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

Please do not quote without permission

Veronica Toffolutti*

University of Padua

November 15, 2010

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

***Acknowledgements**: I would like to thank Luca Nunziata for his outstanding supervision. I am grateful to Giuseppe Bertola, Mark Bryan, Giorgio Brunello, Juan José Dolado, Stephen Jenkins, Eliana La Ferrara, James Malcomson, Raffaele Miniaci, Cheti Nicoletti, Antonio Nicoló, Jörn-Steffen Pischke, Guglielmo Weber, Josef Zweimüller and all the participants at seminars at the University of Padua and at the University of Essex (JESS Seminar), Alicante (ASSET 2010), Buch am Ammersee (ESSLE 2010), Essen (ESPE 2010), Brucchi Lucchino (Padua)and Groningen (SMYE 2011), for their valuable comments on the first draft of this work. Erich Battistin, Emilia Del Bono and Chiara Daniela Pronzato should be separately thanked for the countless discussions. Part of this research is based on a work carried out during my visit to the European Centre for Analysis in the Social Sciences (ECASS) at the Istitute for Social and Economic Research (ISER), both institution are gratefully acknowledged. Usual disclaimers applies. Correspondence can be addressed to the author at Department of Economics "M. Fanno", University of Padua, via del Santo, 33, 35123 Padova (PD) (e-mail: veronica.toffolutti@unipd.it)

firms firing behaviour. Combining Regression Discontinuity Design, with survival techniques this paper 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.

Keywords: Firing costs, Separation hazard rate, Regression Discontinuity, Job Tenure, Program Evaluation
JEL Classification: J24, J41, J63, J64, J65, J83, I18, C21, J18, J38

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 (EPL, henceforth) has often been blamed for the poor performance of some European labor 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. Likewise, the literature highlights the role of the identification strategy used on the results (see the discussion in Kugler, 1999).

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), they reduce the asymmetries between: Capital and labor (Buechtemann, 1993), 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, *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 reasons 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 device 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.

At time of writing, I am aware of only a work which has uses the variation of the probationary period introduced by the afore-mentioned reform: Marinescu (2009). Despite the contribution of her work to the literature, some question for further work arise in this study. Are the result be sensitive to any variation of the identification strategy? Is the 1999 Unfair Dismissal Reform effect homogenous among industries? This paper attempts to fill this gap, by applying a typical treatment evaluation setting in a Regression Discontinuity

Design (RDD, henceforth) on data from the UK Labour Force Survey (LFS), covering the 1996-2001 period.

To test for the economic impact of a probationary period, I 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: workers tenured between 0 and 12 months and those whose tenure is higher than 24 months should be relatively unaffected by the reform. However, the enactment of 1999 Unfair Dismissal Reform halved - from two years to one year - the time that firms have to dismiss workers without any sanction. Therefore, after the reform, the firms could anticipate at the first year the workers screening phase aiming at avoiding any potential litigation in the case of termination. Hence, if the results show 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 1st 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.[1] After the 1st 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 . However, given the extremely flexible nature of the UK labour market,[2] the effects are marginal with respect to other countries such as Italy (Ichino and Riphahn, 2005) or with respect to other UK reforms such as Minimum Wage Implementation in 1999 (Arulampalam, Booth, and Bryan, 2004) or Work Family Tax Credit in 2002 (Blundell, Brewer,

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

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

and Francesconi, 2008, Francesconi and van der Klaauw, 2007), because of the different labour market flexibility - with respect to the Italian case - and because of the different sample of interest - with respect to the other British reforms - the results are expected to be very small.

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-event* method, by combining survival analysis techniques with a RDD framework. The empirical strategy, I use, tackles 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 of 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, this paper investigates the effect and the size of the new probationary period threshold. In so doing, I assume that the counterfactual hazard for the first cohort affected by the reform is approximated by the factual hazard for the last cohort under the no-policy regime.

At a first glance this contribution could seems close to the Lalive, vanOurs, and Zweimüller (2008). The authors study the impact of active labor market policies on the unemployment duration in Switzerland, by offering "direct comparison between the timing-of-event approach and the matching approach". Conversely, the institutional setting of the reform analyzed in the present work - i.e. the presence of two natural discontinuity in a timing-of-event setting - offers the possibility to combine their approach with a RDD one.

Furthermore, since the effect of the reform could be not homogenous among industries, due to their different screening procedure, I 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 EPL literature. Section 3 presents 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 presents the results. Section 8 presents the effect of the afore mentioned reform focusing just on the manufacturing industry. Section 9 provides the robustness checks and finally section 10 concludes.

2 Employment Probationary Periods in the Literature

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 periods 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). Using 1981 cross-section data on last hired workers of 1881 firms, Loh (1994)'s results show that workers self-select into probationary period job, furthermore he finds a 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

Every year more than 20% of the disputes in the British Employment Tribunals concern Unfair Dismissal (for more details see Ministry of of Justice (2010)). Although, as already mentioned in section 1, an essential requirement to claim Unfair Dismissal is the probationary period completion.

Even though, the institution of probationary in UK dates back to the early 1970s, its length changed several times in the last 20 years.[3] This paper focuses on the last probationary period change, introduced when the New Labour legislation came into power in 1997. In that occasion, the qualifying period was lowered from 24 to 12 months by the 1999 Unfair Dismissal and Statement of Reasons for Dismissal (*Variation of Qualifying Period*) Order.

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

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 introduction of the National Minimum Wage. 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 I 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). I will check the effect of the 1999 Unfair Dismissal Reform by gender, in order to find out whether the results are female driven.

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 the best of my knowledge I am aware of 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 my 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 investigates 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.

The first step of this 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.[4] The right hand side replicates her results using the closest sample data to the final sample I use (i.e. using data between 1996 and 2002).

Table 1 shows that using treated and controls according to Marinescu the probationary period shift lead to a stark decrease in the dismissal hazard. Looking at the above-mentioned 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, in this regard I impute the difference to the different sample.[5] Conversely to her, I leave out from the sample those people between

[4]Letting alone the age difference between my sample and Marinescu' one and the difference in the analysis time-span, from my 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.

[5]At this issue, I address the interested reader that the results presented in the first column are in line with the ones presented by Marinescu (2009) for the individuals younger than 40.

Table 1 – *Replication of Marinescu's results*

	Data between 1996 and 2004	Data between 1996 and 2002
0 to 11 months of tenure	-0.249***	-0.217***
	(0.0878)	(0.079)
12 to 23 months of tenure	-0.328***	-0.225**
	(0.101)	(0.095)
Observations	432,823	111,313

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.

16 and 19 and those older than 50 years, hence I am focusing on the reform effect for those people who are mainly attached to the labour market. Looking at the right hand side table panel, we can notice that between 1996 and 2002 the effect of the reform was slightly lower 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 22% for those tenured less than 12 months, by about 23% for those tenured between 12 and 24 months.

Although the relevant research question Marinescu (2009) is investigating on, I 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 1996 to 2002 . Treatment group appears to be younger, less educated, more likely to be nonwhite and there is an higher percentage of females in that group.

Table 2 – *Covariate means and observational control samples*

Variable	Controls	Treated	t-test
Age	33.706	32.844	30.979
Female	0.515	0.532	-10.388
Black	0.016	0.018	-2.905
Other Ethnicity, different from Whites	0.025	0.027	-3.385
Low - Educated	0.235	0.243	-5.710
High-educated	0.324	0.317	4.989
Married	0.669	0.643	16.339
Number of observations	39,672	71,641	

Dehejia and Wahba (2002) using different comparison groups on LaLonde's data found evidence that matching treated and controls, using the Propensity Score Matching (Rosenbaum and Rubin, 1983), in a non-randomized study lead 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 I 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, I adopt the following procedure:[6] First, I 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, I 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, I can

[6]Further details in section 5.

deal with the reform evaluation. To this endI carry out the combination between Regression Discontinuity Design and Survival Data Analysis, [7] as already mentioned in section 1, to the best of my knowledge, would be the first time use in the literature.

5 Identification

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. Let $(h_1(t|c,x), h_0(t|c,x))$ be the two potential outcomes, one would experience after and before the policy implementation status, respectively. Hence, the causal effect of the reform on the hazard of being laid off would be potentially captured, separately by seniority, by the difference: $h_1(t|c,x) - h_0(t|c,x)$. However 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 cohort c at the same tenure level t), 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 the policy implementation status $\bar{c}$. Formally we can rewrite the previous statement in the following way:[8]

[7] More details in section 5.

[8] The interested reader might argue that the above assumption could be rewritten in the following way: $\lim_{c\to\bar{c}^+} h_1(t|C=c, X=x) - \lim_{c\to\bar{c}^-} h_0(t|C=c, X=x)$.

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

Following Imbens and Lemieux (2007)'s notation, $\overline{\mathsf{c}^{-}}$ and $\overline{\mathsf{c}^{+}}$ refer to those individuals who are dismissed just before/after the first cohort affected by the reform implementation (i.e. $\overline{\mathsf{c}}$). In so doing my identification strategy relies on a rather standard assumption made in the treatment evaluation literature (Imbens and Lemieux, 2007): I 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 the counterfactual hazard for the first cohort affected by the reform is well approximated by the factual hazard for the last cohort under the no-policy regime.

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 $\overline{\mathsf{c}}$ (i.e. a continuous pre-program measure). To fix the ideas, when the individuals are deterministically assigned to the treatment group (i.e. affected by the reform) whether they are all one one side of a cut-off score $\overline{\mathsf{c}}$ in our context (i.e. $\mathsf{c} \geq \overline{\mathsf{c}}$), while all the other are, analogously, assigned to the control group (i.e. not affected by the reform). 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.* [9] Even though, the treatment status is a deterministic function of one or more covariates - the cohort in our case - picture 1 clearly shows that the discontinuity drop is lower than one, which could be interpreted as a "imperfect compliance"(Lee and Lemieux, 2009). According to the treatment evaluation definition, the hazard of being dismissed fits neatly the fuzzy design.

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:

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

$$\lim_{c \to \bar{c}^+} h_1(t|C = c, X = x) - \lim_{c \to \bar{c}^-} h_0(t|C = c, X = x).$$

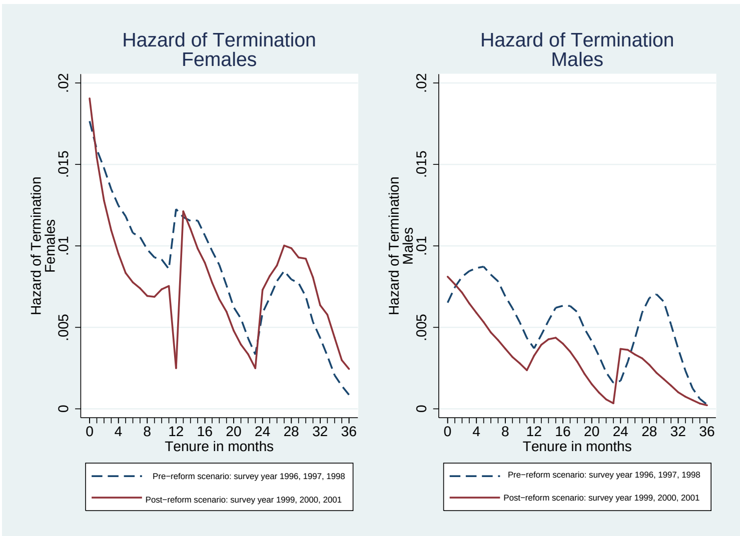
which is interpreted as the average casual effect of the treatment status i at the discontinuity point:

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

In what follows, figure 1 plots the dismissal hazard at tenure level: more precisely for tenures between 0 and 36 months for three pre-reform cohorts (1996, 1997 and 1998, respectively- blue dashed line), i.e. when the probationary period length was 24 months, and for three post-reform cohorts (1999, 2000 and 2001 respectively - red line), i.e. when the 1999 UK Unfair Dismissal Reform was enacted from 24 to 12 months, matching individuals by year of birth and education level, separately by gender.

The figure 1 neatly shows that the reform lead to a discontinuity between the pre and post reform scenario close to the 12th of tenure, especially for females.

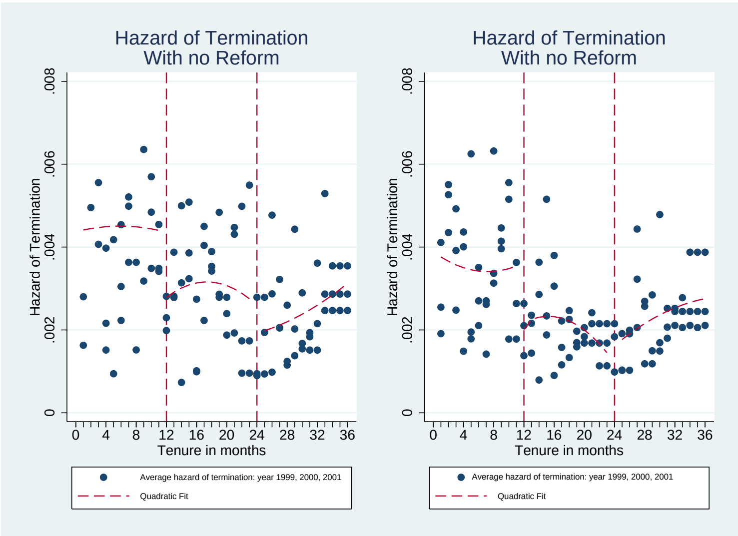
Fig. 1 *– Dismissal Hazard by Reform Scenario*



In this context the discontinuity is represented by 1999 cohort, looking into detail I am interested in analyzing whether the probationary period halving led to a shift in a dismissal hazard. In this regard, I address the interested reader, we could draw two relevant tenure levels 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.

Picture 2, in what follows, depicts the relationship between the mean dismissal hazard function at tenure level for three pre-reform cohorts (1996, 1997 and 1998, respectively - on the left hand side) and for three post-reform cohorts (1999, 2000 and 2001 respectively - on the right hand side) matching individuals by gender, year of birth and education level, ethnicity, industry, region of residence.

Fig. 2 *– Dismissal Hazard by Reform Scenario: tenure between 12 and 23 months*



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

Let be d a dummy variable denoting the policy implementation status (i.e. $\mathsf{d} = 1$ from 1999 onwards), and t the tenure at firm $\mathsf{t} \epsilon \{1, ..., 36\}$.

The effect of the reform on the hazard of being laid off could be empirically captured by the following equation, which , I think, represent a good approximation of the data:

$$\begin{aligned} h &= \alpha_1 + \alpha_2 \cdot \text{(Seniority Year)} \\ &+ \beta_1 \cdot t + \beta_2 \cdot t^2 + \\ &+ \beta_3 \cdot d + \beta_4 \cdot d \cdot t + \beta_5 \cdot d \cdot t^2 \\ &+ \varepsilon \end{aligned} \tag{1}$$

where $d = 1$ after 1998, $d = 0$ otherwise.
which is implemented allowing for interaction of all $\beta's$ terms with the two groups of dummies:

- $t \epsilon [12, 24)$
- $t \geq 24$

6 The Data

The data I use come from the rotating panel of the British Labour Force Survey (LFS)[10] for the period 1996-2001.

The LFS is conducted every quarter since 1992[11] on all aged 16 or older of around 60,000 households.

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 me to work with a reasonable sample size. Second, the detailed

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

[11]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.

employment information included in the LFS allow me 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 I can keep track a limited number of transitions. Second, I am not able to 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 the right to claim unfair dismissal. I 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. [12] Moreover, in the sample of analyzes the vast majority of them have a tenure lower than 24 months which makes identifying the probability of being fired after 2 years difficult, therefore I decide to focus just to permanent workers, reducing the

[12] 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.
  For further details: `http://www.opsi.gov.uk/si/si2002/20022034.htm`

sample by 41.40% of individuals (equal to 37.41% of the job spells). Next, I also exclude from the sample individuals who are 16 to 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.[13] This leaves us people age 20 to 49 years old, equal to 15.27% of the original individuals sample (or 15.65% of the original job spell sample). Furthermore, I drop 685 individuals (equal to 8,105 job spells) because I could not determine the industry in which they were employed, or their occupation.

The final sample consist of 80,302 job spells for 36,197 individuals. Tables 8 and 9 summarize the deletion that yield to the final sample.

In this analysis tenure computation is a central issue. Therefore I rearrange the data aiming at constructing consistent job spells histories.[14] 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.[15]

[13] See appendix for more details??.

[14] When I 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"

[15] I 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.

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 I can not keep track of any job change) I 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), I impute the new hiring date to the previous interview month[16]

For almost all of those workers, who loose their job the reason for the termination is known (more than 90%).[17] Closely related to Marinescu (2009) I 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).

All the individuals 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, I 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 I decided to drop it. In other words, since I aim at calculating the dismissal hazard and we can track it just for

[16]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, I dropped them).

[17]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 I 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.

the first 4 waves, I drop the last wave. Individuals who abandon the sample are supposed to do so at the end of the quarter covered by the interview.

It is worth pointing out that I am 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 September 1999, which would be especially relevant for the construction of the after-reform cohort.[18] Thus for the construction of our cohorts I decide to start from the third quarter (i.e. September/November) of each year: 1996, 1997, 1998, 1999, 2000, 2001. On the basis of this information I define 18 main cohorts conditional on i) their seniority (three main seniority groups defined) ii) on survey quarter (six main survey quarter data), which can be followed for up to 12 months:

Cohort	Description
	First Year in the Job
coh1:	*The group of those individuals who is in probation in the new regime.* More precisely, individuals in their first employment quarter in September 1996.[19]
coh2:	*The group of those individuals who is in probation in the new regime.* More precisely, individuals in their first employment quarter in September 1997.
coh3:	*The group of those individuals who is in probation in the new regime.* More precisely, individuals in their first employment quarter in September 1998.
coh4:	*The group of those individuals who is in probation in the new regime.* More precisely, individuals in their first employment quarter in September 1999.

Continued on next page...

[18]Table 13 presents a placebo regression aims at evaluating whether there is any anticipation effect.

[19]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.

... table 3 continued

coh5:	*The group of those individuals who is in probation in the new regime.* More precisely, individuals in their first employment quarter in September 2000.
coh6:	*The group of those individuals who is in probation in the new regime.* More precisely, individuals in their first employment quarter in September 2001.

Second Year in the Job

coh7:	*The group of those individuals who switch between probation and non probation due to the reform.* More precisely, individuals in their fifth employment quarter in September 1996.[20]
coh8:	*The group of those individuals who switch between probation and non probation due to the reform.* More precisely, individuals in their fifth employment quarter in September 1997.
coh9:	*The group of those individuals who switch between probation and non probation due to the reform.* More precisely, individuals in their fifth employment quarter in September 1998.
coh10:	*The group of those individuals who switch between probation and non probation due to the reform.* More precisely, individuals in their fifth employment quarter in September 1999.
coh11:	*The group of those individuals who switch between probation and non probation due to the reform.* More precisely, individuals in their fifth employment quarter in September 2000.

Continued on next page...

[20]This time span allows us to have exactly 24 months tenure for the first group, at the end of four waves, hence the former probationary period end.

... table 3 continued

coh12:	*The group of those individuals who switch between probation and non probation due to the reform.* More precisely, individuals in their fifth employment quarter in September 2001.

Third Year in the Job

coh13:	*The group of those individuals who have never been in probation.* More precisely, individuals in their ninth employment quarter in September 1996.[21]
coh14:	*The group of those individuals who have never been in probation.* More precisely, individuals in their ninth employment quarter in September 1997.
coh15:	*The group of those individuals who have never been in probation.* More precisely, individuals in their ninth employment quarter in September 1998.
coh16:	*The group of those individuals who have never been in probation.* More precisely, individuals in their ninth employment quarter in September 1999.
coh17:	*The group of those individuals who have never been in probation.* More precisely, individuals in their ninth employment quarter in September 2000.

Continued on next page...

[21]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.

... *table 3 continued*

coh18:	*The group of those individuals who have never been in probation.* More precisely, individuals in their ninth employment quarter in September 2001.

Figure 6 explains graphically the cohorts creation. Table 10 reports some descriptive statistics. Looking at this, we can see that the main reason for terminate a job are other types of termination (44.59%), the second one is quitting (38.48%) and after this to be laid off (16.93%). Analyzing the descriptive statistics here highlightes that the proportion of other type of termination is definitely higher than Marinescu's ones[22], 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 3, which contains the Kaplan - Meier monthly survivor function at firm by cohort and group of tenure in the raw data.

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).[23]

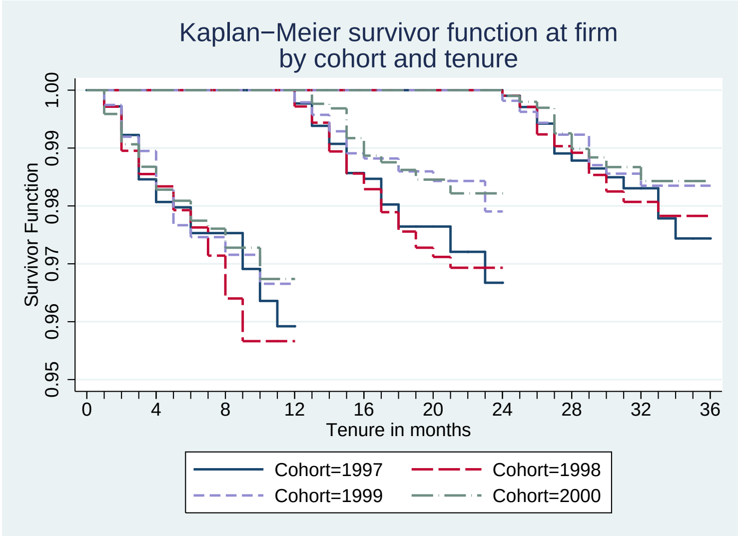
[22] I remind the interested reader that Marinescu's frequencies for each type are respectively: dismissal and redundancies (21.4%), quits (35.4%) and others (22.4%)

[23] To be precise, the survivor function is computed by following for up to 12 months those individuals 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

Fig. 3 – *Kaplan - Meier survivor function at firm*



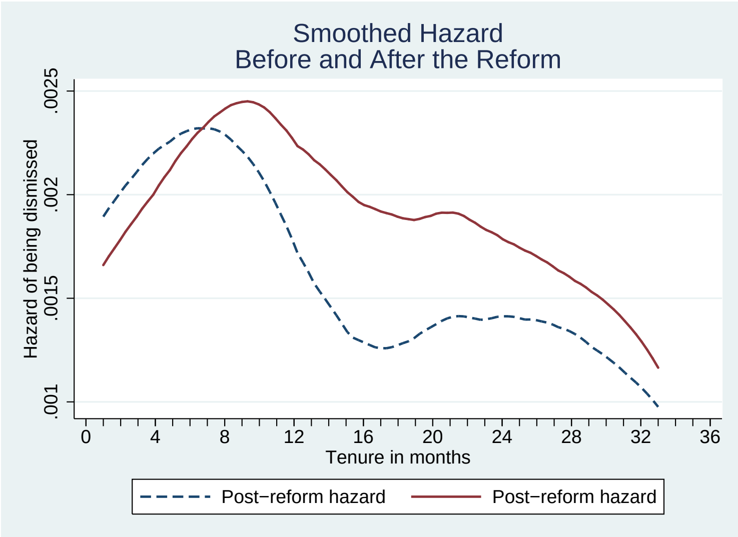
We can draw two main conclusions from the raw data: First, the probability to be discharged decreases with tenure. It is clearly evident that the first two groups have (the left and central panel) a lower survival function compared to the third one (the right panel). With this in mind, the picture, neatly, 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, which in turns 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 3 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.

years cohorts starting from each third quarter year.

Figure 4 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.

Fig. 4 *– 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 is 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 I am 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 my, 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, [24] while I am 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, I am working with the year between 1996 and 2001, hence we are subject to different economic cycles.

In addition to the previous ones, we can draw two main conclusions from picture 4 the implementation of the 1999 UK Unfair Dismissal Reform seems to have led to an increase in the probability of being dismissed in the first seven months, while thereafter it seems that the reform led 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 3. However, one aspect is worth noting: the comparison between the hazard and the survivor function (figure 4 and figure 3) shed light on one main evidence while according to

[24] 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.

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 I present the results of the estimation results.

7 Results

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

First, I define 18 main cohorts according to their tenure and the survey year.

Second, for each cohort, I matched the individual by observables as described above. At this issue, I stratify data by gender and education (i.e. high and low level of education), thus defining four cells. Via Propensity Score Matching, within each cell, I align the distribution of other covariates, namely region of residence, industry (sic codes) and year of birth, ethnicity to the distribution of these variables for those with tenure between 12 and 24 months in 1999. Third, for each survey year(i.e. 1996 - 2001), I compute the average hazard of being dismissed by tenure level (between 0 and 36 months). Fourth, I regress the hazard of being dismissed on a quadratic polynomial in tenure and on the policy implementation dummy and a quadratic polynomial in the interaction between policy implementation dummy and dummies of tenure level as presented in equation 1 in section 5.

The results are summarized in table 4, suggesting that the probationary period shift led to a decrease in dismissal hazard by about 1% (0.8%). More precisely for those tenured between 12 and 24 month by about 0.1%, although this result is only statistically significant at level of 10%. It is worth stressing these results suggest that the cohort after the reform

Table 4 – The Reform Effect on the Dismissal Hazard

	(1) Reform effect Coefficients	S.E.
t	-0.00017	(0.00109)
t^2	0.00007	(0.00013)
d	-0.00846**	(0.00380)
$d \cdot t$	0.00043	(0.00126)
$d \cdot t^2$	-0.00004	(0.00017)
$12 <= t <= 23$	-0.02971**	(0.01349)
$12 <= t <= 23 \cdot t$	0.00367*	(0.00199)
$12 <= t <= 23 \cdot t^2$	-0.00017	(0.00014)
$12 <= t <= 23 \cdot d$	-0.00157*	(0.00088)
$12 <= t <= 23 \cdot t \cdot d$	0.00003	(0.00111)
$12 <= t <= 23 \cdot t^2 \cdot d$	0.00004	(0.00016)
$t >= 24$	-0.03033	(0.03068)
$t >= 24 \cdot t$	0.00173	(0.00244)
$t >= 24 \cdot t^2$	-0.00009	(0.00014)
$t >= 24 \cdot$ d	0.01699	(0.15338)
$t >= 24 \cdot t \cdot d$	-0.00093	(0.01139)
$t >= 24 \cdot t^2 \cdot d$	0.00004	(0.00027)
α	0.00571***	(0.00076)
Survey Year	Yes	
$E\{h\|12^+\} - E\{h\|12^-\}$	-0.00456*	0.00277
$E\{h\|24^+\} - E\{h\|24^-\}$	-0.19137	0.21059
N	222	

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Boostrapped SE-values in parentheses (1000 replications)

Controls are cohorts: 1996, 1997, 1998; while treated are cohorts 1999, 2000, 2001

implementation are less likely to be dismissed compared to their before reform implementation counterpart.

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

The results suggest that the 1999 UK Unfair Dismissal Reform led 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 my findings is quite simple. From 1st 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. I 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 worker 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. Put in other words, the worker's 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.

How do my findings compare with Marinescu's one?

The results here presented partly contradict Marinescu's ones. Analyzing the same reform, 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%.

My main concern on Marinescu identification is due to the controls group she focuses on. Since already 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.

I address this issue matching treated and controls by observable characteristics.

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

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". [25] However, the question of what impact various economic shocks and pieces of labour legislation had on manufacturing labor 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 labor turnover and he finds that this legislation

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

had a significant influence on the demand for labour. Burgess and Nickell (1990) analyzing 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 I aim at evaluating the effect of 1999 Unfair Dismissal Reform on the British Manufacturing dismissal. 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[26]. 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 terms of layoffs compared with the others industries: Such as public administration and defence, primary sector and health and social work.[27] Figure 5[28] 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).

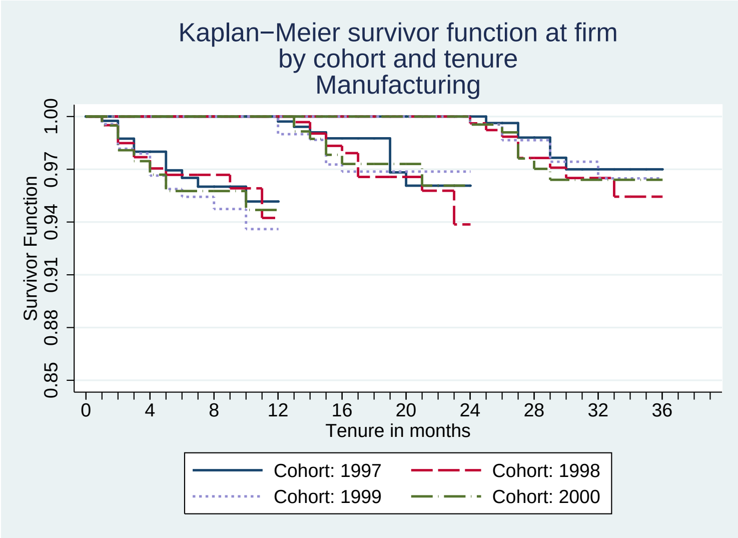
At first glance, while in the aggregate group the picture highlights a sharp difference between the pre and the post reform cohorts just for those people belonging to the second group of tenure and for those workers tenured more than 32 months (see section 6), in the

[26] www.ons.uk

[27] Picture not included but available upon request

[28] 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.

Fig. 5 – *Kaplan - Meier estimate of firing survivor function, by cohort and tenure and industry: Manufacturing*



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 behavior for those belonging to the first group of tenure, while we could not identify any clearly pattern for the others tenure groups. However, it is worth noting that at first view none of this patterns seems to be statistically significant.

The estimation results are presented in table 5.

The estimation results suggest there is any significant difference between the pre and the post reform scenario.

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 lead to reduced 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 firm productive process, either during normal demand periods, the firm will be less likely

Table 5 – *The Reform Effect on the Dismissal Hazard: Manufacturing*

	(1)	
	Reform effect	
	Coefficients	S.E.
t	-0.00046	(0.00350)
t^2	-0.00002	(0.00044)
d	-0.01148	(0.01578)
$d \cdot t$	0.00234	(0.00816)
$d \cdot t^2$	-0.00009	(0.00119)
$12 <= t <= 23$	-0.01609	(0.08002)
$12 <= t <= 23 \cdot t$	-0.00022	(0.01152)
$12 <= t <= 23 \cdot t^2$	0.00010	(0.00056)
$12 <= t <= 23 \cdot d$	-0.00068	(0.01826)
$12 <= t <= 23 \cdot t \cdot d$	0.00074	(0.00739)
$12 <= t <= 23 \cdot t^2 \cdot d$	-0.00004	(0.00116)
$t >= 24$	-0.00890	(0.22614)
$t >= 24 \cdot t$	-0.00138	(0.01674)
$t >= 24 \cdot t^2$	0.00009	(0.00051)
$t >= 24 \cdot d$	-0.04005	(0.58917)
$t >= 24 \cdot t \cdot d$	0.00339	(0.04094)
$t >= 24 \cdot t^2 \cdot d$	-0.00005	(0.00135)
α	-0.00736	(0.02980)
Survey Year	Yes	
$E\{h\|12^+\} - E\{h\|12^-\}$	0.00499	0 .01332
$E\{h\|24^+\} - E\{h\|24^-\}$	0. 08689	0..92815
N	222	

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Boostrapped SE-values in parentheses (1000 replications)

Controls are cohorts: 1996, 1997, 1998; while treated are cohorts 1999, 2000, 2001 to lay him off or during a decline demand period. The firm, in fact, would suffer a capital loss if such workers were permanently lost to the firm.[29]

9 Robustness checks

9.1 Gender differences

My evidence supports the thesis that the probationary period shift leads to an increase in the survival probability at firm. 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 I kept the workers whose wage was above the 10th percentile of the wage distribution.[30] 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.

[29]From my untabulated results, dividing the sample in skilled and unskilled labour, I find that shortening the probationary tends to have an increase in the dismissal hazard, even if negligible, for those unskilled workers tenured less than one year. On the contrary, evidence shows a completely different pattern for the skilled workers. The results presented above are in line with existing literature on human capital Becker (1962). The results show that for those workers whose task are not perceived as specific for the firm, i.e. unskilled, the reform leads to an anticipation effect -i.e. 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.

[30]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.

The estimation results separately by gender are presented in what follows, more precisely: Table 6 presents the estimation results for males, while table 7 presents the results for females.

Table 6 – *The Reform Effect on the Dismissal Hazard: Males*

	(1) Reform effect	
	Coefficients	S.E.
t	0.00500	(0.00347)
t^2	-0.00019	(0.00047)
d	-0.03428**	(0.01694)
$d \cdot t$	0.00142	(0.00895)
$d \cdot t^2$	-0.00055	(0.00136)
$12 <= t <= 23$	0.02502	(0.08758)
$12 <= t <= 23 \cdot t$	-0.00600	(0.01202)
$12 <= t <= 23 \cdot t*^2$	0.00018	(0.00058)
$12 <= t <= 23 \cdot d$	0.00154	(0.00767)
$12 <= t <= 23 \cdot t \cdot d$	-0.00195	(0.00767)
$12 <= t <= 23 \cdot t^2 \cdot d$	0.00061	(0.00132)
$t >= 24$	-0.03626	(0.27696)
$t >= 24 \cdot t$	-0.00123	(0.27696)
$t >= 2 \cdot t^2$	0.00011	(0.00062)
$t >= 24 \cdot d$	1.63880	(1.94313)
$t >= 24 \cdot t \cdot d$	-0.12318	(0.13996)
$t >= 24 \cdot t^2 \cdot d$	0.00281	(0.00304)
α	0.07716*	(0.04081)
Survey Year	Yes	
$E\{h\|12^+\} - E\{h\|12^-\}$	-0.03112**	0.01497
$E\{h\|24^+\} - E\{h\|24^-\}$	-1.57339	1.81212
N	222	

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Boostrapped SE-values in parentheses (1000 replications)

Controls are cohorts: 1996, 1997, 1998; while treated are cohorts 1999, 2000, 2001

The results suggest that the 1999 UK Unfair Dismissal Reform led to a negative effect of the dismissal probability for males only. What is most striking is that the effect on males is more than two times higher than the one for the aggregate group. More precisely, the table 6

Table 7 *– The Reform Effect on the Dismissal Hazard: Females*

	(1) Reform effect	
	Coefficients	S.E.
t	0.01006	(0.00630)
t^2	-0.00075	(0.00088)
d	0.01744	(0.01770)
$d \cdot t$	-0.01154	(0.01549)
$d \cdot t^2$	0.00080	(0.00400)
$12 <= t <= 23$	-0.01216	(0.04531)
$12 <= t <= 23 \cdot t$	-0.00651	(0.00825)
$12 <= t <= 23 \cdot t^2$	0.00064	(0.00088)
$12 <= t <= 23 \cdot d$	-0.00162	(0.01842)
$12 <= t <= 23 \cdot t \cdot d$	0.00941	(0.01385)
$12 <= t <= 23 \cdot t^2 \cdot d$	-0.00074	(0.00396)
$t >= 24$	-0.08752	(0.21078)
$t >= 24 \cdot t$	-0.00429	(0.01587)
$t >= 24 \cdot t^2$	0.00068	(0.00090)
$t >= 24 \cdot d$	-0.03366	(0.50505)
$t >= 24 \cdot t \cdot d$	0.01430	(0.04007)
$t >= 24 \cdot t^2 \cdot d$	-0.00088	(0.00408)
α	0.00853	(0.03128)
Survey Year	Yes	
$E\{h\|12^+\} - E\{h\|12^-\}$	-0.00160	0 .00991
$E\{h\|24^+\} - E\{h\|24^-\}$	0.89795	2.54587
N	222	

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Boostrapped SE-values in parentheses (1000 replications)

Controls are cohorts: 1996, 1997, 1998; while treated are cohorts 1999, 2000, 2001 highlights that the probationary period shortening decreased the dismissal by about 3% for males only.

10 Conclusions

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

First, the paper combines survival data with a RDD framework. Second, by offering a direct comparison between the RDD and DID (Marinescu, 2009), this paper highlights that the identification strategy used matters.Third, since the effect of the reform could be heterogenous among different industries due to their different screening procedure, I investigate the reform effect on a specific industry: Manufacturing.

It is worth pointing out that evidence partly contradicts Marinescu (2009). 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 show consistently that the 1999 Unfair Dismissal led 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 the estimation results here presented and Marinescu's ones we find evidence that using her definition of treated and controls our results are close to hers.

Looking at the reform effect on manufacturing, my evidence show that shortening the probationary period increase the probability of being dismissed for those whose tenure is lower than 12 months.

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.

I interpret the 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. In other words, initially, for the firm waiting to acquire more information on worker's ability is less costly than dismiss him. Thus, the dismissal probability would be higher at the beginning. Put in other words, the worker's 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.

References

Abadie, A. (2005): "Semiparametric difference-in-differences estimators," *The Review of Economic Studies*, 72(1), 1–19.

Angrist, J., and J. Pischke (2009): *Mostly harmless econometrics: an empiricist's companion*. Princeton Univ Pr.

Arulampalam, W., A. L. Booth, and M. L. Bryan (2004): "Training and the new minimum wage," *Economic Journal*, 114(494), C87–C94.

Becker, G. S. (1962): "Investment in Human Capital: A Theoretical Analysis," *Journal of Political Economy*, 70, 9.

Blundell, R., M. Brewer, and M. Francesconi (2008): "Job Changes and Hours Changes: Understanding the Path of Labor Supply Adjustment," *Journal of Labor Economics*, 26(3), 421–453.

Booth, A. L., and G. Zoega (2003): "On the welfare implications of firing costs," *European Journal of Political Economy*, 19(4), 759–775.

Buechtemann, C. E. (1993): "Employment Security and Labor Market Behavior Interdisciplinary Approaches and International Evidence," *Labour*, 7(3), 34–34.

Burgess, S. M., and S. Nickell (1990): "Labour Turnover in UK Manufacturing," *Economica*, 57(227), 295–317.

Campbell, D. T. (1969): "Reforms as Experiments," *American Psychologist*, 24, 109–429.

Commission, L. P. (2003): "The National Minimum Wage: Fourth Report of the Low Pay Commission, Building on Success," Discussion paper.

Davies, P. L., and M. Freedland (1993): *Labour Legislation and Publi Policy: a Contemporary Histoy. Oxford: Claredon Press*.

DeFreitas, G., and A. Marshall (1998): "Labour Surplus, Worker Rights and Productivity Growth: A Comparative Analysis of Asia and Latin America," *LABOUR*, 12(3), 515–539.

Dehejia, R. H., and S. Wahba (2002): "Propensity Score-Matching Methods For Nonexperimental Causal Studies," *The Review of Economics and Statistics*, 84(1), 151–161.

Farber, H. S. (1994): "The Analysis of Interfirm Worker Mobility," *Journal of Labor Economics*, 12(4), 554–93.

Francesconi, M., and W. van der Klaauw (2007): "The Socioeconomic Consequences of 'In-Work' Benefit Reform for British Lone Mothers," *Journal of Human Resources*, 42(1).

Hahn, J., P. Todd, and W. Van der Klaauw (2001): ""Identification and Estimation of Treatment Effects with a Regression-Discontinuity Design"," *Econometrica*, 69(1), 201–09.

Ichino, A., and R. T. Riphahn (2005): "The Effect of Employment Protection on Worker Effort: Absenteeism During and After Probation," *Journal of the European Economic Association*, 3(1), 120–143.

Imbens, G., and T. Lemieux (2007): "Regression Discontinuity Designs: A Guide to Practice," Working Paper 337, National Bureau of Economic Research.

Jovanovic, B. (1979): "Job Matching and the Theory of Turnover," *Journal of Political Economy*, 87(5), 972–90.

Kersley, B., C. Alpin, J. Forth, A. Bryson, H. Bewley, G. Dix, and S. Oxenbridge (2005): ""Inside the Workplace: First Findings from the 2004 Workplace Employment Relations Survey (WERS 2004)"," Working paper, Department of Trade and Industry.

Kugler, A. D. (1999): “The Impact of Firing Costs on Turnover and Unemployment: Evidence from the Colombian Labour Market Reform,” *International Tax and Public Finance*, 6(3), 389–410.

Lalive, R., J. vanOurs, and J. Zweimüller (2008): “The Impact of Active Labour Market Programmes on The Duration of Unemployment in Switzerland,” *Economic Journal*, 118(525), 235–257.

LaLonde, R. J. (1986): “Evaluating the Econometric Evaluations of Training Programs with Experimental Data,” *American Economic Review*, 76(4), 604–20.

Lee, D. S., and T. Lemieux (2009): “Regression Discontinuity Designs in Economics,” NBER Working Papers 14723, National Bureau of Economic Research, Inc.

Loh, E. S. (1994): “Employment Probation as a Sorting Mechanism,” *ndustrial and Labor Relations Review*, 18(3), 471–483.

Marinescu, I. (2009): “Job Security Legislation and Job Duration: Evidence from the United Kingdom,” *Journal of Labor Economics*, 27(3), 465–486.

Maré, D. C. (2006): “Constructing Consistent Work-life Histories: A guide for users of the British Household Panel Survey,” ISER working papers 2006-39, Institute for Social and Economic Research.

Nunziata, L., and S. Staffolani (2007): “Short-Term Contracts Regulations And Dynamic Labour Demand: Theory And Evidence,” *Scottish Journal of Political Economy*, 54(1), 72–104.

OECD (2005): *OECD Employment Outlook*. OECD.

of Justice, M. (2010): “Employment Tribunal and EAT Statistics (GB),” Ministry of justice technical report, Ministry of Justice.

Parsons, D. O. (1972): "Specific Human Capital: An Application to Quit Rates and Layoff Rates," *Journal of Political Economy*, 80(6), 1120–43.

Pissarides, C. A. (2001): "Employment protection," *Labour Economics*, 8(2), 131–159.

Riphahn, R. T., and A. Thalmaier (2001): "Behavioral Effects of Probation Periods: An Analysis of Worker Absenteeism," *Journal of Economics and Statistics (Jahrbuecher fuer Nationaloekonomie und Statistik)*, 221(2), 179–201.

Rosenbaum, P. R., and D. B. Rubin (1983): ""The central role of the propensity score in observational studies for causal effects "," *Biometrika*, 70(1), 41–55.

Sadanand, A., V. Sadanand, and D. Marks (1989): "Probationary Contracts in Agencies with Bilateral Asymmetric Information," *Canadian Journal of Economics*, 22(3), 643–61.

Saint-Paul, G. (1994): "High Unemployment From a Political Economy Perspective," DELTA Working Papers 94-23, DELTA (Ecole normale supérieure).

Smith, P., and G. Morton (2001): "New Labour's reform of Britain's employment law: The devil is not only in the detail but in the values and policy too," *British Journal of Industrial Relations*, 39(1), 119–138.

Stewart, M. B. (2004): "The Impact of the Introduction of the U.K. Minimum Wage on the Employment Probabilities of Low-Wage Workers," *Journal of the European Economic Association*, 2(1), 67–97.

Trochim, W. M. K. (1984): *Reserach Design for Program Evaluation: the Regress Discontinuity Approach. Berverly Hills: Sage Publications.*

Wang, R., and A. Weiss (1998): "Probation, layoffs, and wage-tenure profiles: A sorting explanation," *Labour Economics*, 5(3), 359–383.

Wickens, M. R. (1978): "An Econometric Model of Labour Turnover in U.K. Manufacturing Industries, 1956-73," *Review of Economic Studies*, 45(3), 469–77.

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.

I 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 36 months

individuals satisfy these restrictions 36,197.

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 equal 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.

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

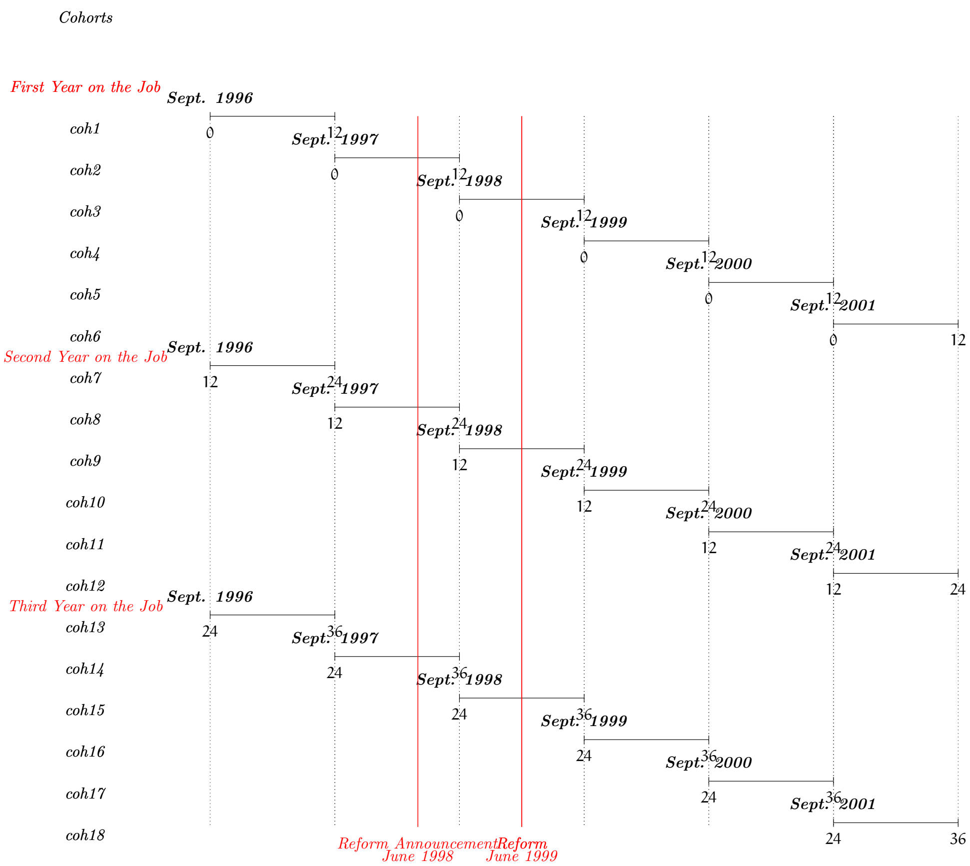


Fig. 6 – *Definition of the Sample*

Table 8 – *Summary Statistics for the sample of full-time employees: Job Spell Level*

	1996	1997	1998	1999	2000	2001	Total	Freq
Original Sample	15,779	14,450	14,410	13,955	12,760	13,674	85,118	100
Deleted								
(1)	7,059	6,401	6,016	5,585	4,998	5,179	35,238	41.40
(2)	2,195	2,003	2,211	2,199	2,152	2,238	12,998	15.27
(3)	152	129	63	105	106	130	685	0.80
Final								
sample	6,373	6,007	6,120	6,066	5,504	6,127	36,197	42.53
(4)	4,817	3,921	3,951	2,979	3,412	3,972	23,052	23.08
Final sample after matching	1,566	2,086	2,169	3,087	2,092	2,155	13,145	15.44

(1) Not permanently employees working more than 16 hr per week
(2) Aged younger than 20 or older than 50
(3) Missing values on key variables
(4) Not matched observations

Table 9 – *Summary Statistics for the sample of full-time employees: Job Spell Level*

	1996	1997	1998	1999	2000	2001	Total	Freq
Original Sample	34,055	32,299	32,799	31,489	28,567	29,148	188,357	100
Deleted								
(1)	14,113	13,054	12,418	11,316	10,021	9,549	70,471	37.41
(2)	4,973	4,726	5,179	5,093	4,979	4,529	29,479	15.65
(3)	1,570	1,365	1,246	1,284	1,129	1,511	8,105	4.30
Final								
sample	13,399	13,154	13,956	13,796	12,438	13,559	80,302	42.63
(4)	11,833	11,036	11,730	8,190	10,260	11,296	64,345	34.16
Final sample after matching	1,566	2,118	2,226	5,606	2,178	2,202	15,896	8.43

(1) Not permanently employees working more than 16 hr per week
(2) Aged younger than 20 or older than 50
(3) Missing values on key variables
(4) Not matched observations

Table 10 – *Summary statistics for the sample of permanent full-time employees*

Cohort	**Age**	**t-test coh. vs. 1999 P-value**	**Female**	**t-test coh. vs. 1999 P-value**	**Lower Degree Education**	**t-test coh. vs. 1999 P-value**	**High School Degree Education**	**t-test coh. vs. 1999 P-value**	**College or Higher Education**	**t-test coh. vs. 1999 P-value**
Tenure between 0 and 12 months										
1996	32.79	0.3304	0.55	0.2160	0.27	0.0000	0.44	0.0237	0.29	0.0000
1997	32.84	0.5088	0.54	0.6925	0.26	0.0000	0.45	0.0039	0.29	0.0000
1998	32.83	0.4558	0.58	0.0000	0.23	0.0000	0.45	0.0012	0.32	0.0012
1999	33.25	0.0821	0.56	0.0281	0.24	0.0000	0.44	0.0458	0.32	0.0000
2000	33.39	0.0106	0.56	0.0375	0.24	0.0000	0.42	0.9039	0.35	0.0000
2001	33.64	0.0001	0.55	0.2027	0.20	0.0667	0.45	0.0022	0.35	0.0000
Total	33.18	0.56	0.24	0.44	0.32					
Tenure between 12 and 24 months										
1996	32.73	0.2274	0.55	0.4684	0.25	0.0000	0.45	0.0063	0.30	0.0000
1997	32.73	0.2321	0.52	0.1021	0.21	0.0042	0.45	0.0192	0.34	0.0000
1998	32.93	0.8964	0.54	0.9035	0.24	0.0000	0.40	0.0282	0.37	0.0163
1999	32.96		0.54		0.19		0.42		0.39	
2000	33.37	0.0306	0.52	0.0453	0.20	0.1050	0.42	0.7202	0.37	0.0922
2001	32.68	0.2501	0.54	0.9714	0.20	0.2329	0.41	0.3448	0.39	0.9927
Total	32.92		0.53		0.22		0.42		0.36	
Tenure between 24 and 36 months										
1996	33.01	0.7873	0.52	0.1311	0.23	0.0001	0.44	0.0516	0.33	0.0000
1997	33.92	0.0000	0.54	0.9110	0.24	0.0000	0.43	0.2588	0.33	0.0000
1998	33.07	0.5473	0.53	0.5345	0.21	0.0465	0.43	0.2570	0.36	0.0053
1999	32.74	0.2683	0.54	0.7287	0.18	0.6162	0.41	0.5386	0.40	0.3089
2000	33.37	0.0392	0.51	0.0270	0.20	0.1573	0.41	0.5117	0.39	0.6292
2001	33.20	0.2327	0.54	0.8819	0.18	0.6076	0.42	0.7217	0.39	0.9608

Table 11 – *Summary statistics for the sample of permanent full-time employees*

Personal Characteristics	
Reasons for leaving last job	
Layoff	16.93
Quit	38.48
Other	44.59
N. observations	4,005

Table 12 – *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	830	956

Table 13 – Placebo Regression

	(1) Reform effect	
	Coefficients	S.E.
t	0.00177	(0.00147)
t^2	-0.00019	(0.00016)
d	-0.00128	(0.00468)
$d \cdot t$	-0.00163	(0.00280)
$d \cdot t^2$	0.00032	(0.00036)
$12 <= t <= 23$	0.00879	(0.01765)
$12 <= t <= 23 \cdot t$	-0.00321	(0.00256)
$12 <= t <= 23 \cdot t^2$	0.00024	(0.00017)
$12 <= t <= 23 \cdot d$	-0.00206	(0.00183)
$12 <= t <= 23 \cdot t \cdot d$	0.00163	(0.00237)
$12 <= t <= 23 \cdot t^2 \cdot d$	-0.00032	(0.00035)
$t >= 24$	0.00431	(0.04116)
$t >= 24 \cdot t$	-0.00207	(0.00330)
$t >= 24 \cdot t^2$	0.00019	(0.00017)
$t >= 24 \cdot d$	-0.03214	(0.06094)
$t >= 24 \cdot t \cdot d$	0.00407	(0.00528)
$t >= 24 \cdot t^2 \cdot d$	-0.00037	(0.00037)
α	0.00473	(0.00291)
$E\{h\|12^+\} - E\{h\|12^-\}$	-0.00201	0 .00125
$E\{h\|24^+\} - E\{h\|24^-\}$	0.32390	0.27066
N	74	

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Boostrapped SE-values in parentheses (1000 replications)

Controls are defined as cohort: 1996 while treated are defined as cohort: 1998