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JOB SECURITY LEGISLATION AND JOB DURATION: EVIDENCE FROM THE UK

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JOB SECURITY LEGISLATION AND JOB DURATION: EVIDENCE FROM THE U.K.[1]

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Abstract:

Job security legislation in most countries causes firing costs to rise with job duration. I analyze a 1999 British policy change that lowered from two to one year the tenure necessary for a worker to be able to sue their employer for unfair dismissal. Empirical results show a 29% decrease in the firing hazard for workers with one to two years of tenure relative to workers with higher tenure. The firing hazard for workers with zero to one year of tenure also decreased, which is consistent with better recruitment. Unemployment duration decreased after the policy change, training increased, and wages were unaffected.

Keywords: Firing costs, separation hazard rate, learning, job tenure

JEL codes: J24, J41, J63, J64, J65, J83

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I. Introduction

US "employment at will doctrine" – the right for employers to dismiss workers whenever they want and for whichever reason, i.e. "at will" – is often contrasted with European job security legislation, also known as employment protection legislation. Job security legislation is often portrayed as one of the causes of high unemployment in Europe (e.g. Lazear, 1990). However, the difference between US and European job security legislation is not quite as stark as it would seem at first glance. In the US, there exist both state-level exceptions and federal statutes, such as anti-discrimination laws, limiting the ability of employers to fire at will. On the other hand, the right not to be unfairly dismissed, introduced in most western European countries in the early 1970's, usually requires that employees have a minimal period of continuous employment to fully qualify for this right[2].

This paper uses a British policy change to identify the impact of job security legislation on worker flows, wages, and training. In the U.K., on June 1st 1999, the number of months necessary to qualify for protection against unfair dismissal was lowered from 24 to 12 months for any termination resulting from dismissal or redundancy[3]. This change amounts, all other things equal, to an increase in the expected firing costs born by firms. Because firing costs tend to reduce both firing and hiring, the impact of such an increase in firing costs on employment is ambiguous. This paper starts with analyzing the impact of the policy change on firing using a difference-in-differences strategy. The group chiefly affected by this policy change is employees with 12 to 23 months of tenure: before this policy change, they had no

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[2] For example, in France, while employees on unlimited term contracts (CDI) can always sue for unfair dismissal, they are only legally entitled to a minimum compensation for unfair dismissal if they have 2 or more years of tenure. In the United Kingdom, the qualifying period is strict: employees cannot sue their employer for unfair dismissal if they have less than the minimum required tenure.

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

right to claim unfair dismissal, but after the policy change they could make such a claim and receive compensation. We therefore expect a lower probability of firing for workers with 12 to 23 months of tenure after the policy change. Employees with more than 24 months of tenure, in principle, were relatively unaffected by the policy change, and could be used as a control group. Employees with less than 12 months tenure are considered as treated. Indeed, they may be 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. Firms could also react to the policy change by monitoring employees more closely during the first months. A model of job separation similar to Jovanovic's (1979) shows 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 U.K. Labour Force Survey longitudinal datasets. The Kaplan-Meier raw hazard reveals that the firing rate is indeed lower after the policy change for employees with 12 to 24 months of tenure. It is also lower for employees with 0 to 12 months of tenure, which is consistent with firms having increased their recruitment efforts after the policy change. Calibrating the model to fit these Kaplan-Meier estimates, I show that recruitment efforts must have indeed increased substantially after the policy change, while monitoring on the job may also have increased slightly. Using employees with 26 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 about 30% both for employees with 12 to 24 months of tenure and for employees with 0 to 12 months of tenure. Prior to the policy change, the 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 university educated workers. After the policy change, firms do not seem to increase recruitment efforts targeted at university educated workers; instead, there is evidence consistent with a moderate increase in monitoring efforts.

I next look at the effects of the policy change on the duration of unemployment, wages, and training. To identify the impact of the policy change on unemployment, I use the fact that only full time workers are protected against unfair dismissal. I find that the policy change was associated with a decrease in unemployment duration for workers looking for full-time jobs. Looking at 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. The policy change had no significant effect on wages. Finally, workers with 0 to 11 months tenure are significantly more likely to get training. The increase in training is small, but consistent with an increase in match quality stemming from better recruitment and monitoring.

Closely related to this paper are Bauer, Bender and Bonin (2004) and Kugler and Pica (2006). Using a difference-in-differences approach, Bauer, Bender and Bonin (2004) 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, Kugler and Pica (2006) 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. By using this source of variation, I can assess the impact of firing costs on the whole economy, and not only on small firms. Overall, my findings thus confirm the results from the small quasi-experimental literature suggesting that strengthening employment protection in Europe does not have any negative impact on employment.

The rest of this paper is organized as follows. Section II briefly reviews the related literature and gives some further institutional background. Section III presents the theoretical hypotheses to be tested, drawing on a model of learning about match quality. Section IV 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 the labor force. Section V analyzes the impact of the policy change on wages, training and the duration of unemployment. Section VI concludes.

II. Background and related literature

A large and well-established body of literature relates firing costs and employment across countries (Djankov et al., 2004) or across countries and time (Lazear, 1990, OECD, 1999, Heckman and Pagès, 2003, Nickell, Nunziata, Ochel, 2005), typically yielding inconclusive results. Pierre and Scarpetta (2004), while still relying on cross-sectional variation, use micro-data on firms. They show that firms in countries with more stringent employment regulations report being more hindered by these regulations, and that firms react to more stringent regulations by providing more training and resorting more to temporary employment. Although very valuable, such cross-sectional evidence may still be plagued by omitted variable biases, in as much as there are many unobservable country-specific factors that may be correlated with both firing regulations and firms' characteristics and behavior

In recent years, several studies have used micro data to assess the consequences of changes in the regulation for one given country. Using regional and temporal variation for the U.S., Autor, Donohue and Schwab (2006) find a negative impact of one wrongful discharge doctrine, the implied-contract exception, on states' employment-to-population ratios. The implied-contract exception arises in some US states when, through words or actions, an employer implicitly promises not to terminate a worker without a good cause; this can thus be seen as a privately granted right not to be unfairly dismissed. However the negative employment impact of the implied contract exception becomes insignificant after six years, and the policy has no effect on firms' productivity (Autor, Kerr and Kugler, 2007).

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, combining the best of the employment at will doctrine and job security. Indeed, on the one hand, firing costs may reduce the burden of economic downturns by making firms internalize the social costs of firing. Moreover, firing costs can increase productivity either by resulting in better job matching or by 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).

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. This rule 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. France went even further by creating a permanent employment contract (the "CNE", "Contrat Nouvelles Embauches") that allows small firms to benefit from a 2 years probationary period during which employment is almost at-will[4].

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

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[4] In 2006, the French government proposed to extend the CNE to young workers, allowing all firms to hire employees below 26 years old under a CNE type contract named the CPE ("Contrat Premier Emploi", i.e. first job contract). Due to historical street protests, the CPE was not implemented. The German government led by Angela Merkel also plans to increase the probationary period from 6 months to 2 years, but the law has not yet been enacted.

[5] 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 (2006) for more details).

[6] "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."

Finally, one should 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, and I will be correcting for this when relevant. 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). These regulations mainly affect women, so it will be crucial to check whether estimated effects are driven by the female labor force. 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. Thus, while the regulatory activity had been intense at the time of the policy change concerning the qualifying period for unfair dismissal, it seems feasible to identify its independent effects.

III. Theory

The right to claim unfair dismissal introduces a discontinuity in the cost of firing as a function of tenure on the job: when tenure becomes larger than the qualifying period, firing

[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. These features also depend on the tenure of the employee on the job, or more precisely continuous employment. The notice period is at least 1 week for more than 1 month and up to 2 years tenure, and at least 2 weeks for more than 2 years tenure, plus one additional week's notice for each further complete year of continuous employment for a period of less than 12 years' continuous employment; and at least 12 weeks' notice if the employee has been employed by the employer continuously for 12 years or more. Redundancy pay is only granted after two years of continuous employment and if the employee was fired for economic reasons. These features of employment law did not change in 1999, so it is important to bear in mind that the two years tenure may still be a meaningful juncture affecting firms' firing policies.

costs are suddenly augmented by the expected costs to the firm of possible unfair dismissal claims. The model I use is based on firm's learning about match quality, a hypothesis whose implications were first formally derived by Jovanovic (1979). The model was shown to be useful in explaining empirical separation hazards. Thus Farber (1994) empirically verifies Jovanovic's prediction about the relationship between tenure and separations. Using the National Longitudinal Survey of Youth, he shows that the monthly hazard of job separation initially increases with time spent on the job, peaks at 3 months, and decreases thereafter. Nagypal (2004) finds learning to be a driving factor of the empirical job separation hazard.

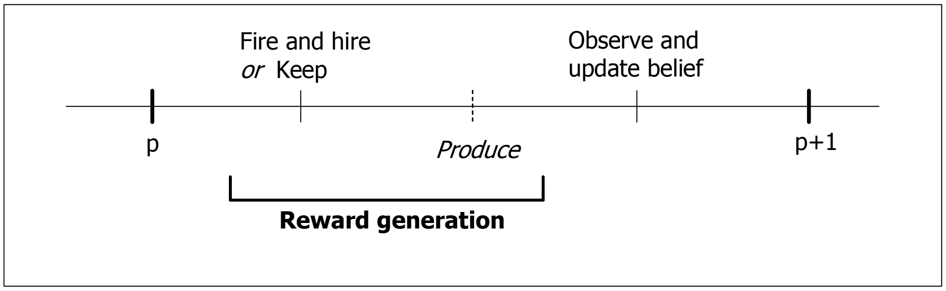
In what follows, I use a model based on dynamic programming further developed in Marinescu (2007) to form testable hypotheses regarding the possible effects of a shortening of the qualifying period on the hazard of firing. The model's aim is to derive the firing hazard stemming from firms' optimal firing behavior in response to a set of parameters among which figures crucially the firing (and hiring) cost. The model necessarily involves many simplifications relative to actual firms' firing behavior. I defer a discussion of the model's limitations to Section C.

A. Assumptions

When a firm and a worker begin their employment relationship they do not perfectly know their match quality[8] but learn about it over time. The worker is assumed to be passive in this model: the firm alone makes separation decisions.

The timing of events within each period p is formalized as follows:

[8] In what follows, I use the term "match quality", which given the literature usage suggests that match quality is idiosyncratic. However, as explained in section III.C, I do not need for the purpose of this model to take a stance with respect to whether match quality is indeed idiosyncratic. Therefore, I could just as well use the term "worker quality" rather than "match quality".



The *set of possible actions* the firm can take is “fire the current worker and hire a new one”, or “keep the current worker”. Therefore, unemployment or the overall level of labor demand are not modeled. The *state of the world* is defined by a vector of two variables: the tenure of the current worker, and the quality of the firm-worker match. Match quality can be either good or bad[9]. I assume that a proportion q of the matches is good whereas a proportion $1-q$ is bad.

Unlike tenure, match quality is not perfectly observed. At each period, the firm observes a normally distributed signal about the quality of the match. The signal for a good match is normally distributed with mean 1 and variance σ^2, whereas for a bad match it is normally distributed with mean -1 and variance σ^2. The belief of the firm that the match is good can be written $b(s,t)$ where s is the sum of all past signals and t is the tenure. Given the quality of the match, the expected value of s after t periods is described by a normal distribution. Using Bayes' rule, one can then compute all possible beliefs $b(s,t)$ (see appendix 1 for the equation).

I can now specify the value as a function of the current belief. As in Jovanovic (1979), I assume that the firm only employs labor and has constant returns to scale. The actual per period return to a good match is 1 whereas the per period return to a bad match is 0.

[9] This simplifying assumption allows me to keep the model intuitive. However, assuming a normal distribution of match quality - as in Jovanovic (1979) - leads to the same qualitative results (see Marinescu, 2007) as those described here.

Moreover, the wage is fixed and set to 0[10]. So if the firm keeps the worker, its expected return will be exactly $b(s,t)$. If the firm fires the worker, it gets the expected value of a new worker and incurs a separation (hiring and firing) cost $c(t)$ which is a function of the tenure t of the current worker.

Let $V^*(b(s,t))$ be the value (i.e. the expected discounted future reward) of the match to the firm obtained when the firm follows the optimal policy.

The value of a worker to the firm if the firm keeps this worker (action K) is given by:

$$
\begin{aligned}
(1) \qquad V(b(s,t),K) = \quad & b(s,t) + \\
& \delta.\{(1-b(s,t)) \int_{-\infty}^{+\infty} f_b(s')*V^*(b(s',t+1))ds' + \\
& b(s,t) \int_{-\infty}^{+\infty} f_g(s')*V^*(b(s',t+1))ds'\}
\end{aligned}
$$

The first line of equation 1 represents the immediate reward for keeping the worker, whereas the two following lines represent future rewards if keeping the worker at the current period, and are thus preceded by the discount factor δ. The second line represents the future rewards if the match is bad weighted by the corresponding belief $1-b(s,t)$, whereas the third line represents the future rewards if the match is good weighted by the corresponding belief $b(s,t)$. For each of the two possible match qualities, the belief at the next period depends on the sum of signals s' that the firm will have observed by tenure $t+1$, or equivalently on the signal at period $t+1$. Given my assumptions, if real match quality is bad and the sum of observations is s (line 2 of equation 1), the probability of reaching a given s' is given by a normal distribution f_b with mean $s-1*(1-b(s,t))$ and variance σ^2 (remember that the mean of the per period signal for the low quality match is -1). A symmetric reasoning applies if the match is good and gives rise to line 3 of equation 1.

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[10] One can also readily specify the wage to be a fixed share of the expected per period return, as would be the case with Nash bargaining. Qualitative results do not change when making this assumption.

Alternatively, if the firm fires the worker (action F), the value is:

$$V(b(s,t),F) = V_{new} - c(t) \tag{2}$$

i.e. it is the value of a new worker minus the firing costs. Note that the value if fire only depends on the tenure due to the existence of tenure-dependent firing costs.

Given the values for keep and fire, the optimal value is given by the Bellman equation:

$$V^*(b(s,t)) = \max(V(b(s,t),K), V(b(s,t),F)) \tag{3}$$

A version of the value iteration algorithm was implemented in Matlab to compute the optimal policy of the firm. The policy can be expressed as a belief threshold $\tau(t)$ for each tenure t such that if the firm's belief is equal to or above $\tau(t)$, then the firm keeps the worker, and otherwise it fires the worker.

The model so far has described the behavior of a representative firm. The behavior of infinitely many single-job firms can be represented by integrating the behavioral response of the firm over all the possible combinations of tenure t and sum of signals s, given the assumed distributions. Thus, under the assumptions I use, it is possible to compute the firing hazard (see Appendix 1 for the equation).

B. Impact of the 1999 policy change

I choose a benchmark case[11] for clarity of exposition. The parameters were chosen so that the shape of the hazard curve is similar to the hazard of firing observed in the United Kingdom in 1996-1999 prior to the policy change (shown in Figure IV).

Let us first consider the case where the firing cost does not vary with tenure but is instead fixed at 7. The firing hazard is plotted in Figure I. It is first increasing and then decreasing in tenure, as in Jovanovic (1979). This is because more and more matches are discovered to be of bad quality as tenure increases. But as firms always dissolve the worst

[11] The parameters for the benchmark case are displayed in Table VIII in Appendix 2.

quality matches, eventually a large proportion of continuing matches will actually be good and so there will be very few workers for whom the belief can fall below the firing threshold. This is why the firing hazard eventually decreases.

To illustrate the effect of the introduction of a probationary period, I assume that at tenure 24, the firing cost goes from 7 to 9. This only affects the firing hazard through the firing threshold. With a higher firing cost after 24 months, the threshold decreases for tenures greater than 24 months, i.e. as firing is more expensive, firms keep workers with lower believed match quality. What happens to the threshold before the end of the probationary period? First, at low tenure, the threshold for firing is the same as in the absence of a probationary period. This means that the hazard will also be the same at low tenure, as seen in Figure I. Then, as tenure increases, firms anticipate that there will be a higher firing cost in the near future, so they *increase* their threshold before the end of the probationary period, thus firing preventively a group of workers whose match quality is fairly low and who would otherwise be likely to get fired at higher cost after the end of the probationary period. This is what creates the spike[12] and the trough in the firing hazard with 24-months probationary period seen in Figure I: indeed, right before the end of the probationary period, more workers get fired because of the higher firing threshold, whereas right after the end of the probationary period, fewer workers get fired because the threshold is lower *and* those who were most likely to fall below it have been fired preventively. If the firing cost increases earlier, i.e. at tenure 12, the spike and trough in the firing hazard will occur around 12 months, while there will be little effect on the firing hazard at high tenures (Figure I).

This analysis however does not take into account the fact that firms could be endogenously reacting to the shortening of the qualifying period by increasing the quality q

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[12] Note that the existence of two peaks in the firing hazard in Figure I is an artifact of discretization, i.e. approximating continuous distributions by a finite number of points. In fact, there is only one peak before the end of the probationary period, and the hazard increases smoothly before that.

of matches when hiring, or by increasing the intensity of monitoring on the job and thus decreasing σ^2. Intuitively, both strategies would reduce the probability that firms should have to fire after the end of the probationary period. To probe whether changing the quality of recruitment or monitoring after the policy change could be an optimal response, I examine the impact of such changes on the value of a new match to the firm. Starting from the reference case, the marginal gain (as measured by the change in the value of a new worker) of increasing either recruitment or monitoring intensity is larger in the 12 months compared to the 24 months probationary period case. This implies that, for a given marginal cost of these technologies, firms should be more willing to invest in them after the policy change.

I now study the effects on the firing hazard of increasing the recruitment quality q from .5 to .7 or increasing the monitoring intensity, i.e. decreasing σ^2 from 16 to 4. The corresponding curves are plotted in Figure II. An increase in recruitment quality results in a decrease in firing at all tenures. Intuitively, this is explained by the fact that there are now fewer bad matches waiting to be dissolved. By contrast, an increase in monitoring results in an increase in firing at low tenures and a decrease in firing at high tenures (Figure II). This is because, with better signals, firms can find out quicker which matches are bad, and so the firing hazard is higher at low tenures. But eventually, as firms can quickly get rid of bad matches, the hazard of firing gets lower. Thus, both increasing the recruitment effort and the monitoring intensity decrease the hazard of firing after the probationary period but they have opposite effects on firing at low tenure (i.e. for tenures between 0 and a few months) : while an increase in recruitment effort decreases firing at low tenure, an increase in monitoring increases it.

C. Limits to the model

The model developed above does not define an explicit cost to the firm of increasing recruitment efforts or monitoring. In reality, these efforts are of course costly and the reduction in uncertainty and increase in match quality will only be obtained if cost-effective. Note however that the costs of these efforts can be viewed as part of the separation cost if assumed to be a fixed cost per match.

A more important limitation of the model is that it relies on partial equilibrium analysis. Thus, I am not modeling the influence of the behavior of one firm on other firms' behavior, nor the aggregate demand for labor. Therefore, I do not need to take a stance with respect to whether match quality is in fact idiosyncratic (Jovanovic, 1979) or whether there is some symmetric (Gibbons, Katz, Lemieux, and Parent, 2005, Moffitt and Jovanovic 1990) or asymmetric learning about general ability (Gibbons and Katz, 1991, Schoneberg, 2004). Nevertheless, the nature of the information imperfection about match quality may have important effects when evaluating the overall efficiency and welfare effects of a change in firing costs. For example, if firing costs increase and there is asymmetric learning about quality, then all else equal, the average quality of terminated matches diminishes, implying that terminated workers have lower reemployment probabilities. However, in this model I am focusing on what drives firms' firing behavior, and it is only when looking at other outcomes such as unemployment duration in the empirical analysis that I will briefly consider the implications of different possible hypotheses about match quality.

D. Main conclusions drawn from the model

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 other parameters such as q or σ^2 change 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. The firing hazard for workers with 12 to 24 months tenure always decreases after the policy change. In all cases, the hazard for workers with more than 24 months tenure remains at similar levels (see Figure II). This implies that workers with more than 24 months tenure should form a reasonable if imperfect control group.

IV. The impact of the policy change on the firing hazard

The focus of this paper, the 1999 policy change, occurs during a phase of steadily growing employment in the UK (Figure III), 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 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. <u>Data</u>

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[13] remains in the sample for 5 quarters. This paper uses the 2-quarters Labour Force survey longitudinal datasets[14] from March 1996 to September 2004. These datasets are put together by the UK Office of National Statistics and they contain all occurrences of individuals in the LFS being observed in two consecutive quarters.

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[13] Households in the sample are identified by their addresses so people who move during the survey drop out of the sample.

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

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 main sample[15] to those employees. 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 group would not be as instructive as for permanent workers. I therefore perform the analysis on the permanent workers only[16].

Having defined the relevant group of workers, I also have to compute their tenure[17]. The date of hiring is present for more than 99% of currently employed workers along with the date of the interview. In most 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, 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, the date when they left their last job is not known, so it has to be imputed[18].

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[15] A different sample will be used to study the duration of unemployment.

[16] I performed the analysis of the impact of the reform on employees on temporary jobs, i.e. fixed term contracts, seasonal work and agencies, and found that there is no impact of the reform (results not reproduced here).

[17] 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.

[18] 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 in order to conform with the distribution of completed unemployment spells. Using the hiring date workers provided in the first quarter of

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. Second, the follow-up also causes a small problem if a job begun *and* ended during the 3-months period between two interviews. In that case, I make a wrong inference about which job was left and when: indeed, I will be assuming that the job left by the second quarter was the job observed at the first quarter, whereas in fact it was another short job that followed in the meantime. However, this problem does not seem to be very important in practice[19].

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 a given type of separation, I treat other types of separations as censoring. In this section, I mainly focus on workers who were fired, i.e. dismissed or made redundant, since they are the ones directly affected by the law.

To summarize, the main sample consists of employees in permanent jobs usually working more than 16 hours per week and having a known tenure. Table I gives summary statistics for the sample used. Note that among the reasons given by workers for leaving their

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observation and the date when they left their job or the imputation thereof, I can thus compute their tenure in months at the moment of termination.

[19] To document the prevalence of such a problem, I compare 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.

last job, dismissals and redundancies represent a sizeable 21.7%, a proportion comparable to the "other" category (22.4%) but lower that quits (35.6%). 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[20]. I now focus on workers who were fired (i.e. dismissals and redundancies).

B. A Kaplan-Meier estimate of the hazard of firing

I plot the non-parametric Kaplan-Meier estimate of the hazard of firing before and after June 1999 (Figure IV). Like Farber (1994), I find a pattern consistent with Jovanovic's 1979 model, and the model developed in section III. 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 (see Appendix 2 Figure VII). 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. The model developed in section III suggests that the observed difference between younger and older workers' firing hazard can be explained by higher firing and hiring costs for older workers, or by a greater uncertainty surrounding older workers' performance (maybe because older workers are involved in tasks that are more difficult to monitor).

Figure IV shows that the shape of the hazard function in the "before" period is very similar to the theoretical hazard curve corresponding to a 24 months probationary period in Figure II: 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

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[20] It is somewhat puzzling that the end of a temporary job is a reason quoted by 3.4% 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.

policy change. 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 [0,12], which indicates that it is likely that the quality of recruitment has increased. Note that while there is no observable change in the firing hazard for the 24 to 48 months tenure group, this does not necessarily contradict the model's predictions in the case of an increase in recruitment effort. Indeed, the decrease in the firing hazard for the 24 to 48 months tenure group engendered by an increase in recruitment quality is likely to be very small (see section III and Figure II).

To build quantitative intuition about the effects of the policy change on the firing hazard, I calibrate the model in section III. The calibration procedure looks for the parameters of the model that minimize the sum of the squared differences between the theoretical and the empirically estimated firing hazard curves[21]. The fixed parameters in the model are the same as in section III. The results of the calibration exercise are shown in Table II. In the first column are parameters inferred from the "before" hazard. In the second and third column are parameters inferred from the "after" hazard. The difference between columns 2 and 3 is that in column 2 firing costs are constrained to be the same as in column 1, while in column 3 they are allowed to adjust to reflect potential increases in hiring costs. Comparing columns 1 and 2, we see that the initial proportion of good matches increased from 43% to 63%, which is a 52% increase. Moreover, monitoring intensity must also have slightly increased as the standard error decreased by 18%. The calibration thus confirms the increase in recruitment effort after the policy change, and suggests that monitoring quality has also improved.

[21] It uses the Matlab function fminsearch to do so. Note moreover that I decide to calibrate the model to best fit the 36 first months of the empirical hazard function in the case of a 24 months probationary period, and the 24 first months of the empirical hazard function in the case of a 12 months probationary period. The model is indeed inadequate at explaining firing hazards at high tenure for structural reasons, and so imposing that the model should fit the firing hazard at high tenure uselessly damages the quality of the fit at low tenure. Indeed, the theoretical firing hazard decreases very fast to 0 for high tenures, as almost all bad matches have been dissolved, whereas the empirical hazard remains roughly at the same level beyond 30 months of tenure. This is very likely due to the fact that match quality is not in reality constant over time, as assumed by the model, but good matches may turn bad (see Marinescu (2007) for a model that includes this feature).

Column 3 shows higher firing and hiring cost during the probationary period after the reform, which is consistent with an increase in recruitment costs.

Could the observed firm behavior be optimal? As in the theoretical section, I compare the marginal gains of increasing recruitment quality or monitoring intensity with a 24 months versus a 12 months probationary period. The only difference is that this time I use the calibrated parameters from the hazard before the policy change. I find that decreasing the standard error σ by 1% increases the value of a new worker by 0.000374 more in the 12 months case compared to the 24 months case. Increasing the proportion of good matches q by 1% increases the value of a new worker by 0.0017 more in the 12 months case compared to the 24 months case, an effect that is 5 times larger than the effect of decreasing σ. I conclude that, for given marginal costs of recruitment and monitoring, it is rational for firms to increase recruitment efforts more than monitoring efforts after the policy change.

To probe the plausibility of the calibration results, I look for other evidence about firms' recruitment and monitoring practices. One such piece of evidence is the 2004 Workplace Employment Relations Survey (WERS 2004). Kersley et al. [2005] show that between 1998 and 2004, there has been no substantial change in the use of tests by employers when recruiting employees. Thus, if recruitment efforts are measured as the use of tests, there does not seem to be a substantial increase in recruitment efforts. However, this measure of recruitment efforts seems overly restrictive. Consistent with an increase in monitoring, performance appraisals are more widely used after the policy change: while 73% of employers used them in 1998, 78% did so in 2004. Another source of evidence on employers' reaction to the qualifying period for unfair dismissal is the Blackburn and Hart (2002) report on small firms' (i.e. with more than one but less than 50 employees) awareness and knowledge of individual employment rights. Employers report that unfair dismissal is the most constraining regulation, after the minimum wage and maternity rights. In July-August

2000, 65% of these small employers were aware that there exists a length of service necessary to qualify for unfair dismissal, but their estimates varied between 1 week and 3 years, with a mean at 15 months, which is somewhat higher than the qualifying period prevailing in 2000. Lastly, employers also reported that because of the risk of an unfair dismissal trial, they are taking more care about who they recruit, which is consistent with an increase in recruitment efforts.

Having thus examined the basic patterns of change in the firing hazard, I move on to a more systematic approach, controlling for other variables that may have affected the hazard of firing.

C. Controlling for covariates using a Cox proportional hazard model

To test the robustness of my findings, I estimate a Cox proportional hazard model with delayed entry[22], controlling for essential covariates. The advantage of such a model is that there is no need to specify the functional form of the baseline hazard (Lancaster, 1990). The specification for the hazard of termination is as follows:

(4) $$\lambda(t,Z) = \lambda_0(t)\exp\{\beta' Z(t) + \gamma_0' Treat + \gamma_1' Treat * After\}$$

Z is a set of controls, including the regional monthly unemployment rate and a full set of year dummies[23]. *Treat* is a set of dummies for different ranges of tenure within the treatment group, i.e. employees with less than 25 months of tenure. *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*. The *Treat* dummies measure how the hazard of termination for the

[22] As explained in section IV.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.

[23] This should control for the impact of economic conditions. 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.

treatment group systematically differs from the hazard of termination for the control group. A test of the negative effect of the policy change on the hazard of termination is that the coefficients in the $\gamma_1^{'}$ vector are negative and significant.

Panel A of Table IV presents the results using basic tenure categories for the treated groups, that is 0 to 11 months and 12 to 23 months. 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 27% for workers with 0 to 11 months tenure and by 29% 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 them 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 change in the definition of the policy change period[24], which suggests that the anticipation of the policy change or the 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, 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 does 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

[24] I also used two other definitions of the reform dummy. In one case, I only allowed for an anticipation effect, excluding the period May 1998 to June 1999, and in the other I only allowed for an adaptation effect by excluding June 1999 to May 2000. The results in presented in Table IV are however unaffected by these alternative definitions.

change on the firing hazard is significant for all subgroups up to month 21, and fades away from month 22 to months 25. The effect is of similar magnitude as in panel A, implying a reduction in the firing hazard of about 30 to 40% for all subgroups from month 5 to month 21, with a somewhat smaller effect for the 0 to 4 months tenure group. The fact that the effect is smaller for that very low tenure group was to be expected from the observation of Figure II (compare the "24 months prob. period" curve with the "12 months prob. period, q=.7" curve) and Figure IV. 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 IV).

In Figure V, 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 IV, but no 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. 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 between .3 and .4, suggesting that the policy change decreased firing hazards by 30 to 40%, a magnitude which is consistent with the effect of the policy change as estimated in Table IV.

Finally, I check whether the impact of the policy change on firing hazards is stronger in industries that have higher firing hazards before the policy change. Manufacturing and construction have the highest firing hazards before the policy change, whereas public administration, education and health have the lowest firing hazards. The impact of the policy change on manufacturing and construction is somewhat stronger than the overall impact, even though the impact on construction is not significant. On the other hand, the policy change does not have any impact on the administration, education and health industry. This is thus broadly consistent with the policy change having had a greater impact on industries that are more likely to fire workers.

D. Impact on 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 V examines the effects of the policy change by gender, age and education. Panel A of Table V shows the break-down by gender. Females see a somewhat lower decrease in their firing hazard than men, even though 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 effect on the 0 to 11 months tenure group is roughly the same for old (over 40) and young workers, whereas the effect for 12 to 23 months tenure group is more pronounced for younger workers.

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 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 education, even though the point estimate of the decrease in the firing hazard for university educated workers with 12 to 23 months tenure is lower and insignificant. Why are university 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 peak in the firing hazard occurs at 7 months, while it occurred at 12 months before the policy change. These results can be explained by calibrating the model in section to fit the Kaplan-Meier estimates of the firing hazard on the sample of college educated workers. The peak in the firing hazard likely occurs later for higher educated workers than for others because these workers are more costly to fire and hire, and harder to monitor: in the "before" period, estimates of firing costs and uncertainty are higher for university educated workers (Table III) than for the whole sample (Table II). However, after the policy change, firms can no longer wait so long before they fire and so the peak in the firing hazard occurs before 12 months after the policy change, consistent with an increase in monitoring effort. Indeed, we see that after the policy change, employers have increased monitoring efforts, with a standard error of the observation process going from 7.6 to 6.9. Recruitment efforts remain roughly the same after the policy change with about 63% of good matches, an estimate that is roughly equal to the full sample estimate *after* the policy change. In other terms, university/college educated workers were already recruited with much more care before the policy change. These findings altogether may explain why the WERS 2004 survey shows no evidence fore an increase in the use of tests for recruitment but does find an increase in the use of performance appraisals. Indeed, if tests and performance appraisals are mainly used for the more qualified workers, then these findings are consistent with the absence of change in recruitment efforts and increase in monitoring efforts found for the higher 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 females, younger and less educated workers are those who see the greatest reduction in their firing hazards. Moreover, heterogeneity in underlying parameters such as firing and hiring costs and the observability of performance does seem to be important, especially when considering different levels of education: thus, the policy change has a different impact on the most educated workers when compared to other educational groups.

E. Impact on other separation hazards

To place firing in the context of other types of separation, I examine the hazard of any job separation after the policy change (Figure VI). 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 see a trough in separations at around 24 months in the "before" period, and the hazards before and after become insignificantly different at tenure 30, and not tenure 24. 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. Moreover, to evaluate the global effect of the policy change, it is interesting to note that while the firing hazard decreases, it is not the case that other types of separations increase at the same time so much as to imply no change in the overall separation hazard. In fact, the separation hazard is lower after the policy change.

While the firing hazard has decreased after the policy change, it is possible that firms have forced some workers to quit in order to avoid firing costs. If so, overall involuntary separations did not necessarily decrease after the policy change. However, Figure VII in Appendix 2 shows that the quit hazard did not increase after the policy change. My findings are therefore not driven by shifts from firings to quits. I next look at the impact of the policy change on other key labor market outcomes such as unemployment duration, training and wages.

V. Impact on other labor market outcomes: unemployment duration, training and wages

To better evaluate the overall welfare effect of the policy change, one should look, beyond the effect on firing, at other positive or negative effects of the policy change. In particular, it is essential to look at unemployment duration since theory predicts that with higher expected firing costs, one should see higher unemployment duration, and an increase in recruitment effort would only reinforce this effect.

However, the theory developed in section III does not directly generate predictions concerning the effects of the policy change on labor market outcomes such as training, wages or unemployment duration. Indeed, that theory only applies to firing decisions taken by the firm. I will therefore have to use theoretical insights from other models of relevance in each particular case. However, because of the lack of appropriate theory and data, it is typically hard to find good control groups, and therefore estimates should be taken with caution.

A. Impact on unemployment duration

There are three reasons why unemployment duration may increase after the policy change. First, if expected firing costs increase, then labor demand may decrease, leading to higher unemployment duration. Second, firms' increased recruitment efforts could imply that it takes longer to pre-screen workers and so unemployment duration should increase. Third, if match quality is not purely idiosyncratic but is also determined by general ability, and if moreover the current employer is better informed about the worker's general ability than the market, then a worker getting fired under higher firing costs sends a worse signal to the market. This would imply that workers fired between 1 and 2 years tenure after the policy change should all other things equal have higher unemployment durations than workers fired between 1 and 2 years tenure before the policy change[25].

Table VI tries to identify the effects of the policy change on unemployment duration. I use a sample of unemployed individuals in the sense of the International Labour Organization (ILO) from the same dataset I used for the employed. Summary statistics for this sample are provided in appendix 2, Table IX. In order to shed light on the impact of a policy change targeted at youth[26], such as the abandoned French CPE, I allow the impact on workers who are below 26 to be different. To identify the effect of the policy change on the duration of unemployment, I use two strategies. First, in panel A of Table VI, I look at the probability of finding a permanent job with more than 16 hours a week (i.e. a treated job) after the policy change. Overall, the policy change is associated with a significant 11% increase in the probability of exiting unemployment towards a treated job. This may be related to the fact that the proportion of permanent jobs among jobs with 16 or more hours a week steadily increases, just as much before as after the June 1999 policy change (not shown). Therefore, it does not seem that the policy change incited employers to massively substitute away from full-time permanent jobs. On the other hand, when we look at the impact on workers below 26, the policy change had a less positive impact: the probability of their exiting unemployment has only increased by 1% (=11%-10%) after the policy change.

A second strategy I use in panel B of Table VI is to look at the exit towards any job and use the difference between those looking for full-time jobs and the others. Because the unfair dismissal provisions only apply to full-time jobs, we expect that workers looking for

[25] Unfortunately, for lack of a long enough follow up period, it is not possible to properly test this specific hypothesis.

[26] Such policies are supported by the fact that young workers have less labor market experience, which makes them riskier hires in as much as their ability is less well known to the market.

full-time jobs take longer to find a job relative to other unemployed workers[27]. I find that, overall, workers looking for a full-time job are 8.6% more likely to exit unemployment after the policy change[28]. On the other hand, consistent with the results from Panel A, we find that workers below 26 have benefited less, since they are only 3% more likely to exit unemployment after the policy change.

Thus, while all workers are more likely to exit unemployment after the policy change, there is some evidence of a negative effect of the policy change for workers under 26 compared to older workers. In conclusion, the policy change has no discernable net negative effect on the duration of unemployment or on the relative supply of permanent jobs with more than 16 hours a week, implying that any negative effects have been overpowered by positive ones.

B. Impact on wages and training

Theoretically, higher firing costs may increase or decrease wages. A first strand of theory argues that higher firing costs give a higher bargaining power to employed workers and so wages increase (Lindbeck and Snower, 2001). A second strand of theory argues that since workers value job security, they should accept lower wages (Summers, 1989). Since workers with 12 to 23 months tenure are more expensive to fire after the policy change, this would imply an increase in their wages relative to the 24 to 48 months tenure group under the first theory, and a decrease in wages under the second theory.

Panel A of Table VII shows the effect of the policy change on wages of workers with different tenures: the results show that the policy change did not affect wages. A decrease in

[27] Part-time workers are actually a good control group because since the Part-time Workers (Prevention of Less Favourable Treatment) Regulations 2000, which came into force on July 1st 2000, they have the same rights as full-time workers in most areas, except precisely for this right to claim unfair dismissal.

[28] Note that being part-time or full-time is left by the LFS to the subjective appreciation of the worker. In practice, 37.55% of workers who say they work part-time and are in permanent jobs work 15 hours or less, and 45.45% work 16 or less hours. Therefore, some of the "part-timers" are de facto also affected by the unfair dismissal provision. This means that any negative effect of the reform will be underestimated if one compares workers looking for full-time jobs versus the others.

the wage could be masked by the introduction of a National Minimum Wage on April 1st 1999. However, restricting the sample to workers above the first decile of the wage distribution – as far as the spillovers of the minimum wage go (Low Pay Commission, 2003) – does not alter this conclusion. I therefore conclude that the policy change had no significant effect on wages[29].

Training can be affected in many ways by a probationary period. First, higher firing costs can increase training as it becomes cheaper to train current marginal employees relative to firing them in the hope of hiring more productive employees. Empirically, firms who perceive higher firing costs are also more likely to train their workers (Pierre and Scarpetta, 2004). Moreover, firing costs increase implicit screening costs for all firms, which increases the value of the informational advantage of the current employer. Therefore, the current employer is more likely to provide training (Acemoglu and Pischke, 1998). These two theories imply that employees newly protected by the 1999 policy change, that is employees with 12 to 23 months tenure, should receive more training after the policy change.

The training of employees with 0 to 11 months tenure may also increase, because while the firing cost incurred by firms *if* they fire workers in that tenure range does not change after the policy change, the *expected* firing cost does increase as the probationary period is now shorter. Training in the very beginning of the employment relationship may also be used as a monitoring device, i.e. by training workers, firms may learn more about their ability than otherwise.

[29] The reader may wander at this point what happens to the main findings on the hazard of firing when restricting the sample to workers above the tenth decile of the wage distribution. Once one corrects for the sample selection this entails (in particular for the under-representation of high tenure workers among the observations where wage data is non-missing), the results are unaffected.

The proportion of workers who get training[30] has increased across the board after the policy change (results not reproduced here), consistently with the idea that employers are trying to select for better matches (Cappelli, 2002), or that they train more precisely because they manage to form better matches and the returns to training are increasing in match quality. Panel B of Table VII documents the effect of the policy change on firms' propensity to train their workers at different tenures. Workers with 12 to 23 months tenure do not get more training after the policy change compared to workers with 24 to 48 months tenure, but workers with 0 to 11 months tenure do[31]. The impact of the policy change on training[32], while very small, is consistent with firms having increased their recruitment and monitoring efforts.

VI. Conclusion

This paper showed 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. Unemployment duration did not increase after the policy change, training increased and wages were unaffected. 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 that increase the length of the probationary period, such as the French CNE contract.

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

[30] Training is here « any training in the last four weeks ». Such training is paid for by the employer in a large majority of cases (71%). However, the information on who pays for training is only available for about a fourth of the sample, so I do not use it. The results are less significant but not different if I use only the sample where the information is available and I define training as "training paid for by the employer".
[31] The share of workers with 0 to 11 months tenure receiving training was 28.4% before the policy change and 33.3% after. The corresponding percentages for the control group are 26.1% and 30.4%.
[32] The results about the impact of the reform on training are unaffected if we restrict the sample to workers unaffected by the introduction of the minimum wage.

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 are more careful about whom they hire, they train their workers somewhat more, and they monitor some of their workers better. Lastly, the government thought that one year is enough time for the initial screening of workers: this does not seem to be confirmed by the data, since the policy change prompted firms to change their human resource management policies, precisely to limit the need for firing past one year of tenure.

This paper 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 I: Summary statistics for the sample of permanent full-time employees

	Obs.	Mean	Std.	Min	Max
Macro situation					
Unemployment rate (claimant count)	436867	3.954	1.706	1.5	11.7
Reason for leaving last job					
dismissed	39954	0.030	0.172	0	1
made redundant,voluntary redundancy	39954	0.183	0.389	0	1
temporary job ended	39954	0.034	0.180	0	1
resigned	39954	0.358	0.479	0	1
gave up work for health reasons	39954	0.046	0.209	0	1
took early retirement	39954	0.024	0.153	0	1
retired	39954	0.026	0.160	0	1
family, personal reason	39954	0.074	0.261	0	1
left for some other reason	39954	0.225	0.417	0	1
Job characteristics					
Tenure	436097	98.456	101.866	0	652
Usual hours worked per week	433442	36.596	8.948	16	97
Gross weekly wage in pounds	167695	333.354	282.744	1	44000
Log real hourly wage	166926	-2.633	0.571	-8.792	2.342
Job training	435358	0.287	0.452	0	1
Person characteristics					
Female	436867	0.460	0.498	0	1
Married and cohabiting	436867	0.580	0.494	0	1
Age	436867	38.850	11.566	16	64
Less than high school educated	436771	0.247	0.432	0	1
University educated	436771	0.278	0.448	0	1
Occupation categories					
Manager	436690	0.161	0.368	0	1
Professional	436690	0.111	0.314	0	1
Associate professional and technical	436690	0.121	0.326	0	1
Administrative and secretarial	436690	0.159	0.366	0	1
Skilled trades occupations	436690	0.107	0.309	0	1
Personal service occupations	436690	0.090	0.285	0	1
Sales and customer service occupations	436690	0.073	0.260	0	1
Process, plant and machine operatives	436690	0.098	0.297	0	1
Elementary occupations	436690	0.081	0.273	0	1
Employer characteristics					
Private sector employer	435832	0.643	0.479	0	1
Manufacturing or construction sector	436699	0.238	0.426	0	1
Administration sector	436699	0.044	0.205	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 II: parameters of the calibrated model

	Before June 1999	After June 1999	After June 1999
Length of probationary period	24 months	12 months	12 months
q	**0.414**	**0.630**	**0.624**
σ	**5.706**	**5.554**	**5.567**
c0	**6.602**	6.602	**6.782**
c1	**6.800**	6.800	6.800
Discount factor	0.995	0.995	0.995
Maximal tenure	200 months	200 months	200 months

Notes: The bold numbers are those that were calibrated, while the other numbers were taken as parameters. c0 is the firing cost during the probationary period and c1 is the firing cost after the probationary period. The model is calibrated to best fit the 36 first months of the empirical hazard function in the case of a 24 months probationary period, and the 24 first months of the empirical hazard function in the case of a 12 months probationary period.

Table III: Parameters of the calibrated model for college/university educated workers

	Before June 1999	After June 1999	After June 1999
Length of probationary period	24 months	12 months	12 months
q	**0.631**	**0.633**	**0.633**
σ	**7.616**	**6.901**	**6.902**
c0	**7.778**	7.778	**7.761**
c1	**7.801**	7.801	7.801
Discount factor	0.995	0.995	0.995
Maximal tenure	200 months	200 months	200 months

Notes: The bold numbers are those that were calibrated, while the other numbers were taken as parameters. c0 is the firing cost during the probationary period and c1 is the firing cost after the probationary period. The model is calibrated to best fit the 36 first months of the empirical hazard function in the case of a 24 months probationary period, and the 24 first months of the empirical hazard function in the case of a 12 months probationary period.

Table IV: 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	-0.245	-0.267	-0.263	-0.282
	(0.072)***	(0.072)***	(0.087)***	(0.087)***
12 to 23 months tenure	-0.272	-0.290	-0.296	-0.326
	(0.084)***	(0.083)***	(0.101)***	(0.100)***
	B. Detailed tenure categories			
0 to 4 months tenure	-0.174	-0.223	-0.168	-0.195
	(0.088)**	(0.088)**	(0.108)	(0.108)*
5 to 11 months tenure	-0.276	-0.292	-0.296	-0.313
	(0.082)***	(0.082)***	(0.100)***	(0.099)***
12 to 17 months tenure	-0.270	-0.292	-0.275	-0.311
	(0.099)***	(0.098)***	(0.120)**	(0.120)***
18 to 21 months tenure	-0.371	-0.393	-0.392	-0.423
	(0.126)***	(0.125)***	(0.150)***	(0.149)***
22 to 23 months tenure	-0.050	-0.041	-0.141	-0.122
	(0.183)	(0.181)	(0.218)	(0.216)
24 to 25 months tenure	-0.032	-0.036	0.014	0.032
	(0.202)	(0.201)	(0.241)	(0.240)
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 tenure categories dummies (same as listed in the table), a dummy for tenure greater than 48 months, and the interaction between this last dummy and the "after" dummy.

Regressions with controls include the following additional controls: unemployment rate, female dummy, married and cohabiting dummy, age, 2 education dummies, 8 occupational dummies, private sector dummy, manufacturing and construction dummy, administration dummy, 3 quarters dummies, year dummies (years are June to May).

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

	A. Gender		
	Males	Females	
0 to 11 months tenure	-0.291	-0.212	
	(0.088)***	(0.125)*	
12 to 23 months tenure	-0.302	-0.255	
	(0.102)***	(0.143)*	
Number of observations	232610	197994	
	B. Age		
	Age<40	Age>=40	
0 to 11 months tenure	-0.290	-0.222	
	(0.090)***	(0.120)*	
12 to 23 months tenure	-0.360	-0.175	
	(0.105)***	(0.136)	
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	-0.352	-0.282	0.043
	(0.127)***	(0.103)***	(0.173)
12 to 23 months tenure	-0.268	-0.413	-0.070
	(0.148)*	(0.121)***	(0.182)
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 the following controls: tenure categories dummies (same as listed in the table), unemployment rate, married and cohabiting dummy, age, 8 occupational dummies, private sector dummy, manufacturing and construction dummy, administration dummy, 3 quarters dummies, year dummies (years are June to May). 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.

Table VI: Impact of the reform on the duration of unemployment

	A. Exit unemployment towards a permanent job with more than 16 hours a week	B. Exit unemployment towards any job
After	0.113	
	(0.026)***	
After*Young26	-0.100	
	(0.037)***	
Looking preferably for full-time employee job *After		0.086
		(0.026)***
Looking preferably for full-time employee job*After*Young26		-0.055
		(0.026)**
Number of observations	27966	27956

Notes: Cox proportional hazard models are used. After is defined as "after June 1999".
Robust standard errors clustered by person in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%.
All regressions include the following controls: unemployment rate, female dummy, married and cohabiting dummy, less than 26 years old dummy, age, 2 education dummies, 3 quarters dummies. Regressions in panel B also include dummies for types of job looked for, dummies for types of job looked for interacted with "after", and year dummies (years are June to May).
The sample is restricted to persons who are ILO unemployed in the first quarter and whose date of leaving their previous job is known. Only the first observation for each unemployment spell (as defined by the date when the last job was left) 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 VII: Impact of the reform on wages and training

	A. Log real hourly wage	B. Training
0 to 11 months tenure	0.008	0.011
	(0.007)	(0.005)*
12 to 23 months tenure	0.005	-0.007
	(0.007)	(0.006)
R-squared	0.44	
Number of observations	166480	433962

Notes: The coefficients reported are the interactions between tenure categories and the "after June 1999" dummy. Panels A reports results from an OLS regression. Panel B reports the marginal effects from a probit model; while the marginal interactions effects are not properly calculated by the dprobit Stata command, the coefficients from a linear probability model are quasi identical.
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 the following controls: tenure categories dummies (same as listed in the table), unemployment rate, female dummy, married and cohabiting dummy, age, 2 education dummies, 8 occupational dummies, private sector dummy, manufacturing and construction dummy, administration dummy, 3 quarters dummies, year dummies (years are June to May). 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 I : the effect of a probationary period on the firing hazard

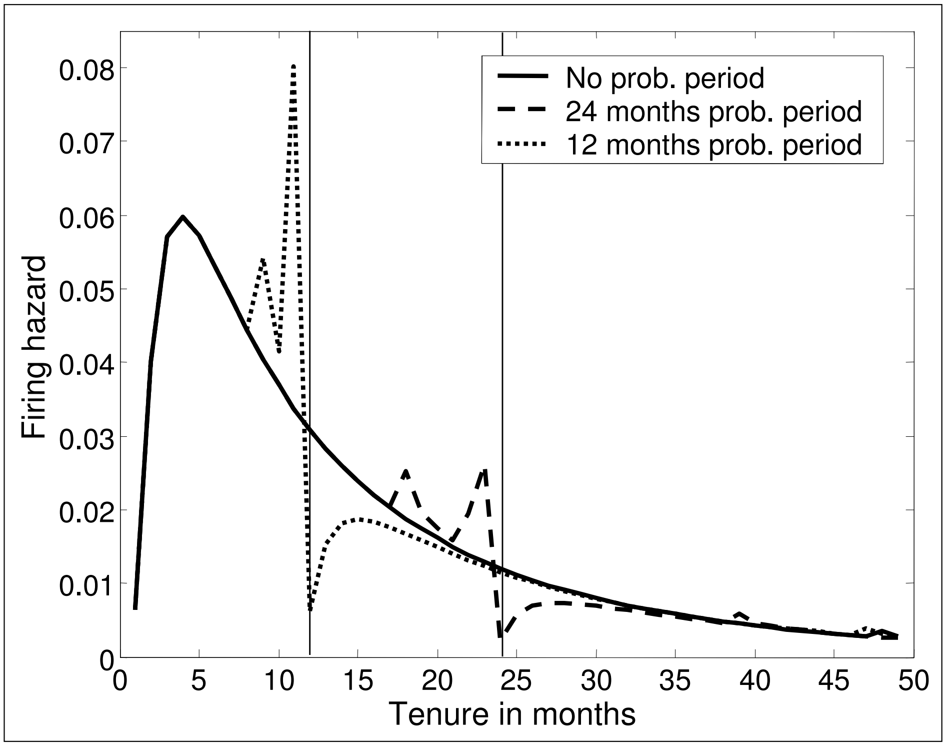


Figure II: the effect of an increase in recruitment effort or monitoring intensity after the reform

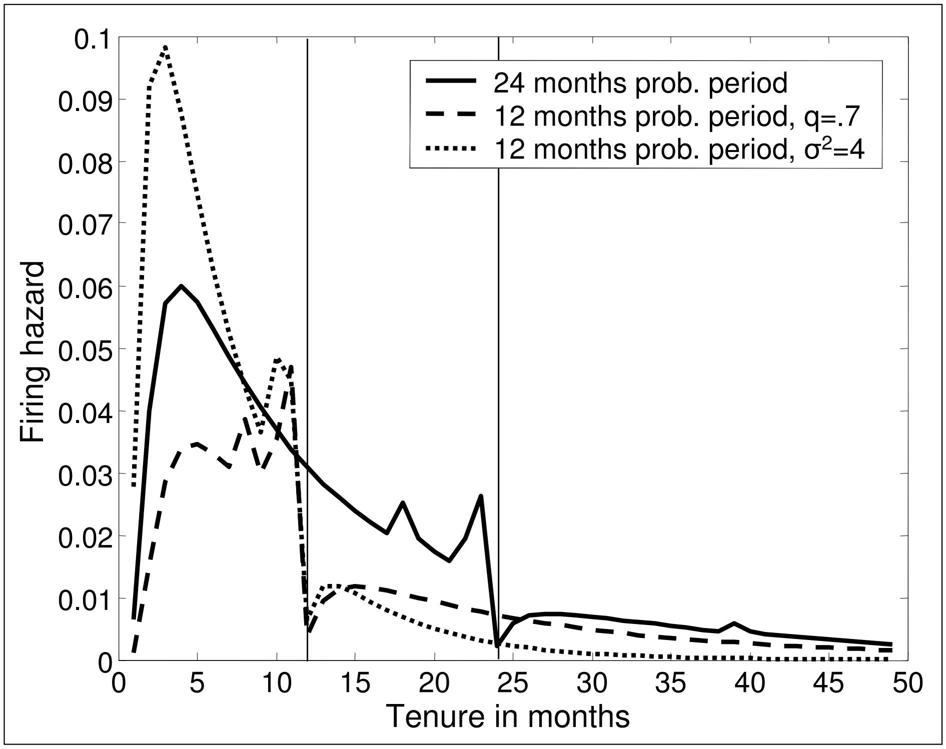
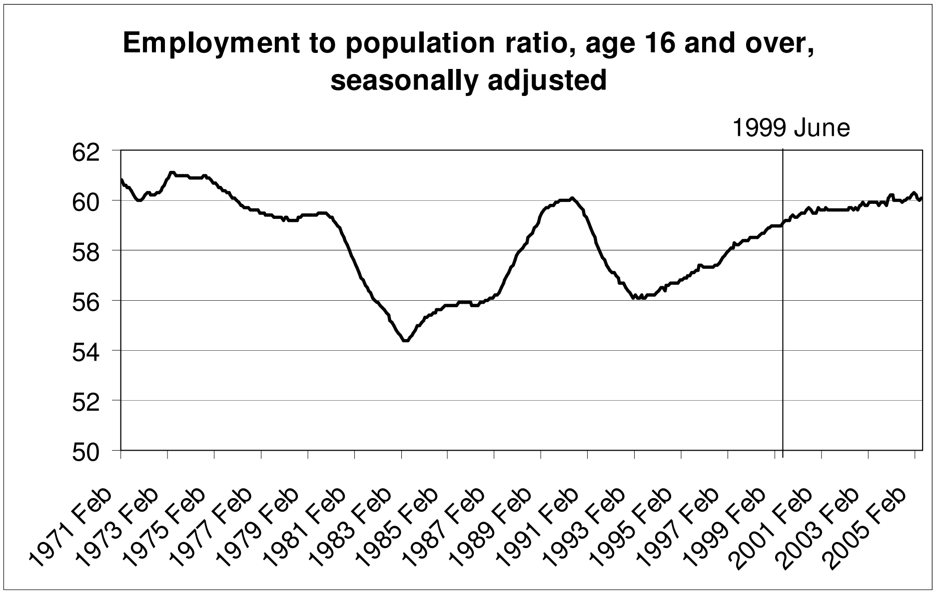
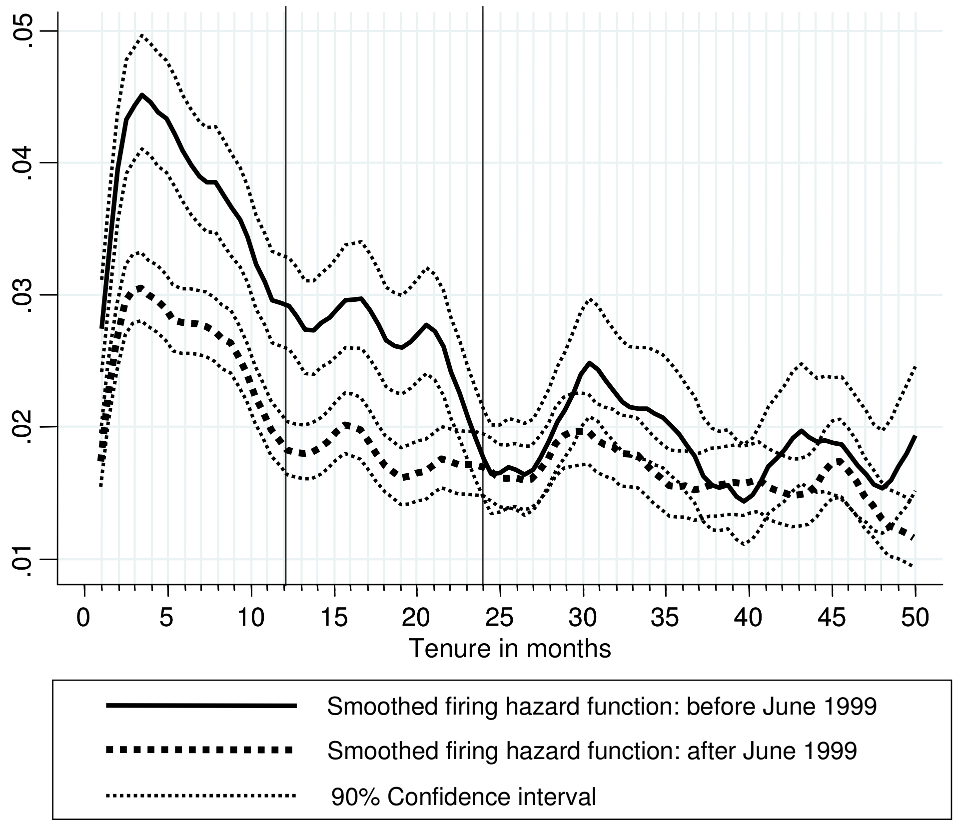


Figure III: The evolution of the employment to population ratio



Source: UK National Statistics, MGSR series, computed from the Labour Force Survey.

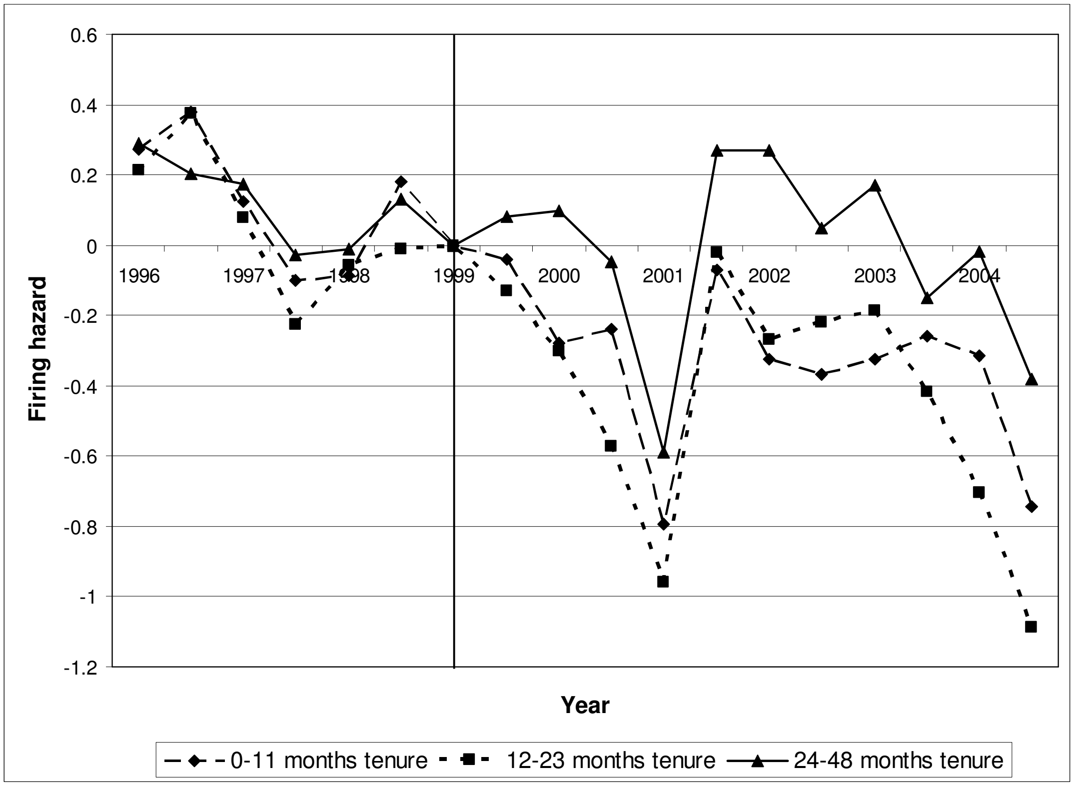
Figure IV: Kaplan-Meier estimates of the firing hazard before and after the reform



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.

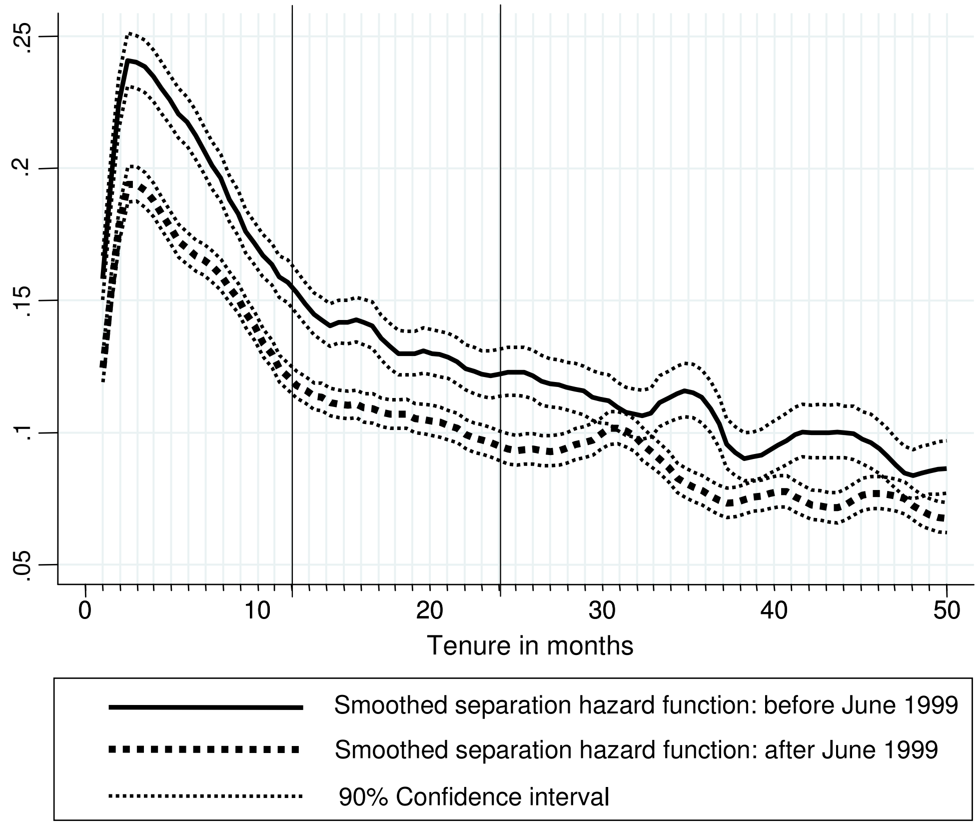
Source: Labour Force Survey Two-Quarter Longitudinal Dataset (www.data-archive.ac.uk).

Figure V: 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 1999.
The regression includes tenure categories dummies for the categories in the graph, an additional dummy for tenures greater than 49 months, and the interaction of the last dummy with half-yearly dummies. The regression further includes the following controls: unemployment rate, female dummy, married and cohabiting dummy, age, 2 education dummies, 8 occupational dummies, private sector dummy, manufacturing and construction dummy, administration dummy, 3 quarters 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.
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 VI: Kaplan-Meier estimates of the separation hazard before and after the reform



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

APPENDIX 1 : Equations for the firm's belief about match quality, and the firing hazard

Belief

The sum of observations out of t periods is described, under my hypotheses, by a normal distribution. Let $g_g(s,t)$ be the probability of getting a sum s of observations at tenure t when the true match quality is good: the distribution is normal with mean t and variance $t.\sigma^2$. Symmetrically $g_b(s,t)$ is normal with mean -t and variance $t.\sigma^2$. Using Bayes' rule we can then compute all possible beliefs. We have:

$$b(s,t) = \frac{q.g_g(s,t)}{q.g_g(s,t) + (1-q).g_b(s,t)}$$

It turns out that t drops out and the formula simplifies to:

$$b(s,t) = \frac{q.\exp\left(\frac{s}{\sigma^2}\right)}{q.\exp\left(\frac{s}{\sigma^2}\right) + (1-q).\exp\left(-\frac{s}{\sigma^2}\right)}$$

Firing hazard

Let $f_t(s)$ be the density of matches with sum of observations s at time t.

The initial values are:

$$f_0(0) = 1$$
$$\forall s \neq 0, f_0(s) = 0$$

Let $p(s \mid s_1)$ be the probability density of getting a total sum of observations s when at the previous period the total sum of observations was s_1.

$$p(s \mid s_1) = \frac{b(s_1)}{\sigma\sqrt{2\pi}} \exp\left(\frac{-(s - s_1 - b(s_1))^2}{2\sigma^2}\right) + \frac{1-b(s_1)}{\sigma\sqrt{2\pi}} \exp\left(\frac{-(s - s_1 + 1 - b(s_1))^2}{2\sigma^2}\right)$$

The evolution of the density of matches is given by the following recursion equation, where $s(\tau(t))$ is the sum of observations corresponding to the belief threshold at tenure t:

$$f_t(s) = \int_{s(\tau(t))}^{+\infty} f_{t-1}(s_1).p(s \mid s_1)ds_1$$

The firing hazard at tenure t is then:

$$h(t) = \frac{\int_{-\infty}^{s(\tau(t))} f_t(s)ds}{\int_{-\infty}^{+\infty} f_t(s)ds}$$

APPENDIX 2: extra tables and graphs

Table VIII : parameters used to compute results in the benchmark case

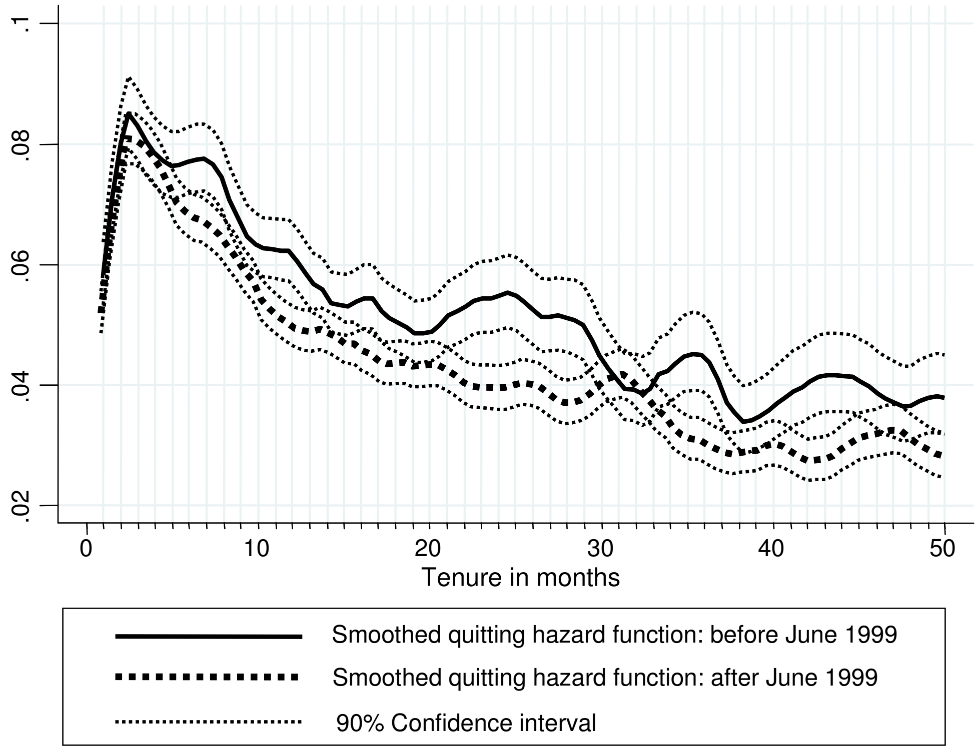
Parameters	**Values**
Discount factor δ	.995
Initial proportion of good matches q	.5
Standard error of signal σ	4
Firing costs c	7
Maximal tenure	200

Table IX: Summary statistics for the sample of ILO unemployed

	Obs.	Mean	Std.	Min	Max
Macro situation					
Unemployment rate (claimant count)	38004	4.437	1.827	1.5	11.7
Unemployment spell characteristics					
Unemployment duration	38004	31.775	52.734	0	482
Seeking full-time employee job	38004	0.513	0.500	0	1
Person characteristics					
Female dummy	38004	0.408	0.491	0	1
Married and cohabiting dummy	38004	0.372	0.483	0	1
Age	38004	36.076	12.667	15	64
Less than high school educated dummy	37997	0.395	0.489	0	1
University educated dummy	37997	0.156	0.363	0	1

Notes: The sample is restricted to persons who are ILO unemployed in the first quarter and whose date of leaving their previous job is known. Only the first observation for each unemployment spell (as defined by the date when the last job was left) 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 VII: Kaplan-Meier estimates of the quit hazard before and after the reform



Notes: same as Figure IV, except that here the failure is quit.