIG) as a share of employees. We check whether the two groups are balanced by replacing the dependent variable in Eq. (1) by each of the covariates, whilst using N = 1,2,3 and h = 6,8,10. The results, reported in the Table 2 below, show that there are generally no significant discontinuities at the 15 employee threshold. While some of the estimated coefficients are statistically significant, for no variable, except mining, are more than half of coefficients statistically significant at 5%. The results for the mining sector rely on a relatively small number of observations which may not be sufficient to give a precise representation of the firm-size distribution.[21] Our preferred specification based on h=10 and N=3 yields only one other variable apart from mining (financial sector) that is statistically significant at the 5% level.

From the three different validity tests discussed above, we conclude that manipulation of the assignment variable, i.e. firm selection, is not a major issue in the present context and a RDD is therefore appropriate.[22]

[20] Using Eq. (1) to detect any possible discontinuities in the probability to grow around the threshold yields qualitatively similar results.

[21] Using a probit model instead of OLS in the context industry and region dummies yields very similar results.

[22] Back-on-the-envelope calculations suggest that the share of missing firms above the threshold should be about 15% to entirely remove the difference in the incidence of temporary work between firms on both sides of the threshold (see Section 5). This calculation is based on asking how many firms with zero incidence of temporary work should be moved from the left to the right of the threshold in order to equalise the average incidence of temporary work. The various validation tests presented in this subsection clearly suggest that selection is at best tiny and cannot remotely account for a share of missing firms of 15% above the threshold.

Table 2
Balancing test for covariates.

Bandwidth	6 – 25			8 – 23			10 – 21		
Order of polynomial	1 order	2 order	3 order	1 order	2 order	3 order	1 order	2 order	3 order
STW take up rate	−0.005	0.001	0.010	−0.002	0.006	0.005	0.004	0.003	−0.002
	[0.006]	[0.009]	[0.012]	[0.007]	[0.010]	[0.013]	[0.008]	[0.011]	[0.016]
Age	−0.522	−0.566	−1.163*	−0.414	−0.919	−1.133	−0.831*	−0.762	−1.133
	[0.346]	[0.511]	[0.683]	[0.385]	[0.573]	[0.763]	[0.439]	[0.654]	[0.886]
Construction	0.024**	0.012	0.013	0.022**	0.011	0.003	0.021	−0.000	0.009
	[0.010]	[0.015]	[0.021]	[0.011]	[0.017]	[0.023]	[0.013]	[0.020]	[0.027]
Manufacturing	−0.027**	−0.017	−0.033	−0.024	−0.017	−0.045	−0.018	−0.024	−0.078**
	[0.014]	[0.020]	[0.028]	[0.015]	[0.023]	[0.031]	[0.017]	[0.026]	[0.036]
Real estate	−0.002	0.005	0.004	0.002	0.002	0.005	0.003	−0.000	0.016
	[0.008]	[0.012]	[0.016]	[0.009]	[0.013]	[0.018]	[0.010]	[0.015]	[0.020]
Transport	−0.003	0.002	0.009	−0.003	0.007	0.011	0.000	0.008	0.020
	[0.006]	[0.010]	[0.013]	[0.007]	[0.011]	[0.015]	[0.008]	[0.012]	[0.017]
Wholesale	0.002	−0.002	0.007	−0.003	0.002	0.014	−0.004	0.010	0.014
	[0.011]	[0.016]	[0.022]	[0.012]	[0.018]	[0.024]	[0.014]	[0.021]	[0.028]
Hotel	0.011**	0.007	0.014	0.011*	0.007	0.018	0.008	0.012	0.025*
	[0.005]	[0.008]	[0.010]	[0.006]	[0.008]	[0.012]	[0.006]	[0.010]	[0.014]
Electricity	−0.001	−0.000	0.001	−0.001	0.000	0.001	−0.000	0.000	0.001
	[0.001]	[0.001]	[0.002]	[0.001]	[0.002]	[0.002]	[0.001]	[0.001]	[0.001]
Mining	−0.005***	−0.004	−0.007**	−0.004**	−0.007**	−0.001	−0.007***	−0.002	−0.001
	[0.002]	[0.002]	[0.003]	[0.002]	[0.003]	[0.003]	[0.002]	[0.003]	[0.004]
Financial	0.001	−0.004	−0.009**	−0.001	−0.006*	−0.005	−0.003	−0.004	−0.007*
	[0.002]	[0.003]	[0.004]	[0.002]	[0.003]	[0.004]	[0.003]	[0.003]	[0.004]
North-east	0.005	0.003	0.009	0.003	0.003	0.025	0.001	0.010	0.033
	[0.012]	[0.018]	[0.025]	[0.013]	[0.021]	[0.028]	[0.015]	[0.024]	[0.032]
North-west	−0.038***	−0.030	−0.017	−0.043***	−0.012	−0.035	−0.034**	−0.010	−0.060*
	[0.013]	[0.019]	[0.026]	[0.014]	[0.022]	[0.029]	[0.016]	[0.025]	[0.033]
Centre	−0.002	0.008	0.002	0.002	0.009	−0.007	0.006	−0.001	0.010
	[0.011]	[0.016]	[0.022]	[0.012]	[0.018]	[0.025]	[0.014]	[0.021]	[0.028]
South	0.035***	0.019	0.006	0.038***	0.001	0.017	0.027*	0.000	0.018
	[0.011]	[0.017]	[0.023]	[0.012]	[0.019]	[0.025]	[0.014]	[0.022]	[0.029]
Observations	35,434	35,434	35,434	23,446	23,446	23,446	15,534	15,534	15,534

Standard errors in brackets.

* $p < 0.1$.
** $p < 0.05$.
*** $p < 0.01$.

5. Main results

In this section, we provide a systematic evaluation of the impact of EP on excessive worker turnover, the incidence of temporary work and labour productivity using the RDD set out in Section 4. Excessive worker turnover 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 turnover, defined as the sum of hires and separations over average employment, and excessive worker turnover represents the net employment change. As it has been demonstrated in the previous section that EP does not affect employment growth, we focus directly on excessive worker turnover here. Fig. 3 summarizes our RDD results of the impact of EP on excessive worker turnover. 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 EP provisions in large firms. The parametric results, reported in Table 4, further show that these results are robust to using different specifications with varying bandwidths and using alternatively linear, quadratic or third-order polynomials to control for the independent effect of firm size.[23] These findings are consistent with the theoretical predictions in

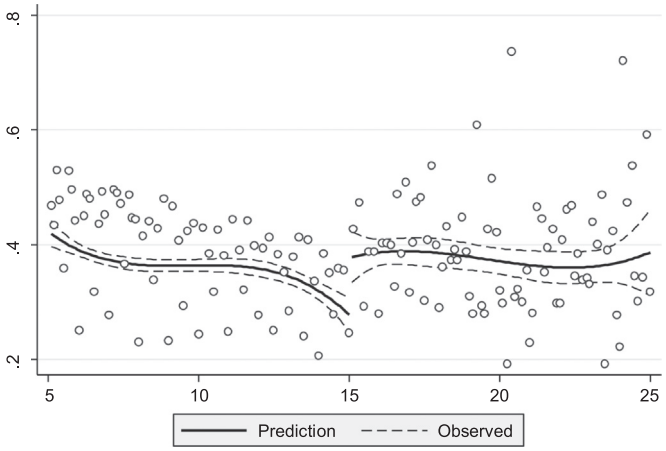

Fig. 3. The impact of employment protection on excessive worker turnover.

Blanchard and Landier (2002) and Cahuc and Postel-Vinay (2002).

From an accounting perspective, the impact of EP on excess worker turnover may reflect different channels. It may reflect a composition effect in the form of an increase in the incidence of non-standard work as large firms substitute workers on open-ended contracts by a sequence of workers on fixed-term contracts or independent contractors. In principle, it may also reflect a technology effect, i.e. the possibility that large firms have higher rates of worker turnover for a

[23] Moreover, qualitatively similar results are obtained when using overall worker turnover, defined as the sum of hires and separations over employment, instead of excess worker turnover.

Table 3
Parametric estimates Selected coefficients.

Bandwidth	6 – 25			8 – 23			10 – 21		
Order of polynomial	1 order	2 order	3 order	1 order	2 order	3 order	1 order	2 order	3 order
Excess worker reallocation rate	0.070***	0.076***	0.099***	0.084***	0.072***	0.097***	0.094***	0.059**	0.132***
for all workers	[0.015]	[0.022]	[0.029]	[0.017]	[0.024]	[0.032]	[0.018]	[0.027]	[0.037]
Incidence of temporary work	0.017***	0.020***	0.025***	0.019***	0.021***	0.028***	0.023***	0.019***	0.034***
in dependent employment	[0.003]	[0.005]	[0.007]	[0.004]	[0.005]	[0.007]	[0.004]	[0.006]	[0.009]
Incidence of independent employees	0.007***	0.000	−0.000	0.004**	0.001	−0.000	0.002	0.001	0.000
in total employment	[0.001]	[0.002]	[0.003]	[0.002]	[0.002]	[0.003]	[0.002]	[0.003]	[0.004]
Excess worker reallocation rate	0.007	0.002	0.008	0.010*	−0.003	0.013	0.007	0.001	0.011
for permanent employees	[0.005]	[0.007]	[0.009]	[0.005]	[0.008]	[0.010]	[0.006]	[0.009]	[0.012]
Excess worker reallocation rate	0.335***	0.275*	0.152	0.392***	0.205	−0.076	0.395***	−0.031	0.077
for temporary employees	[0.112]	[0.158]	[0.207]	[0.122]	[0.174]	[0.230]	[0.138]	[0.199]	[0.267]
Excess worker reallocation rate	0.007***	0.000	−0.000	0.004**	0.001	−0.000	0.002	0.001	0.000
for independent employees	[0.001]	[0.002]	[0.003]	[0.002]	[0.002]	[0.003]	[0.002]	[0.003]	[0.004]
Log labour productivity	−0.063***	−0.057	−0.071	−0.072***	−0.049	−0.094*	−0.081***	−0.037	−0.144**
	[0.024]	[0.036]	[0.047]	[0.026]	[0.039]	[0.052]	[0.030]	[0.045]	[0.060]
Observations	35,434	35,434	35,434	23,446	23,446	23,446	15,534	15,534	15,534

Standard errors in brackets.

* $p < 0.1$.
** $p < 0.05$.
*** $p < 0.01$.

given type of contract. However, this seems less plausible *a priori*, since theory predicts that EP reduces worker turnover, at least among workers with open-ended contracts, leading to lower worker turnover among workers on open-ended contracts in large firms.

In order to systematically analyse the channels through which EP affects excess worker turnover between small and large firms, we make use of a shift-share decomposition, which expresses the difference in excess turnover between small and large firms, as a result of EP, in terms of a between effect and a within effect, as follows:

$$\Delta\hat{XR} = \sum_{i=1}^{N}\sum_{c=1}^{C}\Delta\hat{s}_{ic}\overline{XR}_{ic} + \sum_{i=1}^{N}\sum_{c=1}^{C}\bar{s}_{ic}\Delta\hat{XR}_{ic} \tag{3}$$

where XR represents worker turnover, 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 EP 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 turnover or the composition effect of EP. This term captures differences in excessive worker turnover 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 turnover, or the technology effect of EP. This represents the differential impact of EP 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 EP on each component of excessive worker turnover are reported in Table 3. The RDD results indicate that the overall effect largely reflects the impact of EP on the use of workers on temporary contracts (see also Fig. 4). 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 firm size; iii) for varying definitions of bandwidth. Our preferred estimates, based on bandwidth 6–25 and the use of a third-order polynomial in firm size, suggest that the discontinuity in EP increases the incidence of temporary work by 2.5 percentage points around the threshold. There is no evidence that EP 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) or has any impact on excessive worker turnover by type of contract.

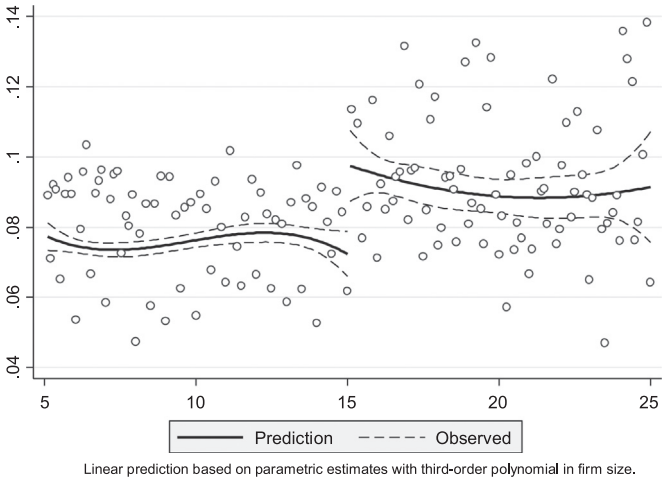

Fig. 4. The impact of employment protection on the incidence of temporary employees.

While the use of temporary workers may allow firms to effectively circumvent the effect of EP on permanent workers, it may have detrimental effects on firm performance if temporary workers are not as productive as permanent workers, even after controlling for differences in labour costs. This may result from individual characteristics as well as lower incentives to invest in the human capital of temporary workers and their motivation. Moreover, although the greater use of temporary workers helps 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.[24]

To shed light on the effects of EP on firm's performance, we focus on labour productivity, defined in terms of sales per worker, below and above the threshold. Moreover, we also present regression 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

[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.

Table 4
Sensitivity analysis Selected coefficients.

	(1)	(2)	(3)	(4)	(5)	(6)
	baseline	symmetric	controls	discrete	Schivardi-Torrini	Leonardi-Pica
Excess worker reallocation rate	0.099***	0.072***	0.078***	0.095***	0.134***	0.277***
	[0.029]	[0.018]	[0.026]	[0.028]	[0.026]	[0.028]
Incidence of temporary work	0.025***	0.017***	0.021***	0.024***	0.020***	0.088***
	[0.007]	[0.004]	[0.006]	[0.006]	[0.005]	[0.005]
Log labour productivity	−0.071	−0.074**	−0.064	−0.075*	−0.207***	−0.093*
	[0.047]	[0.029]	[0.042]	[0.046]	[0.037]	[0.049]

Each coefficient refers to a different regression with a third-order polynomial in firm size and bandwidth 5–25. Standard errors in brackets,
* $p < 0.1$.
** $p < 0.05$.
*** $p < 0.01$.

incidence of temporary employment as an additional covariate. The results, reported in Table 3, show that EP tends to have a negative effect on labour productivity, reducing it by 6 to 14%. Our estimates for labour productivity are statistically significant in five of the nine specifications. Controlling for the incidence of temporary work or the rate of worker turnover consistently reduces the estimated impact of EP on labour productivity (Annex Table A2). 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 or worker turnover may be quite sizeable.

Each coefficient refers to a different regression with the variable in the first column as the dependent variable and across columns varying bandwidths and parameterisations of the independent effect of firm size

6. Sensitivity analysis

In order to check the sensitivity of our results, we conduct a number of further robustness tests. First, we assess the sensitivity of our baseline results to slightly different specifications of our RDD. Second, we implement a regression-in-discontinuities design to control for any institutional aspects that make use of a similar firm-size threshold. Third, we implement a series of falsification tests based on alternative hypothetical thresholds. Finally, we assess whether the results are driven by the specific period considered using a different dataset with a longer time dimension.

6.1. Alternative specifications of the baseline model

In this sub-section, we assess the sensitivity of our results with respect to slightly different specifications of the empirical model and the definition of the threshold. The results are summarised in Table 4. The baseline estimates, which include a third-order polynomial in firm size and make use of the window 5–25 (N=3, h=10), are reported in column 1 as a benchmark (identical to column 3 of Table 3). Column 2 reports the results based on a specification that imposes that the relationship between firm size and the outcome variable of interest is identical on both sides of the threshold ("symmetric"). Column 3 reports the results that include a number of additional controls, including age, age squared, the take up rate of the short-time working scheme (*Cassa Integrazione Guadagni*), industry and region ("controls"). Column 4 report the results based on a discrete measure of employment that only allows for integers and semi-integers ("discrete"). Columns 5 and 6 report the results based on alternative measures of firm size which have been used in the literature and respectively measure firm size as a head count of all employees (Schivardi and Torrini, 2008) or of permanent employees only (Leonardi and Pica, 2013).

The results for excess worker turnover and the incidence of temporary work are robust to all six different RDD specifications. As before, the results for labour productivity are consistently negative, but not statistically significant in all specifications.

6.2. A difference-in-discontinuities 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 may nevertheless be helpful. The main reason in the present context for doing so is to take account of the potential effects of any other regulations that make use of a firm-size threshold around 15, which could lead to a bias in our results.

In order to remove the role of such confounding factors, we propose to complement our standard RDD with a difference-in-discontinuities design (DDD) (Grembi et al., 2016) that exploits differences in the *de facto* role of employment-protection provisions across industries that are unrelated to these confounding factors. [25] Similar to for example Kugler and Pica (2008), we assume that *de facto* differences in the impact of EP across sectors arise as a result of deep technological differences related to the need to regularly adjust the workforce.[26] To measure the "intrinsic" need for making employment adjustments, we focus on the standard deviation of log employment over the period 2001–2008 on average across firms in each sector after netting out the potential effect of EP on firms with more than 15 employees. The potential effect of EP is netted out by estimating Eq. (1) for our measure of employment volatility and using the counterfactual predictions with the threshold dummy set to zero. We thus analyse to what extent the discontinuity around the threshold depends on the intrinsic employment volatility of the sector.[27]

The results based on the difference-in-discontinuities (DDD) design are reported in Table 5. The specification control for the independent

[25] Other reasons for using a difference-in-difference estimator may be to reduce any 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 EP regime or another and does not depend on the *de jure* or *de facto* difference in the stringency of EP as well as to enhance 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.

[26] 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.

[27] A key assumption of our difference-in-discontinuities design 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 McCrary test discussed above by industry. While the difference-in-discontinuities analysis is conducted at the 2-digit industry level, we report results by 1-digit industry in Table A1 of the annex to maintain reasonable cell sizes and for ease of presentation. The McCrary test statistic is never statistically significant. These results, therefore, suggest that the difference-in-discontinuities design described above is appropriate.

Table 5
Difference-in-discontinuity results Selected coefficients.

	(1) Excess worker reallocation rate	(2) Incidence of temporary work	(3) Log labour productivity
Threshold dummy	0.094***	0.025***	−0.066
	[0.029]	[0.007]	[0.047]
Sectoral employment volatility	6.060***	0.857***	−4.018***
	[0.623]	[0.129]	[1.018]
Threshold dummy * sectoral employment volatility	0.021*	0.005**	−0.002
	[0.012]	[0.003]	[0.019]
Constant	0.301***	0.076***	11.924***
	[0.016]	[0.003]	[0.027]
Observations	35,434	35,434	34,736
R-squared	0.045	0.026	0.019

Each coefficient refers to a different regression with a second-order polynomial in firm size and bandwidth 5–25. Standard errors in brackets,
* p < 0.1
** p < 0.05,
*** p < 0.01,

effect of employment volatility and allows for varying firm size profiles according to the degree of employment volatility on each side of the threshold. The DDD estimates indicate a positive and statistically significant effect of EP on excess worker turnover and the incidence of temporary work, and a negative but not statistically significant effect for labour productivity. These results therefore suggest that the baseline results are not driven by confounding factors that are common across industries. Moreover, since the measure of employment volatility is normalized to have zero mean the estimates for the average effect of EP given by the coefficient on the threshold dummy is largely unaffected. Finally, it is worth noticing that employment volatility has important and plausible effects on the outcome variable of interest: positive in the case of worker turnover and the incidence of temporary work and insignificant in the case of labour productivity.

6.3. Placebo test

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 threshold (Lalive et al., 2009). Since the bandwidth is much smaller than in the baseline specifications a quadratic instead of a cubic specification is used. Fig. 5 illustrates that there are no significant discontinuities in any of these points at the 95% confidence interval.

6.4. Different time periods

A final concern is related to the particular period considered for the analysis since this corresponds to the start of the global financial crisis. While this was clearly a very turbulent period with many things changing at the same time, it is not clear whether and how this would affect our analysis. The effects of the global financial crisis are by definition broad-based and it is not immediately clear how this could induce a discontinuity in the incidence of temporary work around the threshold. However, it is possible that it interacts with the threshold, since it increases the need for downsizing, and hence affects the quantitative estimates.

Given the limited time coverage of our data we cannot look at this with the dataset used for the main analysis. In order to nevertheless get some idea of the role of the global financial crisis for our main results, we make use of a different dataset provided by INPS for the period

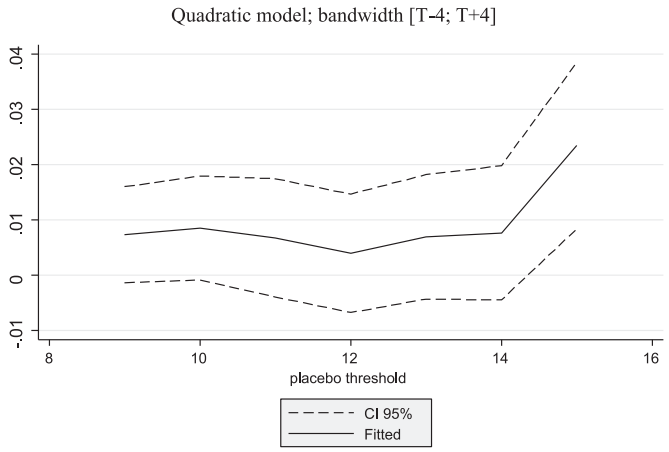

Fig. 5. Placebo tests - Estimating the treatment effect on the incidence temporary work at fake thresholds Quadratic model; bandwidth [T−4; T+4].

1985–2014.[28] In contrast to our main dataset, which is a random sample firms, this is a sample of workers. One implication of this is that firm size cannot be directly measured in the data and that one has to rely on a pre-defined variable for firm size which classifies firms into detailed firm-size categories. Since firm-size is recorded as a categorical variable it cannot be used to conduct the RDD analysis for a larger period. The dataset also does not provide information on labour productivity. However, the firm-size variable in the dataset allows comparing the incidence of temporary work in small firms with 5–14 employees with large firms with 15–24 employees using data from before the global financial crisis only. Hence, it does allow getting an indication of the the focus on 2008–2009 for our main results.

We thus analysis the role of EP for the incidence of temporary work by comparing the probability of being employed on temporary contract among firms with 5 to 14 employees and firms with 15 to 24 employees. Our estimates are based on probit regressions which include controls for age, gender and firm size (based on the categorical variable). The analysis is restricted to the period 2003, the year the Biagi law was implemented, and 2012, the year of the Fornero reform. We provide estimates for the entire period, as well as separate ones for the pre-crisis period (2003–2007), and the crisis period (2008–2012). The results in Table 6 confirm that firms just above the threshold have systematically higher shares of temporary workers than their counterparts just below the threshold: the coefficients are positive and statistically significant for all periods.

7. 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 effect of EP on larger firms not included in the analysis, but which account for the bulk of employment, may differ from that of firms around the threshold. For example, EP may have a different impact on large firms because of differences in their production technology. To the extent that large firms rely more on firm-specific human capital, seek to limit worker turnover and pay higher wages, the scope for substituting permanent workers by workers on temporary contracts may be more limited. Moreover, large firms may differ in the way they respond to idiosyncratic and aggregate shocks. Because of their larger size, it may be easier for them to downsize using natural attrition and reallocate workers between units.[29]

[28] Losai – Sample provided by INPS. https://www.cliclavoro.gov.it/Barometro-Del-Lavoro/Pagine/Microdati-per-la-ricerca.aspx

[29] In addition, the exposure of large firms to output shocks may differ from that of firms around the threshold and thereby affect the incentives large firms to make use of temporary workers.

Table 6
Probit estimates of the probability of being employed on a temporary contract Selected coefficients by time period.

	(1)	(2)	(3)
	2003–2011	2003–2007	2008–2011
Threshold dummy	0.0289	0.0197	0.0348
	(0.0089)	(0.0116)	(0.0125)
	***	*	***
Constant	−1.6238	−2.1241	−0.6995
	(0.0253)	(0.0322)	(0.0321)
	***	***	***
Observations	1,691,572	935,888	755,684

Regressions of workers in firms with 5 to 24 employees that control for age, gender, firm size and year. Standard errors are robust and clustered within firms.
**p < 0.05.
* p < 0.1.
*** p < 0.01.

While these arguments may suggest that there the impact of EP may be smaller for larger firms, this is not obvious in the data, which show that the incidence of temporary work is broadly constant across firms of different sizes.

To address this issue formally, we use semi-parametric estimates of the actual relationship between the incidence of temporary work and firm size and the counterfactual relationship had the employment-protection regime for small firms been applicable to all firms. While, in principle, one could simply use the parametric estimates to extrapolate actual and counterfactual relationships between firm size and the incidence of temporary work beyond the threshold, in practice, this method tends to be sensitive to the specification of the parametric model. This is also the case in the present context. Angrist and Rokkanen (2012) therefore suggest an alternative method that involves using control variables to estimate counterfactual outcomes away from the cutoff under the assumption that treatment is conditionally independent of firm size.

In contrast to the conditional independence assumption (CIA) in the context of conventional quasi-experimental indicators, the CIA assumption invoked here exploits the deterministic nature of the way treatment is assigned in a RDD and, consequently, its validity can be tested by assessing whether the assignment variable has a statistically significant effect on potential outcomes conditional on the controls. The results of such validity tests are reported in Table A3 for three different bandwidths. They show that in only one of the six cases firm size has a statistically significant impact on the incidence of temporary work, conditional on controls. This is when the largest bandwidth is used and the focus is on firms below the cutoff. This is not surprising since there exists a strong positive relationship between the incidence of temporary work and firm size among very small firms with less than 10 employees, while the relationship appears to be essentially constant beyond this point. Close to the cutoff the incidence of temporary work does indeed appear to be independent of firm size, conditional on controls. This means that we can use the controls to analyse the impact of EP on the incidence of temporary work for firms above the cutoff.

Fig. 6 shows the CIA estimates of the effect of EP on the incidence of temporary work based on linear re-weighted regression (Kline, 2011). The solid lines show the fitted lines of the observed incidence of temporary work conditional on the controls. The dashed line shows the counterfactual outcomes conditional on controls in the absence of the treatment. The results suggest that the average treatment effect on the treated does not vary significantly over the domain considered.[30]

[30] We also used an alternative reweighting method which yields qualitatively similar results. In this case, we adopted a flexible parametric model that allowed treatment effects to vary by firm age, region and industry. Using the heterogeneous treatment effects in combination with the characteristics of the population of firms one can construct a re-weighted average treatment effect on the treated that is more representative for the economy as a whole. Reweighting did not change the results significantly.

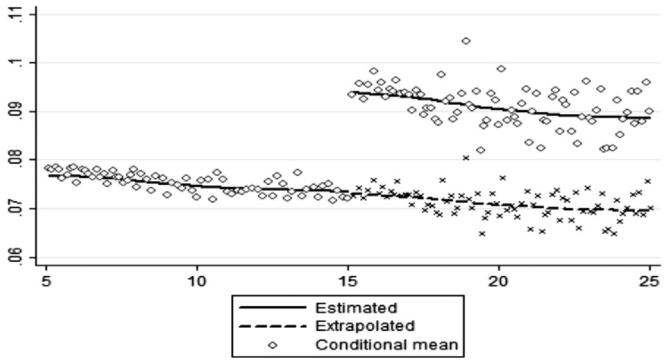

Fig. 6. Estimates of the incidence of temporary work under conditional independence bandwidth 5–25. **Estimated**: Within-sample prediction based on estimates of the incidence of temporary work below the threshold; **Extrapolated**: Out-of-sample prediction above the threshold based on estimates of the incidence of temporary work below the threshold; **Conditional mean**: Conditional mean of the incidence of temporary work within employment-size bin based on estimates of the incidence below the threshold/over the entire domain, respectively.

We conclude that our RDD estimates do not just relate to the impact of EP around the cutoff but also apply to large firms further above the cutoff. While the formal estimates only considered firms with up to 25 employees, the stability of the relationship between the incidence of temporary work and firm size suggests that our estimates are also informative for firms well beyond the 25 mark. Taking our average treatment effect on the treated at face value, this implies that the incidence of temporary work in firms with 15 or more employees would be 7% instead of the observed value of 9%. Put differently, EP accounts for over 20% of the incidence of temporary work among large firms or about 12% for the economy as a whole (the employment share of firms with 15 or more employees in Italy is about 60%).

8. Conclusions

In this paper, we exploit a novel firm-level dataset to investigate the impact of employment protection on firms' workforce characteristics and behaviour by exploiting the variation in employment protection provisions in Italy around a size threshold (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. In the empirical analysis of the threshold effect we use a regression discontinuity design. This is appropriate since the firm-size distribution is continuous around the threshold and firms just below the threshold do not display an unusually low propensity to grow.

Our main findings suggest that employment protection significantly increases the incidence of temporary work and, as a result, tends to dampen the job security of workers in firms above the threshold. Indeed, our empirical results provide clear evidence that firms above the threshold tend to circumvent the stricter regulations on permanent contracts by making more intensive use of temporary employment contracts. In this way, they exploit the market opportunities and economies of scale offered by the larger size without incurring extra adjustment costs in case of downsizing. Our preferred estimates suggest that the discontinuity of EP increases the incidence of temporary work by 2 to 2.5 percentage points (about 20%) in firms above the threshold. Using the recently proposed method to identify the impact of treatment effects away the threshold by Angrist and Rokkanen (2012), we also find that our RDD estimates do not just relate to the discontinuity around the threshold but also apply to larger firms well beyond the threshold. EP may account for about 12% of the incidence of temporary work in Italy.

Our results also suggest that the greater use of temporary employment in firms above the threshold has a negative impact on firm productivity. The effect is again sizeable, although there is some uncertainty about its precise magnitude. Importantly, a considerably

part of the impact of EP on labour productivity appears to come through its impact on the incidence of temporary work (and hence worker turnover). This means that the wide discrepancy in employment protection between workers on open-ended contracts and those on temporary and atypical contracts not only reduces job security on average, but also comes at a cost for employers and the economy in terms of lower labour productivity.

In this context, the recent labour reforms intended to reduce the stringency and uncertainty of employment protection provisions for workers on permanent contracts for firms above the threshold, could, if fully implemented, contribute to better economic performance and tackle at least in part the large dualism in the Italian labour market. These results are relevant for a range of mainly European countries that either have conducted recent reforms or are envisaging reforms to reduce the often large divide in the protection between workers employed on open-ended contracts and those on temporary and other atypical contracts.

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Annex

See Annex Tables A1–A3

Table A1
McCrary test and Schivardi-Torrini test by 1-digit industry.

	Volatility	**McCrary test**	
	Industry mean	Estimated log difference	Standard error
Construction	0.259	0.065	0.108
Electricity, gas and water supply	0.189	0.266	0.823
Financial intermediation	0.258	−0.559	0.890
Hotels and restaurants	0.268	0.399	0.278
Manufacturing	0.229	0.039	0.066
Mining and quarrying	0.215	−0.162	0.842
Real estate	0.271	0.220	0.157
Transport and communication	0.247	0.370	0.207
Wholesale	0.252	0.147	0.107

Table A2
Conditional labour productivity results Third-order polynomial in firm size, bandwidth 5–25.

	(1)	(2)	(3)	4)	(5)
	All firms				
Treated	−0.071	−0.058	−0.054	−0.034	−0.028
	[0.047]	[0.047]	[0.046]	[0.048]	[0.047]
Incidence of temporary work		−0.528***		−0.480***	
		[0.040]		[0.044]	
Worker reallocation rate			−0.165***		−0.155***
			[0.009]		[0.010]
Treated * incidence of temporary work				−0.264**	
				[0.107]	
Treated * worker reallocation rate					−0.074***
					[0.024]
Constant	11.940***	11.978***	11.984***	11.974***	11.982***
	[0.026]	[0.026]	[0.026]	[0.026]	[0.026]
Observations	34,736	34,736	34,736	34,736	34,736
R-squared	0.013	0.018	0.026	0.019	0.027

Standard errors in brackets, *** p < 0.01, ** p < 0.05, * p < 0.1.

Table A3
Tests based on conditional independence assumption Selected coefficients.

Bandwidth	6–25		8–23		10–21	
Treatment status (D)	D=0	D=1	D=0	D=1	D=0	D=1
Firm size	−0.001***	−0.000	−0.000	−0.000	0.000	−0.001
	[0.000]	[0.001]	[0.000]	[0.001]	[0.001]	[0.001]
N	29,850	5,584	18,601	4,845	11,498	4,036

Standard errors in brackets, *** p < 0.01, ** p < 0.05, * p < 0.1. Controls: age, age square and short-time work take-up rate.

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