JOB SECURITY LEGISLATION AND JOB DURATION: EVIDENCE FROM THE U.K.

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Abstract:

Even in countries with high average job security, workers with low tenure typically enjoy very limited job protection. This article analyzes the impact of such a feature on job duration. It uses a 1999 British reform that increased job security for workers with one to two years of tenure. The firing hazard for these workers decreased by 26% relative to the hazard for workers with two to four years of tenure. The firing hazard for workers with zero to one year of tenure also decreased by 19%, which is consistent with better recruitment practices and hence improved match quality.

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I.Introduction

European job security legislation is more stringent than its US equivalent. This difference is often seen as contributing to higher unemployment in Europe (e.g. Lazear, 1990). However, the gap between US and European job security legislation is not as large as it would seem at first glance. In the US, both state-level exceptions and federal statutes, such as anti-discrimination laws, create firing costs for employers. On the other hand, the right not to be unfairly dismissed, introduced in most western European countries in the early 1970's, is usually conditional on employees having reached a legislated minimal tenure in their job. Given the centrality of job security legislation in policy debates, it is crucially important to understand how this kind of legislation affects worker flows and the quality of job matching.

This paper uses a British policy change to identify the impact of job security legislation on firms' firing behavior using a difference-in-differences strategy. In the U.K., on June 1st 1999, the tenure necessary to qualify for protection against unfair dismissal was lowered from 24 to 12 months for any termination resulting from dismissal or redundancy[1]. The group chiefly affected by this policy change is employees with 12 to 23 months of tenure: before this policy change, they had no right to claim unfair dismissal, but after the policy change they could make such a claim and receive compensation. Since the policy change increases the firing cost for workers with 12 to 23 months of tenure, I expect a lower probability of firing for these workers after the policy change. Employees with 24 months of tenure or more were, in principle, unaffected by the policy change, and are used as a control group. Employees with less than 12 months tenure are considered as treated. Indeed, they may be

[1] From now on, "firing" refers to dismissal (discharge for cause) or redundancy (lay off).

affected by the policy change if, for example, employers screen better after the policy change to avoid a potential trial in the event of termination after the shorter qualifying period. Thus, according to Ian MacCartney, the employment minister responsible for the reduction in the qualifying period, firms could respond to the new policy by improving their recruitment methods (Financial Times, 1999). Consistent with this view, small firms with fewer than 50 employees surveyed by Blackburn and Hart (2002) after the policy change report that, because of the risk of an unfair dismissal trial, they are taking more care about whom they recruit. Finally, HR managers polled after the reform report that internal disciplinary and grievance cases[2] are the biggest legal challenge for HR (Personnel Today, 2002). HR managers cite recruitment as the main non-legal challenge facing HR, possibly indicating an increased effort in that domain after the policy change. Besides acting on the recruitment channel, firms could also react to the policy change by monitoring employees more closely during the first months on the job. In the framework of a learning model of job separation similar to Jovanovic's (1979), one can show that a higher recruitment quality implies a lower firing hazard for workers with 0 to a few months tenure, while a higher monitoring effort implies a higher firing hazard for these same workers.

The empirical analysis of the firing hazard estimates duration models on the U.K. Labour Force Survey longitudinal datasets. Using employees with 24 to 48 months of tenure as a control group in a Cox proportional hazard model, I find that the policy change significantly decreased the hazard of termination by 19% and 26% for employees with 0 to 11 months of tenure and for employees with 12 to 23 months of tenure respectively. The decrease in firing for employees with 0 to 11 months of tenure is consistent with firms having increased the quality of new recruits after the policy change. Prior to the policy change, the

[2] Internal disciplinary action is often a step towards the worker's ultimate dismissal. In fact, failure to go through a disciplinary procedure before dismissing a worker could lead to the firm being successfully sued for unfair dismissal.

firing hazards of treatment and control groups evolve similarly over time, and they diverge in the year following the policy change. This supports the validity of the control group, and suggests that the impact of the policy change on the firing hazards of the treated groups is indeed causal. Lastly, I show that while most demographic and educational groups are similarly affected by the policy change, this change has a distinctive effect on college educated workers. After the policy change, firms do not seem to increase recruitment efforts targeted at college educated workers; instead, there is evidence consistent with some increase in monitoring efforts. A brief investigation of the impact of the reform on outcomes other than job separation shows that unemployment duration decreased after this policy change, training increased, and wages were unaffected.

Closely related to this paper are Bauer, Bender and Bonin (2007) and Kugler and Pica (2008). Using a difference-in-differences approach, Bauer et al. (2007) investigate the impact of granting employees the right to claim unfair dismissal on employment in small German firms. While their window of observation may be too short to yield robust estimates, they find no evidence that granting employees unfair dismissal protection affects job flow rates. Using a similar empirical strategy and a longer time frame, Kugler and Pica (2008) found that increasing firing costs for small Italian firms has an offsetting effect on accessions and separations, and thus leaves employment unchanged. While these papers base the assignment to control and treatment groups on firm size, this paper uses workers' job tenure as the basis for such an assignment. The treatment group in this paper is thus more representative of the whole economy. Indeed, all employees go through the stage of being a low-tenure employee and, at any point of time, about a fourth to a third of all workers have less than 24 months of tenure. Overall, my findings confirm the results from the small quasi-experimental literature suggesting that strengthening employment protection in Europe does not have any tangible

negative impact on employment. This paper further suggests that shortening the probationary period may increase the quality of job matching through improved recruitment practices.

The rest of this paper is organized as follows. Section II gives some further institutional background. It also intuitively explains the expected impact of the reform if firms learn about the quality of their workers over time. Section III describes the data, presents the main empirical results about the firing hazard, and analyzes the impact of the policy change on the firing hazard of various sub-groups of workers. Section III also details robustness tests and briefly analyzes the impact of the policy change on wages, training and the duration of unemployment. Section IV concludes.

II.Background and theory

A.Background on probationary periods

Conditioning employment protection on workers having reached a given tenure can be seen as a way to tackle the trade-offs generated by firing costs (Kugler, 2007), combining the best of the employment at will doctrine[3] and job security. Indeed, firing costs may reduce the burden of economic downturns by making firms internalize the social costs of firing. Moreover, these costs may increase productivity by improving job matching or stimulating investment in human capital (Malcomson, 1999). And, for risk averse workers, job security is a benefit in itself. On the other hand, higher firing costs will tend to reduce hiring in as much as they increase the cost of labor (Bertola, 1992). High firing costs may also prevent the sorting of workers into the jobs they are best suited to, thus reducing productivity (Blanchard and Katz, 1997).

[3] This is the default doctrine in the U.S. and means that employers can dismiss workers for any reason they want, i.e. at will. There are now many exceptions to employment at will.

A probationary period mitigates the productivity problem, since firms can fire workers unsuited to the job at low cost at the beginning of the employment relationship (Krueger, 1991). The institution of a probationary period is also related to the "last in, first out" rule, which requires that, when a firm lays off workers, it should first lay off those with lowest tenure on the job. A probationary period allows firms to adjust their workforce at lower cost, while preserving most workers' job security. Tenure-dependant job protection is thus a measure that can balance workers' and firms' objectives.

Tenure-dependent job protection is also an important element in the debate about the reform of European employment policies. Indeed, many European countries developed fixed-term contracts to allow for a probationary period without directly altering their protective legislation. Recently, French and German governments were planning to increase the duration of the probationary period for regular employment contracts. In France, the introduction of more flexible employment contracts for small firms (CNE) and young people under 26 (CPE) ultimately failed due to the combined opposition of public opinion and higher courts. The negative reactions to the French reforms may have made the German government reconsider its plans; in any case, the proposed reforms were not pushed forward.

In the UK, the qualifying period for unfair dismissal[4] was changed several times in the past (Davies and Freedland, 1993). The last change, which is the focus here, occurred after Labour came to power in 1997, when this 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. This measure was part of a package destined to promote new labor practices. In the May 1998 *Fairness at Work* white paper (www.dti.gov.uk/er/fairness/), the

[4] In the U.K., if a worker is dismissed (i.e. for cause) or made redundant (laid off), and satisfies the relevant conditions, he can sue his former employer, claiming that the dismissal was unfair. The Employment Tribunal decides on the case. If the worker's claim is found to be legitimate, the firm has to pay the worker a compensation that is largely based on the worker's wage and seniority (see Marinescu (2008a) for more details).

New Labour government essentially justifies the reduction in qualifying period[5] as a compensation offered to workers in exchange for their consent to a more flexible organization of the labor market[6].

Finally, note that the Labour government introduced a series of other labor market reforms that may potentially affect estimates of the impact of the change in the qualifying period for the right to claim unfair dismissal[7]. First, a National Minimum Wage was implemented in April 1999. Important new regulation has also been passed concerning parental leave and dependent care leave (Employment Relations Act 1999, and Maternity and Parental Leave Regulations 1999) and sex discrimination (Sex Discrimination (Gender Reassignment) Regulations 1999). The effects of these changes will be explicitly accounted for in the analysis below. Lastly, the Employment Relations Act 1999 increased the limits on the awards workers who win a trial for unfair dismissal can get at court. However, the previous limit was already not binding: 95% of the awards workers obtained in 2003 (computed from the Survey of Employment Tribunal Applications, 2003, available on www.data-archive.ac.uk) were lower than the limit prevailing before 1999. It is therefore unlikely that this change has affected firms' behavior. Overall, these other reforms should not

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[5] From a strictly legal point of view, the change in unfair dismissal rights is not equivalent to a change in what is legally defined as the probationary period (which in fact plays a very minor role in UK law). But the terminology of "probationary period" is useful to conceptualize the problem.

[6] The white paper states the following: "As the economy becomes more dynamic, leading to more frequent job changes, the Government is concerned that this period is too long and a better balance between competitiveness and fairness would be achieved if it were reduced: employees would be less inhibited about changing jobs and thereby losing their protection, which should help to promote a more flexible labour market; more employers would see the case for introducing good employment practices, which should encourage a more committed and productive workforce. Some employers claim that a long qualification period is needed to allow mistakes made in recruitment to be rectified without heavy costs. The Government accepts such mistakes happen but believes that the present period is longer than is needed to allow them to come to light and be dealt with. For all these reasons, and to increase protection against arbitrary dismissal, the Government therefore proposes to reduce the qualifying period to one year."

[7] The right not to be unfairly dismissed is but one aspect of employment law regulating the termination of contracts of employment. Other important components are the notice period and the severance (or redundancy) pay rules. The cost to firms of both these regulations is discontinuous at two years of continuous employment. These features of employment law did not change in 1999.

differentially affect the treated and control groups in the same way as the policy change of interest. Moreover, section D will test for the robustness of the results to these changes.

B. Firing costs and employer learning

The existence of a probationary period implies that firing costs become discontinuously higher after the end of the probationary period. Accordingly, I expect all other things equal to see a higher firing hazard before the end of the probationary period than afterwards. Moreover, if some workers are more likely to get fired in the near future than others, then a firm that dynamically optimizes its firing decisions would have an incentive to fire these workers shortly before the end of the probationary period in order to avoid having to fire them at higher cost thereafter. Since a mass of separations are thus shifted from the beginning of the post-probation period to the end of the probationary period, there is a spike in the firing hazard before the end of the probationary period, and a trough thereafter. Thus, after the reform, I expect the spike and trough in the hazard to occur at 11 months rather than 23 months. Importantly, I also expect the firing hazard for workers with 12 to 23 months tenure to be lower than before the reform since firing costs for this group of workers have increased. Apart from the consequences of moving the spike and trough in the firing hazard, there should be no substantial change in the firing hazard for workers with 0 to 11 months tenure or more than 24 months tenure. Indeed, firing costs did not change for these groups.

In a more complex scenario, it is conceivable that employers change their human resources management practices to further reduce the likelihood that they will have to fire workers after the end of the shorter probationary period. Thus, firms could increase the quality of recruitment so that fewer workers need to ever be fired. Alternatively, firms could monitor workers better in order to find out faster which matches are likely to be bad and hence be able to terminate them before the end of the probationary period. To investigate the impact of these strategies on the firing hazard, one can use the theoretical framework of

Jovanovic's 1979 employer learning model. Indeed, this model was found to be empirically relevant in explaining separation hazards (Farber, 1994, Nagypal, 2007). In such a theoretical framework (for details of the model, see Marinescu, 2008b), one can show that an increase in the quality of recruitment would decrease the firing hazard after the reform at all tenures, and especially so at very low tenures since this is where most firing occurs in the first place. If, instead, the quality of monitoring increases, bad matches get weeded out earlier, which increases the firing hazard at low tenures. On the other hand, with better monitoring, the firing hazard at higher tenures decreases, since those workers who survive the initial purge are more likely to be good matches.

I can now summarize the possible effects of the policy change on the firing hazard. First, note that it is not possible to determine in the general case what happens for workers who have tenures just below 12 months: indeed, the shortening in the probationary period implies that there should be a spike before 12 months, but if recruitment efforts increase then this spike may lie below the curve corresponding to a 24 month probationary period. For workers with 0 to a few months tenure, the policy change has no effect if there is no change in recruitment or monitoring quality; the firing hazard increases if monitoring quality improves, while it decreases if recruitment quality improves[8]. The firing hazard for workers with 12 to 23 months tenure always decreases after the policy change. The hazard for workers with more than 24 months tenure stays constant if firms do not change their human resources strategy, and decreases slightly if they increase monitoring or recruiting quality. However, in practice many workers with more than 24 months tenure will have been hired before the reform and they will therefore not be affected by better recruitment practices and be only

[8] It is also possible that, after the policy change, workers increase their effort on the job during the probationary period. This may also decrease the firing hazard for workers with low tenures. However, the effect on the firing hazard will be limited if firms correctly anticipate this strategic behavioral change on the part of workers.

marginally affected by better monitoring. Overall, this implies that workers with more than 24 months tenure should form a reasonable control group.

III.Results

The focus of this paper, the 1999 policy change, occurs during a phase of steadily growing employment in the UK, and the policy change does not have any immediate impact on the growing employment trend. While employment growth does slow down from August 2000 onwards, it is difficult to attribute this to the policy change. By the beginning of 2005, the employment to population ratio (UK National Statistics, MGSR series) reaches an almost all time high; it is only surpassed by the values observed before 1976. Thus, it is unlikely that the 1999 policy change has had any major impact on average labor demand in the British economy.

A.Data

The British Labour Force Survey (LFS) is administrated each quarter and contains questions similar to the Current Population Survey in the US. It is a rotating panel, and each household remains in the sample for 5 quarters. This paper uses the 2-quarters Labour Force survey longitudinal datasets[9] from March 1996 to September 2004. These datasets contain all occurrences of individuals in the LFS being observed in two consecutive quarters.

The right to claim unfair dismissal only applies to employees (i.e. not self-employed) in permanent jobs working usually more than 16 hours a week. I therefore restrict my sample to those employees[10].

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[9] Full documentation about the datasets can be found on www.data-archive.ac.uk.

[10] In principle, workers on fixed-term contracts also have the right to claim unfair dismissal, but before 1999 (Employment Relations Act), they could contractually waive this right. Moreover, the majority of employees on fixed term contracts have a tenure inferior to 2 years, which makes identifying the probability of being fired after 2 years difficult. Altogether, this means that analyzing the effects of the policy change for this

Having defined the relevant group of workers, I also have to compute their tenure[11]. The year of hiring is present for more than 99% of currently employed workers along with the date of the interview. In 61% of cases, both the year and month of hiring are known. When only the year of hiring is known, and the worker has less than 4 years tenure (0.6% of cases), I drop the observation because monthly precision is important in that range; otherwise I keep it and assume the month of hiring was January (this is random with respect to each job). For workers who separate from their jobs, the tenure at separation can also be calculated. For those who are still unemployed by the second quarter, the date when their last job ended is known. If, however, workers have found a new job (7.6% of observations), the date when they left their last job is not known, so it has to be imputed[12].

What are the potential tenure sampling problems? The sample of jobs is what is traditionally called in the duration literature a stock sample (first quarter observations) with follow-up (second quarter observations). As a result, long tenures are overrepresented. Indeed, all the jobs that started x years before the first period of observation and ended in the meantime are not observed. However, it is possible to correct for this bias in survival analysis by specifying the date of entry in the study, which in this case will be the date of the first interview minus one month[13]. Second, the follow-up also causes a small problem if a person separated from two or more jobs during the 3-months period between two interviews. Indeed, the question about the reason why a job ended explicitly refers to the last job held rather than

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group would not be as instructive as for permanent workers. I therefore perform the analysis on the permanent workers only.

[11] Because the dataset is a panel, a job can be observed for two or more consecutive periods. I only keep the first observation for each job. Thus several jobs held by the same person can be present in the sample, but not the same job observed at two or more different points in time. When it is possible, I will therefore cluster by person, and when not I will only keep the first job observed for each person. I also tried clustering by tenure category group, which yielded smaller standard errors.

[12] The distribution of completed unemployment spells lasting 3 months or less and beginning and ending with employment has 3 months as a mode. Therefore, I assume that if a worker separated from the job he was holding in the first quarter and found a new job by the second quarter, then he separated from the first job during the month of the first interview, i.e. I make the unemployment spell as long as possible. This imputation may thus overestimate tenure by at most 3 months for 7.6% of the observations.

[13] In this case, correcting for the bias simply means that jobs that started before the entry date are not counted as being in the risk set before the entry date.

to the job held at the previous interview. In most cases, the last (and lost) job and the job held at the previous interview should be the same, and this is the assumption I make. But if a person separated from two or more jobs, then the characteristics of the job at the first interview will be wrongly assigned to the last job held (and lost). However, this problem does not seem to be very important in practice[14].

If a worker left his job in the previous quarter, he is prompted to indicate the reason why the job ended among a list of the following possibilities: 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. When using duration models to explain firing, I treat other types of separation as censoring.

To summarize, the sample consists of employees in permanent jobs usually working more than 16 hours per week and having a known tenure. Table 1 gives summary statistics for the sample used. Note that among the reasons given by workers for leaving their last job, dismissals and redundancies represent a sizeable 21.4%, a proportion comparable to the "other" category (22.4%) but lower than quits (35.4%). Since the question involves self-reporting, the distinction between dismissals and redundancies has to be taken with skepticism: indeed, workers may prefer to report that they were laid off rather than discharged[15]. I now focus on workers who were fired (i.e. dismissals and redundancies).

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[14] To document the prevalence of such a problem, I compare, within the sample of people who got fired, the characteristics in terms of occupation and industry of the last job held as described in the second quarter interview with those of the job that was held in the first quarter. As it happens, when the information on both jobs is available, there is a discrepancy in only 4% of the cases, and I decide to drop these cases.

[15] It is somewhat puzzling that the end of a temporary job is a reason quoted by 3.2% of workers although the sample includes permanent jobs only; however, while the question asking about permanent jobs prompts the worker to clearly indicate if the job is "objectively temporary" rather than "subjectively temporary because he intends it to be temporary", this distinction is not insisted upon in the question about the reason for leaving the last job. Therefore, it could be that these workers meant that that job was subjectively temporary.

B. Impact of the policy change on the firing hazard: main results

I plot the non-parametric Kaplan-Meier estimate of the hazard of firing before and after June 1999 (Figure 1). Like Farber (1994)[16], I find a pattern consistent with Jovanovic's 1979 model, and the theory sketched in section B. Figure 1 shows that the shape of the hazard function in the "before" period is consistent with theoretical predictions: in particular, one very clearly observes a trough in the firing hazard around 24 months. After 24 months, the hazard function is essentially identical before and after the policy change (the difference is not significant). This confirms that employees with more than 24 months of tenure form a good control group. The hazard of termination after the policy change is significantly lower on the interval [12,23], but also on the interval [0,11], which is consistent with the quality of recruitment having increased[17]. Moreover, if workers hired after the policy change were recruited more carefully than workers hired before it, I would expect that, in the "after" period, the hazard of firing for workers with less than one year tenure hired after the policy change would be lower than the hazard of firing for similar workers hired before the policy change. This is indeed the case in the data, suggesting that there was an increase in recruitment quality after the policy change. Finally, note that there is no observable change in the firing hazard for the 24 to 48 months tenure group, which confirms that this group is a reasonable control group.

I next estimate a Cox proportional hazard model with delayed entry[18], controlling for essential covariates. The advantage of such a model is that there is no need to specify the

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[16] While the peak in terminations occurs at about 3 months as in Farber's work, it is not as sharp. This difference is not due to my looking only at terminations and not at quits, as performing the same analysis on quits yields a similar pattern. It is instead likely to be due to the fact that the NSLY is a sample of young people. Indeed, I find that for people aged less than 40, there is a sharper peak at 3-4 months.

[17] The results do not rule out an increase in both monitoring and recruiting efforts. Rather, the impact of any increase in monitoring efforts is dominated by the impact of the increase in recruitment efforts.

[18] As explained in section A, jobs are at risk of being terminated from the date of hiring but they are only observed from the date of the first interview on, i.e. they enter the study with a delay.

functional form of the baseline hazard (Lancaster, 1990). The specification for the hazard of termination is as follows:

(1) $$\lambda(t,Z) = \lambda_0(t)\exp\{\beta' Z(t) + \gamma_1' Treat * After\}$$

Z is a set of controls, including worker demographics, occupation dummies and their interaction with year dummies, industry dummies and their interaction with year dummies, region dummies, the regional monthly unemployment rate and a full set of calendar quarter-year dummies. *Treat* is a set of dummies for different ranges of tenure within the treatment group, i.e. employees with less than 24 months of tenure. *Treat* alone does not need to be included in the right-hand side variables because the non-parametric estimate $\lambda_0(t)$ of the hazard already accounts for different hazards of termination at different tenures. *After* is a dummy that takes the value one from June 1999 on (or that takes the value 1 from June 2000 on and is missing from June 1998 to May 2000, depending on specifications). *Treat**After* is the interaction between *Treat* and *After*. A test of the negative effect of the policy change on the hazard of termination is that the coefficients in the γ_1' vector are negative and significant.

Panel A of Table 2 presents the results using basic tenure categories for the treated groups. Using After 1999 as the policy change dummy and a full set of controls, I find that the policy change significantly reduced the firing hazard by 19% for workers with 0 to 11 months tenure and by 26% for workers with 12 to 23 months tenure relative to those workers having 24 to 48 months tenure. Note that the estimates without controls are almost identical and the difference between estimates with and without controls is not statistically significant, which suggests that the quasi-experimental design is solid. A problem with using "after June 1999" as the post-policy change period is that firms may have anticipated the policy change and/or it may have taken some time for firms to adjust to the new regulation. Therefore, I use as an alternative measure the after period "after June 1999, but excluding observations from May 1998 to May 2000". The results are not affected by this alternative definition of the

policy change period, which suggests that anticipation or delays in the reaction to the policy change do not play an important role in determining the estimates of the impact of the policy change on the firing hazard.

In panel B of Table 2, I use detailed tenure categories to examine the effects on different tenure subgroups. Again, the inclusion of controls or the definition of the post-policy change period do not substantially affect the results. I therefore concentrate on the specification with controls and using “after June 1999” as the post-policy change period. This is also the specification I adopt in the rest of the paper, unless otherwise specified. Concerning the effect of the policy change on different tenure categories, I find that the negative effect of the policy change on the firing hazard is significant for subgroups with 5 and up to 21 months of tenure, and fades away from month 22 to month 25. The effect is of similar magnitude as in panel A, implying a reduction in the firing hazard of about 30% for all subgroups from month 5 to month 21. The fact that the effect is smaller and insignificant for the 0 to 4 months tenure group was to be expected from the observation of Figure 1. The reduction in the firing hazard is largest for the 18 to 21 months tenure, likely due to the fact that before the policy change there used to be a spike at about 21 months tenure (Figure 1).

In Figure 2, I investigate in more detail the time pattern of the reduction in the firing hazard by allowing the firing hazard to vary half-yearly for each tenure group. The regression includes the same controls as in Table 2, but no quarter-year effects or interactions of any variables with year effects. We can see that before the first half of 1999, which I take as the reference period, the firing hazards for the two treated groups (0 to 11 months and 12 to 23 months tenure) evolve in the same way as the firing hazard of the control group (24-48 months tenure). This suggests that in the absence of the policy change, treatment and control groups respond similarly to macroeconomic trends, which is crucial for identification in a difference-in-differences setting (Blundell and Costas Dias, 2000). After the policy change,

the firing hazards of the two treated groups consistently fall below the firing hazard of the control group, while still following roughly the same time trends. In the second half of 1999, the effect of the policy change is not yet clearly visible, but the pattern becomes much more pronounced from the first half of 2000 onwards, such that the difference between the treated groups' and the control group's coefficients is of a magnitude which is consistent with the effect of the policy change as estimated in Table 2.

C. Impact of the policy change on the firing hazard of different groups

In this section, I test whether the policy change has heterogeneous effects on subgroups of workers. Indeed, numerous papers studying the impact of firing costs found that higher firing costs tended to mostly protect prime-age males and more educated workers while negatively affecting youths, females and the less educated (see for example OECD [1999]).

Table 3 examines the effects of the policy change by gender, age and education. Panel A of Table 3 shows the break-down by gender. Females see a slightly larger decrease in their firing hazard than men, but this difference is not significant. Thus, policy changes in the areas of dependent care and sex discrimination, which intervened at the same time as the policy change of interest, are not driving the results. Panel B shows the break-down by age. The impact of the policy change on workers over 40 is about half as large as for younger workers and statistically insignificant; however, the difference between the two age groups is not significant.

Panel C shows the impact of the policy change on the firing hazard by level of education. The hazard of firing significantly decreases for workers with 0 to 23 months tenure who are less than college[19] educated, but not for those who are college educated. For workers with 12 to 23 months tenure, the hazard of firing decreases for all levels of

[19] In the data, the term used is "university".

education, even though the point estimate of the decrease in the firing hazard for college educated workers with 12 to 23 months tenure is much smaller and insignificant. Why are college educated workers different? When looking at the Kaplan-Meier plot of their hazard of firing before and after the policy change (figure not reproduced here), we see that the positive insignificant effect of the policy change on workers with 0 to 11 months tenure is due to the fact that after the policy change the maximum of the firing hazard occurs at 7 months, while it occurred at 12 months before the policy change. These results could be explained by employers having increased monitoring efforts for more educated workers.

In conclusion, I do not find that males, older or more educated workers are most protected by the policy change. Quite to the contrary, there is some evidence that younger and less educated workers are those who see the greatest reduction in their firing hazards. These results are also consistent with the idea that, since there is more uncertainty about the performance of younger and less educated workers, higher firing costs tend to have more of an impact on firms' behavior towards them (Oyer and Schaefer, 2002).

D. Robustness tests and discussion

First, one may be worried that improving macroeconomic conditions are driving some of the results: low tenure workers may see a greater decrease in their firing hazard as economic conditions improve. Figure 2 discussed above shows that this is unlikely to be the case: economic conditions were improving faster before August 2000 than after, but the firing hazard of low tenure workers decreases mostly from the first half of 2000 onwards. I also interacted the Treat*After dummy with the unemployment rate to allow for different impacts of the reform in regions and months with higher unemployment rate. The interaction with unemployment was however close to zero and statistically insignificant.

Another concern is that other reforms occurring at the same time as the policy change of interest could be driving the results. First, I tested for the potential confounding effect of

the minimum wage. When restricting the sample to workers above the tenth decile of the wage distribution[20], i.e. when excluding those workers who may be affected by the introduction of the minimum wage (Low Pay Commission, 2003), the results are qualitatively unchanged. Second, with respect to the increase in the awards limit at trial, this reform should increase firing costs relatively more for high tenure workers since awards are increasing in tenure and wage. This would imply that the firing hazard decreases more for higher tenure workers. However, I find that, in absolute terms, the firing hazard of low tenure workers decreases whereas it does not decrease for higher tenure workers (Figure 1).

One could also worry that the decrease in the firing hazard after the policy change is at least in part due to firms have forced some workers to quit in order to avoid firing costs. However, the quit hazard did not increase after the policy change (not shown). This also implies that the overall separation hazard is likely to have significantly decreased after the reform. This is confirmed by Figure 3. This figure also strengthens the analysis by showing that overall separations are not influenced by the right to claim unfair dismissal. Indeed, one can see that while all separations significantly decrease after the policy change, they do not follow the same tenure pattern as firings, i.e. one does not, for example, see a trough in separations at around 24 months in the "before" period. Thus the shape of the *firing* hazard seems to be indeed determined by the existence of the right to claim unfair dismissal, while the overall separation hazard is not visibly affected by the consequences of that right.

To better evaluate the overall welfare effect of the policy change, one should look, beyond the effect on firing, at the effects of the policy change on outcomes such as unemployment duration, wages and training (results not shown). However, because of ambiguous theoretical predictions and/or a lack of fully convincing quasi-experimental

[20] The question about wages is only asked in the first and last wave of the survey, and they are therefore missing for a large share of the sample (61%). Thus, missing wages had to be imputed on the basis of a classic wage regression.

designs, results concerning the impact of the reform on these outcomes should be taken with caution. The policy change had no significant effect on wages of treated groups relative to the control group. Workers with 0 to 11 months tenure were significantly more likely to get training after the reform relative to the control group. The increase in training is small (1 percentage point), but consistent with an increase in match quality stemming from better recruitment and monitoring. Finally, to identify the impact of the policy change on unemployment duration, I use the fact that only full time workers are protected against unfair dismissal. I find however that the policy change was associated with a *decrease* in unemployment duration for workers looking for full-time jobs. This is most likely not a causal effect of the reform. In fact, the share of full-time jobs in the British economy increases quite steadily around the reform, and the reform did not substantially affect this trend. Looking finally at young workers who were less than 26 years old, I find that their unemployment duration decreased less than that of older affected workers, which is consistent with the decrease in the probationary period having hurt their relative employment prospects. Overall, the suggestive evidence about the impact of the policy change on outcomes other than firing is consistent with the reform having prompted firms to hire relatively more reliable, older workers, and to train their workers more, while there were no tangible effects on wages or unemployment.

IV.Conclusion

This paper shows that shortening the qualifying period for the right to claim unfair dismissal reduced the hazard of firing for newly covered workers, but also for workers with lower tenure, likely reflecting an increase in the quality of new recruits. The effect of the policy change on the firing hazard was concentrated among workers with less than college

education. There is also some suggestive evidence implying that the reform did not increase unemployment duration. These results provide an important new piece of evidence on the impact of firing costs on employment. They are also of particular interest to predict the impact of policies, such as those considered by France and Germany, that increase the length of the probationary period.

The findings presented here are only partially consistent with the predictions of the British labor government about the impact of the policy change. First, they predicted that it would encourage workers to change jobs, leading to a more flexible labor market. This is not the case however as quits and overall separations have actually decreased. Second, they predicted that employers would adopt better employment practices, thus increasing productivity: this seems to have happened since employers appear to be more careful about whom they hire, and to monitor some of their workers better. Lastly, the government thought that one year is enough time for the initial screening of workers. If the government meant to suggest that firms would not feel the need for any additional screening after the reform, then this is not consistent with the data since firms seem to have changed their human resources management policies in order to limit the need to fire workers past one year of tenure.

This work could be extended along several lines. To better understand the mechanisms at play, it would be helpful to examine countries with different lengths of the probationary period and different firing costs. The United Kingdom is indeed a special case: while its employment law is very similar in structure to that of the countries from continental Europe, firing costs are much lower on average. Examining more typical European countries such as France or Germany should thus shed more light on how a probationary period affects firms' behavior and labor market outcomes in the European institutional context. In general, it would be useful to further investigate how the widespread institution of a probationary period can solve the trade-offs policy makers face when deciding on firing costs.

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Table 1: Summary statistics for the sample of permanent full-time employees

	Obs.	Mean	Std. Dev.	Min	Max
Macro situation					
Unemployment rate (claimant count)	434925	3.954	1.708	1.5	11.7
Reason for leaving last job					
dismissed	38729	0.029	0.169	0	1
made redundant,voluntary redundancy	38729	0.186	0.389	0	1
temporary job ended	38729	0.032	0.175	0	1
resigned	38729	0.354	0.478	0	1
gave up work for health reasons	38729	0.047	0.212	0	1
took early retirement	38729	0.025	0.156	0	1
retired	38729	0.027	0.163	0	1
family, personal reason	38729	0.075	0.263	0	1
left for some other reason	38729	0.224	0.417	0	1
Job characteristics					
Tenure in months	434925	99.255	101.862	1	652
Person characteristics					
Female	434925	0.459	0.498	0	1
White	434925	0.956	0.204	0	1
Married and cohabiting	434925	0.582	0.493	0	1
Age	434925	38.902	11.560	16	64
Less than high school educated	434830	0.103	0.304	0	1
University educated	434830	0.278	0.448	0	1
Occupation categories					
Manager	434759	0.162	0.368	0	1
Professional	434759	0.111	0.314	0	1
Associate professional and technical	434759	0.121	0.326	0	1
Administrative and secretarial	434759	0.158	0.365	0	1
Skilled trades occupations	434759	0.107	0.309	0	1
Personal service occupations	434759	0.089	0.285	0	1
Sales and customer service occupations	434759	0.073	0.260	0	1
Process, plant and machine operatives	434759	0.098	0.297	0	1
Elementary occupations	434759	0.081	0.273	0	1

Notes: The sample is restricted to persons who are employed in the first quarter, in a permanent job, and usually working 16 or more hours a week. Only the first observation for each job (as defined by the hiring date) is kept.

Source: Labour Force Survey Two-Quarter Longitudinal Dataset (www.data-archive.ac.uk). For the unemployment rate, UK National Statistics, Time Series data [NS TSD], Regional claimant count rate, non seasonally adjusted, series code EGU4.

Table 2: Impact of the reform on the hazard of firing by tenure

	Post reform period: After June 1999		Post reform period: After June 1999, excluding May 1998 to May 2000	
	No controls	Controls	No controls	Controls
	A. Basic tenure categories			
0 to 11 months tenure	-0.223	-0.191	-0.225	-0.205
	(0.071)***	(0.067)***	(0.086)***	(0.086)**
12 to 23 months tenure	-0.273	-0.260	-0.275	-0.288
	(0.081)***	(0.077)***	(0.097)***	(0.096)***
	B. Detailed tenure categories			
0 to 4 months tenure	-0.119	-0.057	-0.106	-0.043
	(0.088)	(0.084)	(0.107)	(0.107)
5 to 11 months tenure	-0.304	-0.285	-0.310	-0.311
	(0.080)***	(0.074)***	(0.096)***	(0.095)***
12 to 17 months tenure	-0.294	-0.276	-0.289	-0.301
	(0.095)***	(0.091)***	(0.115)**	(0.114)***
18 to 21 months tenure	-0.328	-0.317	-0.321	-0.339
	(0.120)***	(0.115)***	(0.142)**	(0.140)**
22 to 23 months tenure	-0.103	-0.078	-0.147	-0.138
	(0.163)	(0.159)	(0.193)	(0.190)
24 to 25 months tenure	-0.040	-0.008	-0.015	-0.001
	(0.175)	(0.168)	(0.203)	(0.197)
Number of observations	431935	430604	336887	335782

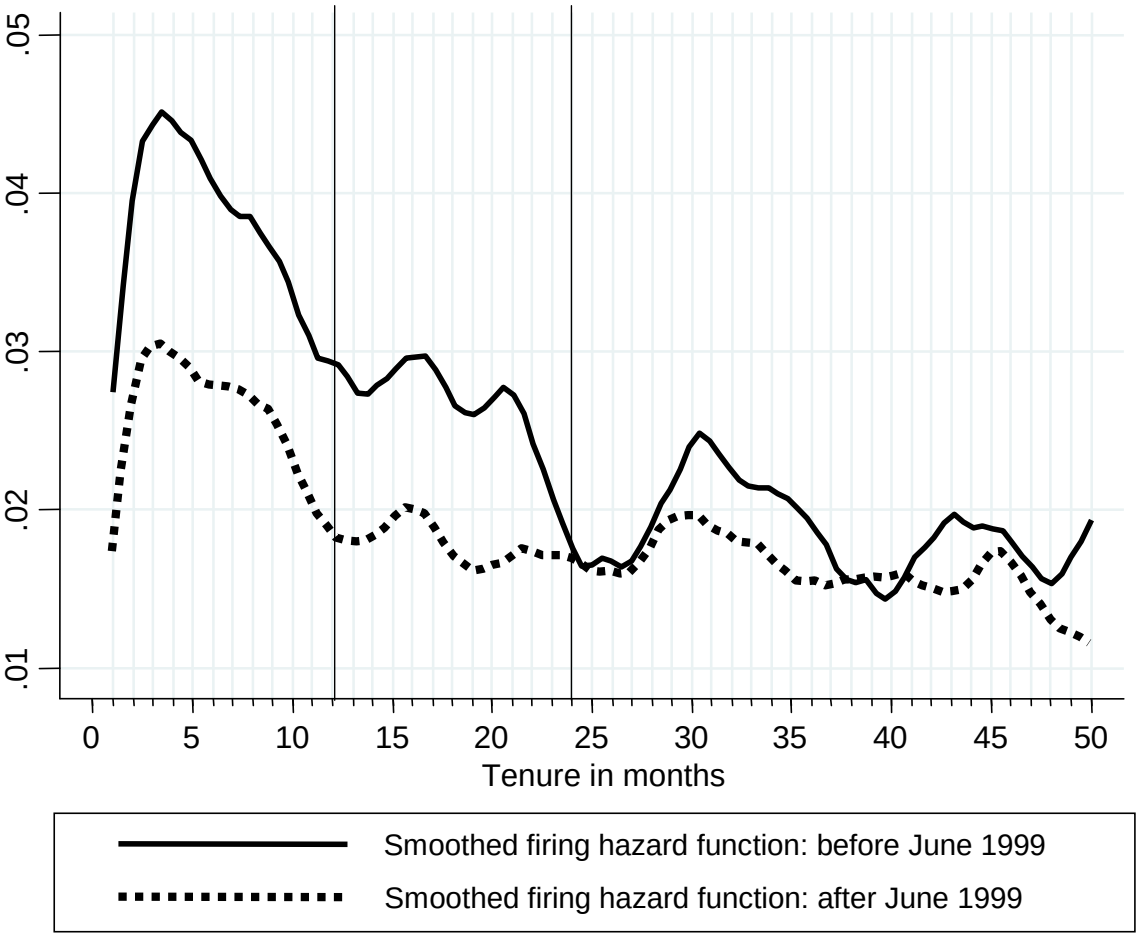
Notes: The coefficients reported are the interactions between tenure categories and "after". Cox proportional hazard models are used.
Robust standard errors clustered by person in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%.
In panel A, the control group has 24 to 48 months tenure, while in panel B the control group has 26 to 48 months tenure.
All regressions include interactions between a dummy for tenure greater than 49 months and the "after" dummy.
Regressions with controls include the following additional controls: unemployment rate, female dummy, white dummy, married and cohabiting dummy, age dummies (one by decade), 2 education dummies, 8 occupational dummies and the interaction of those with year dummies, private sector dummy, 9 industry dummies and the interaction of those with year dummies, 11 region dummies, quarter-year dummies.
The sample is restricted to persons who are employed in the first quarter, in a permanent job, and usually working 16 or more hours a week. Only the first observation for each job (as defined by the hiring date) is kept.
Source: Labour Force Survey Two-Quarter Longitudinal Dataset (www.data-archive.ac.uk). For the unemployment rate: UK National Statistics, Time Series data [NS TSD], Regional claimant count rate, non seasonally adjusted, series code EGU4.

Table 3: Impact of the reform on the firing hazard by gender, age and education

	A. Gender		
	Males	Females	
0 to 11 months tenure	-0.180	-0.199	
	(0.081)**	(0.116)*	
12 to 23 months tenure	-0.243	-0.284	
	(0.094)***	(0.133)**	
Number of observations	232610	197994	
	B. Age		
	Age<40	Age>=40	
0 to 11 months tenure	-0.223	-0.124	
	(0.085)***	(0.111)	
12 to 23 months tenure	-0.331	-0.154	
	(0.098)***	(0.124)	
Number of observations	226339	204265	
	C. Education		
	Less than high school	High school but less than college	University/College educated
0 to 11 months tenure	-0.265	-0.229	0.129
	(0.118)**	(0.095)**	(0.162)
12 to 23 months tenure	-0.235	-0.376	-0.072
	(0.137)*	(0.112)***	(0.167)
Number of observations	106140	204283	120181

Notes: The coefficients reported are the interactions between tenure categories and the "after June 1999" dummy. Cox proportional hazard models are used.
Robust standard errors clustered by person in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%.
The control group is 24 to 48 months tenure.
All regressions include interactions between a dummy for tenure greater than 49 months and the "after" dummy, as well as the following additional controls: unemployment rate, white dummy, married and cohabiting dummy, age dummies (one by decade), 8 occupational dummies and the interaction of those with year dummies, private sector dummy, 9 industry dummies and the interaction of those with year dummies, 11 region dummies, quarter-year dummies. Regressions in panels A and B include 2 education dummies. Regressions in panels B and C also include a female dummy.
The sample is restricted to persons who are employed in the first quarter, in a permanent job, and usually working 16 or more hours a week. Only the first observation for each job (as defined by the hiring date) is kept.
Source: Labour Force Survey Two-Quarter Longitudinal Dataset (www.data-archive.ac.uk). For the unemployment rate: UK National Statistics, Time Series data [NS TSD], Regional claimant count rate, non seasonally adjusted, series code EGU4.

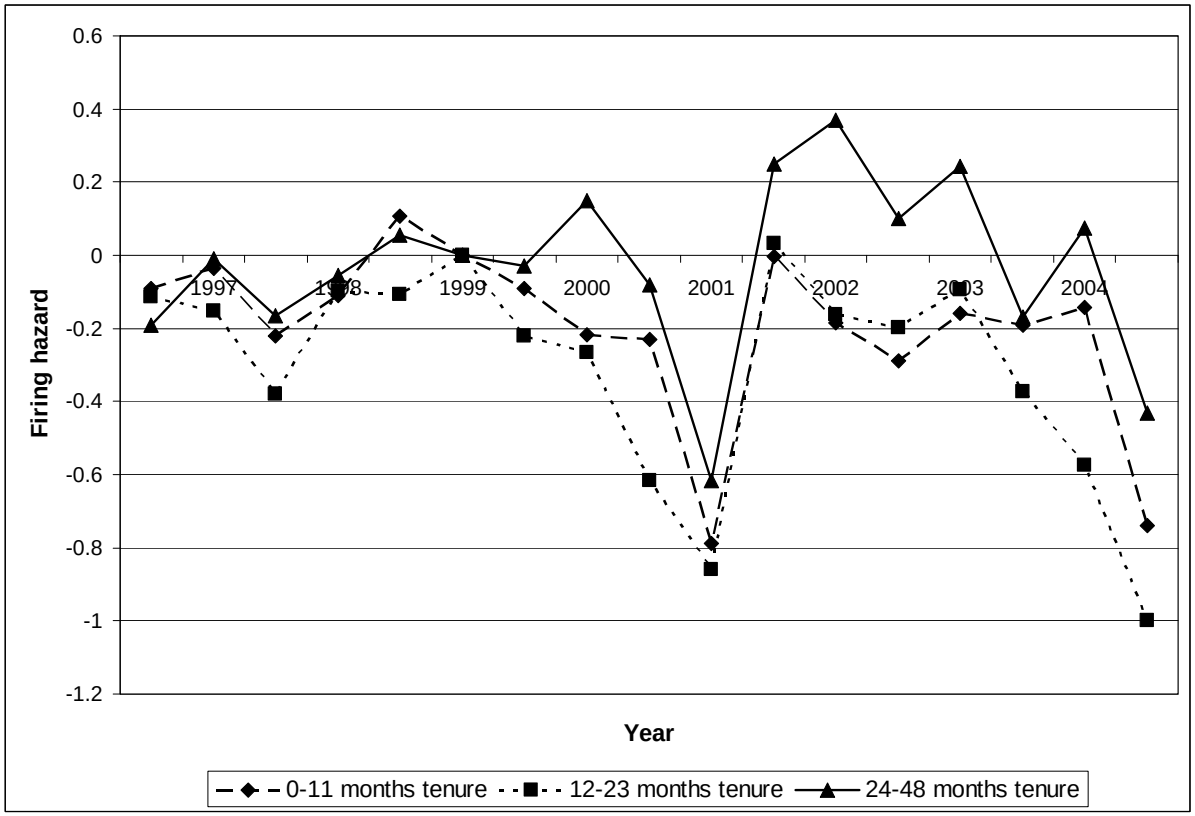
Figure 1: Kaplan-Meier estimates of the firing hazard before and after the reform



Notes: The figure plots smoothed non-parametric Kaplan-Meier firing hazard estimates. Firing is defined as dismissing or making redundant a worker. The sample is restricted to persons who are employed in the first quarter, in a permanent job, and usually working 16 or more hours a week. Only the first observation for each person is kept.

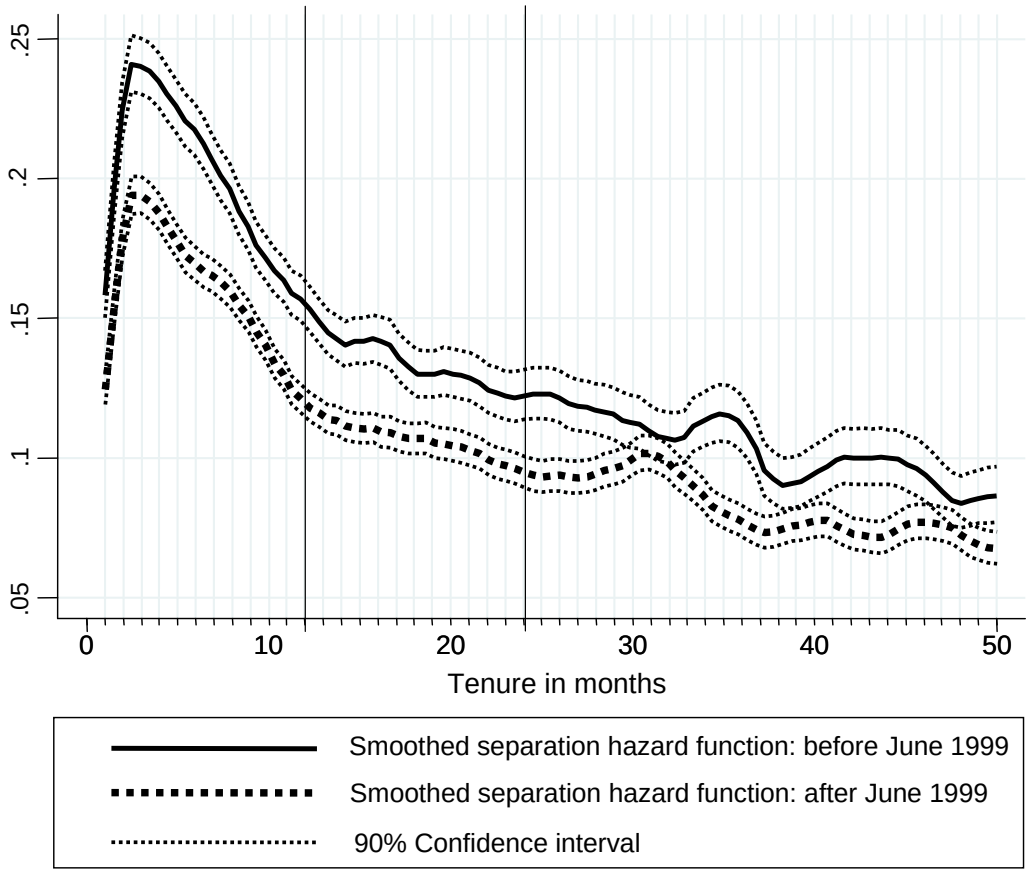
Source: Labour Force Survey Two-Quarter Longitudinal Dataset (www.data-archive.ac.uk).

Figure 2: The effect of the reform on firing hazards: timing



Notes: The series plotted are the coefficients on the interactions between tenure categories and half-yearly dummies in a Cox proportional hazard model. The excluded period is the first half of 1996, and coefficients are normalized to 0 in the first half of 1999.
The regression includes the interaction of a dummy for tenures greater than 49 months with half-yearly dummies. The regression further includes the following controls: unemployment rate, female dummy, white dummy, married and cohabiting dummy, age dummies (one by decade), 2 education dummies, 8 occupational dummies, private sector dummy, 9 industry dummies, 11 region dummies.
The sample is restricted to persons who are employed in the first quarter, in a permanent job, and usually working 16 or more hours a week. Only the first observation for each job (as defined by the hiring date) is kept.
Source: Labour Force Survey Two-Quarter Longitudinal Dataset (www.data-archive.ac.uk).

Figure 3: Kaplan-Meier estimates of the separation hazard before and after the reform



Notes: same as in Figure 1, except that the failure event here is any job separation, instead of dismissals or redundancies only.