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CICLO XXI

# ESSAYS ON POLICY EVALUATION
# OF THE BRITISH LABOUR MARKET:
# TWO CASE STUDIES

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*To my family*

*"The important thing is not to stop questioning.*
*Curiosity has its own reason for existing.*
*One cannot help but be in awe when he contemplates*
*the mysteries of eternity, of life,*
*of the marvelous structure of reality.*
*It is enough if one tries merely to comprehend*
*a little of this mystery every day.*
*Never lose a holy curiosity."*
*(Albert Einstein)*

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# Contents

# List of Figures

# List of Tables

# Introduction

On $18^{th}$ December, 2009, prof. Mario Draghi, currently the governor of the Bank of Italy, has been invested by a "Laurea Honoris Causae" in Statistics from the University of Padova. During his Lectio-magistralis prof. Mario Draghi stressed the fasting growing relevance to "find less syntectic and more consistent information aiming at representing hard to understand phenomena". This thesis attempts at filling this gap as regards the ongoing debate on the implications of Employment Protection Legislation.

With the term Employment Protection Legislation (EPL, henceforth) economists refer to a set of instruments which limit both the hiring and the firing procedures.

Even though, the economic literature, in the last two decades, has investigated the effect of EPL on employment levels and dynamics, the results are far from being conclusive. The main objective of the present thesis is to evaluate the implication of changing EPL focusing on two British reform occurred at the end of the 90s. The UK represents indeed an exceptional breeding ground for this issue. Britain is commonly known to be the country with the lowest EPL in Europe, the second one in the OECD countries, after the USA. Conversely to most of the other European countries, since when the New Labour came into power in 1997, the British labour market legislation has progressively strengthened, leading to a less flexible labour market.

Briefly, this Ph.D thesis is a compendium of two papers, each one corresponding to a chapter. In both chapters we adopt a typical Policy Evaluation Approach. In this regard, the first chapter analyzes the 1999 UK Unfair Dismissal Reform effects on the equilibrium outcome for workers and firms. The second one, using Barbieri and Sestito (2008)'s approach, tackles whether experiencing a temporary job-spell vs. staying in unemployment has an impact in terms of subsequent employment statuses in the UK.

More specifically, the first paper uses the UK Labour Force Survey from 1997 to 2001 to examine 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, leads to a significant decrease in the probability of being laid off amounting to 1% just for the newly covered. In other words, those workers whose tenure is between 12 and 23 months after the reform are less likely to be discharged compared with the same group in the pre-reform period. Our evidence, although, show that 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 workers 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.

The second paper investigates the effect of experiencing a spell of temporary contract vs. a spell of unemployment in the British labour market. Using short panels from the UK Labour Force Survey covering the period from 1997 to 2001, we find evidence that unemployed individuals are significantly less likely to transit to employment in the short-run horizon. However, the difference - in terms of transition to employment- between the two status is progressively declining. What is the reason for this result? Is this news "mask mounting evidence"? Our answer is the enactment of *New Deal Programme*- an active labour market policy designed to move long-term unemployed to work and away from welfare. Using Barbieri and Sestito (2008)'s approach we show that splitting the unemployed into categories: New Deal eligible (i.e. long-term unemployed) and short-term unemployed, the effect of being temporary vs. a spell of unemployment moved from 30% for the younger group (46% for the older group)to 25%(44% for the olders) higher chances to get a job. At the same time, comparing the effect of being New Deal eligible vs stay in unemployment we could observe that the New Deal participants, after the New Deal implementation, are significantly more likely to find a job after nine months by about 5%. Finally, we do not find any significant effect when we look at the probability to transit into permanent jobs.

# Prefazione

In data 18 dicembre 2009, il professor Mario Draghi, attuale gobernatore della Banca d'Italia, è stato insegnito della "Laurea Honoris Causae" in statistica. Durante la sua Lectio-Magistralis, il professor Mario Draghi ha evidenziato la crescente necessitá di "informazioni meno sintetiche e piú capaci di rappresentare fenomeni articolati" (Draghi, 2009). Questa tesi mira a colmare questa lacuna nell' ambito del corrente dibattito sulla legislazione dell'Employment Protection.

Con il termine legislazione Employment Protection (EPL, nel seguito), gli economisti si riferiscono ad un insieme di strumenti destinati a limitare sia le assunzioni sia i licenziamenti.

In gran parte degli ultimi due decenni la letteratura economica ha studiato gli effetti dell' EPL sui livelli occupazionali e sulle sue dinamiche, senza peró raggiungere delle conclusioni. Il principale obiettivo di questa tesi é di valutare le implicazioni dovuti a cambiamenti della legislazione in materia di EPL, focalizzando l'attenzione su due riforme inglesi avvenute alla fine degli anni 90. A tal proposito il Regno Unito rappresenta un terreno fertile per la valutazione delle politiche. Sebbene tale nazione sia conosciuta per essere lo stato europeo con il basso livello di EPL - ed il secondo per quanto riguarda i paesi OECD, dopo gli USA. Sin dalla salita al potere del Partito dei New Labour avvenuta nel 1997 la legislazione in materia di occupazione sembra essersi irrigidita, muovendosi in direzione opposta rispetto maggioranza degli stati europei. Brevemente questa tesi è composta da 2 pepers, ciascuno corrispondente ad un capitolo, caratterizzati entrambi da un approccio tipico delle Valutazione delle Politiche.

A tal riguardo il primo capitolo analizza gli effetti sull'equilibrio tra imprese e lavoratori derivanti dall'attuazione della 1999 UK Unfair Dismissal Reform. Il secondo capitolo, altresì affronta l'effetto di essere lavoratori temporanei "vis-a-vis" essere disoccupati in termini di occupazione in orizzonte di breve periodo.

Piu' dettagliatamente, il primo articolo, utilizzando dati dal 1997 al 2001 provenienti dalla UK Labour Force Survey, eamina l'impatto della 1999 UK Unfair Dismissal Reform - che dimezzo il periodo di prova da due anni ad un anno - in termini di licenziamenti da parte delle imprese. Combinado tra loro tecniche di valutazione delle politiche, quali Difference-in-Differences e Regression Discontinuity Design, con tecniche tipiche della survival analysis i nostri risultati mostrano che la riduzione del periodo di prova, dovuta alla riforma, portó ad una significativa riduzione dei licenziamenti pari all'1% per i trattati - i.e. i lavoratori per i quali la durata dell'impiego è tra i 12 ed i 23 mesi, sebbene l'oltrepassare la soglia dei 12 mesi del periodo di prova dia luogo ad una riduzione non significativa nei licenziamenti. Concentrando la nostra analisi sul settore manifatturiero, i nostri risultati mostrano che il dimezzamento del periodo di prova conduce ad un aumento nella probabilitá di essere licenziati per quelli che hanno un rapporto lavorativo inferione ad un anno. Allo scopo di giustificare questo fenomeno, abbiamo diviso i lavoratori specializzati (*white collars*) da quelli non specializzati (*blue collars*). I nostri risultati supportano l'ipotesi che gli effetti della riforma siano eterogenei rispetto alle capacitá dei lavoratori.

Il secondo articolo compara l'effetto di essere un lavoratore tempoarneo rispetto ad essere disoccupato sul successivo outcome lavorativo nel mercato del lavoro inglese. Utilizzando degli short panels dal 1997 al 2001 con dati provenienti dalle UK Labour Force Survey, le stime da noi riportate evidenziano come i disoccupati abbiano una probabilità significativamente minore, rispetto ai temporanei, di trovare un lavoro nel breve periodo. Tuttavia il divario da noi trovato risulta assottigliarsi nel tempo. Quali potrebbero essere le ragioni di tale comportamento? Questa notizia maschera qualche effetto? La nostra risposta è l'attuazione del *New Deal Programme* - una politica atta a riportare i disoccupati di lungo periodo sul mercato del lavoro sgravando così lo stato sociale. Utilizzando l'approccio Barbieri and Sestito (2008) mostriamo come separando tra di loro i disoccupati in due categorie:

Individui idonei a partecipare al New Deal Programme (i.e. disocuppati di lungo periodo) e disoccupati di breve periodo, l'effetto di essere temporaneo passa da un 30% per il gruppo dei piu' giovani (46% per il gruppo degli adulti) ad un 25% (44%)piu' altre probabilitá di trovare un lavoro. Allo stesso modo, comparando gli individui idonei a partecipare al New Deal ed i disoccupati di breve periodo, dopo l'attuazione del New Deal, i primi risultano avere una probabilitá significativamente piú alta pari ad 5% di trovare un lavoro nove mesi dopo. Infine, guardando alla probabilità di trovare un lavoro permanente le nostre stime non mostrano alcun effetto di rilievo.

# Chapter 1

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

## 1.1 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 power 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.[1]

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

[1] For exceptions we address the interested reader to the appendix A.1

Reform, which halved the probationary period from two years to one year.

This reform was previously analyzed by Marinescu (2009),[2] although the relevance of her contribute to the economic literature some question for further work 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 in a Difference-in-Differences framework and Regression Discontinuity Design 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: workers tenured between 0 and 12 months and those whose tenure is higher than 24 months should be relatively unaffected by the reform. However, as 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 to avoid a potential trial in the event of termination. 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 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.[3] After the 1st of June 1999 the qualifying period was lowered

[2] Further details in section 1.4.

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

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 . However, given the extremely flexible nature of the UK labour market,[4] 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 in 1999 (Arulampalam, Booth, and Bryan, 2004) or Work Family Tax Credit in 2002 (Blundell, Brewer, and Francesconi, 2008, Francesconi and van der Klaauw, 2007), because of the different labour market flexibility - with respect to the Italian case - and because the different impact of the other UK legislation our 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 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 in-

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

vestigate 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 event approach and the matching approach". Conversely, the framework we deal with - i.e. the presence of two natural discontinuity in a timing-of-event setting - offers the possibility to combine the two approaches.

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, [5] i.e. producing a "Difference-in-Differences" (DID, henceforth) estimation.

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 1.2 reviews the EPL literature. Section 1.3 presents the 1999 UK Unfair Dismissal Reform. Section 1.4 presents the previous papers which have dealt with the 1999 UK Unfair Dismissal Reform. Section 1.5 introduces the econometric model. The data and some preliminary statistics are presented in section

[5] More details in section1.5

2.6. Section 1.7 presents the results. Section 1.8 presents the effect of the afore mentioned reform focusing just on the manufacturing industry. Section 1.9 presents a a large battery of robustness checks and finally section 2.8 concludes.

## 1.2 Literature Review

Two literature strands appear to be relevant to our analysis: One provides empirical evidence of the effect of EPL - dividing the main findings coming from the theoretical literature from those coming from the empirical literature - and the other provides a theoretical discussion of employment probationary periods.

### 1.2.1 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, at this issue 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 two EPL specific components: 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 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. At the same time, as stressed at the beginning of this section, this EPL component reduces, also, the hiring procedure. Ljungqvist (2002)'s calibration show that the 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*. Though the lens of the matching model increasing firing costs leads to mixed results. More specifically, as in a typical search model, if the firing costs do not decrease the outside option of the firm in the wage bargain the overall employment rises, otherwise the overall employment falls. Put in other words, when the outside option for the firm is weak, hence 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.

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, an 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. To fix the ideas the authors show that a separation determines a fall in tax base and hence a 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 signal 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.

### 1.2.2 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 10 OECD country cross-sectional aggregate data, 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.

### 1.2.3 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 2.1 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.

## 1.3 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 right for employers to dismiss workers 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. Even though the literature often compares weak employment protection 'a -la "USA-style" with the most protective "European-style" system, some similarities already exist. For example, in the USA, there are quite a few exceptions to the "employment at will" rule. Some of them are due to law and jurisprudence, such as antidiscrimination laws, and others to custom, such as the institution of tenure in USA universities. Still, the majority of the workforce in the USA remains under "employment at will". By contrast, in most European countries, employers can generally only fire workers for a "fair" reason. However, even in the most protective "European-style" employment protection is not given for granted from the beginning.

Even though institution of probationary in UK dates back to the early 1970s, its length changed several times in the last 20 years.[6] 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.

A more flexible labour market organization was the justification used by the new government for the probationary period change. [7] 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 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.[8]

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. [9]

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

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

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

[9](computed from the Survey of Employment Tribunal Applications, 2003, available on

## 1.4 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.

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 1.1 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 1.1 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.

Our evidence show that using treated and controls according to Mari-

www.data-archive.ac.uk) were lower than the limit prevailing before 1999. It seems, also to us, that it is unlikely that this change has affected firms' behavior. Therefore, we look at the variation of qualifying period as independent by the Employment Relations Act 1999.

[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.

Table 1.1: 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* |
| | (0.0878) | (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.

nescu 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, 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 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 1.2 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 1.2: 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 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 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

[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 1.5.

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 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 2.1, to the best of our knowledge, would be the first time use in the literature.

## 1.5 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.

### 1.5.1 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 $t$ had they been 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 need to be as close as possible

[13]More details in section 2.1.

[14]More details in section 1.5.2.

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 $\Delta_t$, we need to make the following assumptions (Abadie, 2005):

$$E[h_t^1 - h_t^0 | X, D = 1] = E[h_t^1 - h_t^0 | X, D = 0] \tag{1.1}$$

Assumption 1.1is 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 1.1 fall in the so-called "selection on observables" (or CIA) requirement, which can be written in the following fashion:

$$H_t^1, H_t^0 \quad \perp \quad D | X \tag{1.2}$$

Assumption 1.2 requires that conditional on observed characteristics X selection bias disappears.

If assumption (1.2) holds the effect of the treatment on the treated conditional on set of covariates X could be written as:

$$\begin{aligned} E[h_1^1 - h_1^0 | X, D = 1] &= \{E[h_1 | D = 1] - E[h_1 | D = 0]\} + \\ &- \{E[h_0 | D = 1] - E[h_0 | D = 0]\}. \end{aligned} \tag{1.3}$$

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

However, as argued in section 1.4 we are concerned whether condition 2.3 may hold. Thus, in order to correct for selection bias based on observable characteristics we perform a matching procedure.[15] Matching is widely

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

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:

$$0 < \Pr(D = 1|X = x) < 1, \ \forall x \in \widetilde{X} \tag{1.4}$$

Assumption 1.4 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 ( 1.1- 2.3 -1.4) hold we can rewrite ATT in the following fashion:

$$\begin{aligned} ATT &= E[h_1^0 - h_0^0|D = 1] = \\ &= E_{p(X)}[(E(h_1^0|D = 1, p(X)) - E(h_0^0|D = 1, p(X)|D = 1] = \\ &\stackrel{CIA}{=} E_{p(X)}[(E(h_1^0|D = 1, p(X)) - E(h_0^0|D = 0, p(X)|D = 1] = \\ &= E_{p(X)}[(E(h^0|D = 1, p(X)) - E(h^0|D = 0, p(X)|D = 1] = \\ &\stackrel{1.1}{=} E[h_1^0 - h_0^0|D = 0], \end{aligned} \tag{1.5}$$

Equation (1.5) 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 (Ashenfelter and Card (1985), Heckman

[16] Aiming at evaluating the the so-called time-invariance assumption in section A.9 are presented the activity rates, the Gross Domestic Product and the unemployment rate growth during in the analyzes period, see for further details graphics (A.1)- (A.3).

(1997)), which relies on the assumption that the average treatment effect on the treated could be identified by the difference between average outcomes for treated and controls over time.

$$\begin{aligned} \beta_{CDID} &= \{E[h_1|p(x), D = 1] - E[h_1|p(x), D = 0]\} + \\ &- \{E[h_0|p(x), D = 1] - E[h_0|p(x), D = 0]\} \end{aligned} \tag{1.6}$$

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 (2.3) holds we can rewrite equation

(1.5):

$$
\begin{aligned}
\beta_{CDID} &= E[h_{g99}|p(X), D=1] - E[h_{g99}|p(X), D=0] = \\
&= E[h_{g99}|p(X), D=1] - E[h_{g98}|p(X), D=0] + \\
&+ E[h_{g98}|p(X), D=0] - E[h_{g99}|p(X), D=0] = \\
&= E[h_{g99}|p(X), D=1] - E[h_{g98}|p(X), D=0] + \\
&- (E[h_{g99}|p(X), D=0] - E[h_{g98}|p(X), D=0]) = \\
&\stackrel{1}{=} [h_{g99}|p(X), D=1] - E[h_{g98}|p(X), D=0] + \\
&- (E[h_{g98}|p(X), D=0] - E[h_{g97}|p(X), D=0]) \qquad (1.7)
\end{aligned}
$$

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.

### Matching Algorithms

As stated in section 2.1, 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 2.1 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

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

the caliper, the pairs would be automatically discharged. [18]

### 1.5.2 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 2.1 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)'s notation, $\overline{t^-}$ and $\overline{t^+}$ refer to those individuals whose tenure is slightly lower or slightly

[18] The covariates comparison between matched and unmatched samples and the estimated propensity score are presented in table A.10.

[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).

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 $\mathcal{T}$. (i.e. a continuous pre-program measure). 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.

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

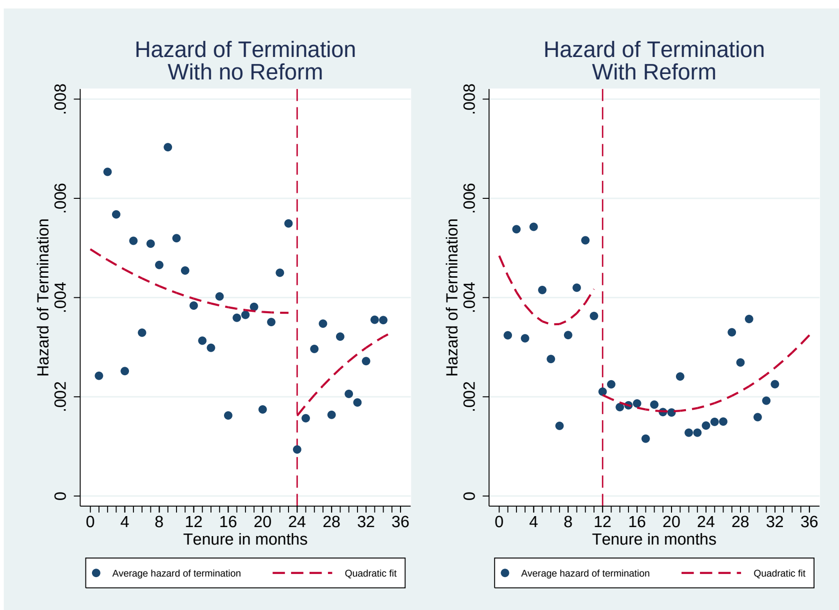
[20]See Trochim (1984) for further details.

discontinuity. Furthermore, after the reform the legal probationary period was shorten to 12 months. Therefore 12 represents another discontinuity.

In what follows, figure 1.1 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 1.1: 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 between 12 and 24 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]

$$\begin{aligned} h_i &= \alpha_i + \gamma_i \cdot \mathsf{Tenure} + \gamma_{i2} \cdot \mathsf{Tenure}^2 + \\ &+ \beta_i D_i + \beta_{i2} D_i \cdot \mathsf{Tenure} + beta_{i3}(D_i \cdot \mathsf{Tenure})^2 + \varepsilon \end{aligned} \tag{1.8}$$

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, 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.

## 1.6 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 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

[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 A.9 for further details.

[22] A brief description of the dataset and the covariates is included in appendix A.7.

[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.

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 the right to 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 majority of them

[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

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 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.[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 A.1 and A.2 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]

[25]See appendix for more detailsA.9.

[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.

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]

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 A.3 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

[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).

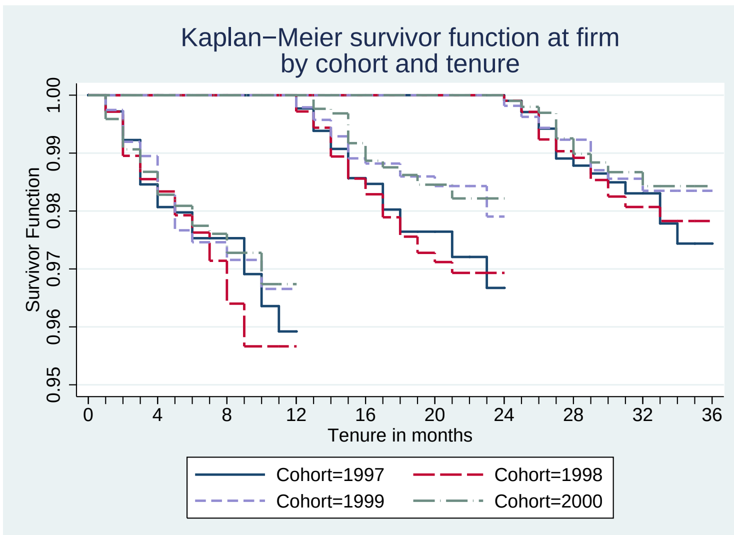
[29]The list of possible reason for terminate a job are: dismissed, made redundant, temporary job finished, resigned, gave up for health reasons, took early retirement, retired, gave up for family or personal reasons, other reason. It is worth noting that in the LFS the individuals declare how they perceive 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%)

job finished.

The empirical relevance of the 1999 UK Unfair Dismissal is clearly evident from figure 1.2, which contains the Kaplan - Meier monthly survivor function at firm by cohort and group of tenure in the raw data.

Figure 1.2: 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]

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

[31] 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)

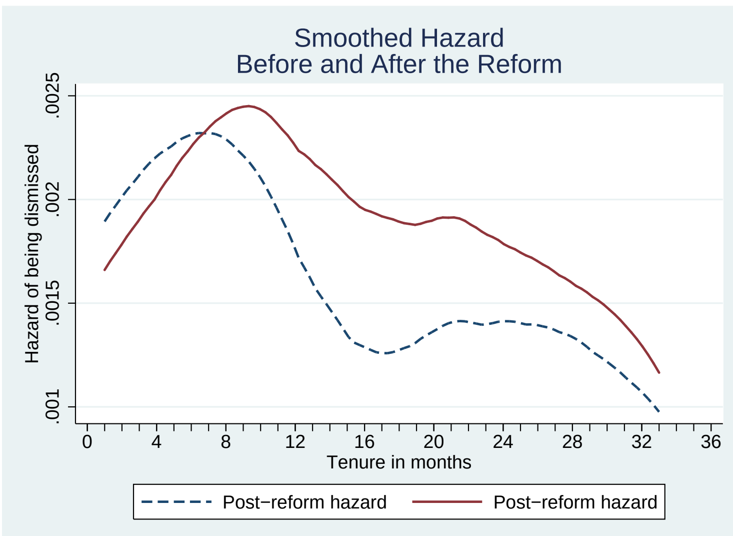
respectively in the third quarter of each year. The reform was enacted in June 1999 (second quarter), thus aiming at getting rid of any possible confounding factors for the 1999 cohort we chose to define the three years cohorts starting from each third quarter year.

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 1.2 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.

Figure 1.3 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 1.3: 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 1.3 the implementation of the 1999 UK Unfair Dismissal Reform seems to have lead to an increase in the probability of being dismissed in the first seven months, while thereafter it seems that the reform lead 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 1.2. However, one aspect is worth noting: the comparison

[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.

between the hazard and the survivor function (figure 1.3 and figure 1.2) 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.

## 1.7 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.

### 1.7.1 Conditional Difference in Differences

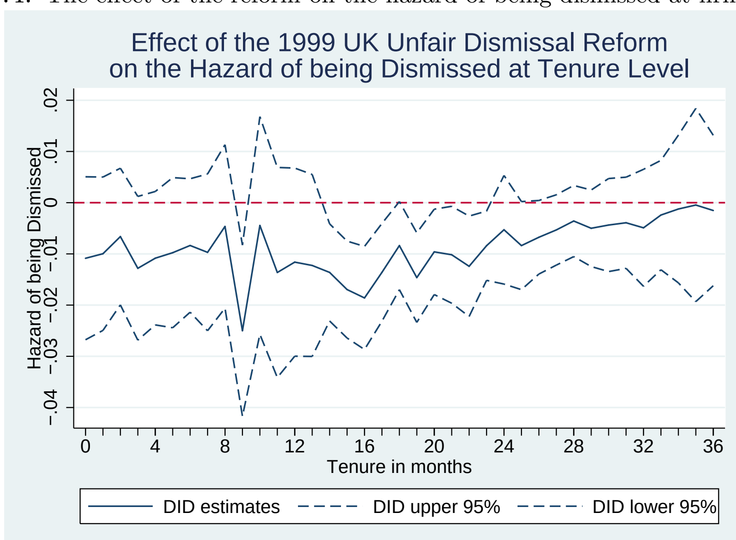
The estimation procedure we take can be described as follows. First, we define three main groups according to their tenure and we define as treated those individuals whose tenure is between 12 and 24 months of tenure after the reform and as controls all the others, namely workers tenured between 0 and 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 1.4). In our setting the assignment 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 1.2) 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 1.4 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 1.4: 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

[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.

level of 5%. This peak might be due to the fact that according to figure 1.3 the ninth month of tenure represents the peak of dismissal hazard in the pre-reform period, while after the reform implementation the peak 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 1.7.2.

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

### 1.7.2 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 1.8.

The results are summarized in table 1.3, 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 1.3: 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. (1.8). 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 1.7.1 close to the threshold - i.e. 12 month of tenure.

### 1.7.3 Discussion of the results: a comparison with the existing literature

Our findings suggest that the 1999 UK Unfair Dismissal Reform lead 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 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.

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 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 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. 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.

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 identification is due to the controls group she focuses on. Since already as mentioned in section 1.4 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 1.4, that using her definition of treated and controls our estimated results are close to Marinescu's ones, while using our approach we find results that partly contradict hers.

## 1.8 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

[34] See section 2.1.

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

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

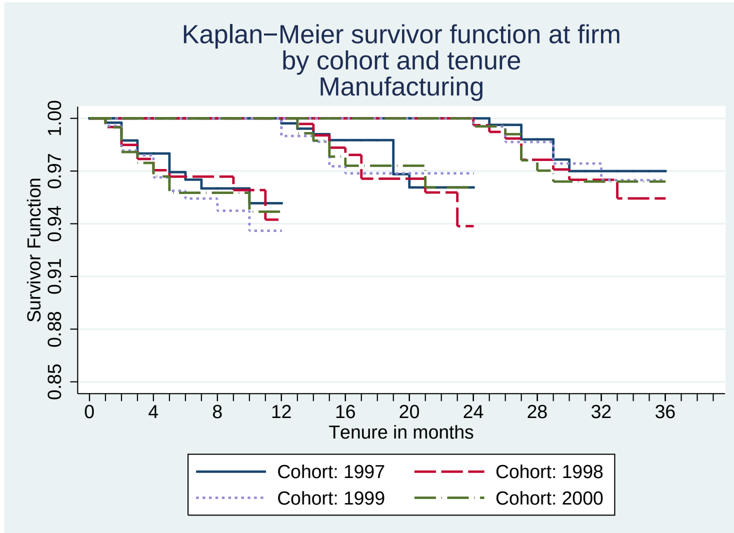
Figure 1.5[38] depicts the relationship between the survivor function at tenure level for two pre-reform cohorts (1997 and 1998, respectively) and for two post-reform cohorts (1999 and 2000, respectively) and by group tenure (those belonging to the first year of tenure, those belonging to the second year of tenure and finally those belonging to the third year of tenure).

[36] www.ons.uk

[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.

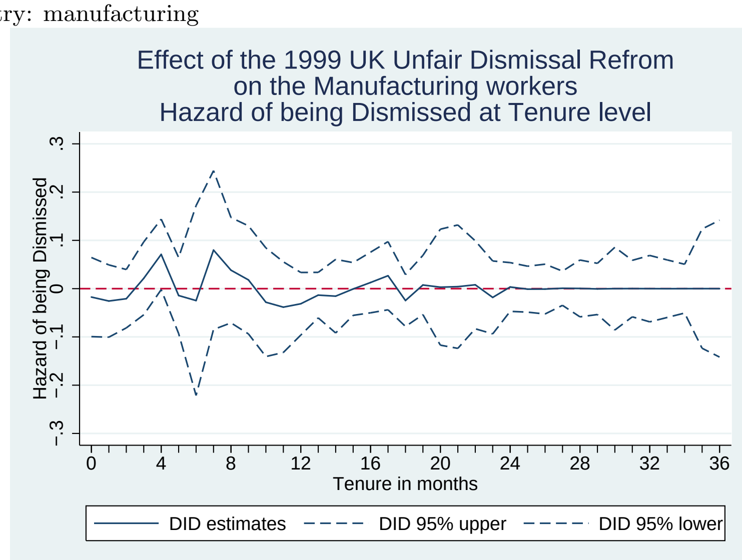
Figure 1.5: Kaplan - Meier estimate of firing survivor function, by cohort and tenure - Females




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 2.6), 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 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.

We report our estimation results by way of graphical illustration. Figure 1.6 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 confirm the finding highlighted by figure 1.5, 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 1.6 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 effects.

Figure 1.6: 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 striking is that our results show that shortening the probationary 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 1.6 show 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 increase the probability of being dismissed of 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 of 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 - we can observe a not 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 - of tenure we do not find a clear pattern, although as stressed in the introduction of this section none of this effect is statistically significant.

Concerning the effect on the threshold the results are summarized in table 1.4, 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.

Using the former approaches described in section 1.5, the results could be summarized in what follows. We report our estimation results by way of graphical illustration. Figure 1.4 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.

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 following graph summarize the results of the DID approach, while table 1.4 summarizes the RDD[39] results.

Table 1.4: 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. (1.8) 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 sta-

[39]The graphs representing the discontinuity effect led by the probationary period end has been reported in sectionA.9

tistically 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 1.6 close to the threshold - i.e. 12 month of tenure.

Our evidence shows that shortening of the probationary period tends to have a positive effect on the dismissal probability for the first group of tenure, while for the second group 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 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 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.

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.

Figure 1.7 and 1.8 display the 1999 UK Unfair Dismissal Reform Effect on the hazard of being dismissed respectively for skilled manufacturing workers and for the unskilled ones.

[40] Manufacturing, with more than 17% of the sample size, represents the larger industry in the raw data.

Figure 1.7: The effect of the reform on the hazard of being dismissed by tenure separately by workers skills: skilled

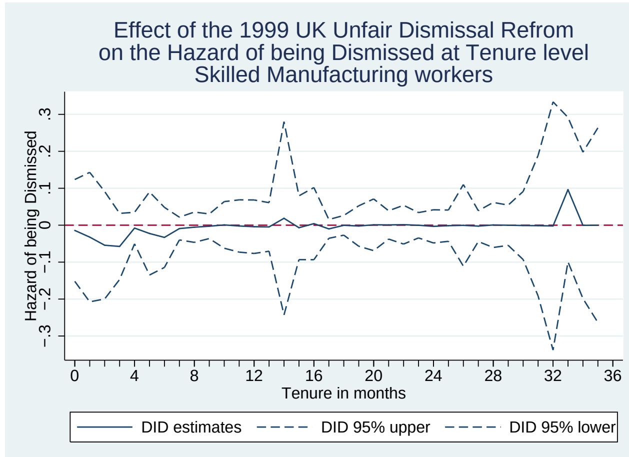
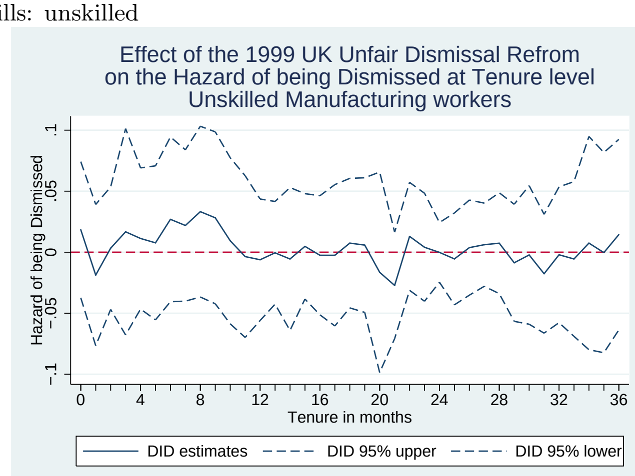



Figure 1.8: The effect of the reform on the hazard of being dismissed by tenure separately by workers skills: unskilled




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 those unskilled workers tenured less than one year. On the other hand, our evidence shows a completely different pattern for the skilled workers. 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 decreases 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 1.5. From table 1.5 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 show 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 1.8 show us that the reform implementation lead an increase in the probability of being laid off for the unskilled manufacturing workers amounting to 1% at the beginning.[41] The probability increase 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 1.6). 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 for those workers whose task

[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%.

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.

Table 1.5: 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. (1.8) 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 1.6: 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. (1.8) 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).

## 1.9 Robustness checks

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

### 1.9.1 Gender differences

Our 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 1.3). 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

[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.

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 (A.15 and A.16), 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.

We report our estimation results by way of graphical illustration. Figures 1.9 and 1.10 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 (1.9) and for males (1.10).

As already mention in section 1.7 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 later.

From figure 1.9 we can see 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 shirking 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.

Table 1.7 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.does not drive to any relevant effect. This validity of this result is confirmed by the results presented in figure 1.9 close to the 12 month of tenure.

Figure 1.9: The effect of the reform on the hazard of being dismissed by tenure separately by gender: Females

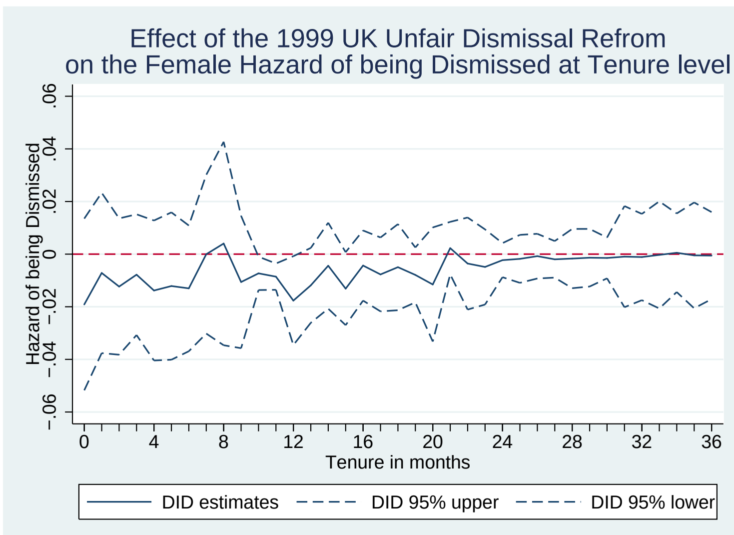


Table 1.7: Impact of the 1999 UK Reform on the termination hazard using RDD approach- Females

| | 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. (1.8) 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).

From figure 1.10 we can see that the probationary period shift 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 1.10: The effect of the reform on the hazard of being dismissed by tenure separately by gender: Males

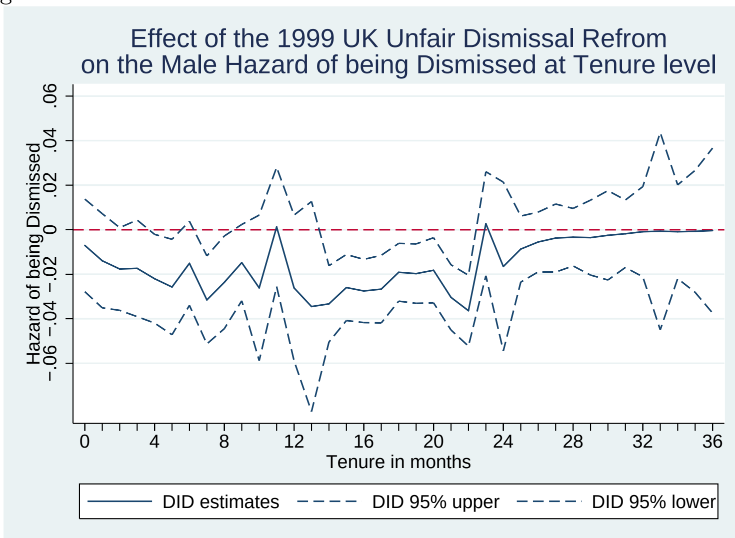



Table 1.8 shows the effect of overcoming the probationary period threshold on the male hazard of being dismissed.

Table 1.8: 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. (1.8) 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.

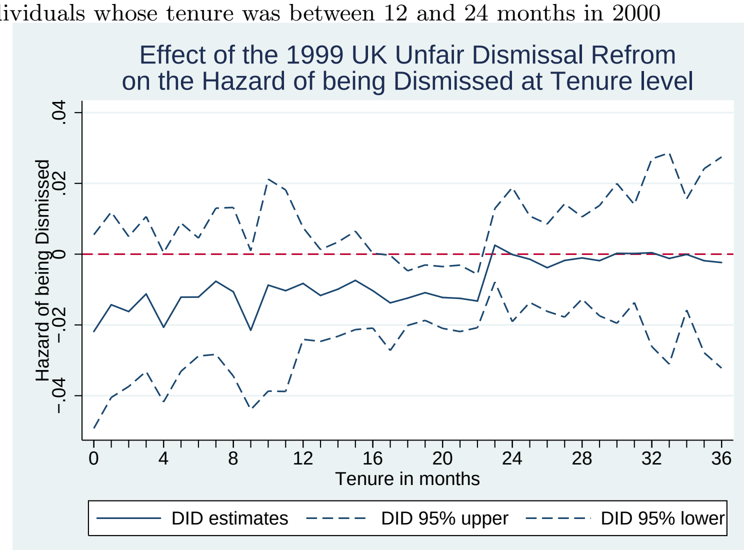
Conversely the new probationary period end - i.e. 12 months - does not drive to any relevant effect for male workers. This result is in line with the effect close to the threshold highlighted in figure 1.10.

### 1.9.2 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, while 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 1.11 the DID estimation,[44] while table 1.9 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 our results confirm that the effect does not start just after the completion of the probationary period - i.e. our evidence confirm that the new probationary period threshold is negative but not significant.

Figure 1.11: 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




[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.

Table 1.9: 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. (1.8). 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.

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 between 14 and 16 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 1.9.

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 1.9 close to the threshold - i.e. 12 month of tenure.

## 1.10 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 survival 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). 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 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 DID and RDD estimation do not find evidence 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 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. 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.

Regarding the other types of terminations (quits and "other reasons"), we do not find significant evidence.

Looking at the reform effect on manufacturing, our evidence show that shortening the probationary period increase 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 the different skilled workers differently. 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. However, it is worth pointing out that our results are not 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 old 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. 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.

# Chapter 2

# Is It Temporary Contract Effect or Is It the New Deal Effect? Evidence from the UK

## 2.1 Introduction

The economic literature often compares highly regulated European labour markets with a *laissez-faire* situation -i.e. without employment protection - characterizing the USA labour markets. At the same time, the persistent unemployment afflicting many European Countries have stimulated many questions about the efficiency of highly regulated labour markets (Bertola, 1992, Cahuc and Postel-Vinay, 2002, Guell and Petrongolo, 2007, Lazear, 1990, Nickell, Nunziata, and Ochel, 2005). Despite the ongoing debate, in the recent decades many European countries have evolved toward more flexibility by creating temporary jobs (Barbieri and Sestito, 2008, Cahuc and Postel-Vinay, 2002, Dolado and Jimeno, 2002, Saint-Paul, 1994) instead of reducing protection for insiders.

Nowadays 14% of EU workers have a limited duration contract (Eurostat). Looking at particular employment groups, such as young individuals this percentage is starkly higher. More specifically it amounts to 39%

for those individual aged between 15 and 24 years, and to 20% for those aged between 25 and 29 years. The role played by temporary contract in countries such as Italy, France and Germany (OECD, 2005) accounts for the large share of total employment growth. The current literature is not conclusive on the issue of temporary contracts representing effective stepping-stones towards permanent employment (Blanchard and Landier, 2002, Booth, Francesconi, and Frank, 2002a). Indeed, temporary contracts can also be used by firms as a cheaper option for adjusting employment, with lower wages and severance payments, and poor training (Booth, Francesconi, and Frank, 2002a). Fixed-term contracts seem to pay lower wages in the United Kingdom (Booth, Francesconi, and Frank, 2002a), France (Blanchard and Landier, 2002), Spain (de la Rica, 2003, Jimeno and Toharia, 1993), and Italy (Cipollone and Guelfi, 2006), offering fairly different prospects of promotion across these countries. While temporary jobs seem to represent stepping-stones to permanent work in the United Kingdom (Booth, Francesconi, and Frank, 2002a), temporary employment turns out to be not so'temporary' in the south of Europe (Blanchard and Landier (2002), for France, Alba-Ramirez (1998) for Spain; Cipollone and Guelfi (2006) for Italy). Hence, "temporary jobs are - from worker's perspective - bad-jobs" (Booth, Francesconi, and Frank, 2002b), however "temporary contracts improves the opportunity for previously unemployed workers to move out of unemployment into permanent employment " (Larsson, Lindqvist, and Skans, 2005). As a consequence, recently the economic literature have moved from the comparison: Permanent versus temporary towards the comparison: Unemployed versus temporary. (Barbieri and Sestito, 2008, Larsson, Lindqvist, and Skans, 2005, Paggiaro, Rettore, and Trivellato, 2009)

Few paper have dealt a proper evaluation impact on the comparison temporary vs. unemployed on subsequent employment histories: among them Barbieri and Sestito (2008), Paggiaro, Rettore, and Trivellato (2009).

Barbieri and Sestito (2008) using four waves from the Italian LFS: 1994, 1997, 2000 and 2003, compare subsequent employment spell outcomes of people who have recently acquired a temporary job with those who remained unemployed, getting rid the selection bias between the two groups via Propensity Score Matching. The authors find that those people who transit through a temporary contract are 37% more likely to find a satisfactory job than those who stay unemployed and continue to search.

Paggiaro, Rettore, and Trivellato (2009) using Barbieri and Sestito's approach on a larger sample of the Italian Labour Force Survey (LFS) find that experiencing a spell of temporary employment leads to a 30% higher employment rate one year later for man, and 35% for women.

Conversely to most of the European countries - such as Spain, Italy, Germany - the UK percentage of temporary contracts has progressively declined from 7.6% in 1997 to 5.4%. At the same time, the UK legislation on employment both permanent and temporary[1] has progressively strengthened.[2]. With respect to temporary contract the UK legislation moved from no-restriction in their use until 2002 to the enactment of the Fixed Term Employees - Prevention of Less Favourable Treatment - Regulations 2002,[3] which basically limited the the use of successive fixed-term contract to four year. Furthermore, in 1998 the enactment of the New Deal program raised significantly the transition from unemployment through employment by about 5% (Blundell, Costa Dias, Meghir, and Van Reenen, 2004, De Giorgi, 2005). In this context two main questions would arise: Firstly, "Does experiencing a temporary job-spell vs. being unemployed have an impact in terms of subsequent employment status in the UK?". Secondly, "If there is any impact in the experiencing temporary contracts vs. being

[1] From now on we would use the terms: Fixed-term contract and temporary as synonyms.

[2] For further details see the appendix

[3] 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 organization.
  Further details: `http://www.opsi.gov.uk/si/si2002/20022034.htm`

unemployed, is it affected by the New Deal enactment?"

This paper tackles the effect of experiencing a spell of temporary job-spell vs. a spell of unemployment on short-term labour outcomes controlling for the New Deal effect, in the UK context.

The remaining of the paper is organized as follows. Section 2.2 describes the UK institutional setting. Section 2.3 provides first evidence on the difference between experiencing a temporary job-spell vs. staying in unemployment in term of subsequent employment. Section 2.4 describes how the New Deal works. Section 2.5 describes the empirical strategy we use, which largely relies on Barbieri and Sestito. The data and some preliminary statistics are presented in section 2.6. Section 2.7 presents the results. Finally section 2.8 concludes.

## 2.2 The UK institutional setting

In this section we aim at describing the New Deal impact on the UK labour market, which is widely recognized as an example of flexible labour market especially in the latter context.[4]

From 1997 - the New Labour settlement - to 2006 the UK overall employment rate rose from 72.9% to 74.6%, leading to the highest female employment rate since the 1970s.[5] Even though the New Labour government period was characterized by a steep increase in the employment rate it is worth pointing out that this process started just after the early 1990s recession, i.e. five years before Labour went to power (Brewer, 2007).

However, as stressed by Dickens, Gregg, and Wadsworth (2000) "this good mask mounting evidence" that the increase in terms of employment rate was not homogenous in the active population, but it was mainly driven by particular portion of labour market such as lone parents and women with non-working partners. Likewise, Dickens, Gregg, and Wadsworth stress how the unemployment was concentrated on selected individuals.

[4] According to the OECD, the UK is characterized by the most flexible labour market in Europe and by the second lowest employment protection, after the USA among OECD countries.

[5] In particular in that time - span the female employment rate rose from 67.2% to 70.2% meanwhile the male employment rate rose from 77.7% to 78.8% (Brewer, 2007).

At this issue various authors consider whether experiencing unemployment rises the chances of future unemployment experience (Arulampalam, 2001, Arulampalam, Booth, and Taylor, 2000).

On the one hand, from a policy point of view "the casual link between past unemployment and current unemployment" (Arulampalam, Booth, and Taylor, 2000) appear to be a central issue, since policies aimed at reducing short term unemployment lead to long-run effects (Arulampalam, Booth, and Taylor, 2000).

On the other hand, past unemployment experiences may be used by firm as a signal of employees productivity (Blanchard and Katz, 1997, Lockwood, 1991, Phelps, 1972, Pissarides, 1992). Since unemployment determines both a loss of human capital and work experience, firm might be reluctant to hire past unemployed people. At the same time, the loss of human capital may induce the unemployed to lower their reservation wage, which in turn lead them to accept "poorer quality jobs that are more likely to be destroyed" (Arulampalam, Booth, and Taylor, 2000), increasing the chances to future unemployment experience.

The central issue for the policy makers becomes to provide people adequate training and/or match properly into job (Arulampalam, 2001) aiming at preventing the 'initial conditions' which may lead to this phenomenon. At this issue the British case is particularly relevant since it commonly believed that UK invests too little in vocational education and in training. Among the other OECD countries the proportion of adults with low skilled qualification is definitely higher, especially when compared with countries such as Sweden, Finland, USA and Germany (Abramovsky, Battistin, Fitzsimons, Goodman, and Simpson, 2009). Although, between 1997 and 2002 the UK has seen a flurry of reforms, mostly of them have been principally targeted to specific categories of individuals such as low skilled, older working age people, lone parents, disables, females, ethnic minorities. Among the New Labour's welfare to work reforms, perhaps the best know is the *New Deal*. This program, which currently has been substituted by the Flexible New Deal, was mainly designed to move unemployed individuals in the UK into work and away from welfare. The program is aimed at a number of different groups, each with various degree of eligibility and degree of compulsion.

These includes:

- New Deal for Young People (NDYP) is targeted to unemployed youth (aged between 18 and 24) who have been unemployed for 6 months or longer.
- New Deal 25+ is targeted to adults (aged 25+) who have been unemployed for eighteen months or more.
- New Deal for Lone Parents addresses, as the name suggests, the employment reintegration needs of single parents with school age children.
- New Deal for the Disabled assists those receiving disability benefits to return to work.
- New Deal 50+ for those aged 50 year old and above.
- New Deal for Musicians for aspiring unemployed musicians.

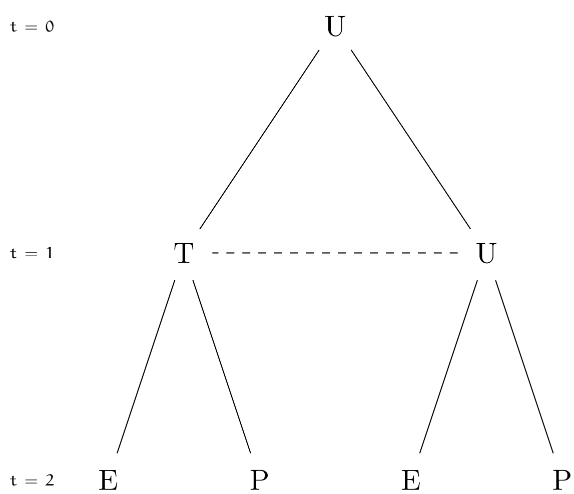
The present work focuses on the first two categories: New Deal for Young People and New Deal 25+ which are explained in details in section 2.4. In what follows we briefly look at difference between experiencing a temporary vs. stay in unemployment.

## 2.3 Temporary vs. Unemployed in the UK

In this section we aim at answering to the first question raised in section 2.1: "Does experiencing a temporary job-spell vs. stay in unemployment have an impact in terms of subsequent employment status in the UK?".

Given the relevance of Barbieri and Sestito (2008) approach for our estimation strategy, we address the interested reader to figure 2.1 .

Figure 2.1: Barbieri and Sestito (2008) Evaluation strategy




At the first interview (i.e. $t = 0$ in our notation) the reference population is made up of all the individuals who are unemployed (i.e. U in our notation). From the initial sample they consider two sub-groups made of those individuals who at $t = 1$, three months later, experience one of three mutually exclusive status: Temporary employment (i.e. T in our notation) and unemployment (i.e. U in our notation). At this point - the second interview - the authors purge the selection on observable characteristics via Propensity Score Matching, where temporary are the treated, while unemployed the controls. Then, after one year ($t = 2$ in our notation) they compare the labour outcomes, in terms of employment (i.e. E in our notation, both temporary and permanent) and in terms of permanent employment (i.e. P in our notation).

Using the strategy described above, we estimate the effect of experiencing a temporary job-spell vs. stay in unemployment in the UK, using two 21 months panel: March 1996- December 1997, March 1999 - December 2000, coming from the Quarterly British Labour Force Survey (LFS).

Table 2.1 presents the estimation results.

Table 2.1: Effect of Experiencing a spell of Temporary contract vs. a spell of unemployment on short-term labour market in the UK, using B&S model.

| | Year: 1996/1997 | | Year: 1999/2000 | |
|---|---|---|---|---|
| Oucome | Impact | | Impact | |
| Employment 9 months later: | 0.7761 | *** | 0.5989 | *** |
| | (0.031) | | (0.0409) | |
| Perm. Empl. 9 months later: | 0.0314 | | 0.0513 | |
| | (0.0281) | | (0.026) | |

* significant at 10%, ** significant at 5%, *** significant at 1%. Effect of being temporary enrolled vs being unemployed. The data used come from the UK LFS. The sample include all the individuals between 16 and 64 years old. In the matching procedure has been used as covariates year of birth, gender, education (2 dummies), marital status, ethnicity (2 dummies), region of residence (4 dummies), regional unemployment rate, regional activity rate, a dummy whether the individual receives a benefit, a dummy for long term unemployment. Bootstrapped standard errors (in parentheses), using 100 replications.

Table 2.1 highlights a stark significant difference between being temporary vs. being unemployed, assessing to a level higher than 77% in the cohort 1996/1997 and to a level slightly lower than 60% in the cohort 1999/2000. Conversely to the employment outcome, we do not find any evidence of experiencing a temporary job-spell vs. stay in unemployment on subsequent permanent labour market outcomes.

What is most striking is the difference between the two cohorts estimation: 77% vs 60% which turns out to be statistically different at conventional levels. In this context two main question would arise: “Do temporary become more likely employed in 1996/1997 compared to 1999/ 2000?” or “Do unemployed tend to transit more likely to employment in 1999/2000 compared to 1996/1997?” As explained in section 2.2, the first reform that impacts on temporary contract occurred in 2002, therefore it would not have any impact on this estimation. Furthermore, since the reform has progressively strengthened the use of temporary contracts the firms might have increased their screening process. In other words, it is well documented that is more costly to discharge a permanent worker than a temporary one, hence the firm would renew the contract for those who are close to the four year threshold if they do not represent a good match for the firm; therefore, the difference between the two categories would be higher. Conversely the difference we find progressively declined, which may imply some changes in the unemployed sample. As explained in section 2.2, one of the main welfare reforms enacted by the new government is the New Deal, which occurred in 1998. According Blundell, Costa Dias, Meghir, and Van Reenen (2004), De Giorgi (2005)[6] the New Deal raised significantly the transition to employment between 5% - 7%, therefore without taking into account the New Deal enactment the effect of experiencing a temporary job-spell vs. stay in unemployment tend to underestimate the real effect in the pre-reform cohort. Aiming at avoiding this bias in our estimation we check for the New Deal effect. In section 2.4 we briefly explain how New Deal works.

## 2.4 The New Deal

In this section we aim at explaining the New Deal program. In this respect we briefly explain the New Deal for young people in section 2.4.1, the New Deal for 25+ in section 2.4.2 and lastly we discuss the previous works on the New Deal in section 2.4.3.

### 2.4.1 New Deal for Young People

The New Deal for Young People (NDYP) was launched in April 1998 to help young unemployed obtaining a job. For all individuals aged between 18 and 24 who have been claiming for at least six month of Job Seekers'Allowance (JSA) - equivalent to Unemployment Benefit - the participation is compulsory[7].

[6] More details in section 2.4.3.
[7] All the eligible people who refuses to participate face the benefit entitlement loss.

During the enrollment in the New Deal, the participants are in three main steps:

1. "Gateway";

2. New Deal options;

3. "Follow-through".

The phase called "gateway", lasting up to 16 weeks, is an intensive job-search assistance phase during which all the participants are assigned to "personal advisor" whose main task is encourage job search.

If the participants during the "gateway" stage has not managed to find a job, then they have the possibility to choose among four possible options. Firstly, the "employer option" a spell of six months on subsidized employment. Secondly, the participants could enroll in a stipulated full-time education or training course. Thirdly, the participants can work in the voluntary sector, for up to six months. Lastly, they could be enrolled in the so-called "Environmental Task Force" which is equivalent to a government job.

If the individual after the option completion are still unemployed they are enrolled in the "Follow-Through", which is mainly an intensive job-assistance lasting up to 13 weeks.

### 2.4.2 New Deal for 25+

The New Deal 25+ was launched in June 1998. The program focuses on all long term unemployed older than 24 years who have been claiming JSA for 18 out of the last 21 months. The function of New Deal 25+ is pretty close to NDYP. Both of them are characterized by three phases and in this regard New Deal 25+ shares with NDYP the "gateway" and the "follow-through" phases. Conversely the second phase, the so-called "Intensive Active Period" (IAP)- lasting no more than 26 weeks- does not allow to choose among four possibility - as the second stage in NDYP - but includes flexible packages of support which can combine work experience, work focused training and help with motivation.

### 2.4.3 Previous works on the New Deal

Up to the best of our knowledge just two studies have looked at the effect of the New Deal implementation, with this regard they have look at the NDYP only: Blundell, Costa Dias, Meghir, and Van Reenen (2004) and De Giorgi (2005).

Blundell, Costa Dias, Meghir, and Van Reenen (2004) examine the labour market impact of the NDYP during the first four months of the treatment - i.e. during the "gateway" stage. Their estimation strategy relies on difference-in-differences estimators using two sources of eligibility to construct comparison groups. First, they compare the labour market impact of those young eligible unemployed living in the first areas to pilot the NDYP (i.e. treated) with the same outcome of young unemployed living in similar areas where the NDYP does not operate (i.e. controls). Second, when the program was ran nationally[8] they compare the labour market outcomes of the eligible with the one of individuals older than 25. Using the above mentioned approach the authors find that the NDYP raised significantly the transition to employment of about 5%.

De Giorgi (2005), using a non-parametric Regress Discontinuity Design, investigates the effectiveness of the program in terms of "enhancing the (re)employment probability" of participant males up to 12 months after starting the "gateway". Using the above mentioned approach De Giorgi (2005) finds evidence that NDYP increases the transition to work by about 6-7%.

## 2.5 The evaluation strategy

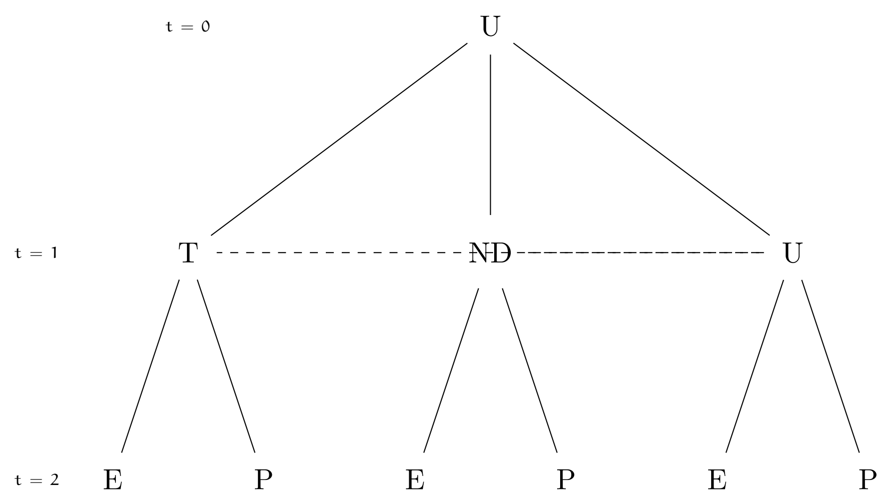
The motivation of our analysis is to assess the effect of experiencing a spell of temporary contract vs. a spell of unemployment on short-term labour outcomes in the UK. In the former section we have shown that UK has progressively strengthened Active Labour Market Policies (ALMPs) for those individuals deemed to be unemployed though the New Deal program, which according to the literature raised the transition to employment by about 5%.

[8] Between January and April 1998 the programme was launched just in six UK regions - Pathfinders.

Now following the discussion in Barbieri and Sestito (2008), we move to the a proper evaluation strategy.

To illustrate our evaluation strategy we address the interested reader to the figure 2.2.

Figure 2.2: Evaluation strategy




At the first interview (i.e. $t = 0$ in our notation) the reference population is made up of all the individuals who are unemployed (i.e. U in our notation) but do not have the requirements to be defined New Deal Eligible (i.e. ND in our notation). In other words, we look at people who are short-term unemployed at time $t = 0$. At this issue appear to be crucial the definition of unemployed, we opt for the ILO definition,[9]instead of using other definitions such as the self-defined status, for two main reasons i) is the definition used

[9]According to ILO definition the term of unemployed refers to people who, during the reference period, were:

- without an employment;
- seeking work i.e. had taken specific steps in a specified reference period to seek paid employment or self-employment;
- currently available for work, i.e. were available for paid employment or self-employment within two weeks.

to compute the employment indicators (such as the unemployment rate) ii) to be in line with Barbieri and Sestito (2008). Hence categories such as housewives and full-time students are excluded from the sample. From the initial sample we consider three sub-groups made of those individuals who at $t = 1$, three months later, experience one of three mutually exclusive status: Temporary employment, New Deal Eligibility and unemployment. At this point the unemployment duration appear to be crucial to split New Deal eligible from unemployed. Although, the comparison among long-term unemployed, temporary workers and short-term unemployed could be affected by some selection which might be not captured controlling for observables. This would particularly crucial for the older group, it is indeed hard to believe that those individuals who are unemployed by more than 18 months would behave - in terms of subsequent job opportunities - as those individuals whose unemployment duration is equal to four months. Therefore, for the group of those individuals aged between 25 and 49 years we add a further requirement for the initial condition: At time t=0 their unemployment duration should be equal at least to 12 months. For sake of clarity in what follows we briefly summarize the initial condition for each group:

**Individuals aged between 18 and 24 years:** At time t=0 their unemployment condition should last less than 6 months.

**Individuals aged between 25 and 49 years:** At time t=0 their unemployment condition should last less between 12 and 17 months.

As briefly explained in section 2.2 a large strand of economic literature has investigated on the unemployment persistence finding that past unemployed people increase the chances to incur in future unemployment. Although we aim at evaluating the effect of being temporary employed vs. being unemployed using a typical policy evaluation approach, hence given the different approach we do not focus on the unemployment persistence issue. At the same time the interested reader may concerns about the deletion of those individuals who at $t = 1$ become permanent and those who become OLF, even though this represent an large percentage of the sample (12% for the young group, 20% for the older[10]), our choice is mainly related to two

[10] See the appendix for further details

main reasons: i) Our research question and ii) temporary and unemployed appear to be the closest categories.

The treatment group definition depends on the research question we aim at answering. When we aim at determining "the impact upon the future labour market status of escaping unemployment through temporary employment" (Barbieri and Sestito, 2008) we define as *treated* ($D = 1$ in our notation) those who are experiencing a spell of temporary job and as *controls* all the others namely: New Deal eligible and unemployed ($D = 2$ and $D = 3$ in our notation). Inasmuch as when we are involved in estimating the effect of being New Deal eligible vs. being short term unemployed, the *treatment status* ($D = 2$ in our notation) is assigned to the New Deal eligible and the *control group* ($D = 3$ in our notation)is represented by the unemployed.

$$D = \begin{cases} 1 & \text{temporary employment in t=1} \\ 2 & \text{New Deal eligible in t=1} \\ 3 & \text{unemployed in t=1} \end{cases}$$

Then, after nine months ($t = 2$ in our notation) we compare the labour outcomes, *in terms of employment* (E in our notation) (i.e. both temporary and permanent) and *in terms of permanent employment* (P in our notation).

For sake of brevity we in what follows we define the controls with the notation $D = 0$.

Let Y be the outcome of interest - i.e. the employment status (either permanent or not permanent) - at time $t = 2$, hence nine months later the treatment status definition. Let $Y^1$, $Y^0$ be the potential outcomes individual would experience being treated or being in the control group respectively.

In this context we are interested in the effect of experiencing a temporary contract vs. staying in unemployment (or long term unemployed in the New Deal case). Therefore we aim at estimating the so-colled Average Treatment Effect on the Treated (ATT):

$$\theta = E(Y^1 - Y^0|D = 1) = E(Y^1|D = 1) - E(Y^0|D = 1) \tag{2.1}$$

Equation 2.1 requires that we observe for the same individual both states, treatment ($D = 1$) and no treatment ($D = 0$). In other words, "What would happened to those individual who are temporary whether they were unemployed?". Using the evaluation terminology the answer to this question is "unobservable" by construction. Therefore, the issue becomes to find a counterfactual that need to be as close as possible to the unobserved outcome. However we could observe $E(Y^0|D = 1)$ which represents the average outcome for the control group. By virtue of this consideration, we can rewrite equation 2.1 in the following way:

$$E[Y^1|D = 1] - E[Y^0|D = 0] = \theta + (E[Y^0|D = 1] - E[Y^0|D = 0]) \qquad (2.2)$$

Equation 2.2 states that the average difference between treated and controls is equal to the ATT plus the selection bias. Aiming at reducing as possible the selection bias we make use of a rather standard assumption of the treatment evaluation literature (Heckman and Smith, 1999, Rosenbaum and Rubin, 1983). Basically using treatment and control who are similar, conditional on a wide range of observable characteristics, which might be relevant either for the treatment status or for the potential outcome we assume that the difference between treated and controls is due to the treatment status only. In other words, no other observable characteristics have impact on the potential outcome. More specifically, we match[11] treated and controls using the propensity score matching which allows to condition just on a scalar, instead of set of variables. Therefore, it becomes fundamental the so-called unconfoundedness or ignorability assumption:

$$Y^1, Y^0 \quad \perp \quad D|X \qquad (2.3)$$

Assumption 2.3 requires that conditional on observed characteristics X, defining the propensity score, being in the treatment or in the control group is independent of the potential outcome.

### Matching Algorithms

As stated in section 2.5 and in line with Barbieri and Sestito (2008), we aim at reducing as possible the selection bias due to observable, which might

[11] In the former graph 2.2 the matching procedure has been pointed out by the dashed line.

arise when treated and control group systematically differ along several dimensions which are relevant to the outcome. As already said in the former section 2.5, 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 2.1 p(x).

Aiming at answering to our research question the matching phase appears to be crucial, since it aims at lowering any possible difference on observables among all the groups we are working on. At this issue, conditional on the effect we focus on we define different treatment statuses and different controls- aiming at implementing this procedure. For sake of clarity we present tables 2.2 - 2.4, which conditional on the estimation outcome define treated and controls.

Table 2.2: Estimation the effect, in terms of subsequent employment status, of experiencing a temporary contract vs. being unemployed (short-term and long-term unemployed). Definition of the treated and control for the propensity score matching estimation.

| | T | Unemployed (U+ND) |
|---|---|---|
| Post-New Deal Scenario | Controls (D=0) | Controls (D=0) |
| Pre-New Deal Scenario | **Treated (D=1)** | Controls (D=0) |

Table 2.3: Estimation the effect, in terms of subsequent employment status, of experiencing a temporary contract vs. being New Deal Eligible (long-term unemployed). Definition of the treated and control for the propensity score matching estimation.

| | T | ND |
|---|---|---|
| Post-New Deal Scenario | Controls (D=0) | Controls (D=0) |
| Pre-New Deal Scenario | **Treated (D=1)** | Controls (D=0) |

Table 2.4: Estimation the effect, in terms of subsequent employment status, of being New Deal Eligible (long-term unemployed) vs. staying in unemployment (short-term unemployed). Definition of the treated and control for the propensity score matching estimation.

| | ND | U |
|---|---|---|
| Post-New Deal Scenario | Controls (D=0) | Controls (D=0) |
| Pre-New Deal Scenario | **Treated (D=1)** | Controls (D=0) |

We perform a nearest neighbour matching on the propensity score with no-replacement using as covariates:year of birth, gender, education (2 dummies), marital status, ethnicity (2 dummies), region of residence (4 dummies), regional unemployment rate, regional activity rate.Furthermore, we use as measure of precise conditioning, the caliper imposition of 0.05, which represents the maximum allowed distance between the treated and the controls of 5%, whether the distance between the treated and the controls would exceed the caliper, the pairs would be automatically discharged. [12]

## 2.6 The data

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

The LFS is conducted every quarter since 1992[13] 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.[14]

In this analysis we construct and pool two 21 months panels: (March 1996 - December 1997 - i.e. pre-New Deal cohort- March 1999 -December 2000 - post-New Deal cohort[15]).[16] We drop data for 1998 since it represents the New Deal enactment year. Moreover, as discussed in section 2.2, conditional on the group of interested and the region of residence[17] the New Deal enactment month could vary between January and June 1998, hence

[12]The covariates comparison between matched and unmatched samples and the estimated propensity score are presented in appendix B.3.

[13]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.

[14]for more details on the working history we address the interested reader to chapter "The Implications of Changing Employment Protection: Evaluating the 1999 UK Unfair Dismissal Reform".

[15]From now on we use the terms panel and cohort as synonyms

[16]Since the NDYP was experimentally ran in six regions between January 1998 and April 1998, we were concerned that using the last wave of each panel: December 1997 - February 1998 could lead to bias estimation, hence we drop it. Aiming at maintaining the same time-span we drop also its companion December 2000-February 2001.

[17]It is worth pointing out that in six pilot regions the New Deal was ran in January.

we prefer dropping this year.

It is worth pointing out that we do not find evidence of relevant changes in the survey during our analysis period.

For each of the two cohorts, we drop all the individuals older than 49 given the relevant of transition to retirement at that age. Furthermore, we also exclude from our sample individuals who are 16 to 17 years old for two main reasons: they would never be New Deal Eligible and given the instability of their attachment to the labour market.

This leaves us people aged between 18 and 49 years. Given the differences between people aged 18-24 years and those older than 24 years both in terms of observable characteristics, such as age, and in terms of eligibility, we split the two groups.

Tables 2.5 and 2.6[18] report the sample size at time $t = 0$ for the two pooled panel, respectively for the younger group and for the older one.[19]

Table 2.5: Sample size and labour force state at t=0, for those aged between 18−24 looking at the cohort

| Age: 18-24 | 1996/1997 | | 1999/2000 | |
|---|---|---|---|---|
| State in t=0 | No | % | No | % |
| Total | 4,692 | 100 | 3,724 | 100 |
| Employed | 3,402 | 72.51 | 2,714 | 72.88 |
| Unemployed | 924 | 19.69 | 794 | 21.32 |
| OLF | 366 | 7.8 | 216 | 5.8 |

Table 2.6: Sample size and labour force state at t=0, for those aged between 25−49 looking at the cohort

| Age: 25- 49 | 1996/1997 | | 1999/2000 | |
|---|---|---|---|---|
| State in t=0 | No | % | No | % |
| Total | 41,077 | 100 | 35,635 | 100 |
| Employed | 33,862 | 82.43 | 29,969 | 84.10 |
| Unemployed | 1,725 | 4.20 | 918 | 2.58 |
| OLF | 5,490 | 13.37 | 4,748 | 13.32 |

Comparing table 2.5 and table 2.6 we can notice that while more than 80% of those aged 25-49 is employed, for the young group this percentage lowers to 72%. Furthermore the former tables shed light on the stark difference between the percentage of unemployed people present in the two groups: while for the older group it is equal to 4.20% in the first panel (2.58% in the second one) for the younger group the percentage is more than triple (amounting to a level of 19.69% in the first cohort and amounting to 21.62% in the second cohort). On the contrary, the percentage of Out of Labour Forces (OLF) for the older group is almost double compared with

[18]A more detailed classification is present in appendix B.2

[19]Unemployed includes just those people who are without an employment and actively seeking for a work in the last month, but do not have the requirements to participate in the New Deal program. Those who are New Deal Eligible are dropped from our sample.

the younger one (more than 13% for the older group compared to a level close to 8% for the younger one).

What is most striking is that the percentage of young unemployed after the New Deal Enactment is increased.[20] Although it is worth pointing out that at the same time it also decreased - by a lower amount - the percentage of young OLF, at a first glance, we interpret this result partly due to a transition from OLF status to unemployment and in particular for this age group a transition from education to labour market, partly due to and increase in the unemployment. Conversely the same percentage for the older group almost halved.

### 2.6.1 Definition of the outcome variables

Following Barbieri and Sestito (2008), Paggiaro, Rettore, and Trivellato (2009) and as described in section 2.2 we consider the following short-term labour outcomes:

- *if the individual at the last interview - i.e. nine months after the second interview - is employed. Using Paggiaro, Rettore, and Trivellato (2009)'s notation we define it "Employment".*

- *if the individual at the last interview holds a permanent contract (in comparison with Paggiaro, Rettore, and Trivellato (2009) we include the self-employed).Using Paggiaro, Rettore, and Trivellato (2009)'s notation we define it "Permanent employment".*

It is worth pointing out that the interval between the second interview and the last one - i.e. nine months in our context- is a crucial issue, particularly with respect to permanent employment. In other words, using a longer time span we would observe an higher difference between those who, at time $t = 1$, become temporary and those who stay in unemployment - both long and short term unemployment.

[20] We test whether the difference is statistically relevant and we do not find any relevance.

## 2.7 Results

In this paper largely following Barbieri and Sestito (2008) we investigate the impact of experiencing a temporary contract versus stain in unemployment, controlling for the New Deal enactment, on short-term subsequent labour market outcomes.

The estimation procedure we adopt can be described as follows. First, for each panel and group we are working on, at the first interview we take all the unemployed who are not eligible for the New Deal. Second, we consider three sub-groups made of those individuals who three months later experience one of three mutually exclusive status: Temporary employment, New Deal Eligibility and unemployment. Third, at this interview we define as treated those who are experiencing a temporary job spell while as controls all the others, namely: New Deal eligible and unemployed. Fourth, aiming at purging any raw difference between treated an controls we match the two groups via propensity score matching. Finally, we look at their employment and permanent employment status nine months later.

Tables 2.7 - 2.10 present the estimation results. With this regard we start presenting the results for the younger group: table 2.7 - 2.8. Beside this, we present the result for the older group 2.9 -2.10.

Table 2.7: Estimated effect before the New Deal Enactment, for those aged 18 -24

| Outcome | Diff. | SE | T-Stat. | Signif. |
|---|---|---|---|---|
| Temporary vs. being Unemployed (Short-term + Long-term unemployed) | | | | |
| Employment at t1+3 | 0.300 | 0.109 | 2.752 | *** |
| Perm. Empl. at t1+3 | 0.015 | 0.023 | 0.652 | |
| Temporary vs. New Deal Eligible | | | | |
| Employment at t1+3 | 0.263 | 0.100 | 2.600 | *** |
| Perm. Empl. at t1+3 | -0.055 | 0.071 | -0.767 | |
| New Deal Eligible vs. Short-term unemployed | | | | |
| Employment at t1+3 | 0.037 | 0.064 | 0.578 | |
| Perm. Empl. at t1+3 | -0.040 | 0.064 | -0.625 | |

* significant at 10%, ** significant at 5%, *** significant at 1%. Effect of being New Deal Eligible vs being unemployed. In the matching procedure has been used as covariates year of birth, gender, education (2 dummies), marital status, ethnicity (2 dummies), region of residence (4 dummies), regional unemployment rate, regional activity rate. Bootstrapped standard errors, using 100 replications.

Our results show undoubtedly that experiencing a temporary contract for those individuals aged between 18 and 24 years in the pre- New Deal scenario leads to a starkly higher probability to get a job nine months later. As regards, the effect amounts to 30% when temporary are compared to the both unemployed: Short-term and long-term. When we move from the comparison temporary contract vs. unemployed to the comparison temporary contracts vs. long-term unemployed the difference between the two categories lowers to 26%, which turns out to be significant at conventional levels. In respect to the comparison long-term unemployed “vis-a-vis” short-term unemployed our evidence shows that this two classification are not statistically different in terms of subsequent job-spells.

When we look at the probability to get a permanent job our results do not deliver any significant effect.

Table 2.8: Estimated effect after the New Deal Enactment, for those aged 18 -24

| Outcome | Diff. | SE | T-Stat. | Signif. |
|---|---|---|---|---|
| Temporary vs. being Unemployed (Short-term + Long-term unemployed) | | | | |
| Employment at t1+3 | 0.250 | 0.04 | 6.250 | *** |
| Perm. Empl. at t1+3 | 0.200 | 0.139 | 1.439 | |
| Temporary vs. New Deal Eligible | | | | |
| Employment at t1+3 | 0.193 | 0.030 | 6.410 | *** |
| Perm. Empl. at t1+3 | -0.163 | 0.116 | -1.410 | |
| New Deal Eligible vs. Short-term unemployed) | | | | |
| Employment at t1+3 | 0.057 | 0.006 | 9.500 | *** |
| Perm. Empl. at t1+3 | 0.037 | 0.036 | 1.028 | |

* significant at 10%, ** significant at 5%, *** significant at 1%. Effect of being New Deal Eligible vs being unemployed. In the matching procedure has been used as covariates year of birth, gender, education (2 dummies), marital status, ethnicity (2 dummies), region of residence (4 dummies), regional unemployment rate, regional activity rate. Bootstrapped standard errors, using 100 replications.

Our results show undoubtedly that experiencing a temporary contract for those individuals aged between 18 and 24 years leads to a starkly higher probability to get a job nine months later, also in the post-reform scenario, even though the overall effect is lower. As regards, the effect amounts to 25% when temporary are compared to the both unemployed: Short-term and long-term. When we move from the comparison temporary contract vs. unemployed to the comparison temporary contracts vs. long-term unemployed the difference between the two categories lowers to 19%, which turns out to be significant at conventional levels. Concerning the effect of experiencing a spell long-term unemployment “vis-a-vis” a spell of short-term unemployment our evidence shows that, in the post-reform scenario, this comparison turns out to be statistically relevant amounting to a level of 5.7%.

Also in the post-reform scenario when we look at the probability to get a permanent job our results do not deliver any significant effect.

Table 2.9: Estimated effect before the New Deal Enactment, for those aged 25 - 49

| Outcome | Diff. | SE | T-Stat. | Signif. |
|---|---|---|---|---|
| Temporary vs. being Unemployed (Short-term + Long-term unemployed) | | | | |
| Employment at t1+3 | 0.468 | 0.079 | 5.924 | *** |
| Perm. Empl. at t1+3 | 0.039 | 0.041 | 0.951 | |
| Temporary vs. New Deal Eligible | | | | |
| Employment at t1+3 | 0.421 | 0.093 | 4.527 | *** |
| Perm. Empl. at t1+3 | 0.011 | 0.084 | 0.131 | |
| New Deal Eligible vs. Short-term unemployed) | | | | |
| Employment at t1+3 | 0.046 | 0.033 | 1.394 | |
| Perm. Empl. at t1+3 | 0.049 | 0.037 | 1.324 | |

* significant at 10%, ** significant at 5%, *** significant at 1%. Effect of being New Deal Eligible vs being unemployed. In the matching procedure has been used as covariates year of birth, gender, education (2 dummies), marital status, ethnicity (2 dummies), region of residence (4 dummies), regional unemployment rate, regional activity rate. Bootstrapped standard errors, using 100 replications.

Looking at the effect of experiencing a temporary contract for those individuals aged between 25 and 49 years, the difference - in term of subsequent job-spells - between the two categories is leads to an even higher probability to get a job nine months later. As regards, the effect amounts to 46%, in the pre New Deal Enactment, when temporary are compared to the both unemployed: Short-term and long-term. When we move from the comparison temporary contract vs. unemployed to the comparison temporary contracts vs. long-term unemployed the difference between the two categories lowers to 42%, which turns out to be significant at conventional levels. Concerning the effect of experiencing a spell long-term unemployment "vis-a-vis" a spell of short-term unemployment our evidence shows that, even for the olders, in the pre-reform scenario the difference between the two categories is not statistically relevant. We would not be able to justify this result in the pre-reform scenario, particularly for the older group. However this finding, as already mentioned, is not statistically significant.

Also in this case when we look at the probability to get a permanent job our results do not deliver any significant effect.

Table 2.10: Estimated effect after the New Deal Enactment, for those aged 25 - 49

| Outcome | Diff. | SE | T-Stat. | Signif. |
|---|---|---|---|---|
| Temporary vs. being Unemployed (Short-term + Long-term unemployed) | | | | |
| Employment at t1+3 | 0.445 | 0.079 | 5.633 | *** |
| Perm. Empl. at t1+3 | -0.003 | 0.032 | -0.094 | |
| Temporary vs. New Deal Eligible | | | | |
| Employment at t1+3 | 0.401 | 0.091 | 4.407 | *** |
| Perm. Empl. at t1+3 | -0.020 | 0.064 | -0.313 | |
| New Deal Eligible vs. Short-term unemployed) | | | | |
| Employment at t1+3 | 0.044 | 0.018 | 2.444 | *** |
| Perm. Empl. at t1+3 | -0.023 | 0.040 | -0.575 | |

* significant at 10%, ** significant at 5%, *** significant at 1%. Effect of being New Deal Eligible vs being unemployed. In the matching procedure has been used as covariates year of birth, gender, education (2 dummies), marital status, ethnicity (2 dummies), region of residence (4 dummies), regional unemployment rate, regional activity rate. Bootstrapped standard errors, using 100 replications.

Moving to the post-reform scenario, the difference between temporary contracts and unemployed lowers also for the older group. The effect of experiencing a temporary contract vs. staying in unemployment for this group decline to a level of 44%. Concerning the effect of experiencing the New Deal eligibility versus short term unemployment our evidence shows that the difference decline, in the post New Deal, to a level of 40%. In line with the post-reform estimates for the younger group, also the ones for the olders show that New Deal eligible have an higher probability to get a job amounting to a level of 4.4%confirming the results of Blundell, Costa Dias, Meghir, and Van Reenen (2004).

With respect to the probability of finding a permanent job our results do not find a clear pattern and any significant result.

## 2.8 Conclusions

The aim of this paper is to find the effect of experiencing a temporary job-spell versus being unemployed in the UK, controlling for the New Deal effects.

In so doing we contribute to the literature in two main ways, we apply the Barbieri and Sestito's approach - and in particular the comparison temporary vs. unemployed - in the UK context, while it was used before just for the Italian case.

Second, thanks to the New Deal implementation we have shown that if the unemployed are better trained - the difference between temporary and unemployed is progressively decreasing.

We find evidence that experiencing a temporary job spell increase significantly the probability of find a job nine months later, both for the younger and for the older group. At the same time our results show that the difference between temporary workers and unemployed (both New Deal eligible and short-term unemployed) decreased after the New Deal implementation. Comparing the effect of being New Deal eligible vs stay in unemployment we could observe that the New Deal participants, after the New Deal implementation, are significantly more likely to find a job after nine months by about 5% a result which is in line with previous findings.

Concerning the no-effect on permanent employment, as already mentioned this could be mainly driven by the short time-span of our data. Unfortunately, long panels, such as BHPS, do not have a sufficient sample size to lead to robust results.

# Appendix A

# Appendix to Chapter 1

## A.1 The Employment Contract Termination The case of UK Legislation

## A.2 Unfair Dismissal

By virtue of UK Employment Rights Act 1996 (ERA), all the employees have the right not to be unfairly dismissed. One the one hand, under this regulation, the employer is required to provide the Employment Tribunal that the reason for the dismissal falls within one of potentially fair reasons[1].

On the other hand, for a dismissed employee [2] to claim unfair dismissal,

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[1] The potentially fair reasons according to the section 98 of ERA are:

- lack of capability or qualification;
- misconduct;
- redundancy : this means that there is no more necessity of that worker, or there is not enough work for him/her
- a statuary restriction;
- another substantial reason;
- retirement: from 1 October 2006. This means that those individuals above the retirement age (65 years) could be fairly dismissed.

[2] According the UK legislation, besides the self-employed there are some employees that are not allowed to claim unfair dismissal, in particular:

- police officers;

he/she should have worked continuously for his/her employer at least for the minimum probationary period. The probationary period is an essential requirement to claim unfair dismissal, except for those cases when the dismissal is considered Automatically Unfair [3]. However, claims must be presented to (i.e. actually received at) the Employment Tribunal within three months of the effective date of employment termination. These time limits are strictly enforced and therefore if the employees miss the deadline the Employment Tribunal may refuse to hear his/her case.

According to the British legislation, if firing is not sustained by fair reasons, the Employment Tribunal could force the firm to three possible solutions: take back the employee to his/her previous job (re-instatement), to a different job for the same employer (re-engagement) or in most cases to a compensation award.

There are two main elements to compensate for unfair dismissal: basic award and compensatory award[4]. Strictly speaking, the basic award is roughly equivalent to a statuary redundancy payment[5] and is calculated

- members of the armed forces;
- share fishermen;
- people working outside Great Britain;
- registered dock workers;
- employees above the normal retirement age.

[3](See appendix A.5 for major details)

[4]The maximum compensatory award for loss suffered following a dismissal was £ 56,800 until 1 February 2006. From 1 February 2006 to 1 February 2008 it was increased up to £ 58,400. From 1 February 2008 it has further been increased up to £ 63,000.

[5]Under the 1996 ERA the following categories of employees have no right to redundancy payments:

- members of the armed forces;
- House of Lords and House of Commons staff;
- apprentices whose service ends at the end of the apprenticeship contract;
- employees at the end of a fixed term contract which was agreed, renewed or extended before 1 October 2002 and lasted at least two years where they have already given written agreement to waive their entitlement to a redundancy payment at the end of the contract. Any waivers inserted into contracts agreed, renewed or extend after 1 October 2002 will not be valid and fixed-term employees will have the right to

with respect to employee's age, length of service and rate of pay[6], while the compensatory award keeps into account broader components, such as earnings lost for the period from the date of the dismissal to the date of the Employment tribunal hearing, future loss of earnings, fringe benefits and loss of statuary employment rights.

## A.3 Wrongful discharge

Another important feature of the Employment Legislation is the Wrongful Dismissal. A wrongful dismissal occurs where an employee has been dismissed either without notice or without adequate notice, unless the employer was acting in response to a serious breach of the contract by the employee.

Claims for wrongful dismissal can be brought either the Employment Tribunal (within three months from the dismissal date)or before the Court (within six years from the date of the dismissal).

Compensation for wrongful dismissal is usually limited to payments that would have fallen due to the employee during the notice period. However, in some cases, it is possible to add some awards to compensate for:

- loss fringe benefits;
- commission and bonus payments due under contractual schemes;
- payments under profit sharing schemes;
- loss of pension contributions;
- back pay and holiday pay;

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statuary payments if they have been continuously employed for two years or more and are made redundant;

- domestic servants working in private home;
- share fishermen paid only by a share in the proceeds of the catch;
- crown servants or employees in a public office;
- employees of Government of an oversees territory.

[6] See appendix A.6 for major details

- share options.

In some circumstances, a wrongful dismissal claim can be brought if the dismissal was in breach of contractually binding disciplinary rules and procedures. In these cases, damages may be calculated by reference to how long it would have taken to comply with the contractual procedure in addiction to the notice period.

## A.4 Constructive dismissal

Constructive dismissal happens when an employee is forced to quit his/her job against his/her will because of his/her employer's conduct.

Typical examples of reason to claim constructive dismissal are:

- a serious breach in the contract (i.e. no payment);
- changes of the employment contract without employee's agreement;
- dangerous work condition.

The first steps against conductive dismissal are grievance procedure and mediation, but if they do not work and the employee is forced to quit the employer, he/she is entitled to claim the Jobseeker's Allowance, which is an weekly amount of money depending on the age and on the family conditions.

## A.5 Automatically unfair reasons for dismissal

The requirement of full completion of the probationary period is not necessary for those employee who can prove that they are fired for at least one of the following reasons:

- exercise their statutory rights, like the right to write particulars of their terms and conditions;
- pregnancy;
- take/ask to take statutory maternity, paternity or adoption leave;

- are or intend to be a trade union member or refuse to join a union;
- exercise their rights under the National Minimum Wage Act;
- complain about a health and safety problem;
- report wrongdoing at work ('whistleblowing');
- exercise the rights in connection with a statutory grievance or disciplinary procedure;
- take part in official industrial; action that lasts less than 12 weeks;
- take time off for jury service;
- ask to work flexibly if they have that right;
- exercise their rights under the Working Time Regulations.

## A.6 Calculation of the basic award

For the basic award, the tribunal gives:

- half a week's gross basic pay for each year of service, if the employees is younger than 22 years old.
- one week's gross basic pay for each year of service, if the employees is between 22 and 41 years old.
- one and a half weeks'gross basic pay for each year of service for those over the age of 41.

The maximum numbers of years which can be compensated is 20.

## A.7 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.

## A.8 Tables

Table A.1: 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 A.2: 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 A.3: 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 A.4: 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 A.5: 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 A.6: 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 |

### A.8.1 Descriptive statistics for robustness checks

Table A.7: 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 A.8: 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 |

## A.9 Figures

Figure A.1: UK Unemployment Rate by Quarter

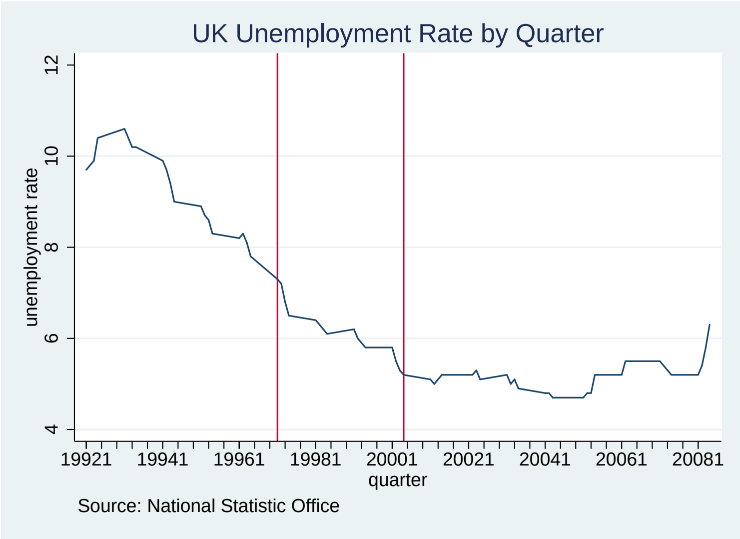


Figure A.2: UK Activity Rate

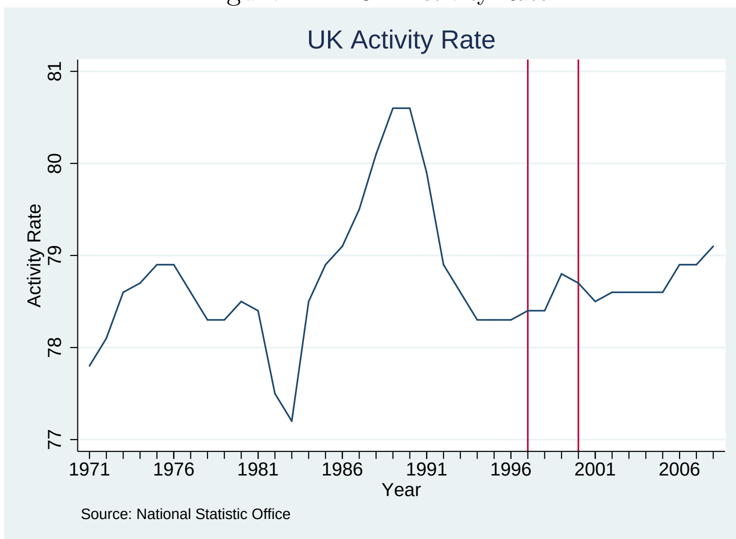



Figure A.3: UK Gross Domestic Product by Quarter

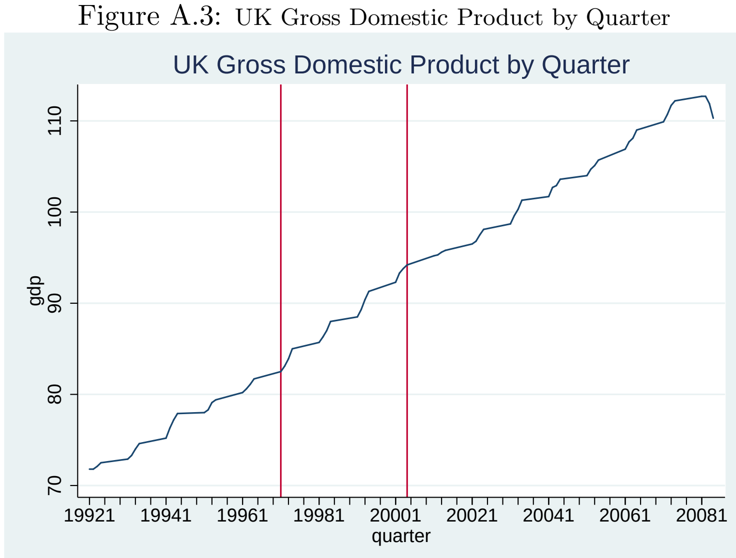


Figure A.4: UK Gross Domestic Product Growth by Quarter

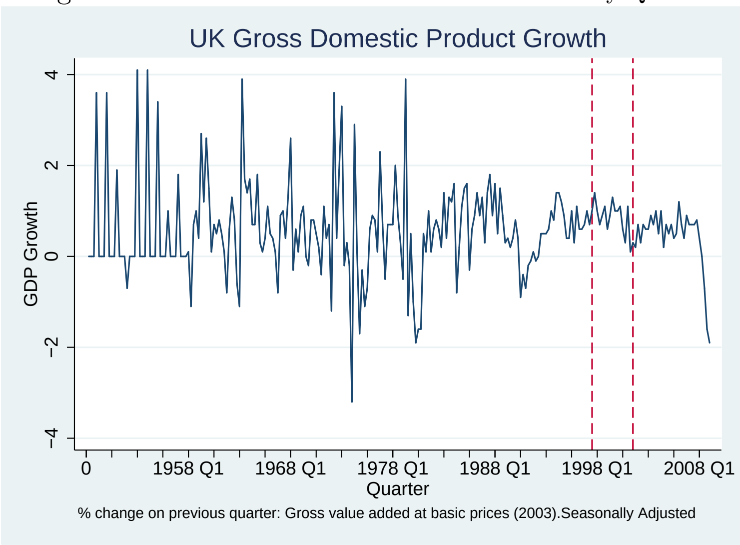



Figure A.5: Survival at firm, comparison between workers younger than 50 with those older

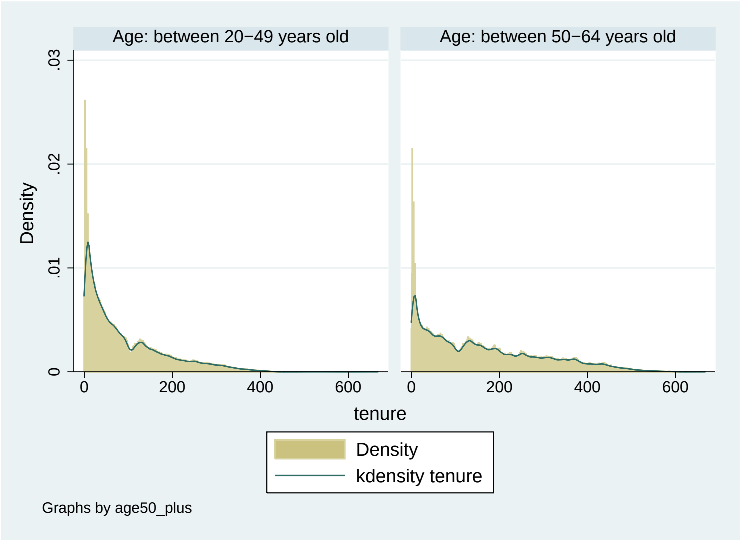


Figure A.6: Kaplan - Meier estimate of firing survivor function, by cohort and tenure

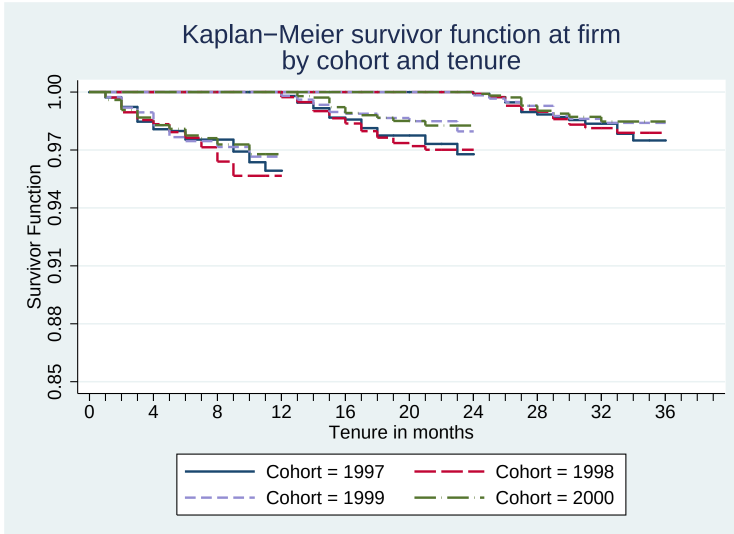


Figure A.7: Kaplan - Meier estimate of quitting survivor function , before and after the reform

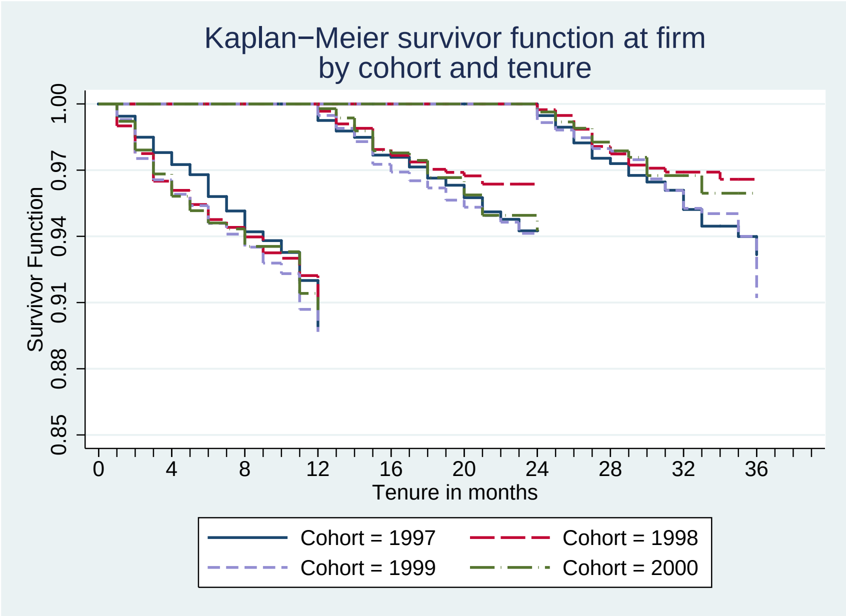



Figure A.8: Kaplan - Meier estimate of other types of termination survivor function , before and after the reform

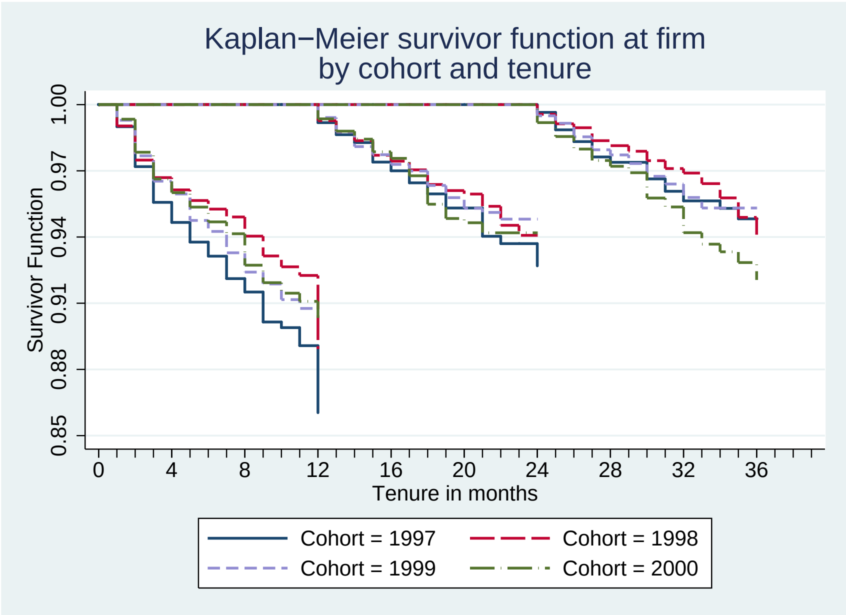


Figure A.9: Estimation of the causal effect of the reform on the hazard of termination

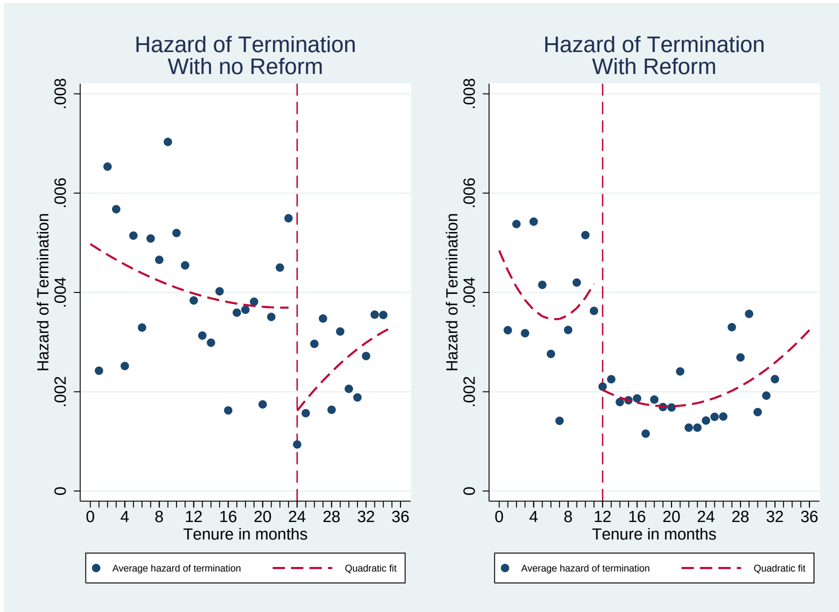


Figure A.10: In the graph 'treated' refers to those workers tenured between 12 and 24 months in 1999. 'controls' refers to all the others. Each graph shows the propensity score comparing the treated and each control

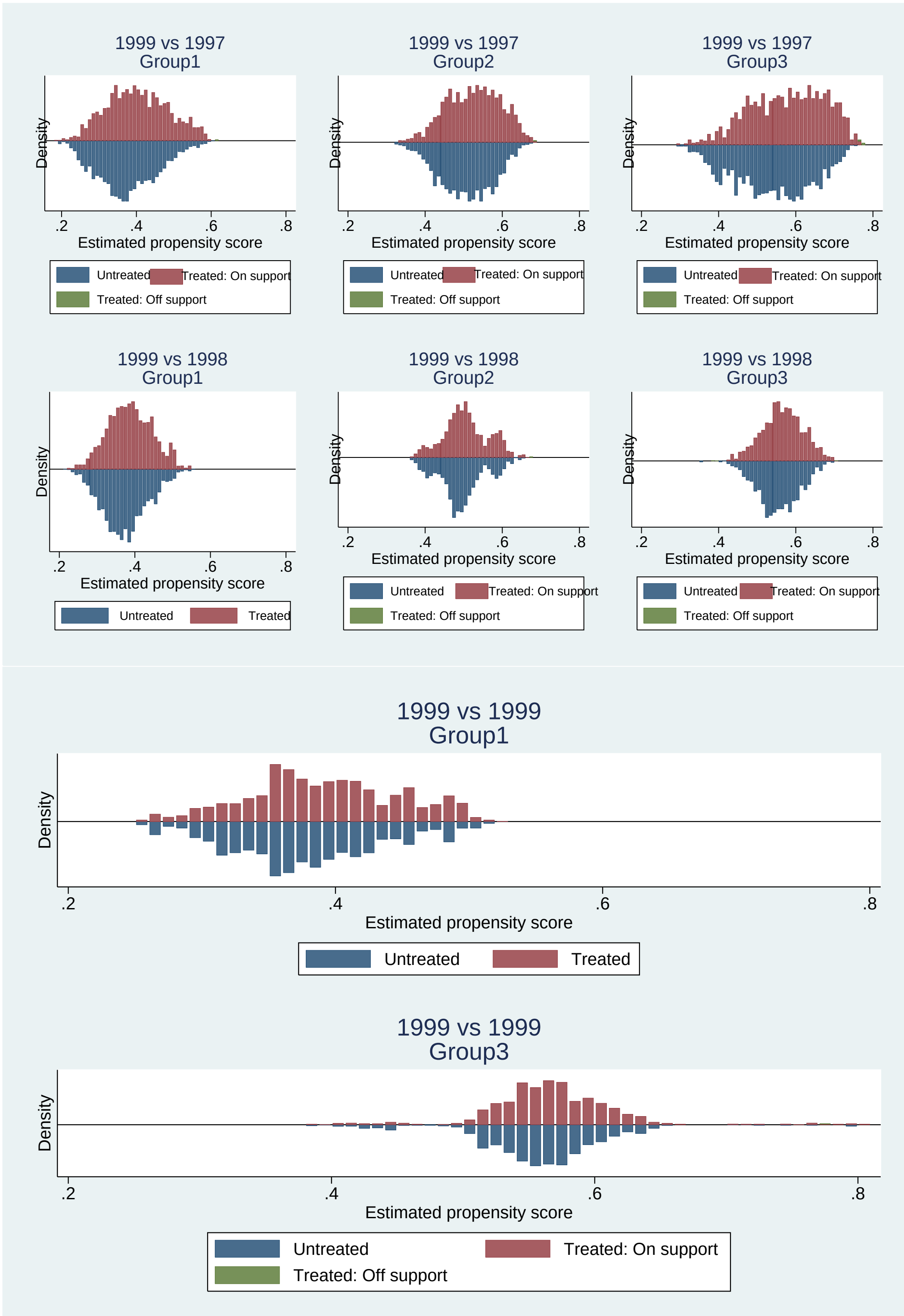


Figure A.11: Kaplan - Meier estimate of firing survivor function, by cohort and tenure - Separately by industry: Manufacturing

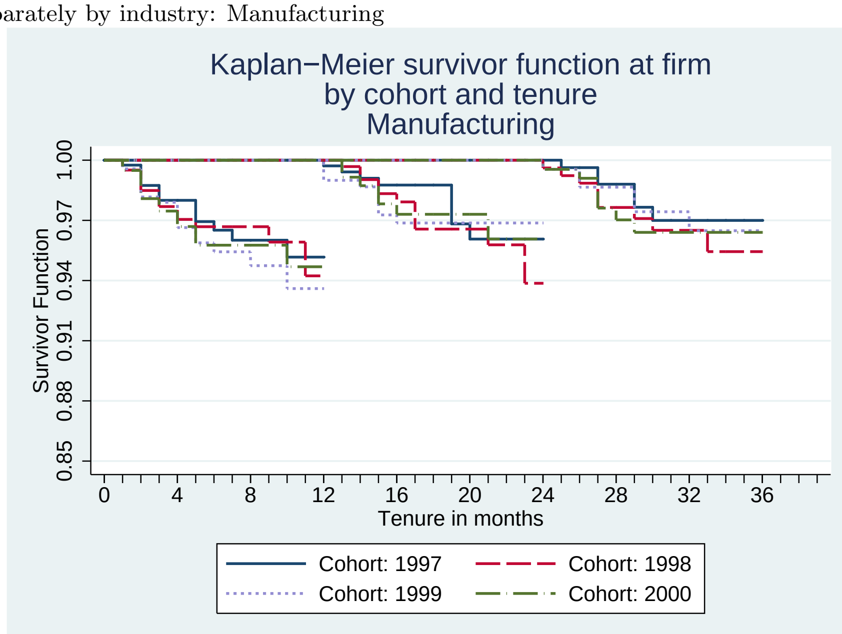



Figure A.12: Estimation of the causal effect of the reform on the hazard of termination - Separately by industry: Manufacturing

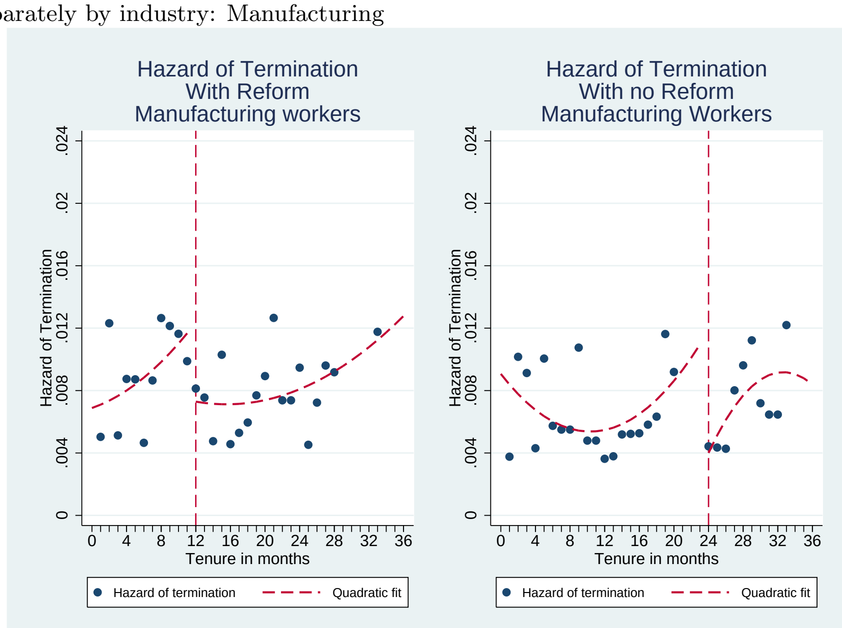


Figure A.13: Estimation of the causal effect of the reform on the hazard of termination - Separately by workers skills: Skilled

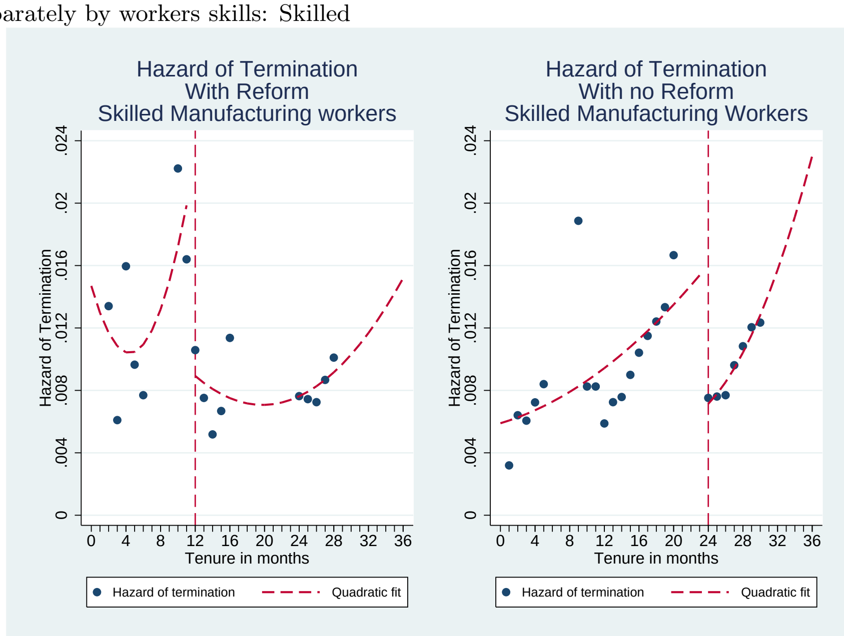



Figure A.14: Estimation of the causal effect of the reform on the hazard of termination - Separately by workers skills: Skilled

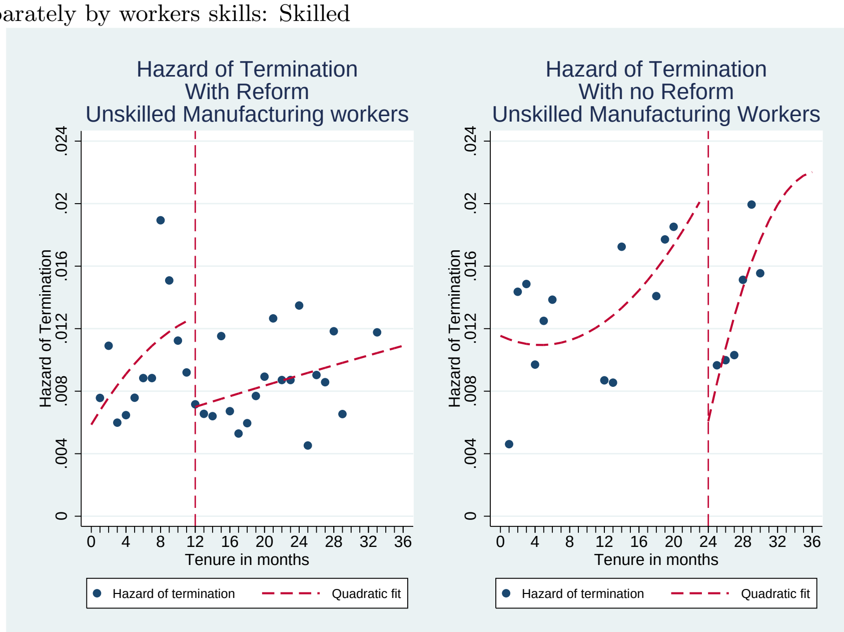


Figure A.15: Kaplan - Meier estimate of firing survivor function, by cohort and tenure - Females

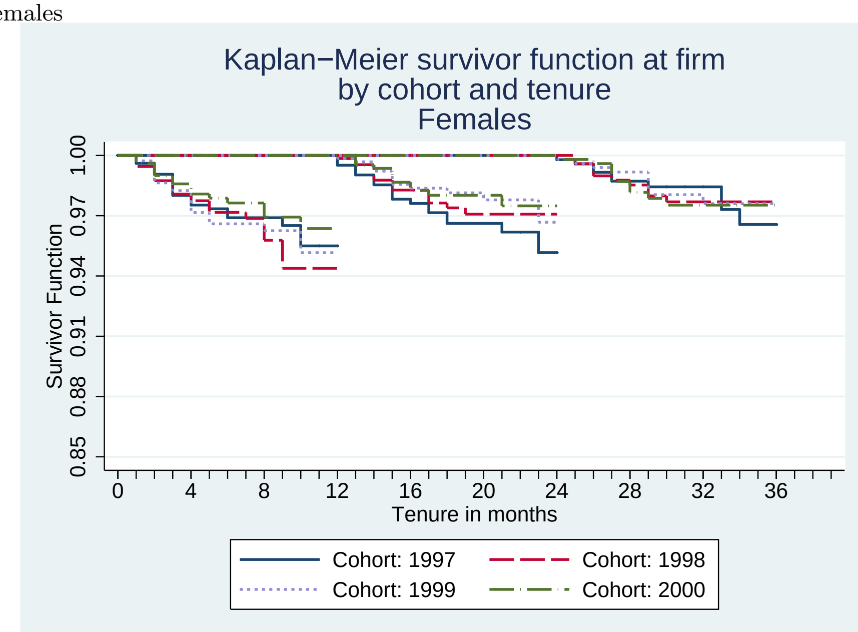



Figure A.16: Kaplan - Meier estimate of firing survivor function, by cohort and tenure - Males

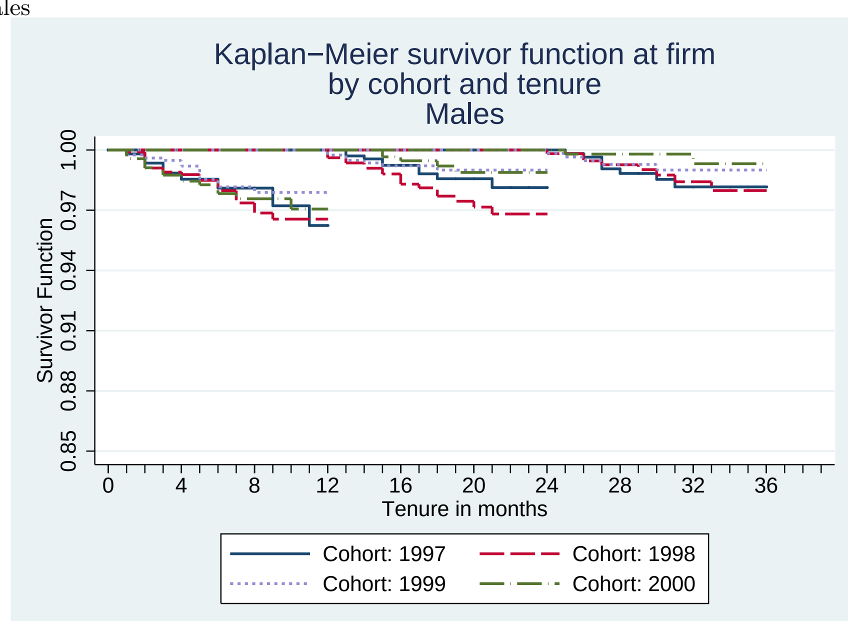


Figure A.17: Estimation of the causal effect of the reform on the hazard of termination - Females

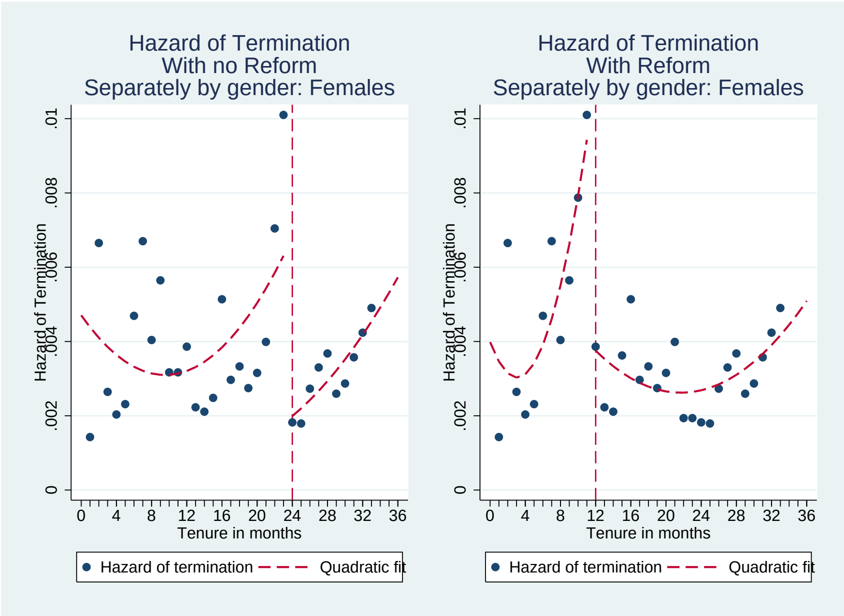
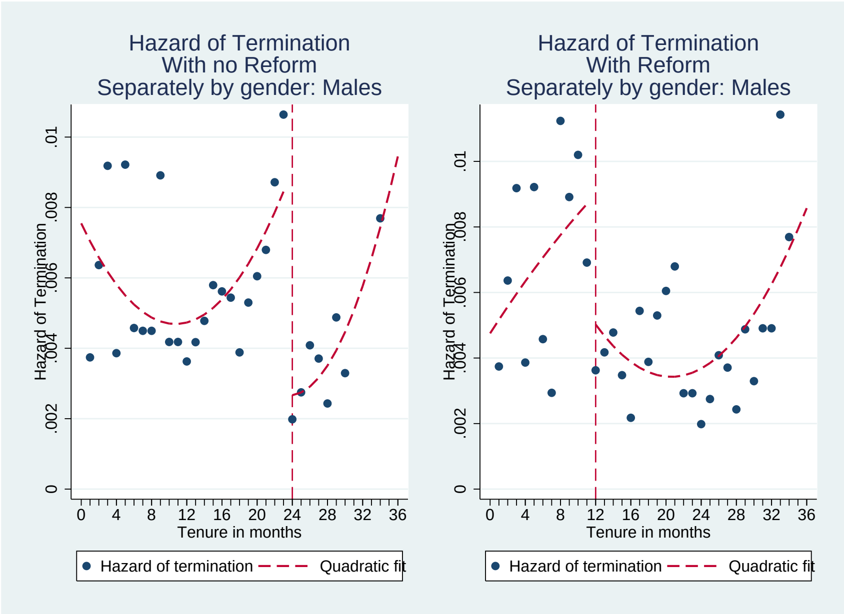



Figure A.18: Estimation of the causal effect of the reform on the hazard of termination



## A.10 Covariates sample mean in unmatched and matched samples

| Variable | Sample | *Treated: workers tenured between 12 and 24 months after the reform (in 1999)* | *Controls: workers tenured less than 12 months in 1997* |
|---|---|---|---|
| Year of birth | **Unmatched** | 1966.1 | 1964.2 |
| | **Matched** | 1966.1 | 1966.4 |
| Year of birth * gender | **Unmatched** | 1060.1 | 1066 |
| | **Matched** | 1059.2 | 1043.6 |
| Black ethnicity | **Unmatched** | 0.02108 | 0.01519 |
| | **Matched** | 0.02012 | 0.01398 |
| Other Etnicity | **Unmatched** | 0.03627 | 0.03627 |
| | **Matched** | 0.03631 | 0.02306 |
| Female | **Unmatched** | 0.53922 | 0.54278 |
| | **Matched** | 0.53876 | 0.53091 |
| Less than high school educated | **Unmatched** | 0.18824 | 0.26054 |
| | **Matched** | 0.18842 | 0.17812 |
| University educated | **Unmatched** | 0.39265 | 0.29154 |
| | **Matched** | 0.39205 | 0.38027 |
| North West | **Unmatched** | 0.09314 | 0.10834 |
| | **Matched** | 0.09323 | 0.09887 |
| Yorkshire and Humber | **Unmatched** | 0.08039 | 0.09253 |
| | **Matched** | 0.08047 | 0.08759 |

*Continued on next page...*

*... table A.9 continued*

| | | | |
|---|---|---|---|
| East Midlands | **Unmatched** | 0.07377 | 0.08385 |
| | **Matched** | 0.07385 | 0.06845 |
| West Midlands | **Unmatched** | 0.08603 | 0.09222 |
| | **Matched** | 0.08611 | 0.08734 |
| East | **Unmatched** | 0.10368 | 0.08912 |
| | **Matched** | 0.10378 | 0.11138 |
| London | **Unmatched** | 0.15588 | 0.11051 |
| | **Matched** | 0.15505 | 0.15432 |
| South East | **Unmatched** | 0.14755 | 0.14228 |
| | **Matched** | 0.14769 | 0.16021 |
| South West | **Unmatched** | 0.08113 | 0.08912 |
| | **Matched** | 0.08121 | 0.08121 |
| Wales | **Unmatched** | 0.04779 | 0.04262 |
| | **Matched** | 0.04784 | 0.02625 |
| Scotland | **Unmatched** | 0.07034 | 0.07889 |
| | **Matched** | 0.07041 | 0.07777 |
| Northern Ireland | **Unmatched** | 0.02868 | 0.02356 |
| | **Matched** | 0.0287 | 0.01497 |
| Primary & Construction | **Unmatched** | 0.21471 | 0.23729 |
| | **Matched** | 0.21492 | 0.19014 |
| Public Sector | **Unmatched** | 0.25662 | 0.23559 |
| | **Matched** | 0.25589 | 0.25908 |

Table A.9: Workers tenured 12 months or less in 1997 as conterfactual to Treated

| Variable | Sample | *workers tenured between 12 and 24 months after the reform (in 1999)* | *Controls: workers tenured between 12 and 24 months in 1997* |
|---|---|---|---|
| Year of birth | **Unmatched** | 1966.1 | 1964.3 |
| | **Matched** | 1966.1 | 1965.9 |
| Year of birth * gender | **Unmatched** | 1060.1 | 1022.1 |
| | **Matched** | 1059.4 | 1040.6 |
| Black ethnicity | **Unmatched** | 0.02108 | 0.01625 |
| | **Matched** | 0.02109 | 0.02011 |
| Other Etnicity | **Unmatched** | 0.03627 | 0.04167 |
| | **Matched** | 0.0363 | 0.03507 |
| Female | **Unmatched** | 0.53922 | 0.52044 |
| | **Matched** | 0.53888 | 0.52931 |
| Less than high school educated | **Unmatched** | 0.18824 | 0.21436 |
| | **Matched** | 0.18837 | 0.17807 |
| University educated | **Unmatched** | 0.39265 | 0.33962 |
| | **Matched** | 0.3922 | 0.40029 |
| North West | **Unmatched** | 0.09314 | 0.09172 |
| | **Matched** | 0.09321 | 0.09566 |
| Yorkshire and Humber | **Unmatched** | 0.08039 | 0.0815 |
| | **Matched** | 0.08045 | 0.07432 |
| East Midlands | **Unmatched** | 0.07377 | 0.07233 |
| | **Matched** | 0.07383 | 0.07334 |

*Continued on next page...*

*... table A.10 continued*

| | | | |
|---|---|---|---|
| West Midlands | **Unmatched** | 0.08603 | 0.09722 |
| | **Matched** | 0.08609 | 0.07726 |
| East | **Unmatched** | 0.10368 | 0.09801 |
| | **Matched** | 0.10375 | 0.07898 |
| London | **Unmatched** | 0.15588 | 0.12526 |
| | **Matched** | 0.15526 | 0.18985 |
| South East | **Unmatched** | 0.14755 | 0.15094 |
| | **Matched** | 0.14766 | 0.15036 |
| South West | **Unmatched** | 0.08113 | 0.08281 |
| | **Matched** | 0.08119 | 0.08462 |
| Wales | **Unmatched** | 0.04779 | 0.04298 |
| | **Matched** | 0.04783 | 0.04979 |
| Scotland | **Unmatched** | 0.07034 | 0.07862 |
| | **Matched** | 0.07039 | 0.07775 |
| Northern Ireland | **Unmatched** | 0.02868 | 0.03512 |
| | **Matched** | 0.0287 | 0.02183 |
| Primary & Construction | **Unmatched** | 0.21471 | 0.24921 |
| | **Matched** | 0.21486 | 0.20554 |
| Public Sector | **Unmatched** | 0.25662 | 0.23087 |
| | **Matched** | 0.25607 | 0.21609 |

Table A.10: Workers tenured between 12 and 24 in 1997 as conterfactual to Treated

| Variable | Sample | *Treated: workers tenured between 12 and 24 months after the reform (in 1999)* | *Controls: workers tenured between 24 and 36 months in 1997* |
|---|---|---|---|
| Year of birth | **Unmatched** | 1966.1 | 1963.1 |
| | **Matched** | 1966.1 | 1965.9 |
| Year of birth * gender | **Unmatched** | 1060.1 | 1056.4 |
| | **Matched** | 1058 | 1112 |
| Black ethnicity | **Unmatched** | 0.02108 | 0.01638 |
| | **Matched** | 0.02088 | 0.03021 |
| Other Etnicity | **Unmatched** | 0.03627 | 0.03213 |
| | **Matched** | 0.03635 | 0.03095 |
| Female | **Unmatched** | 0.53922 | 0.53827 |
| | **Matched** | 0.5382 | 0.56571 |
| Less than high school educated | **Unmatched** | 0.18824 | 0.23496 |
| | **Matched** | 0.18865 | 0.20535 |
| University educated | **Unmatched** | 0.39265 | 0.33071 |
| | **Matched** | 0.3913 | 0.37214 |
| North West | **Unmatched** | 0.09314 | 0.11748 |
| | **Matched** | 0.09334 | 0.08966 |
| Yorkshire and Humber | **Unmatched** | 0.08039 | 0.08346 |
| | **Matched** | 0.08057 | 0.09162 |
| East Midlands | **Unmatched** | 0.07377 | 0.06488 |
| | **Matched** | 0.07394 | 0.07025 |

*Continued on next page...*

*... table A.11 continued*

| | | | |
|---|---|---|---|
| West Midlands | **Unmatched** | 0.08603 | 0.08724 |
| | **Matched** | 0.08622 | 0.08204 |
| East | **Unmatched** | 0.10368 | 0.11244 |
| | **Matched** | 0.10391 | 0.10243 |
| London | **Unmatched** | 0.15588 | 0.11654 |
| | **Matched** | 0.15402 | 0.18251 |
| South East | **Unmatched** | 0.14755 | 0.13764 |
| | **Matched** | 0.14788 | 0.13952 |
| South West | **Unmatched** | 0.08113 | 0.07276 |
| | **Matched** | 0.08131 | 0.07738 |
| Wales | **Unmatched** | 0.04779 | 0.04472 |
| | **Matched** | 0.0479 | 0.04053 |
| Scotland | **Unmatched** | 0.07034 | 0.08378 |
| | **Matched** | 0.0705 | 0.07197 |
| Northern Ireland | **Unmatched** | 0.02868 | 0.0378 |
| | **Matched** | 0.02874 | 0.0199 |
| Primary & Construction | **Unmatched** | 0.21471 | 0.24787 |
| | **Matched** | 0.21518 | 0.20339 |
| Public Sector | **Unmatched** | 0.25662 | 0.28031 |
| | **Matched** | 0.25718 | 0.2449 |

Table A.11: Workers tenured between 24 and 36 in 1997 as conterfactual to Treated

| Variable | Sample | *Treated: workers tenured between 12 and 24 months after the reform (in 1999)* | *Controls: workers tenured less than 12 months in 1998* |
|---|---|---|---|
| Year of birth | **Unmatched** | 1966.1 | 1965.2 |
| | **Matched** | 1966.1 | 1965.7 |
| Year of birth * gender | **Unmatched** | 1060.1 | 1148.6 |
| | **Matched** | 1060.1 | 1011.7 |
| Black ethnicity | **Unmatched** | 0.02108 | 0.01736 |
| | **Matched** | 0.02108 | 0.01716 |
| Other Etnicity | **Unmatched** | 0.03627 | 0.0419 |
| | **Matched** | 0.03627 | 0.0326 |
| Female | **Unmatched** | 0.53922 | 0.58456 |
| | **Matched** | 0.53922 | 0.51471 |
| Less than high school educated | **Unmatched** | 0.18824 | 0.22763 |
| | **Matched** | 0.18824 | 0.17279 |
| University educated | **Unmatched** | 0.39265 | 0.32161 |
| | **Matched** | 0.39265 | 0.40417 |
| North West | **Unmatched** | 0.09314 | 0.09683 |
| | **Matched** | 0.09314 | 0.09755 |
| Yorkshire and Humber | **Unmatched** | 0.08039 | 0.08186 |
| | **Matched** | 0.08039 | 0.07353 |
| East Midlands | **Unmatched** | 0.07377 | 0.07662 |
| | **Matched** | 0.07377 | 0.06863 |

*Continued on next page...*

*... table A.12 continued*

| | | | |
|---|---|---|---|
| West Midlands | **Unmatched** | 0.08603 | 0.0871 |
| | **Matched** | 0.08603 | 0.08529 |
| East | **Unmatched** | 0.10368 | 0.10057 |
| | **Matched** | 0.10368 | 0.10368 |
| London | **Unmatched** | 0.15588 | 0.12362 |
| | **Matched** | 0.15588 | 0.14877 |
| South East | **Unmatched** | 0.14755 | 0.14906 |
| | **Matched** | 0.14755 | 0.16054 |
| South West | **Unmatched** | 0.08113 | 0.09024 |
| | **Matched** | 0.08113 | 0.08725 |
| Wales | **Unmatched** | 0.04779 | 0.03846 |
| | **Matched** | 0.04779 | 0.04804 |
| Scotland | **Unmatched** | 0.07034 | 0.08905 |
| | **Matched** | 0.07034 | 0.0777 |
| Northern Ireland | **Unmatched** | 0.02868 | 0.02439 |
| | **Matched** | 0.02868 | 0.02206 |
| Primary & Construction | **Unmatched** | 0.21471 | 0.21176 |
| | **Matched** | 0.21471 | 0.20637 |
| Public Sector | **Unmatched** | 0.25662 | 0.23526 |
| | **Matched** | 0.25662 | 0.24926 |

Table A.12: Workers tenured 12 months or less in 1998 as conterfactual to Treated

| Variable | Sample | *Treated: workers tenured between 12 and 24 months after the reform (in 1999)* | *Controls: workers tenured between 12 and 24 months in 1998* |
|---|---|---|---|
| Year of birth | **Unmatched** | 1966.1 | 1965.1 |
| | **Matched** | 1966.1 | 1966.3 |
| Year of birth * gender | **Unmatched** | 1060.1 | 1062.6 |
| | **Matched** | 1060.9 | 1074 |
| Black ethnicity | **Unmatched** | 0.02108 | 0.01372 |
| | **Matched** | 0.01988 | 0.02429 |
| Other Etnicity | **Unmatched** | 0.03627 | 0.03404 |
| | **Matched** | 0.03632 | 0.03706 |
| Female | **Unmatched** | 0.53922 | 0.54078 |
| | **Matched** | 0.53963 | 0.54626 |
| Less than high school educated | **Unmatched** | 0.18824 | 0.23782 |
| | **Matched** | 0.18847 | 0.19656 |
| University educated | **Unmatched** | 0.39265 | 0.36713 |
| | **Matched** | 0.39313 | 0.39656 |
| North West | **Unmatched** | 0.09314 | 0.10997 |
| | **Matched** | 0.09325 | 0.09988 |
| Yorkshire and Humber | **Unmatched** | 0.08039 | 0.08327 |
| | **Matched** | 0.08049 | 0.07902 |
| East Midlands | **Unmatched** | 0.07377 | 0.08719 |
| | **Matched** | 0.07387 | 0.05742 |

*Continued on next page...*

*... table A.13 continued*

| | | | |
|---|---|---|---|
| West Midlands | **Unmatched** | 0.08603 | 0.09454 |
| | **Matched** | 0.08613 | 0.08859 |
| East | **Unmatched** | 0.10368 | 0.09895 |
| | **Matched** | 0.1038 | 0.10405 |
| London | **Unmatched** | 0.15588 | 0.11732 |
| | **Matched** | 0.15485 | 0.16172 |
| South East | **Unmatched** | 0.14755 | 0.16238 |
| | **Matched** | 0.14773 | 0.15975 |
| South West | **Unmatched** | 0.08113 | 0.05805 |
| | **Matched** | 0.08123 | 0.0692 |
| Wales | **Unmatched** | 0.04779 | 0.03796 |
| | **Matched** | 0.04785 | 0.04712 |
| Scotland | **Unmatched** | 0.07034 | 0.08768 |
| | **Matched** | 0.07043 | 0.06601 |
| Northern Ireland | **Unmatched** | 0.02868 | 0.03159 |
| | **Matched** | 0.02871 | 0.04098 |
| Primary & Construction | **Unmatched** | 0.21471 | 0.23194 |
| | **Matched** | 0.21497 | 0.18871 |
| Public Sector | **Unmatched** | 0.25662 | 0.25349 |
| | **Matched** | 0.25571 | 0.24344 |

Table A.13: Workers tenured between 12 and 24 in 1998 as conterfactual to Treated

| Variable | Sample | *Treated: workers tenured between 12 and 24 months after the reform (in 1999)* | *Contols: workers tenured between 24 and 36 months in 1998* |
|---|---|---|---|
| Year of birth | **Unmatched** | 1966.1 | 1965 |
| | **Matched** | 1966.1 | 1966.3 |
| Year of birth * gender | **Unmatched** | 1060.1 | 1045.3 |
| | **Matched** | 1059.2 | 1111.4 |
| Black ethnicity | **Unmatched** | 0.02108 | 0.01724 |
| | **Matched** | 0.02085 | 0.013 |
| Other Etnicity | **Unmatched** | 0.03627 | 0.04669 |
| | **Matched** | 0.03557 | 0.03214 |
| Female | **Unmatched** | 0.53922 | 0.53212 |
| | **Matched** | 0.53876 | 0.56526 |
| Less than high school educated | **Unmatched** | 0.18824 | 0.20683 |
| | **Matched** | 0.18817 | 0.17149 |
| University educated | **Unmatched** | 0.39265 | 0.36102 |
| | **Matched** | 0.3923 | 0.39794 |
| North West | **Unmatched** | 0.09314 | 0.09433 |
| | **Matched** | 0.09323 | 0.10378 |
| Yorkshire and Humber | **Unmatched** | 0.08039 | 0.08085 |
| | **Matched** | 0.08047 | 0.08023 |
| East Midlands | **Unmatched** | 0.07377 | 0.08305 |
| | **Matched** | 0.07385 | 0.0844 |

*Continued on next page...*

*... table A.14 continued*

| | | | |
|---|---|---|---|
| West Midlands | **Unmatched** | 0.08603 | 0.09433 |
| | **Matched** | 0.08562 | 0.10059 |
| East | **Unmatched** | 0.10368 | 0.11219 |
| | **Matched** | 0.10378 | 0.10967 |
| London | **Unmatched** | 0.15588 | 0.13193 |
| | **Matched** | 0.15554 | 0.13886 |
| South East | **Unmatched** | 0.14755 | 0.12974 |
| | **Matched** | 0.14769 | 0.13813 |
| South West | **Unmatched** | 0.08113 | 0.08179 |
| | **Matched** | 0.08121 | 0.08489 |
| Wales | **Unmatched** | 0.04779 | 0.04011 |
| | **Matched** | 0.04784 | 0.03508 |
| Scotland | **Unmatched** | 0.07034 | 0.08555 |
| | **Matched** | 0.07041 | 0.07311 |
| Northern Ireland | **Unmatched** | 0.02868 | 0.02883 |
| | **Matched** | 0.0287 | 0.02134 |
| Primary & Construction | **Unmatched** | 0.21471 | 0.23253 |
| | **Matched** | 0.21492 | 0.21443 |
| Public Sector | **Unmatched** | 0.25662 | 0.2811 |
| | **Matched** | 0.25613 | 0.23626 |

Table A.14: Workers tenured between 24 and 36 in 1998 as conterfactual to Treated

| Variable | **Sample** | *Treated: workers tenured between 12 and 24 months after the reform (in 1999)* | *Controls: workers tenured less than 12 months in 1999* |
|---|---|---|---|
| Year of birth | **Unmatched** | 1966.1 | 1965.8 |
| | **Matched** | 1966.1 | 1966.4 |
| Year of birth * gender | **Unmatched** | 1060.1 | 1103 |
| | **Matched** | 1060.1 | 1078.3 |
| Black ethnicity | **Unmatched** | 0.02108 | 0.01937 |
| | **Matched** | 0.02108 | 0.02304 |
| Other Etnicity | **Unmatched** | 0.03627 | 0.03431 |
| | **Matched** | 0.03627 | 0.03113 |
| Female | **Unmatched** | 0.53922 | 0.56123 |
| | **Matched** | 0.53922 | 0.54853 |
| Less than high school educated | **Unmatched** | 0.18824 | 0.24478 |
| | **Matched** | 0.18824 | 0.1701 |
| University educated | **Unmatched** | 0.39265 | 0.31676 |
| | **Matched** | 0.39265 | 0.39828 |
| North West | **Unmatched** | 0.09314 | 0.11545 |
| | **Matched** | 0.09314 | 0.1027 |
| Yorkshire and Humber | **Unmatched** | 0.08039 | 0.07671 |
| | **Matched** | 0.08039 | 0.08824 |
| East Midlands | **Unmatched** | 0.07377 | 0.06466 |
| | **Matched** | 0.07377 | 0.08211 |

*Continued on next page...*

*... table A.15 continued*

| | | | |
|---|---|---|---|
| West Midlands | **Unmatched** | 0.08603 | 0.0941 |
| | **Matched** | 0.08603 | 0.08162 |
| East | **Unmatched** | 0.10368 | 0.09166 |
| | **Matched** | 0.10368 | 0.12108 |
| London | **Unmatched** | 0.15588 | 0.11941 |
| | **Matched** | 0.15588 | 0.14216 |
| South East | **Unmatched** | 0.14755 | 0.15617 |
| | **Matched** | 0.14755 | 0.14755 |
| South West | **Unmatched** | 0.08113 | 0.08296 |
| | **Matched** | 0.08113 | 0.0848 |
| Wales | **Unmatched** | 0.04779 | 0.04591 |
| | **Matched** | 0.04779 | 0.03627 |
| Scotland | **Unmatched** | 0.07034 | 0.08998 |
| | **Matched** | 0.07034 | 0.06765 |
| Northern Ireland | **Unmatched** | 0.02868 | 0.02715 |
| | **Matched** | 0.02868 | 0.01814 |
| Primary & Construction | **Unmatched** | 0.21471 | 0.21016 |
| | **Matched** | 0.21471 | 0.1973 |
| Public Sector | **Unmatched** | 0.25662 | 0.25377 |
| | **Matched** | 0.25662 | 0.24142 |

Table A.15: Workers tenured 12 months or less in 1999 as conterfactual to Treated

| Variable | Sample | *Treated: workers tenured between 12 and 24 months after the reform (in 1999)* | *Contols: workers tenured between 24 and 36 months in 1998* |
|---|---|---|---|
| Year of birth | **Unmatched** | 1966.1 | 1966.3 |
| | **Matched** | 1966.1 | 1966.8 |
| Year of birth * gender | **Unmatched** | 1060.1 | 1052.5 |
| | **Matched** | 1059.7 | 1041.5 |
| Black ethnicity | **Unmatched** | 0.02108 | 0.00856 |
| | **Matched** | 0.02012 | 0.02012 |
| Other Etnicity | **Unmatched** | 0.03627 | 0.03645 |
| | **Matched** | 0.03631 | 0.04097 |
| Female | **Unmatched** | 0.53922 | 0.53534 |
| | **Matched** | 0.53901 | 0.52969 |
| Less than high school educated | **Unmatched** | 0.18824 | 0.18352 |
| | **Matched** | 0.18842 | 0.17664 |
| University educated | **Unmatched** | 0.39265 | 0.40475 |
| | **Matched** | 0.39279 | 0.38739 |
| North West | **Unmatched** | 0.09314 | 0.08336 |
| | **Matched** | 0.09249 | 0.10476 |
| Yorkshire and Humber | **Unmatched** | 0.08039 | 0.08494 |
| | **Matched** | 0.08047 | 0.08587 |
| East Midlands | **Unmatched** | 0.07377 | 0.071 |
| | **Matched** | 0.07385 | 0.06649 |

*Continued on next page...*

*... table A.16 continued*

| | | | |
|---|---|---|---|
| West Midlands | **Unmatched** | 0.08603 | 0.08875 |
| | **Matched** | 0.08611 | 0.08881 |
| East | **Unmatched** | 0.10368 | 0.11252 |
| | **Matched** | 0.10378 | 0.10451 |
| London | **Unmatched** | 0.15588 | 0.11949 |
| | **Matched** | 0.15579 | 0.15088 |
| South East | **Unmatched** | 0.14755 | 0.14802 |
| | **Matched** | 0.14769 | 0.14524 |
| South West | **Unmatched** | 0.08113 | 0.08875 |
| | **Matched** | 0.08121 | 0.07434 |
| Wales | **Unmatched** | 0.04779 | 0.04342 |
| | **Matched** | 0.04784 | 0.05373 |
| Scotland | **Unmatched** | 0.07034 | 0.07924 |
| | **Matched** | 0.07041 | 0.066 |
| Northern Ireland | **Unmatched** | 0.02868 | 0.04913 |
| | **Matched** | 0.0287 | 0.0287 |
| Primary & Construction | **Unmatched** | 0.21471 | 0.21173 |
| | **Matched** | 0.21492 | 0.20363 |
| Public Sector | **Unmatched** | 0.25662 | 0.27892 |
| | **Matched** | 0.25687 | 0.22939 |

Table A.16: Workers tenured between 24 and 36 in 1999 as conterfactual to Treated

# Appendix B

# Appendix to Chapter 2

## B.1 Main Employment Reforms

This section is aimed at describing the main reforms occurred in the UK Labour market after the New Labour settlement.

**Minimum Wage Implementation (1999):** The adult rate was set at £3.60 per hour, with a lower your rate of £3.00 per hour for those age 18-21. The youth rate subsequently rose £3.20 per hour in June 200 and the adult one rose to £3.70 per hour in October 2000. Stewart (2004) looking at the effects on Minimum wage implementation on subsequent employment probability does not find any significant result. Arulampalam, Booth, and Bryan (2004) find an increase in training and monitoring due to the introduction of the Minimum Wage. Commission (2003) shows that spillovers may have taken place on the wage distribution up to the first decile.

**Unfair Dismissal Reform (1999):** The probationary period to claim unfair dismissal was halved from two year to one year. Marinescu (2009) looking at the probationary period shortening on the dismissal hazard finds that the reform leads 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. Analyzing the same reform, on the probability of being dismissed, we find a significant decline amounting to 1% just for the newly covered - i.e. between 12 and 24 months of tenure.

**Parental leave (1999):** Three months of paid or unpaid time off work to care for a child or make arrangements for the child's welfare. Up to our knowledge no previous work on it.

**Work Family Tax Credit (1999)** :Introduction of a new child-care tax credit for low-income families claiming Working Families' Tax Credit, giving a maximum amount of £70 a week for one child and £150 for two. To claim this benefit the parents have to work at least 16 hours per week (the sum of them). Blundell, Duncan, McCrae, and Meghir (2000) using estimates from structural model to simulate likely response; looking only at initial level of Working Family Tax Credit (WFTC) find that women with employed partners tend to decrease their employment supply by 0.6%, whereas lone mothers increase it by 2.2%. Brewer, Duncan, Shephard, and Suarez (2006) find that women with employed partners reduce their employment by 0.6%, while those with non-employed partners tend to increase their employment by 0.1%, whereas lone mothers increase it by 5.1%.

**Work and Family Bill & longer maternity leave (2005):** from 26 to 52 weeks and paid paternity leave for fathers if the mother returns to work before the end of her maternity leave. Up to our knowledge no previous work on it.

## B.2 Summary Statistics

Table B.1: Employment status at t=0 in more detailed categories for those aged between 18-24 looking at cohort.

| Age: 18-24 | 1996/1997 | | 1999/2000 | |
|---|---|---|---|---|
| State in t=0 | No | % | No | % |
| Total | 4,692 | 100 | 3,724 | 100 |
| Self-employed | 136 | 2.9 | 96 | 2.58 |
| Permanent employee | 3,047 | 64.94 | 2,453 | 65.87 |
| Temporary employee | 219 | 4.67 | 165 | 4.43 |
| Unemployed | 597 | 12.72 | 488 | 13.1 |
| New Deal Eligible | 327 | 6.97 | 306 | 8.22 |
| OLF | 366 | 7.8 | 216 | 5.8 |

Table B.2: Employment status at t=0 in more detailed categories for those aged between 25-49 looking at cohort.

| Age: 25- 49 | 1996/1997 | | 1999/2000 | |
|---|---|---|---|---|
| State in t=0 | No | % | No | % |
| Total | 41,077 | 100 | 35,635 | 100 |
| Self-employed | 4,520 | 11.00 | 3,635 | 10.2 |
| Permanent employee | 28,270 | 68.82 | 25,506 | 71.58 |
| Temporary employee | 1,072 | 2.61 | 828 | 2.32 |
| Unemployed | 924 | 2.25 | 608 | 1.71 |
| New Deal Eligible | 801 | 1.95 | 310 | 0.87 |
| OLF | 5,490 | 13.37 | 4,748 | 13.32 |

Table B.3: Employment status at t=1 in more detailed categories for those aged between 18-24 who were unemployed at time t=0 looking at cohort.

| Age: 18-24 | 1996/1997 | | 1999/2000 | |
|---:|---:|---:|---:|---:|
| State in t=1 | No | % | No | % |
| Total | 597 | 100 | 488 | 100 |
| Self-employed | 2 | 0.34 | 4 | 0.67 |
| Permanent employee | 47 | 7.87 | 50 | 10.25 |
| Temporary employee | 89 | 14.91 | 94 | 19.26 |
| Unemployed | 239 | 40.03 | 183 | 37.50 |
| New Deal Eligible | 196 | 32.83 | 148 | 30.33 |
| OLF | 24 | 4.02 | 9 | 1.84 |

Table B.4: Employment status at t=1 in more detailed categories for those aged between 25-49 who were unemployed at time t=0 looking at cohort.

| Age: 25-49 | 1996/1997 | | 1999/2000 | |
|---:|---:|---:|---:|---:|
| State in t=1 | No | % | No | % |
| Total | 924 | 100 | 608 | 100 |
| Self-employed | 5 | 0.54 | 7 | 1.15 |
| Permanent employee | 52 | 5.63 | 46 | 7.57 |
| Temporary employee | 156 | 16.88 | 132 | 21.71 |
| Unemployed | 336 | 36.36 | 292 | 48.03 |
| New Deal Eligible | 206 | 22.29 | 59 | 9.70 |
| OLF | 144 | 15.58 | 72 | 11.84 |

## B.3 Figures

Figure B.1: In the graph 'treated' refers to those who hold a temporary contract after the New Deal Enactment. 'controls' refers to all the unemployed: long-term and short-term. Each graph shows the propensity score comparing the treated to each control, by age: Young group

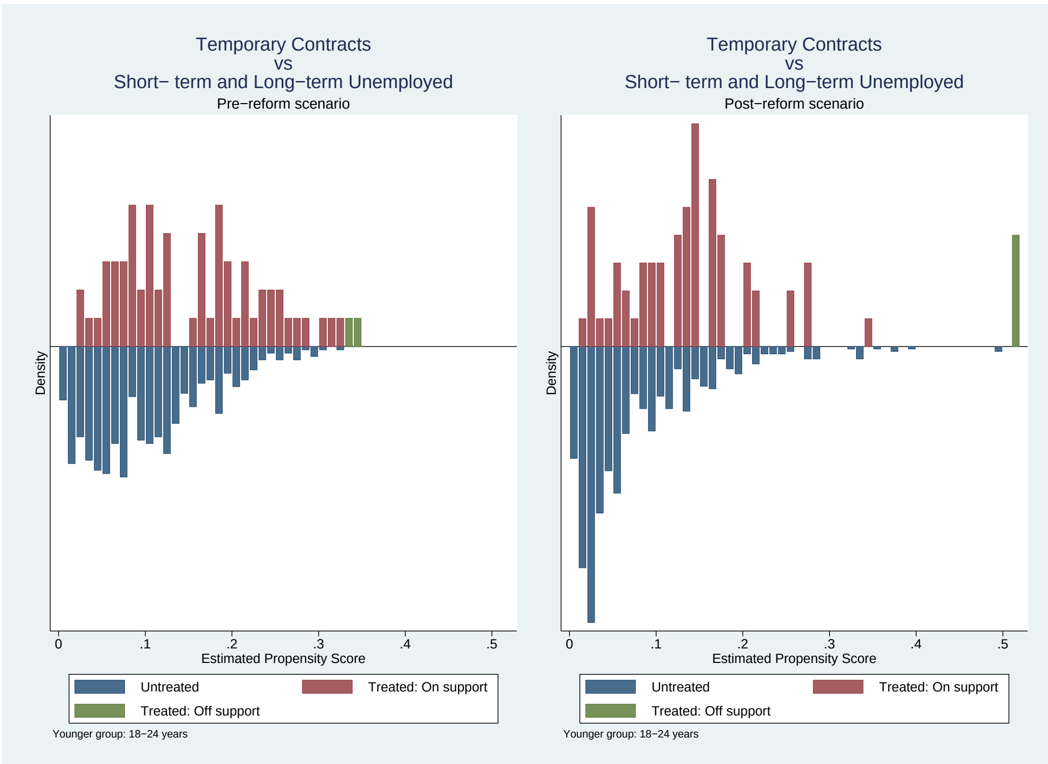


Figure B.2: In the graph 'treated' refers to those who hold a temporary contract after the New Deal Enactment. 'controls' refers to all the others. Each graph shows the propensity score comparing the treated to each control, by age: Young group

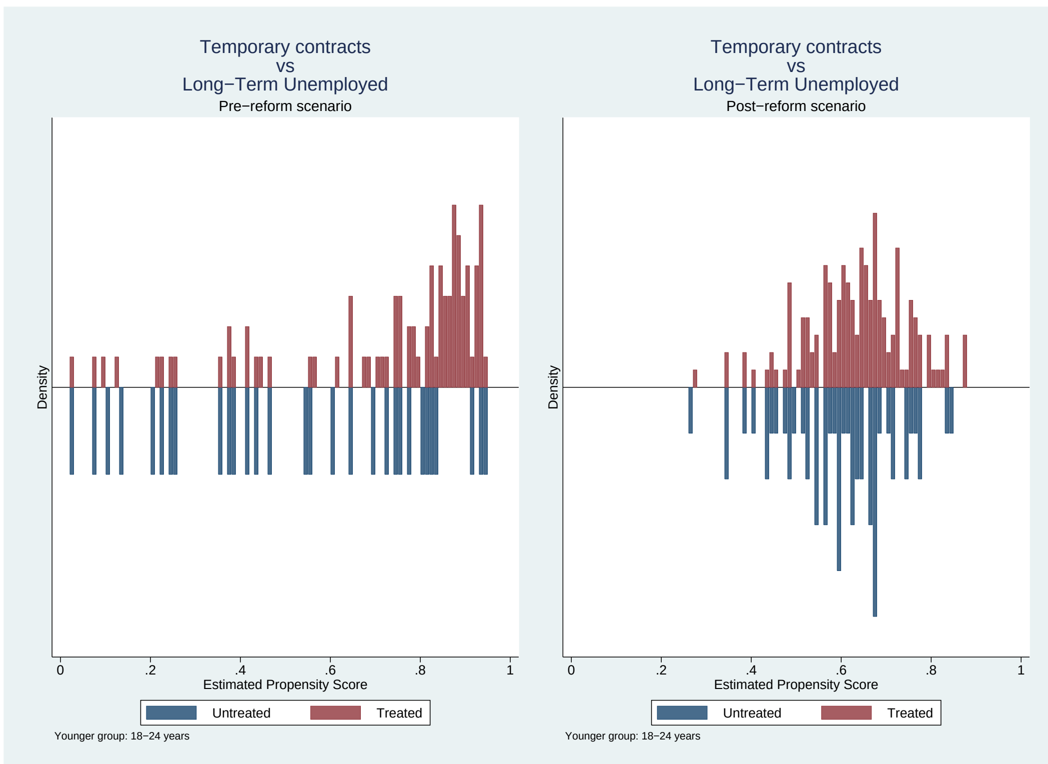


Figure B.3: In the graph 'treated' refers to those who hold a temporary contract after the New Deal Enactment. 'controls' refers to all the unemployed: long-term and short-term. Each graph shows the propensity score comparing the treated to each control, by age: Young group

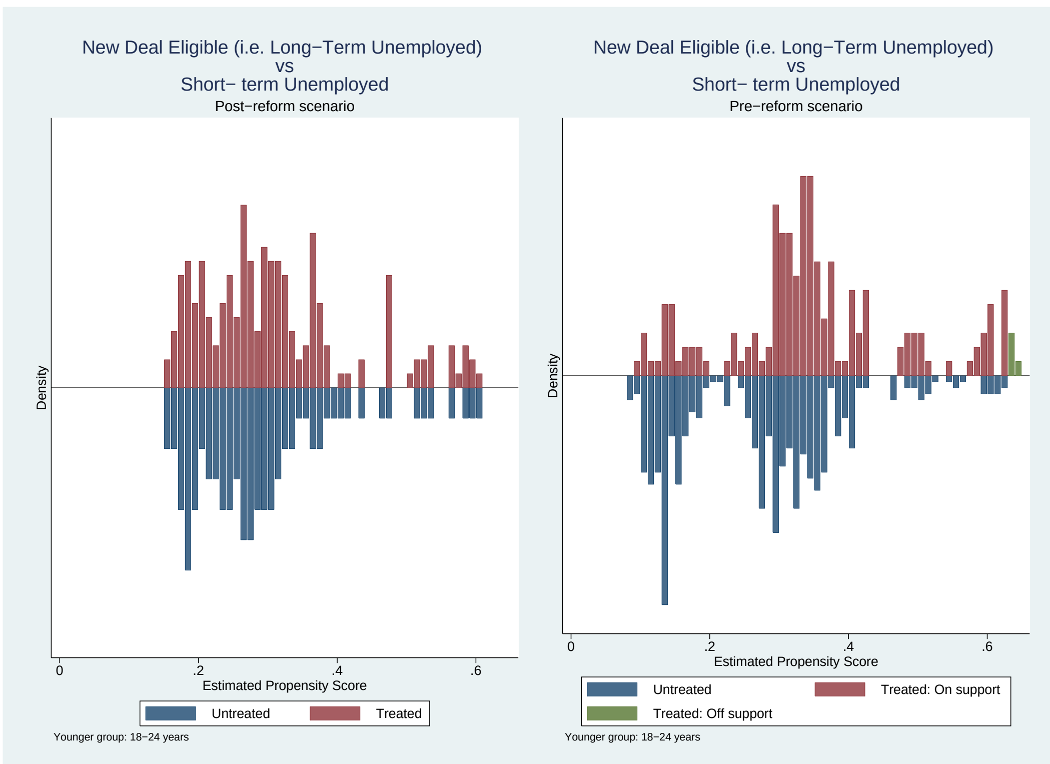


Figure B.4: In the graph 'treated' refers to those who hold a temporary contract after the New Deal Enactment. 'controls' refers to all the unemployed: long-term and short-term. Each graph shows the propensity score comparing the treated to each control, by age: Old Group

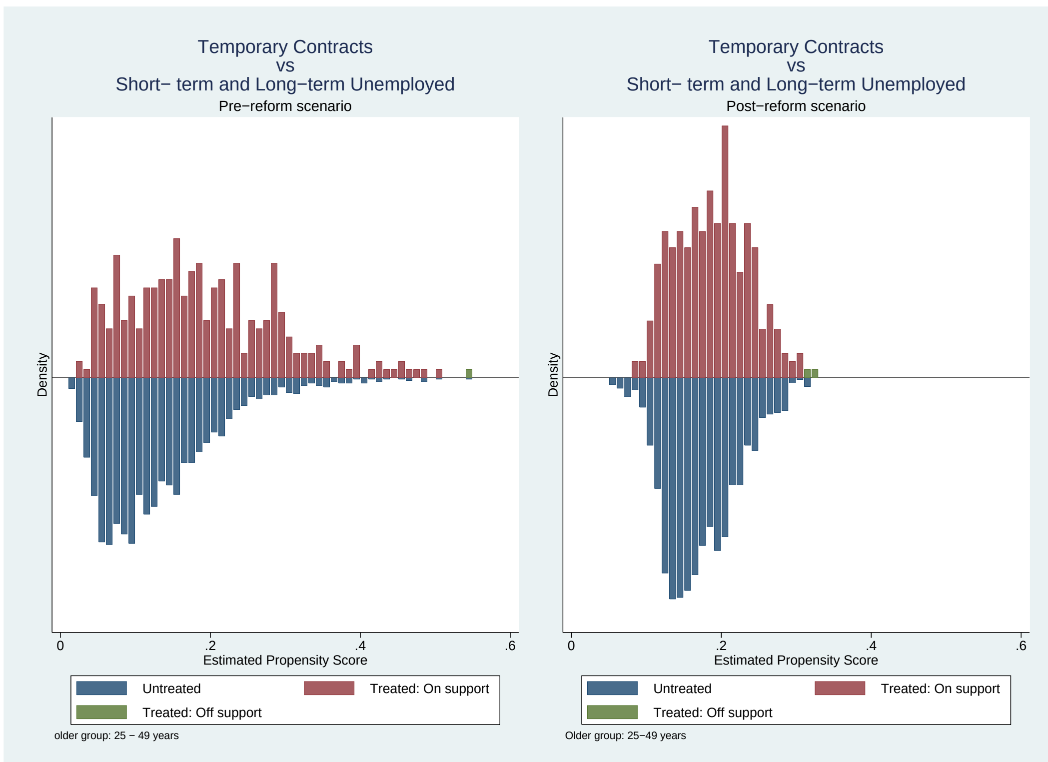


Figure B.5: In the graph 'treated' refers to those who hold a temporary contract after the New Deal Enactment. 'controls' refers to all the others. Each graph shows the propensity score comparing the treated to each control, by age: Old group

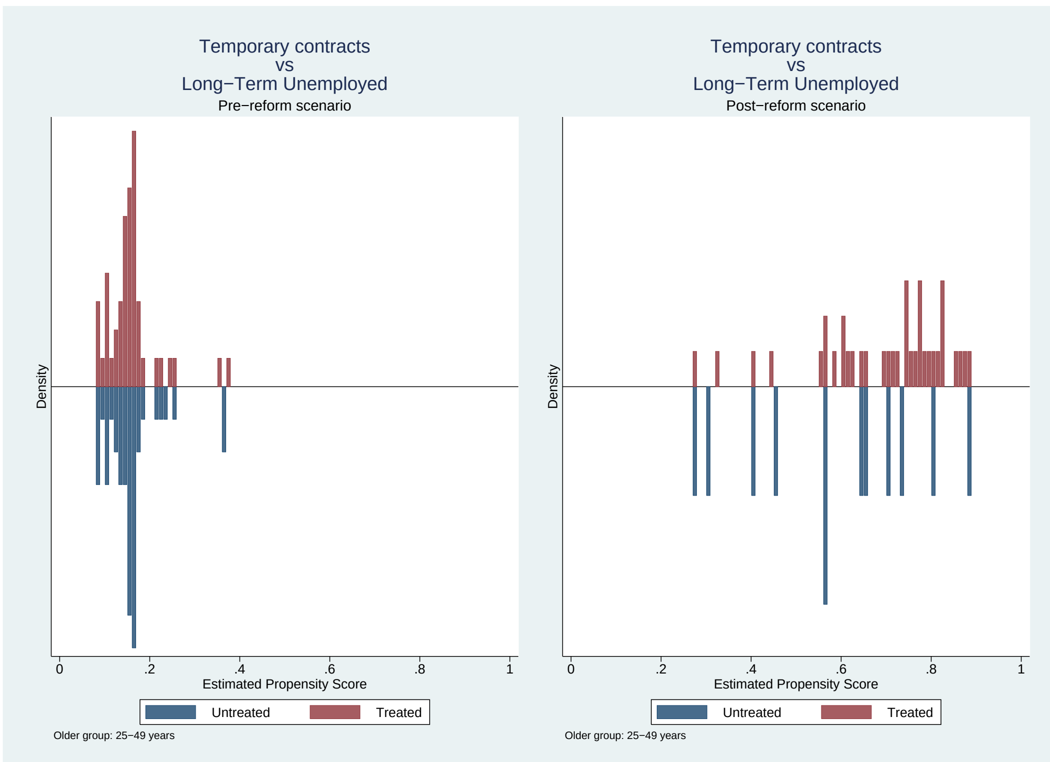
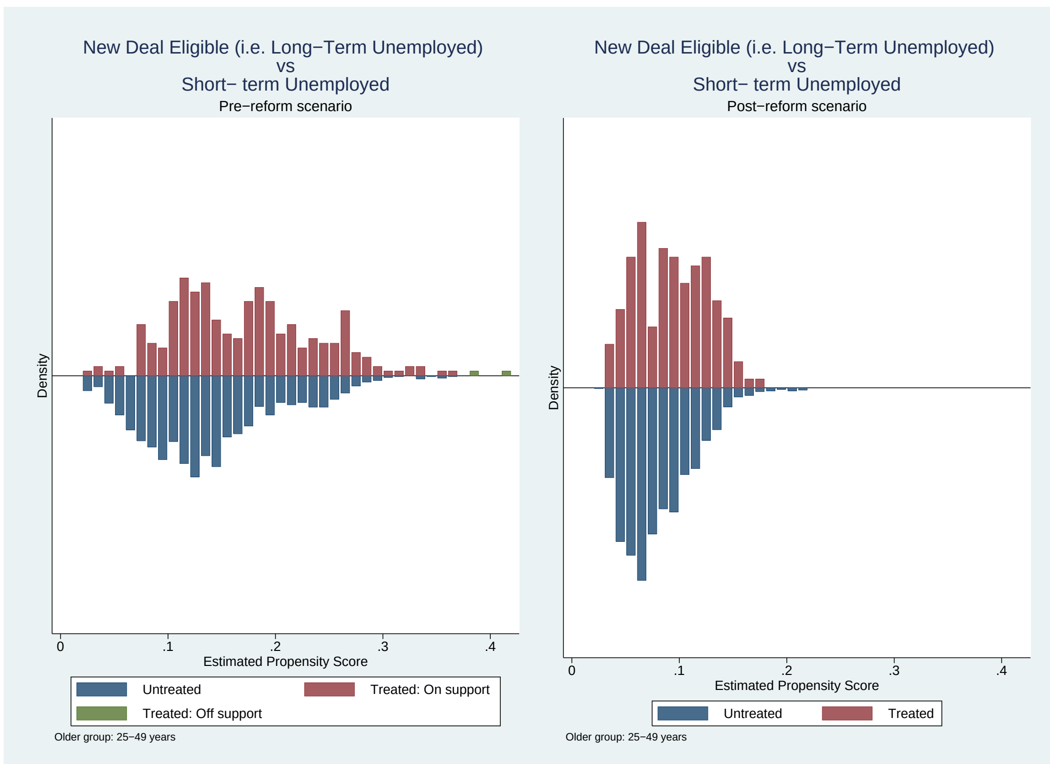


Figure B.6: In the graph 'treated' refers to those who are long-term unemployed (i.e. New Deal Eligible) after the New Deal Enactment. 'controls' refers to the short-term unemployed. Each graph shows the propensity score comparing the treated to each control, by age: Old Group



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