The Implications of Changing Employment Protection: Evaluating the 1999 UK Unfair Dismissal Reform

Very preliminary - Please do not quote

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Abstract

The empirical results on the net impact of job security provision on employment have not been conclusive. Using the UK Labour Force Survey from 1997 to 2001, this paper examines the impact of the 1999 British Unfair Dismissal Reform on firms firing behaviour. Combining treatment evaluation techniques, namely Difference-in-Differences and Regression Discontinuity Design, with survival techniques our results show consistently that the probationary period shortening, occurred during the reform, led to a significant decrease in the probability of being laid off amounting to 1% just for the newly covered - i.e. those workers whose tenure is between 12 and 24 months, even though, the new probationary period threshold is found to be not significant. Looking at the effects of the reform on manufacturing, our evidence shows that shortening the probationary period increases the probability of being dismissed for those whose tenure is lower than 12 months. Aiming at evaluating whether this pattern was driven by a particular compositional effect we split white from blue collar workers. Our evidence supports the thesis that the effect of the reform is heterogeneous across skills.

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1 Introduction

In the last two decades of the XX century the economic literature has witnessed a steep increase in analysis aimed at explaining the persistent unemployment afflicting many European Countries. Strict employment protection legislation has often been blamed for the poor performance of some European labour markets (see the discussion in Nunziata and Staffolani, 2007). However, the impact of job security provision on unemployment is still not conclusive, since, as argued by Kugler (1999), it depends on whether the regulation has a greater effect on the exit rates into or out of unemployment. In this regard, a number of studies have tried to investigate the extent to which the level of European unemployment can be influenced by firing costs, although without delivering a clear-cut message.

On the one hand, it is well documented that high firing costs increase the unemployment duration (Saint-Paul, 1994).

On the other hand, firing costs represent an insurance for the workers in the absence of perfect insurance market (Pissarides, 2001). In this regard Pissarides (2001) shows that if there were optimal severance payments there would be not a reduction in the job creation. Moreover, by mandating firing costs, there is some bargaining powers given to workers and the asymmetries between labour and capital are balanced (Buechtemann, 1993). Another hypothesis is that firing costs affect the redistribution between employed and unemployed, or between skilled and unskilled workers (Saint-Paul, 1994). Furthermore, since redundancy payments or firing costs represent a burden for firms, the employers would be encourage to reduce dismissals, which would lead to a decline in the number of unemployment benefit claimed (Booth and Zoega, 2003).

In the UK - the country analyzed in this paper - the redundancy payments depend on the tenure at work and on the cause (i.e. whether the reason is *fair* or *unfair*). At this issue just *fairly* dismissed workers could legally require the redundancy payment, but after two years only. At the same time, if the employee had been sacked for other reason but her qualification or conduct, she could claim *unfair* dismissal, but after having completed the probationary period only. Currently the British probationary period amounts to one year.

In the framework of asymmetric information, probationary period represents a fixed-length period during which the firm screens the new hire's abilities (Loh, 1994). It is well documented how workers adjust their behavior during probation: They reduce their work absences (Ichino and Riphahn, 2005, Riphahn and Thalmaier, 2001), self-select in those job which are suited for (Loh, 1994) and accept lower wages (Wang and Weiss, 1998).

Although the probationary period interpretation as screening devices and the workers adjustment behavior in term of absence has been empirically tested, little is still know about the equilibrium outcome between workers and firms. The present work, hence, analyzes this adjustment process by investigating whether firms respond to the 1999 British Unfair Dismissal Reform, which halved the probationary period from two years to one year.

This reform was previously analyzed by Marinescu (2009),[1] although the relevance of her contribute to the economic literature some question for further work

[1]Further details in section 4.

arise in this study. Does the probationary period end lead to an effective decrease in the dismissal probability? Do the result be sensitive to any variation in the control group definition? Is the 1999 Unfair Dismissal Reform effect homogenous among industries? Is the 1999 Unfair Dismissal Reform effect homogenous among the workers skill? Put in other words: Do low-skilled workers react as high-skilled workers to the probationary period shift? This paper attempts to fill this gap, by applying a typical treatment evaluation setting, namely Difference-in-Differences and Regression Discontinuity Design, in a timing-of-events framework on data from the UK Labour Force Survey (LFS), covering the 1997-2001 period.

To test for the economic impact of a probationary period, we define as *treated* the workers whose tenure is between 12 and 24 months, since they are the group directly affected by the reform. Whereas the other groups: i) Workers tenured between 0 and 12 months and ii) workers whose tenure is higher than 24 months should be relatively unaffected by the probationary period shift. However, the reform halved - from two years to one year - the time that firms have to dismiss workers without any sanction. Therefore, aiming at avoiding any potential trial in the event of termination, the employers might decide to discharge the unsuited workers, without any sanction, during the first year - *anticipation effect*. Hence, if we find any effect on those people tenured less than 12 months we can interpret them as an anticipation effect. Conversely, since the reform increases the cost to discharge those workers tenured between 12 and 23 months, we expect that the reform lowers the probability of being dismissed for those workers.

Before the 1^{st} of June 1999, the number of months necessary to qualify (qualifying period) to claim unfair dismissal was 24 months. Therefore just dismissed workers tenured two years or more were entitled to claim unfair dismissal.[2] After the 1^{st} of June 1999 the qualifying period was lowered from 24 to 12 months, thus the dismissed or "made redundant" workers whose tenure was between 12 and 23 months were automatically entitled to claim unfair dismissal, differently from before. The reform implied, therefore, an increase in Employment Protection Legislation (EPL, henceforth) for British workers. Although, the results, in term of subsequent separation, are expected to be very small. The reasons beyond this phenomenon are two: The extremely flexible nature of the UK labour market and the extreme rare event - the dismissals in the UK - we are analyzing. The UK represents, indeed, an exceptional breeding ground [3] therefore the effects are expect to be marginal with respect to other countries such as Italy (Ichino and Riphahn, 2005, Kugler and Pica, 2008),[4] characterized by an higher level of EPL. Kugler and Pica (2008), analyzing a companion reform occurred in Italy,[5] find that increasing employment protection in a country characterized by an high level of EPL, decreases accession and separation by about 10%. Likewise, others UK reforms, such as New Deal in 1999 (Blundell, Costa Dias, Meghir, and Van Reenen, 2004) or Work Family Tax Credit in 2002

[2]The probationary period is an essential requirement, except in cases when the dismissal is automatically unfair like in discrimination cases.

[3]According to the OECD the UK labour market is the the second most flexible country in the OECD, after the USA (OECD, 2005).

[4]According to Bertola (1990), Italy was the country characterized by the highest EPL among the OECD countries. More recently the OECD ranks Portugal as the country with the strictest EPL, followed by Italy, Spain and Greece (Kugler and Pica, 2008).

[5]See section employment 2.2

(Blundell, Brewer, and Francesconi, 2008, Francesconi and van der Klaauw, 2007), seem to have a larger impact in term of job-flows. In our mind the main reason for this difference is the different sample of individuals who the reforms look at: On the one hand unemployed (Blundell, Costa Dias, Meghir, and Van Reenen, 2004) or low-income families (Blundell, Brewer, and Francesconi, 2008, Francesconi and van der Klaauw, 2007), on the other hand dismissed workers whose previous tenure was between 12 and 24 months. As explained in section 6 dismissal in UK represent an extreme rare event.

In addition to contributing to the international employment protection literature by providing some evidence on the way workers and firms respond to an increase in employment protection, this study also offers a rigorous evaluation of the impact of 1999 British Unfair Dismissal Reform on the probability of terminating a job. It contributes to the EPL ongoing debate in two main ways.

First, the main novelty of the present paper is the estimation of the impact of a discontinuity in the presence of *timing-of-events* method, by combining survival analysis techniques with a Regression Discontinuity Design (RDD, henceforth) framework. The empirical strategy, we use, allows us to cope with two main problems. One the one hand, the timing of event method is the only approach to to consistently estimate models of transitions dealing with censoring. On the other hand, RDD deals with unobserved heterogeneity. RDD is a quasi-experimental design in which the probability of being treated is a discontinuous function one or more continuous underlying variables. The probability of being laid off varies discontinuously after the end of probationary period, hence the reform offers two natural discontinuities at the probationary period end: One in the ante-reform period (i.e. 24 months of tenure before June 1999) and one in the post-reform period (i.e. 12 months of tenure after June 1999). Using a RDD on survival data, we investigate the effect and the size of the new probationary period threshold. In so doing, our identification strategy relies on a rather standard assumption made in the treatment evaluation literature (Imbens and Lemieux, 2007). More specifically, in this context we assume that in the absence of the reform no discontinuity would be observed in the hazard of being dismissed or made redundant around the threshold (i.e. the probationary period end).

At a first glance this contribution could seems close to the Lalive, vanOurs, and Zweimüller (2008). The authors study the impact of active labour market policies on the unemployment duration in Switzerland, by offering "direct comparison between the timing-of-events approach and the matching approach". Conversely, the framework we deal with - i.e. the presence of two natural discontinuity in a timing-of-events setting - offers the possibility to combine the two approaches: RDD and timing-of-events.

Second, since the central issue in evaluating the impact of the UK 1999 Unfair Dismissal Reform is to establish the impact of the probationary period variation on firms firing behavior also beyond the threshold. To this end, we compare how the change in survival probability over time (the difference between the the post-reform scenario and the pre-reform scenario) differs between treated and controls, [6] i.e. producing a "Difference-in-Differences" (DID, henceforth) estimation.

[6] More details in section5

Furthermore, since the effect of the reform could be not homogenous among industries, due to their different screening procedure, we investigate reform effects on a specific industry: manufacturing. What impact various economic shocks and pieces of labour legislation had on manufacturing labour turnover? Since the 70s this question has been widely analyzed (Burgess and Nickell, 1990, Wickens, 1978), hence manufacturing was a natural choice to make.

The remaining of the paper is organized as follows. Section 2 reviews the literature. Section 3 gives a brief description of the 1999 UK Unfair Dismissal Reform. Section 4 presents the previous papers which have dealt with the 1999 UK Unfair Dismissal Reform. Section 5 introduces the econometric model. The data and some preliminary statistics are presented in section 6. Section 7 describes the main findings. Section 8 presents the effect of the afore mentioned reform focusing just on the manufacturing industry. Section 9 outlines a large battery of robustness checks and finally section 10 concludes.

2 Literature Review

Two literature strands appear to be relevant to our analysis: One provides evidence of the EPL effect and the other provides a theoretical discussion of employment probationary periods. More precisely, section 2.1 discusses the main findings coming from the theoretical EPL literature. Section 2.2 reviews those coming from the empirical EPL one, while section 2.3 reviews the literature on probationary periods.

2.1 Theoretical Considerations on EPL

With the term Employment Protection Legislation (EPL) the economists refer to a set of instruments which limit both the hiring (e.g. training procedures, rules favouring disadvantaged groups, limitation on the use of fixed term contracts) and the firing procedures (e.g. redundancy procedures, appeal procedures for wrongful dismissal and severance payments, special requirements for collective dismissals) (OECD, 2005).

In the recent years the economic literature has seen a flurry of works, both theoretical and empirical, aiming at explaining the effects of EPL on the employment and unemployment levels, on their dynamic over time, and on the firm profits without delivering a clear cut message.

It is well documented that high employment protection increases the firm dismissal cost (Bertola, 1990, Kugler and Saint-Paul, 2004), hence reducing the propensity to dismiss workers. At the same time employers fear the high dismissal cost, hence firm reduce also their propensity to hire. Therefore EPL reduces both job destruction and job creation, however the net effect on average unemployment and employment is not a priory identifiable (Bertola, 1990). Despite the effects of EPL on short-term unemployment are ambiguous, it is well documented that stricter legislations increase the long-term unemployment (Di Tella and MacCulloch, 2005, Nickell, 1997, OECD, 2005).

Herein, we briefly rehear the theoretical consideration of one EPL specific component: Firing costs, which among all the EPL components - we think- better match the central issue of this paper.

An insightful paper by Bentolila and Bertola (1990) shed light on the impact of firing costs on hirings, while the previous literature was devoted to discharging precudures only. Looking at a partial equilibrium scenario, the authors consider the optimal firm firing and hiring procedures in presence of linear costs - both firing and hiring ones - and modelling the productivity as a random walk. Their results show that firing costs affect both hiring and dismissal policies, although the outflow from the employment is slightly larger compared to the inflow into employment.

Moving to a general equilibrium setting, Ljungqvist (2002) analyzes the firing costs effect on labour demand - more specifically firing taxes that are redistributed to workers in forms of lump sum transfers - finding ambiguous results. Although, his main result shows that when firing costs increase wages or when they reduce labour supply, the overall effect of firing costs on employment is more likely to be negative. To this end the author uses three main framework of employment literature: Search models, matching models and model with employment lotteries. Looking into detail, the search model highlights that in the presence of high firing costs it becomes more costly for the workers search another job - "the workers are 'locked into' their job" (Ljungqvist, 2002) - leading to an higher employment. To fix the ideas a firing costs increase leads to a firm higher burden to discharge unproductive workers, likewise, it also increases the cost of searching another job, both this effects drive to a decline in the dismissal policies. At the same time, as stressed at the beginning of this section, this EPL component reduces, also, the hiring procedure. Ljungqvist (2002)'s calibrations show that higher firing costs lead to a larger drop in the firing policy compared to the hiring one. To fix the ideas an increment in the firing cost leads to a firm higher burden to discharge unproductive workers, similarly, as stressed previously, it also increases the cost of searching another job, both this effects drive to a decline in the firing policy.

The assumptions behind the second model considered by Ljungqvist (2002) - the matching model- are that firms do the searching and the wages are determinated by bargaining *ex post*. Through the lens of the matching model, when the outside option for the firm is weak, the workers will be able to claim higher wages, which drives to less hiring.

The negative implication of higher lay-off are captured by the last model presented by Ljungqvist (2002), a employment lottery model à-la Hopenhayn and Rogerson (1993). Loosely speaking, the idea behind this results is that there are some invisible factors, which in turn imply to have less than full employment combined with full insurance.

Concerning the welfare implication of lay-off taxes, Ljungqvist shows that effect on welfare may be different from the employment one, namely the Pareto optimal equilibrium.

Firing costs may have beneficial effects in the workers specific investments in a framework with *ex post* bargaining (Belot, Boone, and Ours, 2007). In an employment contract neither the firm nor the worker could protect their specific investments. In an *ex-post* setting this may lead to an hold-up problem where workers under-invest. Therefore, a firing costs augment may have indirect effect on specific investments. Broadly speaking, since firing costs reduce lay-off, they may length tenures, which in turn rise the period for specific investments. Letting alone the fiscal externalities, Belot et al. find a possible welfare gain. The authors show that a separation determines a fall in tax base and hence an increase in unemployment insurance premiums leading to an higher social return compared to the private one. Furthermore, Belot, Boone, and Ours (2007)' model shows that the effects may vary across different workers groups. More specifically, the same level of firing costs may generate winners - workers who face the hold-up problem - and losers- who do not care about the specific investments. Similarly to Belot, Boone, and Ours (2007), the different effect across different groups have been analyzed by Kugler and Saint-Paul (2004). The authors study the firing costs effects on discrimination against unemployed job seekers. The underline idea workers can be hired either from the employed pool (job-to-job transition) of from the unemployed pool. The pool could be used by the firms as a signal of employees productivity, this highlights a lower productivity for the unemployed compared to the employed. This make firm more reluctant to hire people from the unemployed pool, leading to an higher difference in employment probabilities between insiders an outsiders.

2.2 Empirical Studies on EPL

Since the seminal paper of Lazear (1990) empirical analysis aiming at providing evidence of the EPL effect have spurred.

Using a dynamic model on cross-sectional aggregate data of 10 OECD countries, Bertola (1992) finds that job security provision does not bias labor demand toward lower average employment at a given wage. On the other hand, Grubbs and Wells (1993), using cross-sectional data on 11 EC countries for the late 80s, find that stricter provisions are negatively correlated with employment. These mixed results could be driven by the nature of cross-sectional data, which might be subject to omitted variables biases, simultaneity problems, and potential endogeneity of regulation.

Even those studies which addressed some of these problems using pooled time-series or panel data have delivered a clear-cut message. Using pooled time-series data on 22 OECD countries over 29 years, Lazear (1990) finds that the severance of payments and advance notice requirements reduce employment. Using data on OECD countries from the 1960s to the 1990s, Nickell, Nunziata, and Ochel (2005) find that shifts in labour market institutions can explain unemployment across OECD countries, although EPL is found not significant. Although very valuable, such strand of literature may still be plagued by omitted variable biases as omitted factors vary over time and therefore may not be captured by country and time fixed effects.

A possible alternative approach is to examine the impact of variation in statuary firing cost within a single country. Recently, several studies using micro data have studied the impact of Employment Protection Legislation on changes in regulation for within a single country. Kugler (1999) explores the impact of the 1990 Colombian Labour Market Reform, which decreased firing costs on worker turnover (exit rates into and out of employment). Using a quasi-experimental setting on repeated cross section from the Colombian National Household (NHS) and controlling for possible selection bias into the formal and informal sector, the author compares the exit rates of formal and informal workers who are affected differently by the labour market reform, but subject to the same non-treatment shocks. She finds that the aforementioned reform increases the rate into and out of employment by over 1% for formal workers when compared to the informal ones.

Kugler and Pica (2008) study the impact of dismissal cost on worker and job flows on the Italian Labour market. Using administrative data from the Italian Social Security Institute (INPS), the authors examine the effect of the 1990 Italian Reform which increased employment protection for workers under permanent contracts in firms with less than 15 employees relative to those in firms with more than 15 employees. Using a Difference-in-Differences approach, they find that accessions and separations decreased after the reform by about 10% for both genders in small firms compared to larger ones. Moreover, they find that employment changes fell by about 15% in small firms after the reform. Regarding the impact of the reform on firms' external margins of adjustment: Entry and exit rate; the authors find that the reform lead to a statistically significant decline in the small firms entry rate compared to the larger one, by 34%. Moreover, small firms, according to the authors' evidence, are more likely to exit the market after the reform compared to large firms. Autor, Donohue, and Schwab (2006), using regional and temporal variation on Current Population Survey (CPS) from 1978 to 1999, analyze the effects of wrongful discharge protection on employment and wages, adopted by the U.S. states courts. The authors find a negative impact (from 0.8% to 1.7%) of one wrongful discharge doctrine, the implied-contract exception, on states' employment-to-population rates.

2.3 Employment Probationary Periods

In 1999, the OECD provided some indicators aiming at assessing the level of job protection among the most developed countries. The summary index drawn up by the OECD relies on three main components: a) Difficulty of dismissal (i.e. the legislative provision establishing the conditions under which a dismissal is fair) b) Procedural inconveniences that the employer may face in the potential trial in case of termination c) Notice and severance pay provision. As already mentioned in section 1 just workers who has completed the probationary period could claim *unfair* dismissal, hence this paper focuses on the core component of regulation protection against dismissal (a).

From an economic point of view, probation plays a relevant role in firms behavior for two main reasons. The first is the so-called "screening effect": Probation serves as check for the employee quality when this information is unavailable before hiring. Therefore the unsuited workers could be discharged at a low cost. The second reason is the so-called "sorting mechanism" (Loh, 1994): Trial period could be used by the firms to discourage poor workers from applying to jobs which they are potentially unsuited for. Furthermore, in some countries, like the USA, during probationary periods the workers do not enjoy some rights which are guaranteed just after the seniority, such as access to health insurance or to pension plans.

From the theoretical perspective, the literature has often compared probationary periods versus recontracting employment schemes (Sadanand, Sadanand, and Marks, 1989). Studying the determinants of the optimal length of probationary periods, Wang and Weiss (1998) analyze the relationship between probation and wage-tenure profiles. The authors, comparing probationary periods jobs (i.e. jobs which start with probationary periods) with non-probationary jobs, find that those jobs which start with probation tend to have lower wages at the beginning, but, also, their wage-increase tend to be higher after probationary period completion. This theoretical study has been empirically confirmed by Loh (1994), who using 1981 cross-section data on last hired of 1881 firms, finds evidence of self-selection into probationary jobs and positive correlation between probation and wage-tenure profiles.

From the empirical perspective, the literature analyzes the relationship between probationary period and workers absenteeism (Ichino and Riphahn, 2005, Riphahn and Thalmaier, 2001). Riphahn and Thalmaier (2001) find evidence of large jumps in terms of absenteeism, at the end of probation. To this end, the authors use full sample of employees in new employment situation, dividing the workers in three main categories: Blue collar, white collar and white collar public sector employees.

Their evidence has been confirmed by Ichino and Riphahn (2005). The authors, using weekly observations for 545 men and 313 females hired as white-collars workers in a large Italian bank between January 1993 and February 1995, find evidence of a large increase in the number of absence days, after the probationary period completion

More recently, Kersley, Alpin, Forth, Bryson, Bewley, Dix, and Oxenbridge (2005) have investigated on the relationship between the probationary period and higher employer's monitoring effort. Using Workplace Employment Relations Survey (WERS 2004), Kersley et al. show that between 1998 and 2004, there has been no substantial change on the recruitment efforts. The authors use as a measure for the recruitment efforts the tests submit by the employer to the new hired. However, the authors find an increase in the performance appraisals used after the reform: while 73% of employers used them in 1998, 78% did so in 2004.

3 The 1999 Unfair Dismissal Reform

According to the OECD, the most flexible countries are the USA and the UK, while Southern European Countries are ranked as the strictest ones.

While the USA labour market is characterized by the "employment at will" legislation – the unsuited workers could be legally discharged by the employers whenever they want and for whichever reason, i.e. "at will"– the European labour market is characterized, by contrast, by a generally stricter job security legislation. Notwithstanding, in the USA, the majority of employment contract are characterized by the "at will" rule, even in the USA, a workers could not be dismissed for reasons such that their ethnicity or for their gender - antidiscrimination laws. On the contrary, Europe is characterized by a stricter labour market regulation, where the employer could discharge the unsuited workers just for their misconduct or their capabilities: i.e. for "fair" reasons. However, even in the most protective "European style" employment protection is not granted from the beginning, but after a amount of period required by law: the so-called *probationary period* or *qualifying period*.

Even though the institution of the qualifying period in the UK dates back to the early 1970s, its length changed several times in the last 20 years.[7] This paper focuses

[7]For further details we address the interested reader to Davies and Freedland (1993)

on the last probationary period change, introduced by the New Labour legislation came in 1999. In that occasion, it was lowered from 24 to 12 months by the 1999 Unfair Dismissal and Statement of Reasons for Dismissal (*Variation of Qualifying Period*) Order.

A more flexible labour market organization was the justification used by the new government for the probationary period change. [8] It is worth stressing that the variation of the probationary period was just one of the numerous reforms implemented by the new government. Perhaps the best known was the implementation of the National Minimum Wage in April 1999. The literature is not quite conclusive about the effect of the National Minimum Wage introduction. In fact, Stewart (2004) finds no effect on the labour market, while Arulampalam, Booth, and Bryan (2004) find an increase in training and monitoring due to the introduction of the Minimum Wage. Moreover, Low Pay Commission (2003) shows that spillovers may have taken place on the wage distribution up to the first decile.

In this context we chose to follow the explanation of Low Pay Commission (2003), looking only at workers above the first decile of the wage distribution.

An additional problem may be the fact that the female labour supply may have been particularly affected by the introduction of parental leave and dependent care leave(Employment Relations Act 1999, and Maternity and Parental Leave Regulations 1999) and sex act discrimination (Sex Discrimination (Gender Reassignment) Regulations 1999). We will check the effect of the 1999 Unfair Dismissal Reform by gender, in order to find out whether the results are female driven.[9]

Finally, the Employment Relations Act 1999 increased the limits on the awards workers who win a trial for unfair dismissal can get at court. However as argued by Marinescu (2009), the previous limit was already not binding: 95% of the awards workers obtained in 2003.

4 Previous works on the 1999 UK Unfair Dismissal

Up to our knowledge there exists just one paper which deals with 1999 UK Unfair Dismissal Reform: Marinescu (2009). This section aims at explaining her analysis and at showing in which way our paper could be considered a further contribution to the literature.

Using the Two Quarter British Labour Force Survey (LFS) from 1996 to 2004, Marinescu (2009) evaluates the effect of the 1999 UK Unfair Dismissal Reform, which halved the probationary period from two years to one year. She evaluates the effect of the already mentioned reform using a Cox Proportional Hazard Model comparing the difference in the propensity of being laid off between the controls (i.e. all the individuals whose tenure is higher than 24 months) and two separate treatment groups (i.e. those whose tenure is less than 12 months and those whose tenure is between 12 and 24 months). She finds evidence that the British probationary shift led to a decline in probability of being laid off by 19% for workers with 0 to 11 months tenure and by 26% for workers with 12 to 23 months tenure.

[8] For further details we address the interested reader to www.dti.gov.uk/er/fairness/

[9] The estimates are not significant for women, the results have to been added.

The first step of our empirical research is to replicate Marinescu's analysis. Aiming at outlining similarities and differences in the data and in the definition of the variables table 1 reports the replicated results using Marinescu's definition of treated and controls in a sample of individuals aged between 20 and 50 years old. In this regard the left panel of table 1 presents the results using LFS from 1996 and 2004 trying to reconstruct the data as close as possible to Marinescu's definition.[10] The right hand side replicates her results using our analysis sample.

Table 1: Replication of Marinescu's results

	Data between 1996 and 2004	Data between 1997 and 2001
0 to 11 months of tenure	-0.249***	-0.274*
	(0.088)	(0.164)
12 to 23 months of tenure	-0.328***	-0.420**
	(0.101)	(0.206)
Observations	432823	53332

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

The estimated coefficients represent the interaction between tenure and after dummy in a Cox Proportional Hazard Model. Closely related to Marinescu this specification includes all her controls i.e.: 2 cohort dummies, 2 education dummy, female dummy, white dummy, 8 occupational dummies, 9 industry dummies, private sector dummy, 11 region dummies, quarter-year dummies.

Our evidence shows that, using treated and controls according to Marinescu the probationary period shift lead to a stark decrease in the dismissal hazard. Looking at our results, using data from 1996 to 2004, we find that the 1999 UK Unfair Dismissal Reform drove to a decrease significantly in the dismissal probability by about 25%, for those tenured between 0 and 11 months, and by about 33% for those tenured between 12 and 23 months. This results are significantly higher than Marinescu's ones, in this regard we impute the difference to the different sample. Conversely to her we leave out from the sample those people between 16 and 19 and those older than 50 years hence we are capturing the reform effect to those people who are mainly attached to the labour market. Looking at the right hand side table panel, we can notice that between 1997 and 2001 the effect of the reform was strikingly higher than in the time span 1996-2004. We find evidence that the British probationary shift led to a decline in the lay-off probability by about 27% for those tenured less than 12 months, by about 42% for those tenured between 12 and 24 months. It is worth pointing out that moving the observational period, in

[10]Letting alone the age difference between our sample and Marinescu' one and the difference in the analysis time-span, from our understanding in her sample Marinescu includes all the individuals who are permanently employed and working more than 16 hours in the first wave not in all waves.

addition to leading to higher coefficients it also decreases the significance for those tenured less than 12 months.

Although the relevant research question Marinescu (2009) is investigating on, we tried to contribute to literature on a purely methodological base. Since the treatment assignment is not randomly selected, but defined according to some observable characteristics, the job tenure in our context, some pre-treatment factors may affect both the treatment status and the potential outcome.

Table 2 reports some descriptive statistics for the treatment and the control group, defined according to Marinescu (2009), using UK LFS from 1997 to 2001 . Treatment group appears to be younger, less educated, more likely to be nonwhite and there is an higher percentage of females in that group. [11]

Table 2: Covariate means and observational control samples

Variable	Controls	Treated
Age	33.24	32.93
Female	0.54	0.55
Black	0.01	0.01
Other Ethnicity, different from Whites	0.03	0.02
Low - Educated	0.21	0.23
High-educated	0.30	0.33
Married	0.67	0.65
Number of observations	12309	41023

Dehejia and Wahba (2002) using different comparison groups on LaLonde's data found evidence that matching treated and controls, using the Propensity Score matching, in a non-randomized study leads to results close to a randomization. Regarding the circumstances where propensity score matching provides more reliable estimates, compare with regression, the literature does not deliver a clear-cut message (Angrist and Pischke, 2009). In this context we find evidence that Propensity Score matching may be a way to "correct" the treatment effects estimation, controlling for the existence of these confounding factors, based on the idea that the bias, due to the selection, is reduced when comparison of outcomes is performed using treatment and control who are similar, conditional on a set of covariates (Dehejia and Wahba, 2002, Rosenbaum and Rubin, 1983). Hence, we adopt the following procedure:[12] First, we define as treated those workers tenured between 12 and 24 months after the reform enactment, and as controls all the others (i.e. those workers whose tenure is higher than two years, those workers whose tenure is lower than one year and those whose tenure is between 12 and 24 in the pre-reform scenario). Second, we match, via Propensity Score matching technique (Rosenbaum and Rubin, 1983) on a set of observable characteristics namely: year of birth, gender, ethnicity, education, region of residence, job industry. Third, once the treated and the controls are similar on a set of covariates, we can deal with the reform evaluation. To

[11] We test, using a simple t-test, whether the difference between treated and controls appear to be significant. We find evidence that the two group are significantly different at level 5%.

[12] Further details in section 5.

this end we perform a Difference-in-Differences estimator.[13]

Furthermore, this paper contributes to the literature identifying the reduction, in term of dismissal hazard, due to probationary period end. To this end we carry out the combination between Regression Discontinuity Design and Survival Data Analysis, [14] as already mentioned in section 1, to the best of our knowledge, would be the first time use in the literature.

5 Identification

This paper assesses the identification of the reform impact using two different approaches typical of the evaluation analysis, namely Conditional Difference-in-Difference approach and Regression Discontinuity Design, which are explained in the following sections.

5.1 The Conditional Difference in Differences Approach

Let h_{it}^{D} be the potential outcome of interest for individual i (i.e. the hazard function at firm, in our setting) at time t in state D, where $D = 1$ if exposed to the treatment (i.e. tenure between 12 and 24 months) and 0 otherwise. Let treatment take place at time t (from June 1999, in this context). In this setting one wants, ideally, to observe for the same individual i both states, treatment $(D = 1)$ and no treatment $(D = 0)$, in order to capture the impact of the treatment for the same individual, which is given by: $\Delta_{it} = h_{it}^{1} - h_{it}^{0}$. Unfortunately, this is impossible. Therefore, the issue becomes to find a counterfactual that needs to be as close as possible to the unobserved outcome. The overwhelming majority of the econometric literature uses, if provided with a convenient control group, estimates of the average effect of the treatment on the treated (ATT).

For the rest of the paper, the individual index will be dropped to reduce notation.

Given this consideration, in order to identify Δ_t, we need to make the following assumptions (Abadie, 2005):

$$E[h_t^1 - h_t^0 | X, D = 1] = E[h_t^1 - h_t^0 | X, D = 0] \tag{1}$$

Assumption 1 is the crucial for the DID model identification. It requires that conditional on a set of covatiates X, the average outcomes for treated and controls would have followed parallel paths in absence of the treatment.

As noted by Abadie (2005) when $E[h_t^0|X, D = 1] = E[h_t^0|X, D = 0]$ assumption 1 fall in the so-called "selection on observables" (or CIA) requirement, which can be written in the following fashion:

$$H_t^1, H_t^0 \quad \perp \quad D|X \tag{2}$$

Assumption 2 requires that conditional on observed characteristics X selection bias disappears.

[13]More details in section 5.1.
[14]More details in section 5.2.

If assumption (2) holds the effect of the treatment on the treated conditional on set of covariates X could be written as:

$$\begin{aligned} E[h_1^1 - h_1^0 | X, D = 1] &= \{E[h_1 | D = 1] - E[h_1 | D = 0]\} + \\ &- \{E[h_0 | D = 1] - E[h_0 | D = 0]\}. \end{aligned} \tag{3}$$

where $t = 0$ before June 1999 and $t = 1$ after June 1999, respectively.

However, as argued in section 4 we are concerned whether condition 2 may hold. Thus, in order to correct for selection bias based on observable characteristics we perform a matching procedure.[15] Matching is widely used in the Policy Evaluation literature to "correct" the treatment effects estimation, controlling for the existence of these confounding factors. This procedure is based on the idea that the bias, due to the selection, is reduced when comparison of outcomes is performed using treatment and control who are similar, conditional on a set of covariates (Dehejia and Wahba, 2002, Rosenbaum and Rubin, 1983).

To carry out the Propensity Score matching implementation we need to make the following further assumption:

$$0 < \Pr(D = 1 | X = x) < 1, \ \forall x \in \widetilde{X} \tag{4}$$

Assumption 4 requires that the propensity score support for the treated is a subset of propensity score support for the controls. The quantity $\Pr(D = 1 | X = x) = p(X)$ represents the Propensity Score (Rosenbaum and Rubin, 1983), thus if assumption (1- 3 -4) hold we can rewrite ATT in the following fashion:

$$\begin{aligned} ATT &= E[h_1^0 - h_0^0 | D = 1] = \\ &= E_{p(X)}[(E(h_1^0 | D = 1, p(X)) - E(h_0^0 | D = 1, p(X) | D = 1] = \\ &\overset{CIA}{=} E_{p(X)}[(E(h_1^0 | D = 1, p(X)) - E(h_0^0 | D = 0, p(X) | D = 1] = \\ &= E_{p(X)}[(E(h^0 | D = 1, p(X)) - E(h^0 | D = 0, p(X) | D = 1] = \\ &\overset{1}{=} E[h_1^0 - h_0^0 | D = 0], \end{aligned} \tag{5}$$

Equation (5) states that the evolution of the outcome variable for the treated (D=1) in the event that they would not be treated would be the same as actually observed for the individuals not exposed to the treatment (D = 0). In other words the previous equation states that the average outcomes for treated and controls would have followed parallel paths over time if there had been no treatment, the so-called time-invariance assumption. [16]

Given the previous conditions, we can adopt a typical Conditional Difference −in −Differences (CDID) estimator (Heckman (1997)), which relies on the assumption

[15] With this regard we implement a Propensity Score Matching (Rosenbaum and Rubin, 1983) using the Stata-package psmatch2 Leuven and Sianesi (2003)

[16] Aiming at evaluating the the so-called time-invariance assumption in section C are presented the activity rates, the Gross Domestic Product and the unemployment rate growth during in the analyzes period, see for further details graphics (12)- (14).

that the average treatment effect on the treated could be identified by the difference between average outcomes for treated and controls over time.

$$
\begin{aligned}
\beta_{CDID} &= \{E[h_1|p(x), D = 1] - E[h_1|p(x), D = 0]\} + \\
&- \{E[h_0|p(x), D = 1] - E[h_0|p(x), D = 0]\}
\end{aligned} \tag{6}
$$

In what follows, we briefly explain the identification strategy we use in order to estimate our outcome.

The individuals in our data set are asked for up to five consecutive quarters whether they are employed, and how many months they have been in the current state. They are also asked the year and the month in which they started the current job. From this information, we can construct tenure from their hiring date by the present firm up to the last interview. However, the reason why they left the previous job is missing in the fifth quarter, hence we decided to drop it. In other words, since we aim at calculating the dismissal hazard and we can track it just for the first 4 waves, we drop the last wave. Individuals who abandon the sample are supposed to do so at the end of the quarter covered by the interview. This allows us to calculate the monthly empirical survivor function on the basis of complete durations of entrants and surviving non-censored samples, it is worth noting that we can follow an individual for up to 12 months. It is worth pointing out that we are concerned about the 1999 UK Unfair Dismissal Reform enacting date, in fact while according Marinescu (2009) the enacting date was June 1999, according to Smith and Morton (2001) the enacting date was October 1999, which would be especially relevant for the construction of the after-reform cohort. Thus for the construction of our cohorts we decide to start from the third quarter (i.e. September/November) of each year: 1997, 1998, 1999. On the basis of this information we define three main groups that can be followed for up to 12 months:

1. *The group of those individuals who is in probation in the new regime.* More precisely, those workers who start the first year of employment, particularly those whose tenure is between 0 and 3 months at the time of the first interview. This time span allows us to have exactly 12 months tenure for the first group, at the end of four waves, hence when the new probationary period ends.

2. *The group of those individuals who switch between probation and non probation due to the reform.* More precisely, those workers who start the second year of employment, particularly those whose tenure is between 12 and 15 months at the time of the first interview. This time span allows us to have exactly 24 months tenure for the second group, at the end of four waves, hence the former probationary period end.

3. *The group of those individuals who have never been in probation.* More precisely, those workers who start the third year of employment, particularly those whose tenure is between 24 and 27 months at the time of the first interview. This time span allows us to have exactly 36 months tenure, at the end of four waves. Even though, in principle, we may use as controls workers tenured more than three years (i.e. given that the third tenure year does not

represent any relevant tenure deadline), we decide to maintain the same observational period, aiming at comparing the observational time span among the thee groups.

Ideally we can identify the effect of the reform, for each group g, looking at the difference in terms of two different survival function cohorts, one pre-reform and one after-reform, but belonging to the same group. However, the result of this difference is a biased estimate of the effect of the reform, since it includes some confounding factors, such as the trend in firing between two different years.

However if assumption (3) holds we can rewrite equation (5):

$$\begin{aligned}
\beta_{CDID} &= E[h_{g99}|p(X), D = 1] - E[h_{g99}|p(X), D = 0] = \\
&= E[h_{g99}|p(X), D = 1] - E[h_{g98}|p(X), D = 0] + \\
&+ E[h_{g98}|p(X), D = 0] - E[h_{g99}|p(X), D = 0] = \\
&= E[h_{g99}|p(X), D = 1] - E[h_{g98}|p(X), D = 0] + \\
&- (E[h_{g99}|p(X), D = 0] - E[h_{g98}|p(X), D = 0]) = \\
&\stackrel{1}{=} [h_{g99}|p(X), D = 1] - E[h_{g98}|p(X), D = 0] + \\
&- (E[h_{g98}|p(X), D = 0] - E[h_{g97}|p(X), D = 0]) \qquad (7)
\end{aligned}$$

Where 97, 98, 99 define the cohort year, more specifically 1997, 1998, 1999. Using a typical evaluation approach, the former difference identifies the effect the reform, the latter the bias. In what follows we present the second identification strategy, namely Regression Discontinuity Design.

5.1.1 Matching Algorithms

As stated in section 1, a possible source of bias might arise when treated and control group systematically differ along several dimensions which are relevant to the outcome. The so-called matching estimators are useful when selection into treatment is on observables only. Among the great variety of matching estimators, we choose the *Propensity Score* (Rosenbaum and Rubin, 1983), which rather than matching the regressors matches the conditional probability of receiving treatment given x, denoted in section 5.1 p(x).

Defining as treated those individuals tenured between 12 and 24 months and as controls all the others,[17] we perform a nearest neighbour matching on the propensity score with no-replacement using as covariates: Year of birth, ethnicity, gender, education, region and industry. Furthermore, we use as measure of precise conditioning, the caliper imposition of 0.01, which represents the maximum allowed distance between the treated and the controls of 1%, whether the distance between the treated and the controls would exceed the caliper, the pairs would be automatically discharged. [18]

[17] Particularly the matching has been realized comparing the treated and each control group one at a time

[18] The covariates comparison between matched and unmatched samples is available upon request, while the estimated propensity score is presented in figure 21 .

5.2 The Regression Discontinuity Design Approach

This section presents the basic feature of regression discontinuity analysis following the discussion in Hahn, Todd, and Van der Klaauw (2001).

Despite this approach goes back to the 60s (Campbell, 1969), quite few papers have relied on it until relatively recently. Following the notation of potential outcome approach to causal inference, let (h_1, h_0) be the two potential outcome, one would experience by being treated or not being treated. In our setting, h_1, h_0 represents the hazard [19] of being laid off for the treatment and the control group respectively. The causal effect of the reform on the hazard of being laid off would be potentially captured by the difference: $h_1 - h_0$. However, as noted in section 5.1 since the counterfactual 'policy-off' situation can never be observed in the 'policy-on' situation (i.e. we could not observed both status for the same individual at the same time), we have to use alternative strategies to estimate the effect using a suitable comparison group.

Let L be the binary variable denoting the layoff status, with $L = 1$ for those individuals who has been dismissed, and $L = 0$ otherwise. According to the evaluation setting, the identification of a treatment effect could be addressed using a *Regression Discontinuity Design* when the probability of receiving a treatment is a discontinuous function of one or more continuous underline variables. In our setting, the probability of being laid off varies discontinuously with the observable variable tenure $\mathcal{T}$. Formally we can we rewrite the previous statement in the following way:

$$\Pr\{L = 1|\overline{t^+}\} \neq \Pr\{L = 1|\overline{t^-}\}$$

Following Battistin, Brugiavini, Rettore, and Weber (2009) and Imbens and Lemieux (2007)' s notation, $\overline{t^-}$ and $\overline{t^+}$ refer to those individuals whose tenure is slightly lower or slightly higher than the probationary period threshold. In so doing our identification strategy relies on a rather standard assumption made in the treatment evaluation literature (Imbens and Lemieux, 2007): We assume that in the absence of the reform no discontinuity would be observed in the hazard of being dismissed or "made redundant" around the threshold. In other words this means that only treatment status accounts for a possible discontinuity in L at the cutoff point, as there are no other factors accounting for such a discontinuity.

Depending on the discontinuity size, the design could be *fuzzy* or *sharp*. More specifically, a *sharp* design, is characterized by the fact that the selection process is a deterministic function $\mathcal{T}$. To fix the ideas, when the individuals are deterministically assigned to the treatment group whether they are all one one side of a cut-off score t^* in our context (i.e. $\mathcal{T} \geq t^*$), while all the other are, analogously, assigned to the control group. When the probability of being treated is not a deterministic function of reaching the threshold level, according to literature the RDD design is called *fuzzy*.[20] Since the treatment status is a deterministic function of one or more covariates - the tenure in our case - according to the treatment evaluation definition, the hazard of being dismissed fits neatly the sharp design.

[19] "The hazard rate is defined as the probability per time unit that a case that has survived to the beginning of the respective interval will fail in that interval" (Lancaster, Econometric Society Monographs No. 17).

[20] See Trochim (1984) for further details.

Imbens and Lemieux (2007) shows that, as long as the continuity assumption holds, the average casual effect of the treatment is given by the following outcome:

$$\lim_{t \downarrow \bar{t}} \mathbb{E}[h_i | T_i = t] - \lim_{t \uparrow \bar{t}} \mathbb{E}[h_i | T_i = t]$$

which is interpreted as the average casual effect of the treatment status i at the discontinuity point:

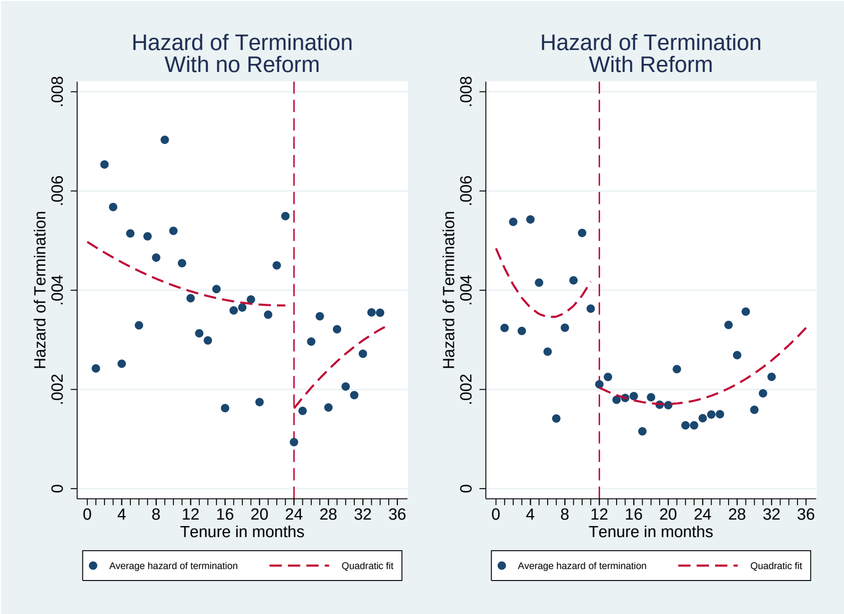
$$ATE = \mathbb{E}[h_1 - h_0 | T = t]$$

In our context we could draw two points of discontinuity represented by the 12th and 24th months of tenure. Since the pre-reform formal length of the probationary period was 24 months, therefore 24 represents a point of discontinuity. Furthermore, after the reform the legal probationary period was shorten to 12 months. Therefore 12 represents another discontinuity.

In what follows, figure 1 depicts the relationship between the dismissal hazard function at tenure level for two pre-reform cohorts (1997 and 1998, respectively - on the left hand side), i.e. when the probationary period length was 24 months, and for two post-reform cohorts (1999 and 2000, respectively - on the right hand side), i.e. when the 1999 UK Unfair Dismissal Reform was enacted, halving the probationary period from 24 to 12 months.

The picture neatly shows that in both cases the probationary period end corresponds to a discontinuity in the dismissal hazard, i.e. with a large drop in the hazard of being laid off. In this regard we can aptly see a large drop at the 24th month of tenure for the left hand side (ante-reform scenario) picture and a large drop, corresponding to the 12th month of tenure for the right hand side (post-reform scenario).

Figure 1: Estimation of the causal effect of the reform on the hazard of termination



The dashed red line represents, in both cases, the approximation, which we think, better empirically capture the phenomenon. In what follows we briefly explain the empirical strategy to estimate the drop in term of dismissal hazard due to the probationary period end.

Let D_1 a dummy variable denoting the treatment status (after the reform), with $D = 1$ for those individuals tenured more than 12 months, and $D = 0$ otherwise.

Let D_2 a dummy variable denoting the control status (pre-reform scenario), with $D_2 = 1$ for those individuals tenured more than 24 months, and $D_2 = 0$ otherwise. The effect of the reform on the hazard of being laid off could be empirically captured by the following equation, which , we think, represent a good approximation of the data: [21]

$$\begin{aligned} h_i &= \alpha_i + \gamma_i \cdot Tenure + \gamma_{i2} \cdot Tenure^2 + \\ &+ \beta_i D_i + \beta_{i2} D_i \cdot Tenure + beta_{i3}(D_i \cdot Tenure)^2 + \varepsilon \end{aligned} \tag{8}$$

where D_i represents the treatment status, respectively D_1 the no-reform scenario, while D_2 represent the reform scenario. More specifically, we run the two regressions,

[21]In what follows we show graphically the hazard distribution and the model, we use to estimate the reform impact on the probability of job termination. See graphics 20 for further details.

the pre-reform and the post-reform scenario, separately, pooling two years for each regression, particularly 1997 and 1998 for the pre-reform scenario; 1999 and 2000 for the post-reform scenario.

6 The Data

The data we use come from the rotating panel of the British Labour Force Survey (LFS)[22] for the period 1997-2000.

The LFS is conducted every quarter since 1992[23] on all individuals aged 16 or older of around 60,000 households. One fifth of the sample is renewed quarterly: hence we can observe the labour market situation of the individuals for up to five waves. However, since we can determine the termination reason up to the fourth one, we stop our observation setting to the fourth wave.

The UK LFS has a number of advantage for the analysis of the probationary period change effect. First, by focusing on extreme rare event, such as dismissal in UK, the LFS with such large sample, allows us to work with a reasonable sample size. Second, the detailed employment information included in the LFS allow us to determine job tenure with high precision. The LFS provides two retrospective sections: one regarding the labour market situation in the previous year and another regarding the labour market situation in the previous three months. In addition to these two sections, there are also some retrospective information regarding the current situation, such as how long the individual has been in the current job, or how long the individual is looking for a job.

However, this dataset has at least two important shortcomings. First, it is a short panel, hence we can keep track a limited number of transitions. Second, we can not identify those spells lasting less than three months. Since the job termination reason is a central issue in the analysis, this means that short tenure patterns cannot be examined.

Only permanent employees working more than 16 hours a week could claim unfair dismissal. We do not consider temporary workers for two main reasons: On the one hand, until 2002 (Fixed Term Employees - Prevention of Less Favourable Treatment - Regulations 2002) fixed-term contract and permanent ones have different treatment with respect permanent ones.[24] Moreover, in our sample the vast

[22] A brief description of the dataset and the covariates is included in appendix A.

[23] From 1979 to 1983 the LFS was carried out every two years. Following a change in the requirements of the EC Regulation, from 1984 to 1991 it was an annual survey. In 1984, the ILO definition of unemployment was adopted in the UK Labour Force Survey. Source: http://www.statistics.gov.uk.

[24] The regulations provide protection for fixed-term employees in a number of areas:

- The right not to be treated less favorably than a comparable open contract employee in respect of contractual terms and conditions or being subjected to any other detriment on grounds of status as a fixed-term employee;
- The right to a statutory redundancy payment where the expiry of a fixed-term contract gives rise to a redundancy situation;
- Limiting the use of successive fixed-term contracts unless the continued use of a fixed-term contract can be justified on objective grounds;
- The right to be informed of open contract vacancies within the organisation.
  Source: `http://www.opsi.gov.uk/si/si2002/20022034.htm`

majority of them have a tenure lower than 24 months which makes identifying the probability of being fired after 2 years difficult, therefore we decide to stress our attention just to permanent workers, reducing the sample by 29.22% of individuals (equal to 32.89% of the job spells).

Next, we also exclude from our sample individuals who are aged between 16 and 19 years old given the instability of their attachment to the labour market, and people aged 50 or older, due to the relative small probability to be dismissed at that age and due to the importance of transition to retirement at that age.[25] This leaves us people age 20 to 49 years old, equal to 51.98 % of the original individuals sample (or 45.27% of the original job spell sample). Furthermore, we drop 7 individuals (equal to 20 job spells) because we could not determine the industry in which they were employed, or their occupation.

The final sample consist of 41213 job spells for 22349 individuals. Tables 10 and 11 summarize the deletion that yields to the final sample.

In this analysis tenure computation is a central issue. Therefore we rearrange the data aiming at constructing consistent job spells histories[26]. The tenure computation is obtained comparing the job situation at three subsequent quarters: t, t+1, t+2. We classify an individual in the same spell if among this three waves the following conditions apply:

- she does not change the industry where she works;
- she does not change the hiring date;
- the variable "Reason why you left the previous job (redylft)" is missing.[27]

Where there is not an exact match of job characteristics among the three quarters (i.e. the industry is different but the hiring date is the same or viceversa and using others variables we can not keep track of any job change) we tried to reconcile the spells where possible by relying on information from prior or subsequent spells.

For those workers who change their employment and do not declare the new hiring date (or the date when they left the previous job), we impute the new hiring date to the previous interview month[28]

[25]See appendix for more details C.

[26]When we use the term "consistent histories" we adopt the Maré (2006) definition "When I refer to "consistent" work-life histories, I have in mind two quite different meanings. The difference between these meanings is at the heart of the problems of extracting histories from the BHPS data. The first meaning is that the resulting history should be consistent with the responses given by respondents. The second meaning is that the resulting history should be internally consistent, meaning that it is a non-overlapping sequence of spells that accounts for all of the respondent's experience"

[27]We dropped the individual when the hiring date is partially missing (i.e. where month or year information is missing) and can not be detected by relying on information from prior or subsequent spells.

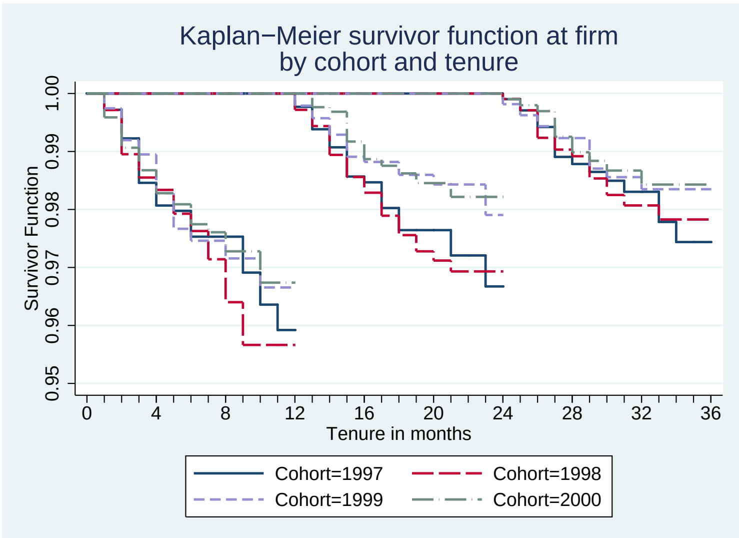
[28]Those individuals represent less than 5% of the sample (4.6%). This imputation strategy could overestimate the new tenure by a maximum of three months. Although, if those individuals are tenured, after the reform, between 11 and 14 months, at the end of the observational window, we dropped them from our sample).

For almost all of those workers, who loose their job we know the reason (more than 90%).[29] Closely related to Marinescu (2009) we divide the type of separations in three main categories: dismissal (dismissed or made redundant), quits (resigned), others (gave up for health reasons, took early retirement, retired, gave up for family or personal reasons, other reason, temporary job finished).

Table 12 reports some descriptive statistics. Looking at this, we can see that the main reason for terminate a job are other types of termination (43.51%), the second one is quitting (38.83%) and after this to be laid off (17.66%). Analyzing our descriptive statistics, we find that the proportion of other type of termination is definitely higher than Marinescu's ones[30], however it is worth pointing out that conversely us in Marinescu's classification "Other types of terminations" leave out: gave up for health reasons, took early retirement, retired, gave up for family or personal reasons, temporary job finished.

The empirical relevance of the 1999 UK Unfair Dismissal is clearly evident from figure 2, which contains the Kaplan - Meier monthly survivor function at firm by cohort and group of tenure in the raw data.

Figure 2: Kaplan - Meier survivor function at firm



The picture depicts the relationship between the survivor function at tenure level for two pre-reform cohorts (1997 and 1998, respectively) and for two post-reform cohorts (1999 and 2000, respectively).[31]

[29]The list of possible reason for terminate a job are: dismissed, made redundant, temporary job finished, resigned, gave up for health reasons, took early retirement, retired, gave up for family or personal reasons, other reason. It is worth noting that in the LFS the individuals declare how they perceive their contract: permanent or temporary. There are some people who perceive their contract as temporary even if it is permanent. This type of distinction is not present in the reason for leaving a job. Therefore we conclude that those people who declare as reason for job termination "temporary job ended" perceived their contract as temporary even if their contract was permanent.

[30]We remind the interested reader that Marinescu's frequencies for each type are respectively: dismissal and redundancies (21.4%), quits (35.4%) and others (22.4%)

[31]To be precise, the survivor function is computed by following for up to 12 months those indi-

We can draw two main conclusions from the raw data: First, the probability to be discharged decreases with tenure. It is clearly evident that the first two groups have (the left and central panel) a lower survival function compared to the third one (the right panel). With this in mind, the picture, neatly, shows that as tenure goes by, the survival probability drop is lower (i.e. it is evident that the biggest survival function drop could be observed in the first group, hot on the heels of the less tenured workers we can observe the survival function drop of the second group and lastly with the lowest fall in the survival function we could observe workers tenured between 24 and 36 months).

Second, the 1999 UK Unfair Dismissal Reform drives an increase in the survival function. Figure 2 highlights a sharp difference between the pre and the post reform cohorts just for those people belonging to the second group of tenure and for those workers tenured more than 32 months. One aspect is worth noting: while in principle the difference for those tenured between 12 and 24 months should be due either to observable characteristics or to the reform, for those tenured 32 months or more it should be due to the observable characteristics only.

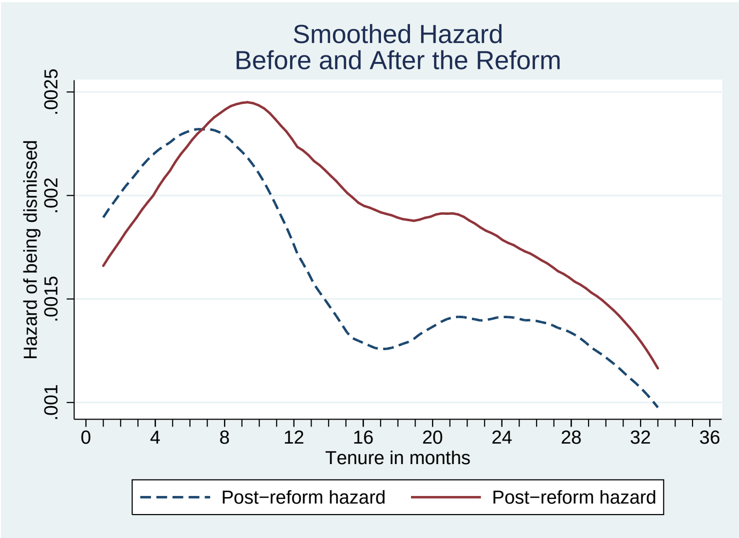
viduals who are tenured between :

- 0 and 3 months (for the left panel)
- 12 and 15 months (for the central one)
- 24 and 27 months (for the right panel)

respectively in the third quarter of each year. The reform was enacted in June 1999 (second quarter), thus aiming at getting rid of any possible confounding factors for the 1999 cohort we chose to define the three years cohorts starting from each third quarter year.

Figure 3 contains the empirical monthly hazard function for job ending by dismissal at tenure level. With this regard the dashed line, i.e. the pre reform hazard, is obtained using the 1997 and 1998 cohort, whereas the line one, i.e. the hazard of being dismissed after the reform implementation, is obtained using the data from the 1999 and 2000 cohort.

Figure 3: Comparison between the pre and the post smoothed hazard of being dismissed



What is most striking is the non-monotonicity of the hazard function in tenure. It is clearly evident that the hazard ia relatively low in the first month at 0.0017 before the 1999 UK Unfair Dismissal Reform implementation (0.0019 after the reform implementation) rising to a peak amounting to 0.0024 at the ninth month of tenure (0.0022 in the seventh month of tenure after the 1999 UK Unfair Dismissal Reform implementation) and sharply decreasing thereafter before leveling off at the level 0.0018 corresponding to 17th month of tenure (0.0014 corresponding to the 16 month of tenure in the post reform scenario). From the 17th month of tenure in the pre-reform scenario (16th month of tenure in the post-reform scenario) it starts slightly to increase before leveling off at the level 0.00185 corresponding to the 22nd month of tenure (0.0014 corresponding to 27th month of tenure) and sharply decline thereafter.

The empirical hazard function confirms the Jovanovic classical model. Jovanovic (1979) predicts that initially for the firms the value of separating is higher than value of waiting to learn more about the real productivity of a match (whose current productivity is low), that means that at the beginning the hazard of termination should sharply increase. After some time, only the most productive matches should remain and therefore the hazard of termination decreases. In particular, Farber (1994), who empirically tested Jovanovic 's ideas, looking at the job termination of young workers using the National Longitudinal Survey of Youth (NSLY), finds that the peak of termination occurs around the third months. However, as previously stated in the raw data which we are working on the dismissal peak occurs at the ninth month of tenure before the reform implementation (seventh month of tenure in the post reform scenario). It is worth pointing out that there are at least three main differences between Farber's data and ours, which may explain the different peak in the hazard functions. Firstly, the different age composition between the two sample: Farber analyzes the labour turnover on a sample of young workers aged between 16 and 30, [32] while we are working on individuals aged between 20 and 49 years old. Secondly, the labour context. Even though, The UK is consider the second country with the lowest employment protection after the USA (OECD, 2005), some differences still exists between the two countries. Thirdly, the different time span between the two analyzes. Whereas Farber time span cover the year between 1979 and 1988, we are working with the year between 1996 and 2000, hence we are subject to different economic cycles.

In addition to the previous ones, we can draw two main conclusions from picture 3 the implementation of the 1999 UK Unfair Dismissal Reform seems to lead to an increase in the probability of being dismissed in the first seven months, while thereafter it seems that the reform drives to a sharp decrease in the probability of being laid off, particularly for those tenured between 12 and 24 months that was apparent from the survivor function in figure 2. However, one aspect is worth noting: The comparison between the hazard and the survivor function (figure 3 and figure 2) shed light on one main evidence while according to the survivor function people tenure between 24 and 32 months were, not significantly different in the pre and in the post reform, from the hazard function, at the first glance it appears that the reforms drives to a decrease, although as previously stated this difference should be due to the observable characteristics only.

In what follows we present the results of our estimation results.

7 Results

In this section we briefly explain the results of the probationary period change on the dismissal hazard. With this regard we will start with presenting the DID results and thereafter the RDD results. We conclude this section comparing our results with Marinescu's ones.

7.1 Conditional Difference in Differences

The estimation procedure we take can be described as follows. First, we define three main groups according to their tenure and we define as treated those individuals whose tenure is between 12 and 24 months of tenure after the reform and as controls all the others, namely workers tenured between 0 and 12 months, workers tenured between 24 and 36 months and workers tenured between 12 and 24 months before the reform implementation.

Second, a relevant issue, which we accounted for in this evaluation setting, is whether the 1999 UK Unfair Dismissal Reform impact is heterogeneous with respect to observable characteristics (see section 4). In our setting the assignment

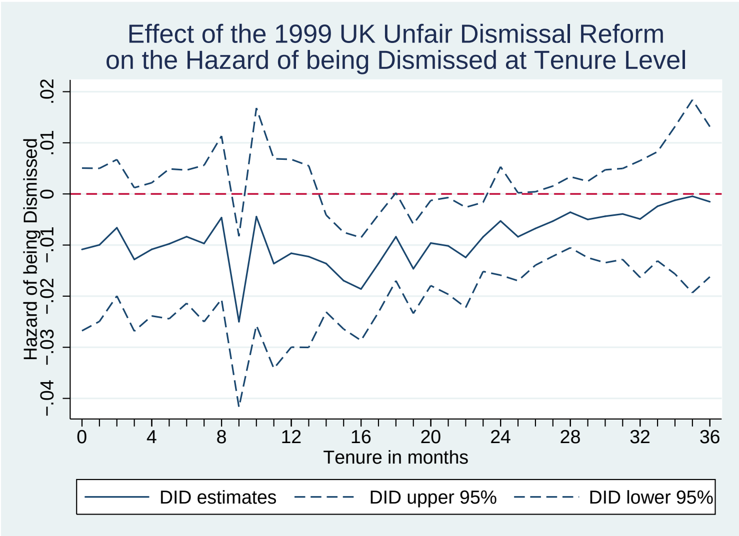
[32] With this regard, Farber's used NLSY data from 1979 though 1988, covering all the individual aged between 16 and 21 in 1979. Hence the final sample contains individuals aged between 16 and 30.

is not random, in fact it depends on the tenure and on the relevant time period (i.e. before or after the reform implementation). One possible way to address this issue is to guarantee that treatment and controls have the same distribution of the relevant characteristics, namely year of birth, gender, industry, region, education and ethnicity. To this end we implement the propensity score matching.

Third, we identify the effect of the reform using a DID estimator. In so doing we relied on a rather standard assumption of the treatment evaluation literature which states that the evolution of the outcome variable for those treated in the event that they would not be treated would be the same as actually observed for the individuals not exposed to the treatment, which seems a quite a plausible assumption. Looking at the crude data (figure 2) it seems that there is no significant differences in the two cohorts before the reform (i.e. 1996 and 1997) implementation for all the groups considerer.

We report our estimation results by way of graphical illustration. Figure 4 displays graphically the estimated effect of the 1999 UK Unfair Dismissal Reform on the hazard of being dismissed, the estimated effect is rounded by a 95% confidence interval.[33]

Figure 4: The effect of the reform on the hazard of being dismissed at firm by tenure



The reform implementation leads to a significant effect whether the confidence intervals around the line do not contain the red dash line corresponding to zero. The reform effect varies according to the tenure level. We observe a general negative trend since the first month, which is progressively declining till reaching the largest drop corresponding to the ninth month of tenure. In that particular tenure level the decline, in term of dismissal hazard, varies between -4% to -0.8% which is also statistically significant at level of 5%. This peak might be due to the fact that according to figure 3 the ninth month of tenure represents the peak of dismissal hazard in the pre-reform period, while after the reform implementation the peak

[33]For the estimation of the confidence intervals has been used a bootstrap procedure with 300 replications, furthermore for the first group of tenure we clustered the estimation by individual.

was anticipated to the seventh month. From the 10th to the 13th month of tenure the picture starkly highlights that the difference between the post-reform hazard and the pre-one is steeply vanishing.

With regard to the treatment group, i.e. those workers tenured between 14 and 24 months, the probationary period shortening drives to a statistically significant decrease in the probability of being dismissed amounting to 1%, except for those tenured 18 months, which turns out to be not statistically different from zero. It is worth pointing out that close to the probationary period threshold -i.e. 12 months - we do not find any significant result. To gather evidence on the validity of this result we address the interested reader to results in section 7.2.

Furthermore, the former graphic emphasizes that the reform does not lead to any relevant effect for those tenured more than two year.

7.2 Regression Discontinuity Design

The Regression Discontinuity Design approach aims at evaluating the effect of the probationary period end on the threshold. The estimation procedure we take can be summarized as follows.

First, respectively for two pre-reform years, i.e. 1997 and 1998, and for two post-reform years, i.e. 1999 and 2000, we compute the average hazard of being dismissed by tenure level (between 0 and 36 months). Second, we run two separate regression one for the pre-reform scenario and one for the post-reform scenario. For both scenarios we regress the hazard of being dismissed on a quadratic polynomial in tenure and on the treatment dummy and a quadratic polynomial in the interaction between treatment dummy and tenure as presented in 8.

The results are summarized in table 3, suggesting a drop in the dismissal hazard both in the pre-reform - i.e. with no reform - on the left-hand side and in post-reform scenario - i.e. with the reform - on the right-hand side.

Table 3: Impact of the 1999 UK Reform on the termination hazard using RDD approach

	Coefficients	
Variables	**With no Reform**	**With Reform**
Impact	-0.034 ***	$-3.26 \cdot 10^{-5}$
	(0.001)	$(4.71 \cdot 10^{-6})$
N. observations	27110	26330

* significant at 10%, ** significant at 5%, *** significant at 1%. The regression model is the one specified in eq. (8). Bootstrapped standard errors (in parentheses), using 1000 replications. Particularly for the pre-reform scenario has been used data up to two years before the reform introduction (particularly 1997 and 1998), while for the post reform scenario has been used data for up to 1 year after the reform (particularly 1999 and 2000).

Results for the former probationary period end -i.e. 24 months - suggest a dismissal probability drop of around 3.4%, which is statistically different from zero at conventional levels. In other words workers tenured two years or more, in the pre-reform scenario, tend to be, for a significant 3.4%, less likely dismissed compared with the less tenured peers.

Conversely the new probationary period end - i.e. 12 months - does not drive to any relevant effect. This validity of this result is confirmed by the results presented on section 7.1 close to the threshold - i.e. 12 month of tenure.

7.3 Discussion of the results: a comparison with the existing literature

Our findings suggest that the 1999 UK Unfair Dismissal Reform leads to a small negative effect of the dismissal probability for the treated, i.e. those tenured between 12 and 24 months only, although the probationary period end is found not significant. With respect to this particular group the economic interpretation of our findings is quite simple. From 1^{st} of June 1999 the qualifying period was lowered from 24 to 12 months, thus the dismissed or "made redundant" workers whose tenure was between 12 and 23 months were automatically entitled to claim unfair dismissal, differently from before. The reform implied, therefore, an increase in EPL for British workers. At the same time the reform implementation leads to firms higher costs for dismissing those workers. This higher burden implies, after the reform, a lower probability of being dismissed for this group.

At the same time, the reform decreases the dismissal probability for those tenured less than one year, although the decline turns out to be not significant. We interpret this results at the light of Jovanovic's model. At the beginning of the employment relationship the firm is not able to distinguish "bad types" workers from "good types" workers. In the initial phase the principal (i.e. the firm) puts more weight on worker's output deciding whether dismiss the workers or not. In other words, initially, for the firm waiting to acquire more information on worker's ability is less costly than dismiss her. Thus, the dismissal probability would be higher at the beginning. From the worker's perspective, her effort would be higher at the beginning and just the "good types" would remain in the firm (Ichino and Riphahn, 2005). After some time, only the most productive matches remain, thus the dismissal probability would be lower.

Concerning the effect of probationary period ending, i.e. 12 months, both the DID and RDD estimation do not find evidence of any significant results. In other words, those workers who have just completed the probationary period do not show a significant decline in the probability of being dismissed compared to those who are quite close to end the required period. However, analyzing the former probationary period end, i.e. 24 months, our results suggest a dismissal probability drop of around 3.4%, which is statistically different from zero at conventional levels. Broadly speaking, workers tenured two years or more, in the pre-reform scenario, tend to be, for a significant 3.4%, less likely dismissed compared with the less tenured peers.

How do our findings compare with Marinescu's one?

The results are out of the line with Marinescu's ones. Analyzing the same reform as us, she finds a decrease in the probability of being dismissed amounting to 26% for the treatment group. For less tenured workers, i.e. less than 12 months, she finds a drop in the probability of being dismissed of around 19%.

Our main concern on Marinescu's identification is due to the controls group she focuses on. Since already as mentioned in section 4 the treatment assignment is not randomly selected but defined according to some observable characteristics, the job tenure in our context, some pre-treatment factors may affect both the treatment status and the potential outcome. In such a case, the difference in observed characteristics creates a "non-parallel outcome dynamics for treated and untreated groups"(Abadie, 2005) leading to biased estimation. In this context the fundamental assumption of DID estimation may be implausible leading to bias estimations.

We address this issue matching treated and controls by observable characteristics.[34]

To enhance our concerns about Marinescu's approach we find evidence, in section 4, that using her definition of treated and controls our estimated results are close to hers, while using our approach we find results that partly contradict refMarinescu's ones.

8 The case of Manufacturing

Manufacturing has been severely hit by the global financial crises. In April 2009, the UK Office of National Statistics estimated an output fell by 12.7% compared to prior year for the month of May. Fortunately, for the UK economy,"the latest purchasing managers' index (PMI) survey data suggests that after months of gloom and doom, there are some signs of relief for the UK manufacturing sector".[35] However, the question of what impact various economic shocks and pieces of labour legislation had on manufacturing labour turnover since the 70s is of great interest for two main reasons. First, the magnitude of its labour turnover (Burgess and Nickell, 1990). Second, the data availability for this sector. Wickens (1978) analyzes the effect of labour legislation in 1965/6 on labour turnover and he finds that this legislation had a significant influence on the demand for labour. Burgess and Nickell (1990), analysing the impact of economic fluctuation on labour turnover in UK manufacturing, find that EPL strongly influences the speed at which firms adjust their labour force, particularly they find that the degree of labour market tightness strongly influences and move pro-cyclically quits, which has outweighed the reduction in the layoff-rate. Considering other countries, a part for the UK, DeFreitas and Marshall (1998), using a sample of Latin American and Asian manufacturing industries, find that strict EPL has a negative impact on labour productivity growth.

In this section we aim at evaluating the effect of 1999 Unfair Dismissal Reform looking at the manufacturing industry. Beside the existing literature, it worth stressing that at the beginning of the century, manufacturing accounted for about 20% of the national economy employing more than four million people, representing roughly 14% of the working population in the UK.[36] Furthermore, in the same period manufacturing industry provided 60% of the UK's exports. Moreover, looking at the raw data, the afore mentioned industry shows a completely different pattern in

[34]See section 5.1.

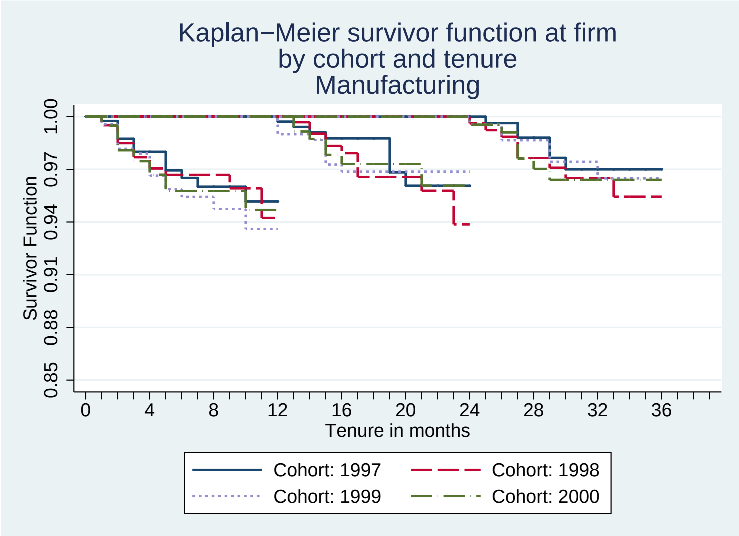
[35]David Noble - the chief executive of the Chartered Institute of Purchasing and Supply (July 01, 2009).

[36]www.ons.uk

terms of layoffs compared with the others industries: Such as public administration and defence, primary sector and health and social work.[37]

Figure 5[38] depicts the relationship between the survivor function at tenure level for two pre-reform cohorts (1997 and 1998, respectively) and for two post-reform cohorts (1999 and 2000, respectively) and by group tenure (those belonging to the first year of tenure, those belonging to the second year of tenure and finally those belonging to the third year of tenure).

Figure 5: Kaplan - Meier estimate of firing survivor function, by cohort and tenure - Females



At first glance, while in the aggregate group the picture highlights a sharp difference between the pre and the post reform cohorts just for those people belonging to the second group of tenure and for those workers tenured more than 32 months (see section 6), in the manufacturing sector we could not get a glimpse of a possible reform effect. Although, looking at the raw data it seems that the 1999 Unfair Dismissal Reform leads to an increase in the firms firing behaviour for those belonging to the first group of tenure, while we could not identify any clear pattern for the others tenure groups. However, it is worth noting that at first view none of this patterns seems to be statistically significant.

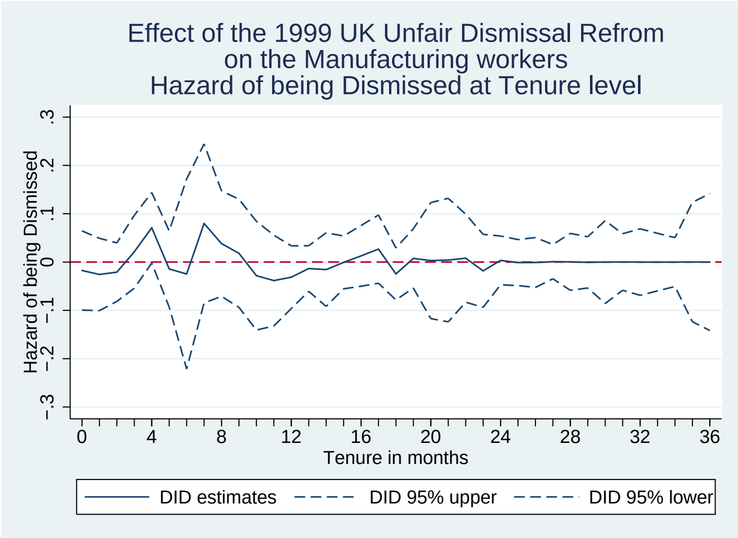
We report our estimation results by way of graphical illustration. Figure 6 depicts the estimated effect of the 1999 UK Unfair Dismissal Reform on the hazard of being dismissed for those employed in the manufacturing sector, the estimated effect is rounded by a 95% confidence interval. The picture confirms the findings highlighted by figure 5, hence the probationary period shortening does not lead to any relevant effect in this sector. In other words, it is clearly evident from the former picture 6 that, for all the time span we consider: 0-36 months of tenure, the confidence intervals always contain the red dash line corresponding to zero - i.e. no

[37]Picture not included but available upon request

[38]It is worth stressing that all graphs presented in this section have a different scale compared to the graphs in the other sections, therefore it would not be possible to compare them. The reason for this choice is given by the higher firing hazard present in the manufacturing sector.

effects.

Figure 6: The effect of the reform on the hazard of being dismissed by tenure separately by industry: manufacturing



Even though the results do not highlight any significant effect, what is most striking is that our results show that shortening the probationary period tends to have an increase in the dismissal hazard, even if negligible, for those tenured less than one year. Since the first month of tenure, picture 6 shows us a general "jagged" trend for the first 12 months of tenure, while thereafter the mean effect overlaps with the red dash line corresponding to zero. Concerning the first group of tenure we observe that the reform increases the probability of being dismissed by about 2% in the first months and it is progressively increasing till reaching the level of 7% corresponding to the fourth month of tenure. Between the fifth and sixth month of tenure the probability of being dismissed, after the reform, decreases by about 2%, while from the seventh month of tenure it jumps again at the level of 7%. Around the new probationary period end threshold - i.e. 12 months - our evidence shows a not statistically significant drop in the hazard by about 3%, which confirm our RDD results (explained later). For the second - i.e. between 12 and 24 months - and the third - i.e. between 24 and 36 months - group of tenure we do not find a clear pattern. As stressed in the introduction of this section none of this effect turns out to be statistically significant.

Concerning the effect on the threshold the results are summarized in table 4, suggesting a drop in the dismissal hazard both in the pre-reform - i.e. with no reform - on the left-hand side of the panel and in post-reform scenario - i.e. with the reform - on the right-hand one.

Table 4 summarizes the RDD[39] results.

Table 4: Impact of the 1999 UK Reform on the termination hazard using RDD approach- Manufacturing

	Coefficients			
Variables	With no Reform		With Reform	
Impact	$-.0335$	***	$-.0374$	*
	(.003)		(.022)	
N. observations	5041		4027	

* significant at 10%, ** significant at 5%, *** significant at 1%. The regression model is the one specified in eq. (8) but the sample includes just manufacturing workers . Bootstrapped standard errors (in parentheses), using 1000 replications. Particularly for the pre-reform scenario has been used data up to two years before the reform introduction (particularly 1997 and 1998), while for the post reform scenario has been used data for up to 1 year after the reform (particularly 1999 and 2000).

Our evidence show that the former probationary period end -i.e. 24 months - suggest a dismissal probability drop of around 3.3%, which is statistically different from zero at conventional levels. In other words workers tenured two years or more, in the pre-reform scenario, tend to be, for a significant 3.3%, less likely dismissed compared with the less tenured peers.

Conversely, the new probationary period end - i.e. 12 months - drives to a decrease in a dismissal probability of about 3.7%, which turns out to be statistically significant at 10% relevant effect. This results is pretty close to the one presented in picture 6 close to the threshold - i.e. 12 month of tenure.

Our evidence shows that the probationary period shortening tends to have a positive effect on the dismissal probability for the first group of tenure - i.e. between 0 and 12 months of tenure - while for the second group of tenure - i.e. between 12 and 24 months of tenure - we are not able to identify a clear pattern. However is worth pointing out that none of these results is significant at usual statistical levels.

In the existing literature (Jovanovic (1979), Parsons (1972), Becker (1962)) the value a worker has to a particular firm may be due to skills and knowledge peculiar to the firm. Large investments in firm-specific human capital, either by the firm or the worker, are likely to reduce labor mobility, since the economic cost of worker-job separations is increased. Thus, the firm would be less likely to lay off that worker whose skills are particularly relevant for the firm productive process, either during normal demand periods or during a decline demand period. The firm, in fact, would suffer a capital loss if such workers were permanently lost to the firm.

Given the large sample size[40] we aim at investigating whether shortening the probationary period affected differently skilled and unskilled workers. To this end we split blue from white collar workers.

[39]The graphs representing the discontinuity effect led by the probationary period end has been reported in section C

[40]Manufacturing accounts for more than 17% of the sample size, represents the largest industry in the raw data.

Figure 7 and 8 display the 1999 UK Unfair Dismissal Reform Effect on the hazard of being dismissed respectively for skilled manufacturing workers and for the unskilled ones.

Figure 7: The effect of the reform on the hazard of being dismissed by tenure separately by workers skills: skilled

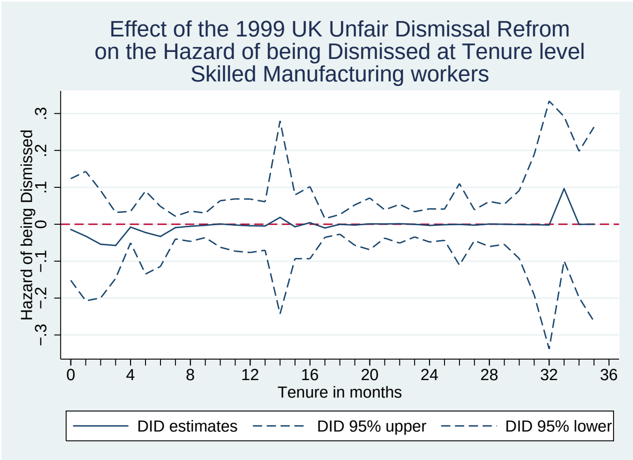
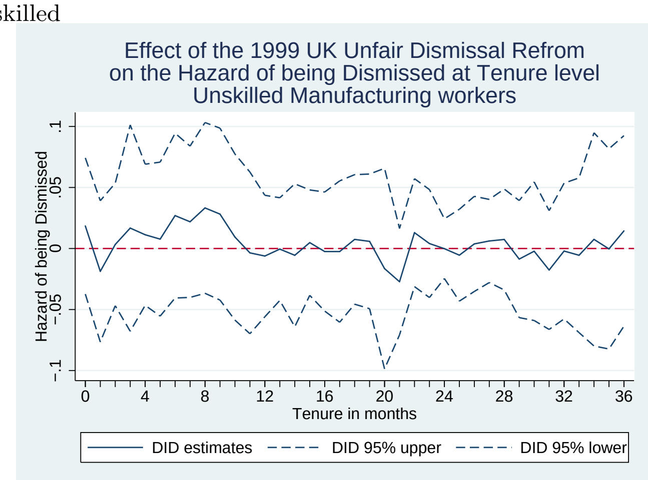


Figure 8: The effect of the reform on the hazard of being dismissed by tenure separately by workers skills: unskilled



Even though the results do not highlight any significant effect, what is striking is that our results show that shortening the probationary tends, on the one hand, to have an increase in the dismissal hazard, even if negligible, for the unskilled workers tenured less than one year. On the contrary, our evidence shows a completely different pattern for the skilled ones. Since for those tenured two years or more, both for skilled and unskilled worker, the mean effect of the reform almost overlaps perfectly with the dash red line corresponding to zero we address the interpretation explanation just or the first group of tenure, i.e. 0-12 months.

On the one hand, for the skilled workers our results show a sharp decrease in the dismissal hazard until the third month of tenure, amounting to 5%. From the forth month of tenure, the drop in the dismissal probability progressively declines, assessing to a level lower than 0.5% from the 11th month of tenure. With respect to the slight decrease close to the probationary period threshold, i.e. 12 months, this result is confirmed by RDD estimation results shown in table 5. From table 5 can be seen that overcoming the probationary period drives to a decrease in the hazard of been maid redundant amounting to 0.1%, however this result is not statistically different from zero. Conversely, our evidence shows that for the same sample overcoming the former probationary period decrease the probability of being dismissed by 3.2%. In other words, workers tenured two years or more, in the pre-reform scenario, tend to be, for a significant 3.2%, less likely dismissed compared with the less tenured peers

On the other hand, since the second month of tenure, picture 8 shows us that the reform implementation leads to an increase in the probability of being laid off for the unskilled manufacturing workers amounting to 1%.[41] The probability increases till reaching the maximum (3.3%) in the eight month of tenure. However, as for the aggregate group our evidence shows a decline in the above mentioned probability close to the new probationary period.[42] The effect on the threshold is confirmed by the RDD estimation results (see 6). Tables 5 and 6 present the estimation results using the RDD technique.

[41] With this regard the exact amount is 1.6% in the third of tenure, 1.1% in the fourth one.

[42] With this regard from the tenth month our evidence show a decrease in the above mentioned probability, which amount goes from 0.3% to 0.6%.

Table 5: Impact of the 1999 UK Reform on the termination hazard using RDD approach - Skilled Manufacturing Workers

	Coefficients		
Variables	**With no Reform**		**With Reform**
Impact	$-.033$	***	$-.002$
	$(.005)$		$(.002)$
N. observations	1921		1462

* significant at 10%, ** significant at 5%, *** significant at 1%. The regression model is the one specified in eq. (8) but the sample includes just skilled manufacturing workers . Bootstrapped standard errors (in parentheses), using 1000 replications. Particularly for the pre-reform scenario has been used data up to two years before the reform introduction (particularly 1997 and 1998), while for the post reform scenario has been used data for up to 1 year after the reform (particularly 1999 and 2000).

Table 6: Impact of the 1999 UK Reform on the termination hazard using RDD approach - Unskilled Manufacturing Workers

	Coefficients		
Variables	**With no Reform**		**With Reform**
Impact	$-.021$	***	$-.003$
	$(.005)$		$(.005)$
N. observations	3120		2565

* significant at 10%, ** significant at 5%, *** significant at 1%. The regression model is the one specified in eq. (8) but the sample includes just unskilled manufacturing workers . Bootstrapped standard errors (in parentheses), using 1000 replications. Particularly for the pre-reform scenario has been used data up to two years before the reform introduction (particularly 1997 and 1998), while for the post reform scenario has been used data for up to 1 year after the reform (particularly 1999 and 2000).

Around the new probationary period end threshold - i.e. 12 months - we can observe a not significant drop in the hazard by about 0.3%, which is close to DID estimation results. Conversely the former probationary period end - i.e. 24 months - for the same sample (unskilled manufacturing workers) suggests a drop in the probability of being laid off by 2%, which is statistically different from zero at conventional levels.

The results presented above are in line with existing literature on human capital Becker (1962). Our results show that the reform leads to an anticipation effect for those workers whose skills are not firm specific, i.e. unskilled. Put in other words, in the post reform period, the firm, since has less time to screen the individuals, tends to anticipate dismissal of "bad types" workers. Although, when the worker has firm specific skills, i.e. skilled workers in our context, our results show that the reform leads to a decrease in the probability of being dismissed also for those workers not covered by the reform.

9 Robustness checks

In this section we present a number of robustness checks for both estimation techniques: Difference-in-Differences and Regression Discontinuity Design. First, we present the results separately by gender. Second, we asses the sensitivity of the estimates changing the definition of treated.

9.1 Gender differences

Our evidence supports the thesis that the probationary period shift leads to a decrease in the dismissal probability. Although, as previously stated, 1999 was a particularly rich period in terms of enacting reforms (see section 3). Aiming at avoiding the confounding factors coming from the implementation of the National Minimum Wage we kept the workers whose wage was above the 10th percentile of the wage distribution.[43] Moreover, given the introduction of reform particularly addressed to female labour participation: i.e. parental leave and dependent care leave (Employment Relations Act 1999, and Maternity and Parental Leave Regulations 1999) and sex act discrimination (Sex Discrimination (Gender Reassignment) Regulations 1999) we want to check if our results are mainly female driven.

In figures 26 and 27, the raw data show that the reform decreased the probability of being dismissed for both genders (i.e. we can see that for all the groups the survival functions after the reform are higher compared with the before ones). However, the difference seems significant just for men belonging to the second group of tenure and for males workers tenured more than 28 months. One aspect is worth noting: While in principle the difference for those tenured between 12 and 24 months should be due either to observable characteristics or to the reform, for those belonging to the third it should be due to the observable characteristics only.

[43]For those whose wage was not available we looked at the education level if the worker has a college or an high school degree we classify him/her above the 10th percentile. When the worker's education was low we looked at the house tenure - i.e. the individual rents freely the house - and at the types of allowances the individual is entitle to.

We report our estimation results by way of graphical illustration. Figures 9 and 10 display graphically the estimated effect of the 1999 UK Unfair Dismissal Reform on the hazard of being dismissed, the estimated effect is rounded by a 95% confidence interval, separately for females (9) and for males (10).

As already mention in section 7 the 1999 UK Unfair Dismissal Reform leads to a significant effect whether the confidence intervals around the line do not contain the red dash line corresponding to zero.

In what follows we procede our analysis describing the results for females, earlier, and for males afterwards.

Figure 9 neatly shows that the probationary period shift leads to a decrease in the dismissal hazard amounting to 2% until the fifth month of tenure. From the sixth month of tenure the effect is progressively reducing leveling off at an increase amounting to 0.1% at tenth month of tenure. Close to the threshold the decline effect is progressively increasing reaching the statistically significant level of about 0.6% in the 12th month. From the 13th month of tenure the reform effect is vanishing.

Figure 9: The effect of the reform on the hazard of being dismissed by tenure separately by gender: Females

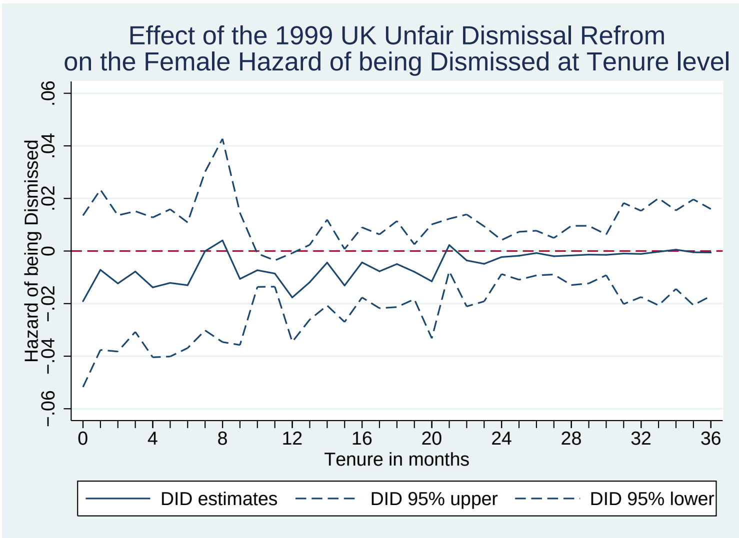


Table 7 shows the effect of overcoming the probationary period threshold on the female hazard of being dismissed.

Results for the former probationary period end - i.e. 24 months - suggest a dismissal probability drop of around 0.7%, which is statistically different from zero at conventional levels. In other words, females workers tenured two years or more, in the pre-reform scenario, tend to be, for a significant 0.7%, less likely dismissed compared with the less tenured peers. At the same time, the new probationary period end - i.e. 12 months - drives to a decrease of about 0.5%. In other words females workers tenured one year or more, after the probationary period shift, tend to be, for a significant 0.5%, less likely dismissed compared with the less tenured peers. The validity of this result is confirmed by the results presented in figure 9 close to the 12 month of tenure.

Table 7: Impact of the 1999 UK Reform on the termination hazard using RDD approach- Females

	Coefficients	
Variables	**With no Reform**	**With Reform**
Impact	−.007 ***	−.006 ***
	(.002)	(.002)
N. observations	14861	14110

* significant at 10%, ** significant at 5%, *** significant at 1%. The regression model is the one specified in eq. (8) but the sample includes just females . Bootstrapped standard errors (in parentheses), using 1000 replications. Particularly for the pre-reform scenario has been used data up to two years before the reform introduction (particularly 1997 and 1998), while for the post reform scenario has been used data for up to 1 year after the reform (particularly 1999 and 2000).

Figure 10 show that the reform leads to a statistically significant decrease in the dismissal hazard amounting to a 2.2% between the fourth and the fifth of tenure. From the seventh month of tenure the effect is progressively vanishing - from 3% in the seventh month of tenure to 0% in the 10th month of tenure. Although it worth pointing out that between the seventh and ninth month of tenure the decline turns out to be statistically different from zero. Starting from the 14th month of tenure the reform leads to a decline by roughly 3% which progressively decline before leveling off at 1% corresponding to 20th month of tenure, and it decreases again at the level of 3% between month 21 and 22 of tenure. In all this interval, i.e. between 14th and 22nd month of tenure, the effect is statically different from zero. From the 24th month of tenure the effect almost overlaps the red dash line corresponding to no-effect.

Figure 10: The effect of the reform on the hazard of being dismissed by tenure separately by gender: Males

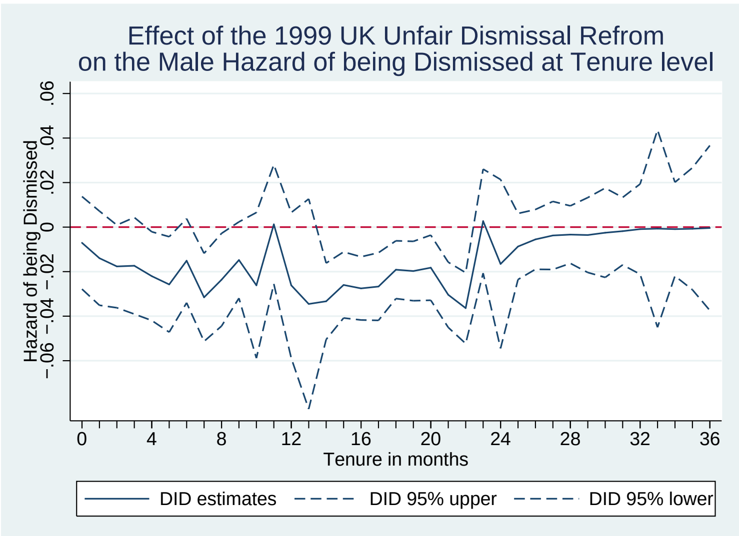


Table 8 shows the effect of overcoming the probationary period threshold on the male hazard of being dismissed.

Table 8: Impact of the 1999 UK Reform on the termination hazard using RDD approach- Males

	Coefficients		
Variables	**With no Reform**		**With Reform**
Impact	$-.041$	***	$-.0000437$
	(.004)		(.0002251)
N. observations	12249		11936

* significant at 10%, ** significant at 5%, *** significant at 1%. The regression model is the one specified in eq. (8) but the sample includes just males . Bootstrapped standard errors (in parentheses), using 1000 replications. Particularly for the pre-reform scenario has been used data up to two years before the reform introduction (particularly 1997 and 1998), while for the post reform scenario has been used data for up to 1 year after the reform (particularly 1999 and 2000).

Results for the former probationary period end -i.e. 24 months - suggest a dismissal probability drop of around 4%, which is statistically different from zero at conventional levels. In other words, males workers tenured two years or more, in the pre-reform scenario, tend to be, for a significant 4%, less likely dismissed compared with the less tenured peers.

Conversely, the new probationary period end - i.e. 12 months - does not drive to any relevant effect for male workers. This result is in line with the effect close to the threshold highlighted in figure 10.

9.2 Defining Eligibility

To gather evidence of the validity of our results we implemented a number of robustness checks for both techniques. The idea behind these tests is to exploit whether the improve matching effect of the reform is driven by the particular treatment group we chose.

Consider for instance the case in which the firms reacted just after the reform and as time went by the effect vanished. On the one hand, the labour turnover adjustment could take more than one year, hence not all the firms would comply to the new probationary period in 1999 - in this case our estimates would underestimate the real reform effect. On the other hand, the firms would adjust their dismissal process immediately after the reform only, hence using a larger time-span window we would find a lower effect. Therefore we decided to slightly change the treated definition using those individuals whose tenure was between 12 and 24 months in 2000 dropping the 1999 cohort. Results are presented in what follows, particularly graph presents 11 the DID estimation,[44] while table 9 presents the RDD estimation. Our robustness checks confirm that the 1999 Unfair Dismissal Reform leads to a non-transitory better matching effect for the treated group, however the effect does

[44]For the estimation of the confidence intervals has been used a bootstrap procedure with 300 replications, furthermore for the first group of tenure we clustered the estimation by individual.

not start just after the completion of the probationary period - i.e. our evidence highlights that the new probationary period threshold is negative but not significant.

Figure 11: The effect of the reform on the survival at firm by tenure using as treated those individuals whose tenure was between 12 and 24 months in 2000

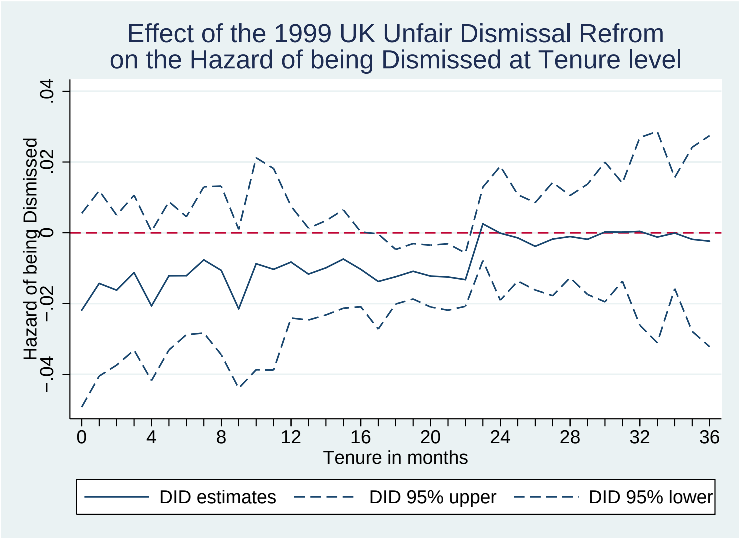


Table 9: Impact of the 1999 UK Reform on the termination hazard using RDD approach

	Coefficients	
Variables	**With no Reform**	**With Reform**
Impact	−0.034 ***	−.0001295
	(0.001)	(.0001837)
N. observations	27110	26046

* significant at 10%, ** significant at 5%, *** significant at 1%. The regression model is the one specified in eq. (8). Bootstrapped standard errors (in parentheses), using 1000 replications. Particularly for the pre-reform scenario has been used data up to two years before the reform introduction (particularly 1997 and 1998), while for the post reform scenario has been used data for up to 2 year after the reform (particularly 2000 and 2001).

We observe a general negative trend since the first month, which is progressively declining till reaching the minimum corresponding to the fourth month of tenure. In that particular tenure level the decline, in term of dismissal hazard, varies between -4% to -0.01% although the effect is not statistically different from zero at conventional levels. From the fifth to the sixteenth month of tenure the picture highlights a negative effect of the reform amounting to 1%, although as before the effect is not statistically relevant. Although the decline, amounting to 1.3%, turns out to be statistically different from zero in the interval 17 - 22 months. With respect to the threshold, from the 10th to the 13th month of tenure, the picture starkly highlights that the difference between the post-reform hazard and the pre-one is steeply vanishing.

Concerning the treatment group, i.e. those workers tenured between 12 and 24 months, the probationary period shortening drives to a statistically significant decrease in the probability of being dismissed amounting to 1%, except for those tenured between 12 and 16 months, for those workers the effect turns out to be not statistically different from zero. It is worth pointing out that close to the probationary period threshold - i.e. 12 months - we do not find any significant result. To gather evidence on the validity of this result we address the interested reader to results in section 9.

With respect to those tenured more than 24 month, as expected - i.e. for the control group we should not find any relevant evidence - the mean effect overlaps almost perfectly the red dash line corresponding to no-effects.

Concerning the effect of the probationary period threshold, results for the former probationary period end - i.e. 24 months - suggest a dismissal probability drop of around 3.4%, which is statistically different from zero at conventional levels. In other words workers tenured two years or more, in the pre-reform scenario, tend to be, for a significant 3.4%, less likely dismissed compared with the less tenured peers.

Conversely the new probationary period end - i.e. 12 months - does not drive to any relevant effect. This validity of this result is confirmed by the results presented on section 9 close to the threshold - i.e. 12 month of tenure.

10 Conclusions

This paper analyzes the impact of the 1999 British Unfair Dismissal Reform on the probability of job termination. In so doing we contribute to the ongoing literature on employment protection in several ways.

First, we combine survival data with a Regress Discontinuity Design framework. Second, aiming at investigating the effect of the reform beyond the threshold we compare how the change in dismissal probability over time (the difference between the post-reform scenario and the pre-reform scenario) differs between those workers directly affected by the reform to those not directly affected. It is referred to as "Difference-in-Differences" estimation.

Third, since the effect of the reform could be heterogenous among different industries due to their different screening procedure, we investigate the reform effect on a specific industry: Manufacturing.

It is worth pointing out that our evidence partly contradicts Marinescu (2009)'s ones. In fact, while her results show a roughly 30% decrease in the firing hazard for workers with zero to two years of tenure relative to workers with higher tenure, our evidence highlights consistently that the 1999 Unfair Dismissal leads to a significant decrease in the probability of being dismissed by roughly 1% at firm level just for the newly covered - i.e. those workers whose tenure is between 12 and 24 months, even though, the new probationary period threshold is found to be not significant. With respect with the comparison between our estimates and Marinescu's ones we find evidence that using her definition of treated and controls our results are close to hers.

Concerning the effect of probationary period ending, i.e. 12 months, both the CDID and RDD estimation shed light of any significant results. In other words, those workers who has just completed the probationary period do not show a significant decline in the probability of being dismissed compared to those who are quite close to end of the required period. However, analyzing the former probationary period end, i.e. 24 months, our results suggest a dismissal probability drop by about 3.4%, which is statistically different from zero at conventional levels. In other words, workers tenured two years or more, in the pre-reform scenario, tend to be, for a significant 3.4%, less likely dismissed compared with the less tenured peers.

Looking at the reform effect in the manufacturing industry, our evidence show that shortening the probationary period increases the probability of being dismissed for those whose tenure is lower than 12 months. Aiming at evaluating whether this particular pattern was driven by a particular compositional effect, we split white from blue collar workers. Our evidence supports the thesis that the reform affects differently skilled and unskilled workers. While the probationary period shortening increases the survival probability at firm for skilled workers, both for newly covered and for those workers still subject to probation, it increases the dismissal probability in the first year tenure for the unskilled workers, only. However, it is worth pointing out that none of our results are statistically significant. Concerning the probationary period threshold, the results presented for manufacturing (also separately for compositional skills) show that both the new probationary period threshold and the older one decrease the dismissal probability, even though just the older one is found to be significant.

This is important from a policy point of view: In the UK contexts, where the average level EPL is low, increasing workers' EPL may have beneficial effects for job turnover.

We interpret our results at the light of the model proposed by Jovanovic (1979), which predicts a rise followed by a fall in the hazard of separation with tenure. In particular, in his seminal paper Jovanovic (1979) predicts that, initially, for the firms the value of separating is higher than value of waiting to learn more about the real productivity of a match (whose current productivity is low). This means that at the beginning the hazard of termination should sharply increase. After some time, only the most productive matches should remain, and therefore the hazard of termination decreases.

In other words, at the beginning of the employment relationship the firm is not able to distinguish "bad types" workers from "good types" workers. In the initial phase the principal (i.e. the firm) puts more weight on worker's output deciding whether dismiss the worker or not. Broadly speaking, initially, for the firm waiting to acquire more information on worker's ability is less costly than dismiss her. Thus, the dismissal probability would be higher at the beginning. Looking at the workers' behaviour, their effort would be higher at the beginning and just the "good types" would remain in the firm (Ichino and Riphahn, 2005). After some time, only the most productive matches remain, thus the dismissal probability would be lower.

Even though the economic literature have stressed the relevance of firing costs on the entrepreneur propensity to hire and to dismiss, some question for further work arise in this study. What remains unexplained is why a variation in the probationary period should influence the dismissal decision of productive workers? Could our results be driven by the particular UK flexible setting?

The contract of employment is one of the most discussed subjects in the economic literature. Therefore it is worth noting that the impact of any reform is difficult to evaluate or might have a small impact, since individual productivity is not observable.

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A Database description

Individual Data Source. Rotating panel from the British Labour Force Surveys from 1997:III to 2000:III, provided by the National Statistical Office.

Sample. From a sample of individuals of 20-49 years of age, who were a t the first interview permanent employed working more than 16 hour per week.

We exclude those

- in the military or the substitute civil service
- never in the labour force during the observed period
- observed only once
- who are full-time students (from the moment they become so)
- employed who do not answer the question about how long they have been in their current job
- with a missing interview between two valid interviews
- with tenure longer than 4 years

individuals satisfy these restrictions 41213.

Tenure. Tenure is measured in months, the smallest unit allowed by the data. We start from the information provided the first time he answers the question "How long have you been in the current job?" and in particular those individuals, who stated the The year and the month in which they started the current job. For those who left the job during the survey and did not declare the date and started a new job in the subsequent survey, we impute he date of separation in the month of the previous survey, in order to keep the unemployment long as possible, the mode would be qual to three months.

The following dummy variables used in the estimation are taken at their values at the beginning of the spell:

- *Economic sector at the previous job.* Grouped as primary (including farming and fishing), manufacturing (including mining as well), construction and services, wholesale, retail and motor, hotels and restaurants, financial intermediation, real estate, renting and business activities, public administration and defence, education, health and social work, others.
- *Year of birth* .
- *Education Three groups:* illiterate, no schooling, and primary education; secondary education and vocational training; and university education. o

Aggregate and Sectoral Variables

- *Regional dummies*: the LFS provides 12 main regions: North West, Yorkshire and Humber, East Midlands, West Midlands, East London, South East, South West, Wales and Scotland.
- *Treated.* Dummy variable, which takes value 1 for those whose tenure is between 12 and 24 months after June 1999.

B Tables

Table 10: Summary statistics for the sample of permanent full-time employees.

Disposition of Sample

Initial Sample	43013	Individuals
Deletions:		
	Number of observation	Freq. In percentage
People not permanently employed (i.e. Temporary workers, unemployed, self-employed)	12568	29.22
People under 20 or people over 50	8089	18.81
Missing data on key variable	7	0.02
Total Deletions	20664	48.04
Final Sample	22349	51.96

Table 11: Summary Statistics for Job Spells
Disposition of Sample

Initial Sample	91070	Job Spells
Deletions:		
	Number of observation	Freq. In percentage
People not permanently employed (i.e. Temporary workers, unemployed, People not permanently employed (i.e. Temporary workers, unemployed, self-employed)	29954	32.89
People under 20 or people over 50	19883	21.83
Missing data on key variable	20	0.02
Total Deletions	49857	54.75
Final Sample	41213	45.25

Table 12: Summary statistics for the sample of permanent full-time employees

Personal Characteristics	
Gender	
Female	54.83
Male	45.17
Education	
Less than high school educated	22.70
High educated	43.35
University educated	33.95
Industry	
Primary	0.81
Manufacturing	17.68
Energy	0.53
Construction	4.88
Wholesale, Retail and Motor	17.12
Hotels and Restaurants	5.17
Transport, Storage and Communication	6.66
Financial Intermediation	4.72
Real Estate, Renting and Business Activity	12.39
Public Administration & Defence	3.73
Education	9.13
Health and Social Work	12.34
Other	4.84
N. observations	41213

Table 13: Summary statistics for the sample of permanent full-time employees

Personal Characteristics	
Reasons for leaving last job	
Layoff	17.66
Quit	38.83
Other	43.51
N. observations	1857

Table 14: Summary statistics for the sample of permanent full-time employees, looking at the tenure and cohort

		Group		
Cohort	**1**	**2**	**3**	**Total**
1997	6456	3828	3177	13461
1998	6682	4083	3191	13956
1999	6559	4082	3155	13796
Total	19697	11993	9523	41213

Table 15: Other types of termination divided by gender

Reason for leaving the job: **Other types of terminations**	**Males**	**Females**
temporary job ended	17.22	9.64
gave up work for health reasons	9.37	8.39
retired (at or after statutory ret. age)	0.00	0.42
gave up wk for family,personal reason	12.69	36.48
left for some other reason	60.73	45.07
Total	100.00	100.00
N. observations	331	477

B.1 Descriptive statistics for robustness checks

Table 16: Summary statistics for the sample of permanent full-time employees

Personal Characteristics	
Reason for last leaving a job	
Layoff	16.95
Quit	37.64
Oher	45.41
N. Observation	3,552

Table 17: Summary statistics for the sample of permanent full-time employees, looking at the tenure and cohort

	Group			
Cohort	**1**	**2**	**3**	**Total**
1996	6,625	3,848	2,926	15,395
1997	6,456	3,828	3,177	13,461
1998	6,682	4,083	3,191	13,956
1999	6,559	4,082	3,155	13,796
2000	5,954	3,589	2,895	12,438
2001	6,666	4,007	2,935	13,608
Total	38,942	23,437	18,279	80,658

C Figures

Figure 12: UK Unemployment Rate by Quarter

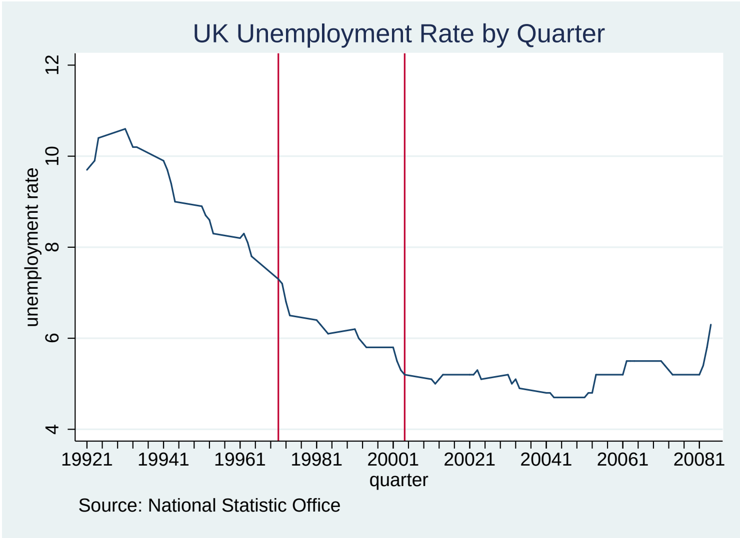

Figure 13: UK Activity Rate

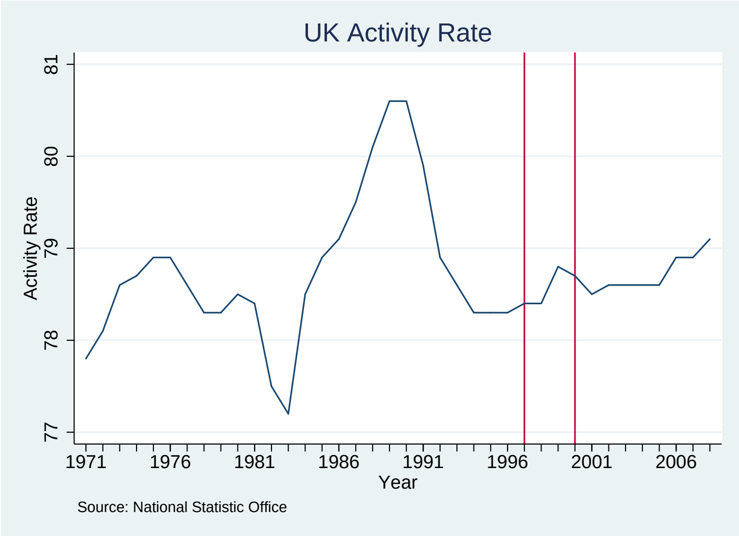

Figure 14: UK Gross Domestic Product by Quarter

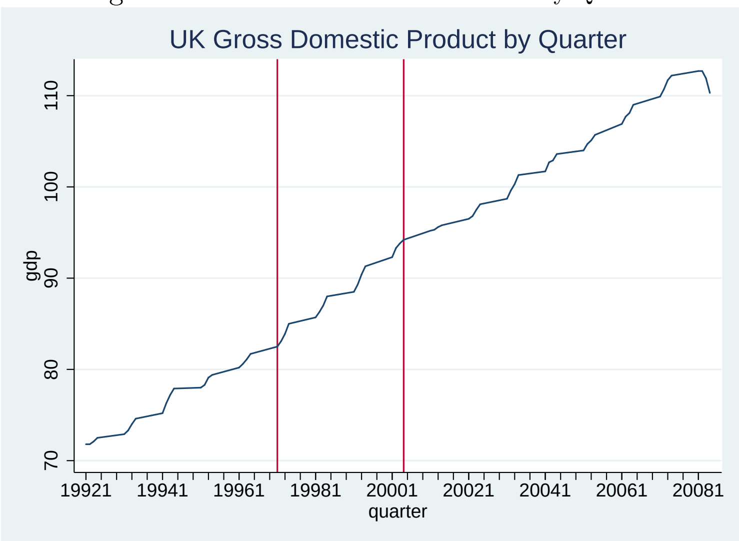

Figure 15: UK Gross Domestic Product Growth by Quarter

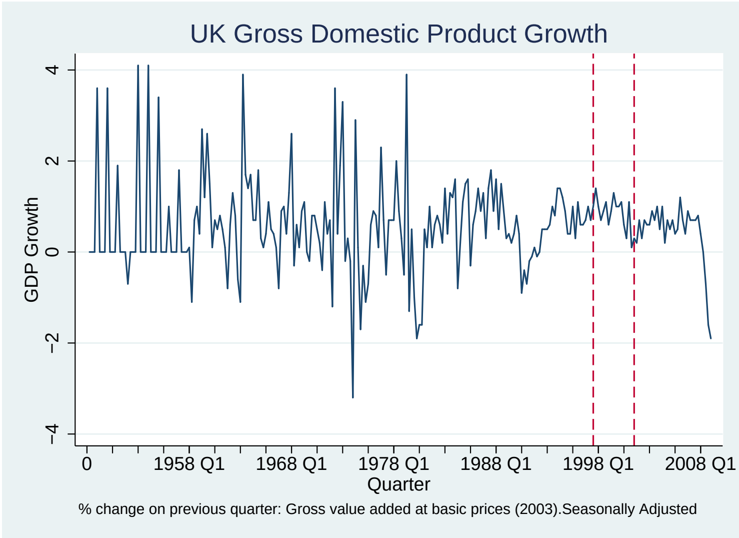

Figure 16: Survival at firm, comparison between workers younger than 50 with those older

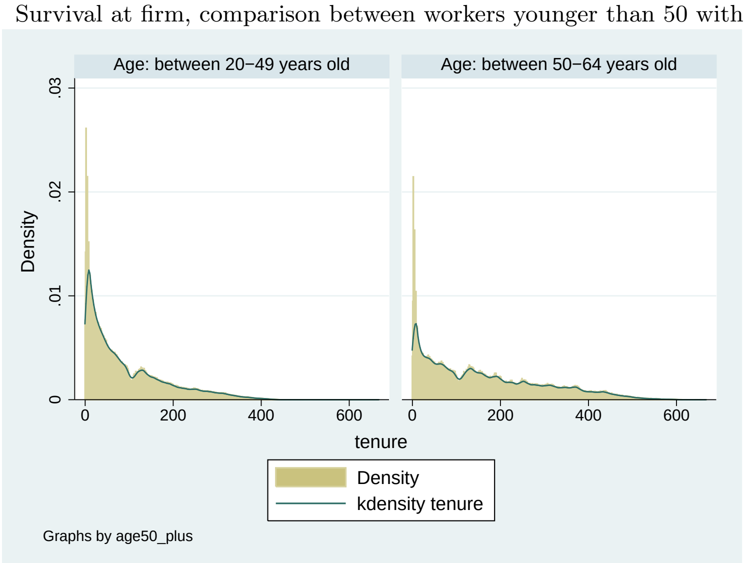

Figure 17: Kaplan - Meier estimate of firing survivor function, by cohort and tenure

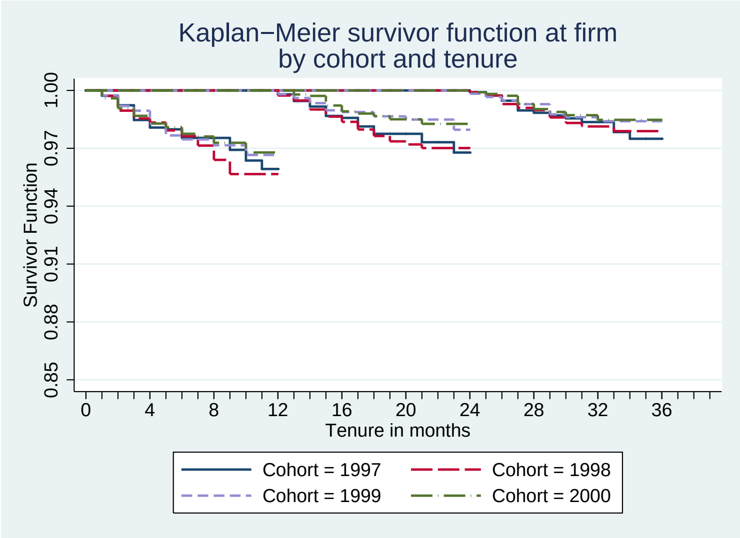

Figure 18: Kaplan - Meier estimate of quitting survivor function , before and after the reform

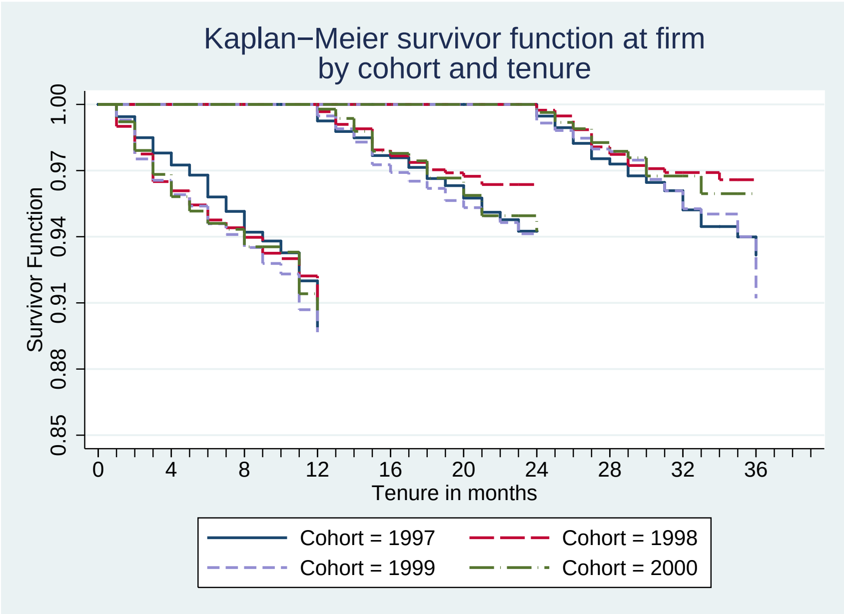

Figure 19: Kaplan - Meier estimate of other types of termination survivor function , before and after the reform

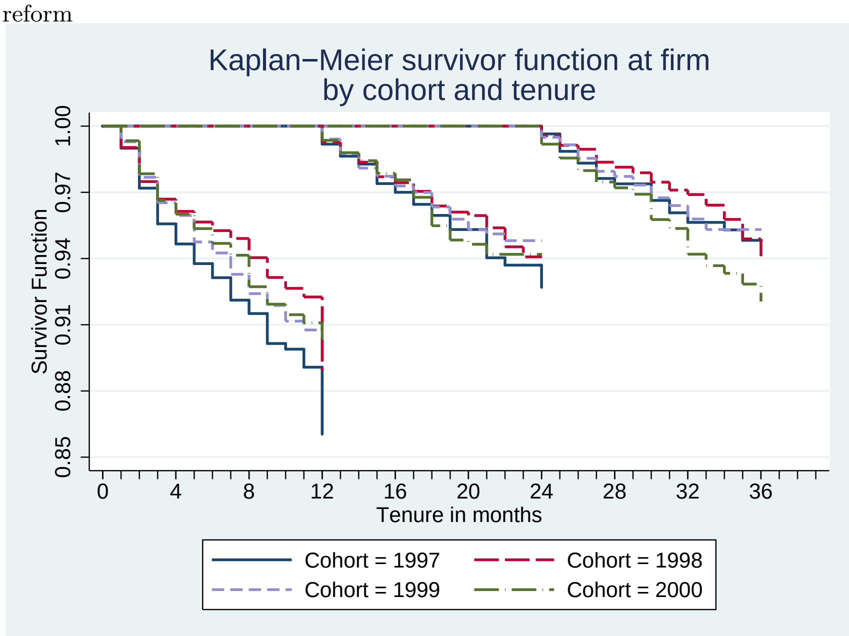

Figure 20: Estimation of the causal effect of the reform on the hazard of termination

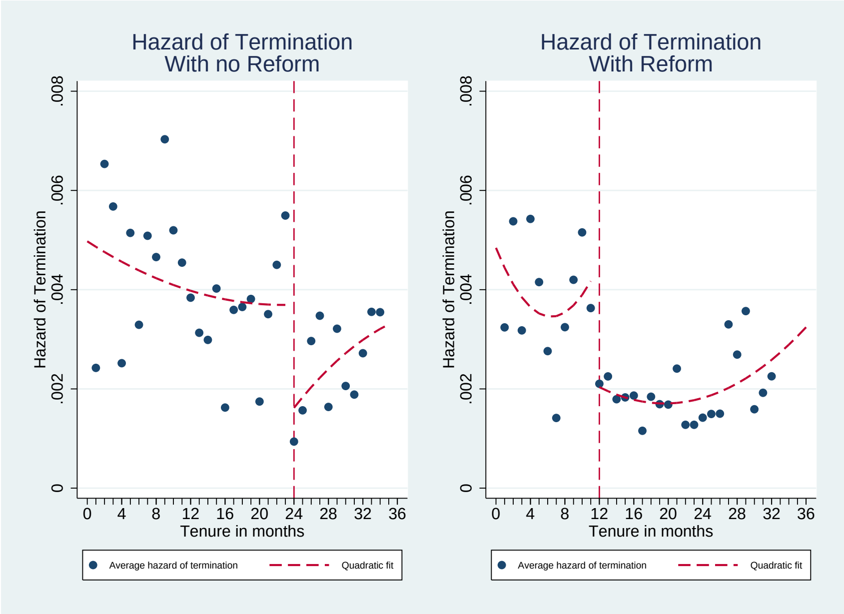

Figure 21: In the graph 'treated' refers to those workers tenured between 12 and 24 months in 1999. 'controls' refers to all the others. Each graph shows the propensity score comparing the treated and each control

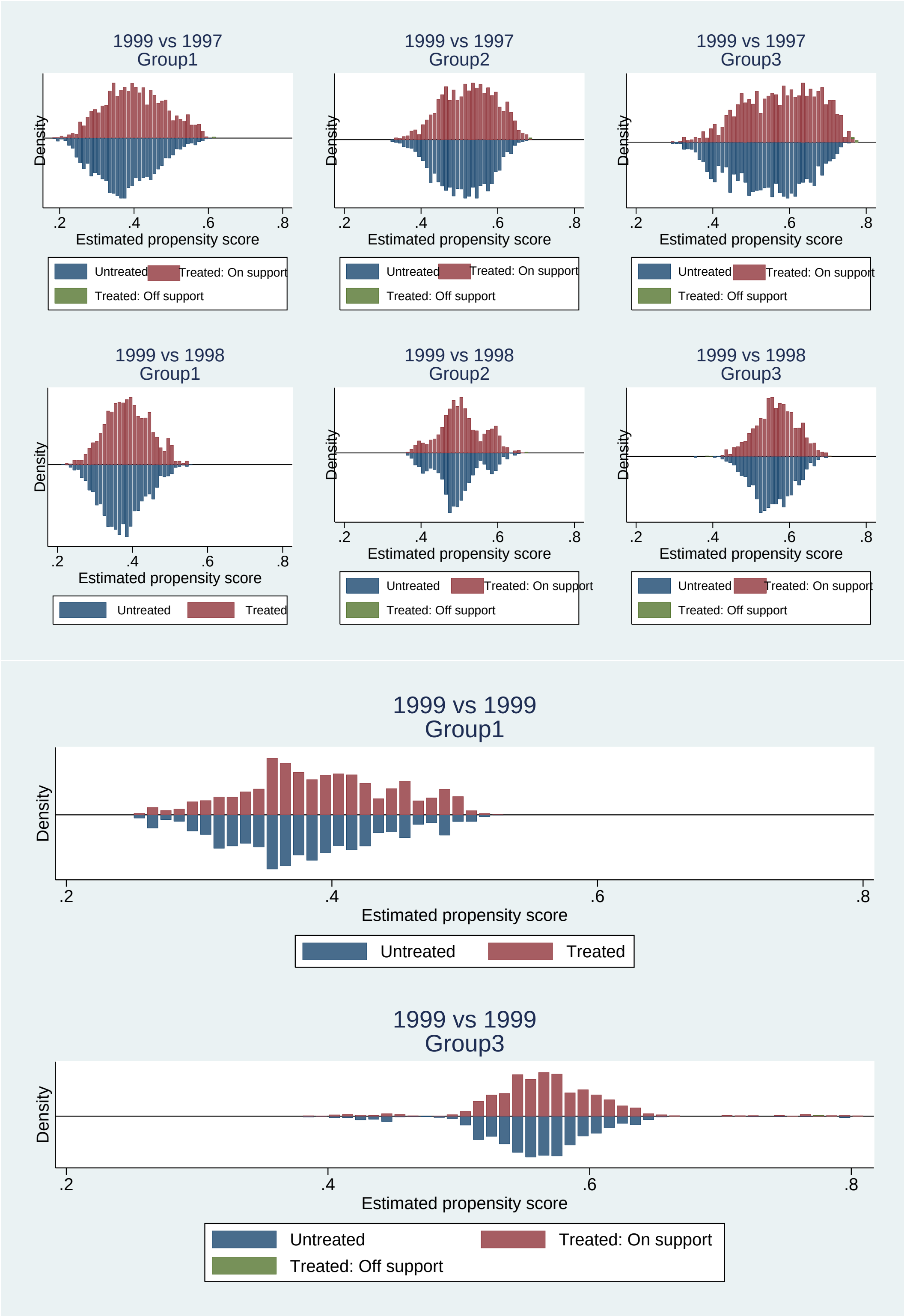

Figure 22: Kaplan - Meier estimate of firing survivor function, by cohort and tenure - Separately by industry: Manufacturing

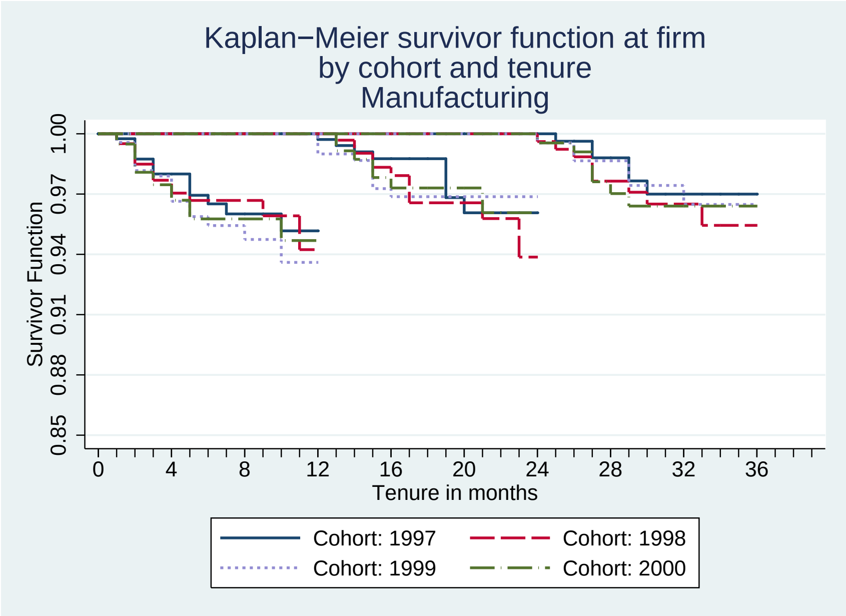

Figure 23: Estimation of the causal effect of the reform on the hazard of termination - Separately by industry: Manufacturing

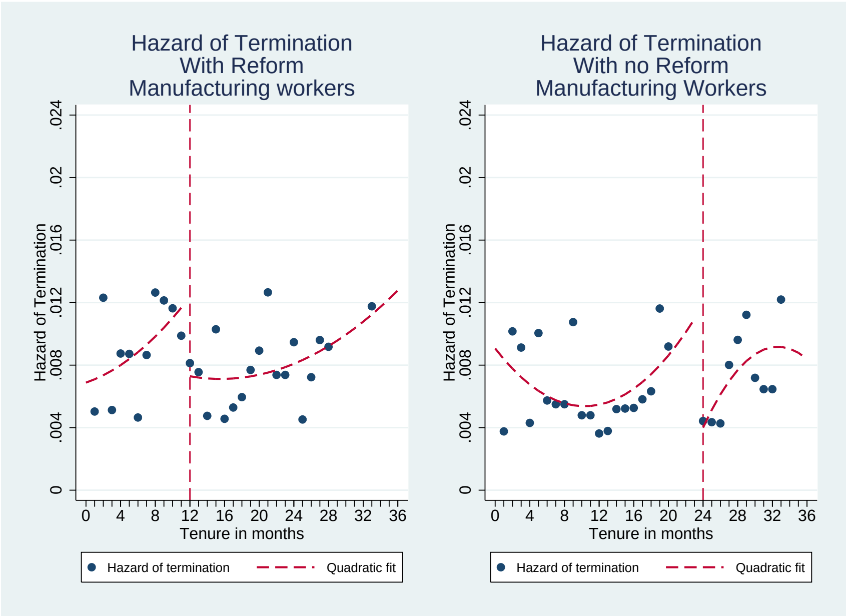

Figure 24: Estimation of the causal effect of the reform on the hazard of termination - Separately by workers skills: Skilled

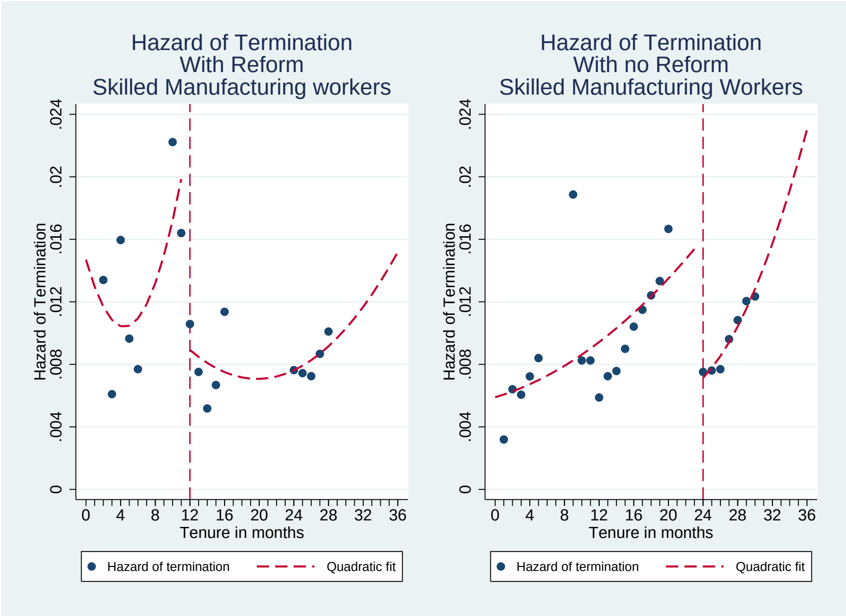

Figure 25: Estimation of the causal effect of the reform on the hazard of termination - Separately by workers skills: Skilled

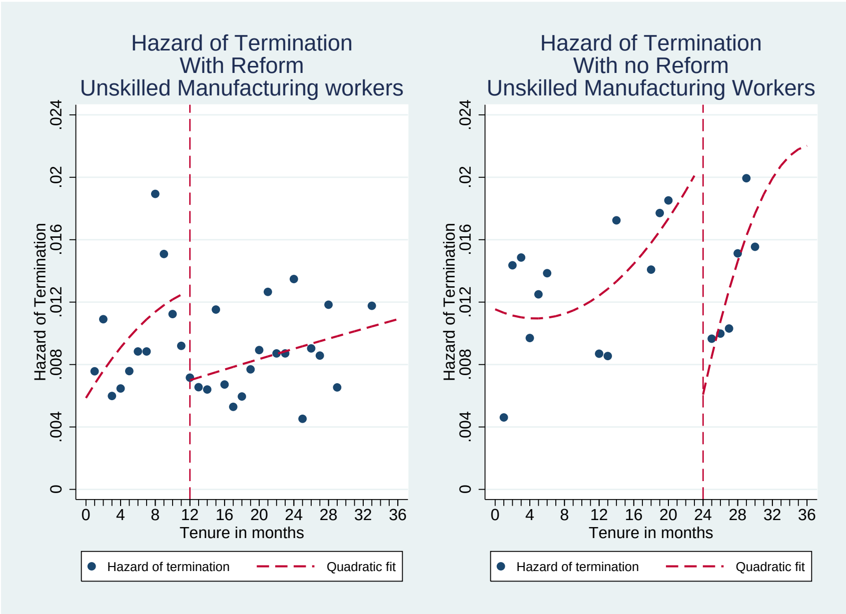

Figure 26: Kaplan - Meier estimate of firing survivor function, by cohort and tenure - Females

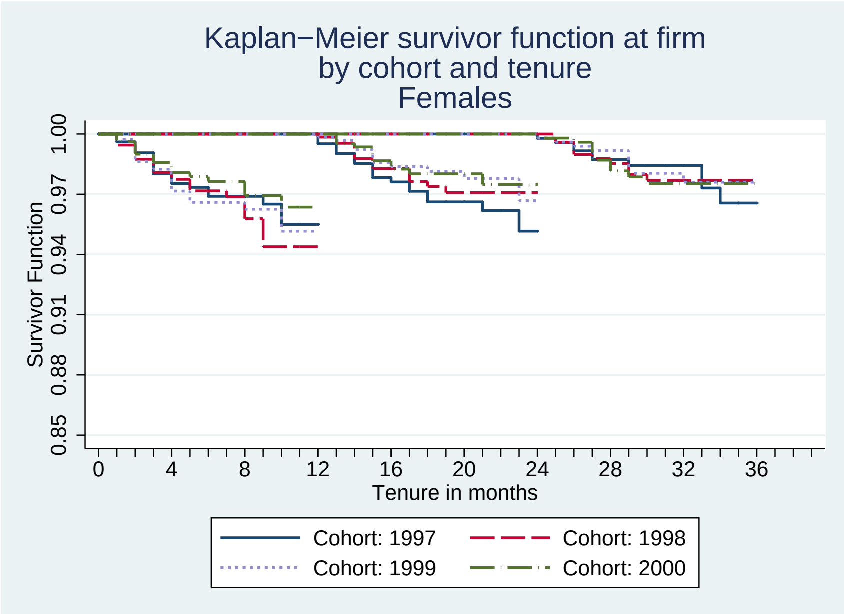

Figure 27: Kaplan - Meier estimate of firing survivor function, by cohort and tenure - Males

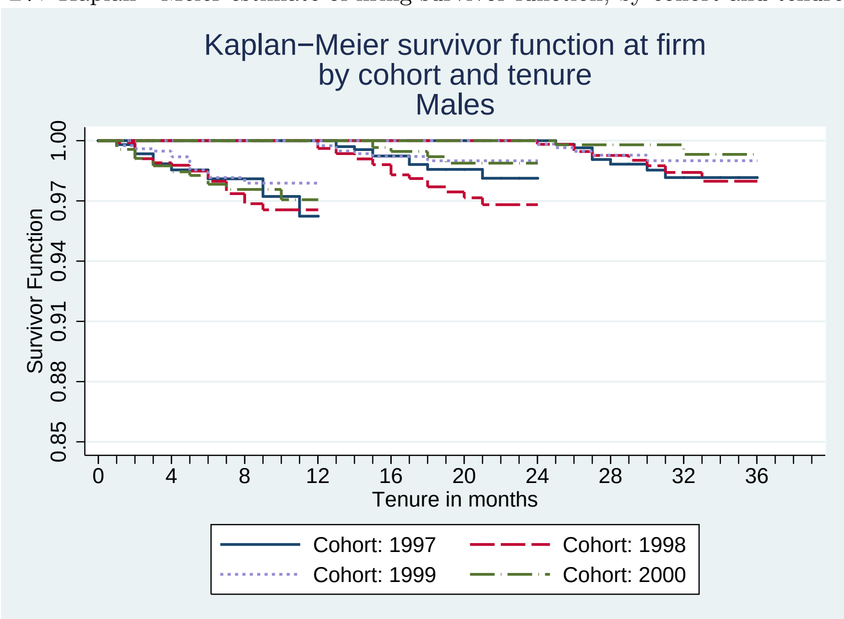

Figure 28: Estimation of the causal effect of the reform on the hazard of termination - Females

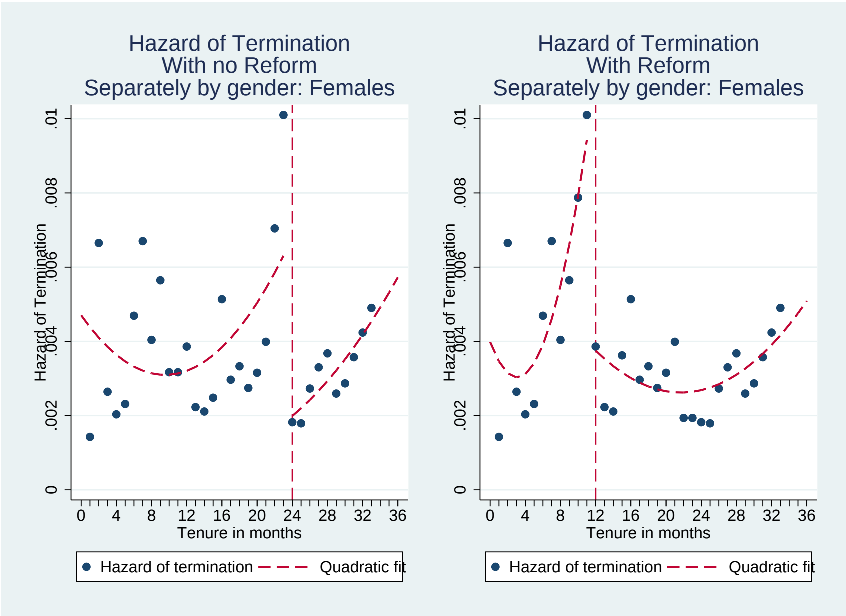

Figure 29: Estimation of the causal effect of the reform on the hazard of termination