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SHORTENING THE TENURE CLOCK: THE IMPACT OF STRENGTHENED U.K. JOB SECURITY LEGISLATION

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Abstract
This paper uses the fact that firing costs are tenure dependent to analyze their effect on turnover and productivity. I exploit a 1999 British reform 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 roughly 30% decrease in the firing hazard for workers with zero to two years of tenure relative to workers with higher tenure. Training increased after the reform, unemployment duration decreased, and wages were unaffected. Theory suggests that the decrease in firing for low tenure workers is mainly due to a sizeable increase in the quality of recruitment.

Keywords: Firing Costs, Separation Hazard Rate, Learning, Job Tenure

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

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I. Introduction

US "employment at will" – 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. In particular, job security is commonly portrayed as one of the causes of high unemployment and slow growth in Europe. However, the difference between US and European job security legislation is not quite as stark as it would seem at first glance. For example, in the US, there are quite a few exceptions to the employment at will rule. Some of them are due to law and jurisprudence, such as anti-discrimination laws, and others to custom, such as the institution of tenure in US universities. Still, the majority of the workforce in the US remains under "employment at will". By contrast, in Europe, and in most developing countries, employers can generally only fire workers for a "fair" reason. However, it is usually not the case that workers benefit from such job security from day one of the employment relationship. Instead, they are only granted full job protection rights once they have worked for their employer for the full length of a probationary period. Even in countries with high firing costs, dismissal costs are thus usually very low in the beginning of the employment relationship, and they significantly increase with tenure.

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 employment at will 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 (for a theoretical illustration of the trade-off, see 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 latter problem, since firms can fire workers unsuited to the job at low cost at the beginning of the employment relationship. 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.

This paper analyzes a specific example of a probationary period provision in the United Kingdom. The right for dismissed workers to sue their employer for unfair dismissal is only granted after a given tenure on the job: before June 1999, this required tenure was two years, and after June 1999 it was reduced to one year. This source of variation allows me to shed light on two questions. First, what are the effects of having such a probationary period[2] on firms' firing behavior? Second, what is the impact of a reduction in the

[2] 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 this terminology is useful to conceptualize the problem.

probationary period on firms' personnel management practices and workers' labor market outcomes? The answers to these questions are of particular interest in the context of European employment policies. Indeed, many European countries developed fixed-term contracts to allow for a probationary period without directly altering their protective legislation, and France, taking a further step, introduced in August 2005 a new employment contract, the CNE ("contrat nouvelles embauches", i.e. "contract for new hires"). The latter allows firms with less than 20 employees to benefit from a 2 years probationary period during which employment is almost at will, while standard job protection is granted after the end of the probationary period. In the early days of 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). The CPE was seen by a sizeable number of French people as a step towards complete liberalization of the labor market, which they strongly oppose. Consequently, on March 28th and April 4th 2006, millions of people demonstrated against the reform, participating in the largest demonstrations in French history. As a result, the proposal was withdrawn. 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.

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 behaviors.

It is thus important to examine the impact of variations in statutory firing costs within a single country. In recent years, several studies have used micro data to assess the consequences of changes in the regulation for one given country (e.g., Hunt [2000], Blanchard and Landier [2001], Kugler [2004], Kugler and Pica [2005]). Most studies, whether cross-country or within countries, focus on the costs firms have to bear with certainty when firing under the regulations in place, setting aside the possibility of further intervention by labor courts. An exception is the study by Autor, Donohue and Schwab [2006] on the United States: using regional and temporal variation, they find a negative impact of one wrongful discharge doctrine, the implied-contract exception, on states' employment-to-population ratios. The implied-contract exception arises when, through words or actions, an employer implicitly promises not to terminate a worker without a good cause. Thus, the implied contract exception, a privately-granted right not to be unfairly dismissed, slightly reduces employment.

The timing of separations and the resulting duration of jobs have been subjected to both theoretical and empirical studies. A classic model by Jovanovic [1979] predicts a rise followed by a fall in the hazard of separation

with tenure. Productivity is job-specific and time-invariant; it is not known *ex ante* but becomes progressively evident as workers and firms observe output in succeeding periods. The probability of separation increases initially with the elapsed time because, as knowledge becomes more precise, the value of separating increases relative to the value of waiting to learn more about the real productivity of a match whose current productivity is low. After some time, observed separation decreases because only the more productive matches remain. 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.

Here, I introduce three new elements of analysis. First, like Autor et al. [2006], I focus on labor courts' induced firing costs, and more specifically on the right not to be unfairly dismissed. But, instead of examining the indirect effects of firing costs on employment, I directly analyze the effects of these costs on the probability of workers getting fired at different tenures. Second, I analyze the impact of firing costs on the timing, and not only the level, of firing. Third, I give this analysis a formal theoretical basis.

To test for the economic impact of a probationary period[3], I use the change in UK law mentioned above. Thus, the number of months necessary to qualify, or qualifying period, was lowered from 24 to 12 months for any termination (dismissal or redundancy) occurring after the 1st of June 1999.

[3] While I am examining a tenure-dependant firing cost, another strand of literature examines the effect of a tenure-dependant quitting cost. Thus, in Canada, the tenure on the job necessary to qualify for unemployment benefits has varied, and a series of papers studies the effect of those changes on job duration (Baker and Rea [1998], Christofides and McKenna [1996]).

Employees with 12 to 23 months of tenure were not protected before the reform whereas they had the right to claim unfair dismissal if fired after the reform; this implies that their probability of being fired should diminish after the reform. Employees with more than 24 months of tenure should be, in principle, relatively unaffected by the reform, and could be used as a control group. Employees with less than 12 months tenure may be affected by the reform if, for example, employers screen better after the reform to avoid a potential trial in the event of termination after the shorter qualifying period. The formal model I develop in Marinescu [2006] and summarize in section III gives further insights about the possible consequences of the reform on the firing hazard. The model's setup is very similar to Jovanovic's 1979 model, but some simplifying assumptions make it intuitive, and suitable for calculating the impact of firing costs and other parameters on the hazard of firing. The model allows predicting how the hazard of firing should change after the reform if firms keep their personnel management policies fixed and only react to the shorter probationary period. The model also predicts how the firing hazard changes if firms optimally react to the reform by increasing their recruitment or monitoring efforts, and it shows that these two strategies have significantly different effects. Thus, a higher recruitment effort 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 uses duration models on the 2-quarters Labour Force Survey longitudinal datasets. A simple Kaplan-Meier estimate reveals that the firing hazard is indeed lower after the reform for employees with 12 to 24 months of tenure. The hazard is also found to be

lower for employees with 0 to 12 months of tenure, which is consistent with firms having increased their recruitment efforts after the reform. Calibrating the model to fit these Kaplan-Meier estimates, I show that recruitment efforts must have indeed increased substantially after the reform (+52%), while monitoring on the job must also have increased slightly. Using all employees with more than 24 months of tenure as a control group in a Cox proportional hazard model, I find that the reform has a significant and large negative impact on the hazard of termination for those employees with 12 to 23 months tenure, and also for those with 0 to 11 months tenure. This result also holds if the control group is limited to employees with 26 to 48 months tenure. The estimated reduction in the firing hazard for workers with less than 2 years tenure relative to those with 2 to 4 years tenure is around 30%, with some small variation depending on the specification and the tenure sub-group considered. Lastly, I show that while most demographic and educational groups are similarly affected by the reform, the latter has a distinctive effect on university educated workers. After the reform, firms do not seem to increase recruitment efforts targeted at this latter group; instead, there is evidence consistent with a moderate increase (+18%) in monitoring efforts.

I next look at the effects of the reform on wages, training, and the duration of unemployment. While this analysis is useful to better gauge the total impact of the reform on the economy, one should note two related caveats. First, the analysis lacks a firm theoretical basis as the theory developed in section III of this paper does not make direct predictions about these outcomes. Second, it is empirically weaker in as much as it is relatively hard to find reasonable control groups to identify the effects of interest. With

these caveats in mind, results are as follows. First, no significant effect on wages can be established. Second, workers with 0 to 11 months tenure are significantly more likely to get training. The increase in training is consistent with an increase in match quality stemming from better recruitment and monitoring. The reform was not associated with an increase in the duration of unemployment overall, but coincided instead with a decrease in unemployment duration for affected workers. To probe whether the abandoned French CPE may have been a good idea, I then allow the reform to have a differential effect on 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 rest of this paper is organized as follows. In Section II, the tenure restriction to the right not to be unfairly dismissed is put into historical perspective. 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 reform on the firing hazard of various sub-groups of the labor force. Section 0 analyzes the impact of the reform on wages, training and the duration of unemployment. Section VI concludes.

II. The unfair dismissal qualifying period: historical background

The right not to be unfairly dismissed, introduced in most western European countries in the early 1970's, is usually restricted in several ways.

One of the main restrictions is that employees must have a minimal period of continuous employment to fully qualify for this right[4]. In the UK, after Labour came to power in 1997, 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 gave the following justification for the reduction in qualifying period:

> "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

[4] 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. This condition was set in 1973 when unfair dismissal legislation was first introduced, and has never been changed since. The introduction of the CNE contract in August 2005 could however be seen as an attempt to change this state of affairs since under that contract employees cannot sue their employer at all during the first 2 years of tenure, but the contract is identical to a CDI after two 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. Unlike France, the UK experimented a lot with the length of the qualifying period. Thus, while the initial 1971 (Industrial Relations Act) qualifying period had also been set to 24 months, it subsequently changed 7 times (Davies and Freedland [1993]). Initially, all parties agreed to lower progressively the qualifying period so that all employees could be covered, and so by March 1975, the qualifying period had been reduced to 6 months. The main reason why the diminution in the qualifying period was to be progressive was that the newly created Industrial Tribunals could not immediately cope with a huge caseload.
However, by the end of the 1970's, and in particular after Mrs. Thatcher became prime minister in 1979, the terms of the debate changed. The right of employees to claim unfair dismissal was seen as a burden to businesses, in particular to small ones. By the time Mrs. Thatcher came to power, the qualifying period was down to 6 months. She immediately increased it to 12 months with the 1979 unfair dismissal (variation of qualifying period) order. Then the 1980 Employment Act increased this qualifying period again to 24 months for firms with less than 20 employees. Lastly, the 1985 "Unfair dismissal (variation of qualifying period)" order increased the qualifying period to 24 months for firms with more than 20 employees as well, which meant that by 1985 the qualifying period was 24 months for all employees.

> 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."

Thus, the reduction in the qualifying period is mainly seen as compensation offered to workers in exchange for their consent to a more flexible organization of the labor market.

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[5]. 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

[5] 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 latter 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.

(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 reform concerning the qualifying period for unfair dismissal, it seems feasible to identify its independent effects.

III. Model of the impact of firing costs on the timing of firing decisions

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 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] and that was recently shown by Nagypal [2004] to be a driving factor of the empirical job separation hazard.

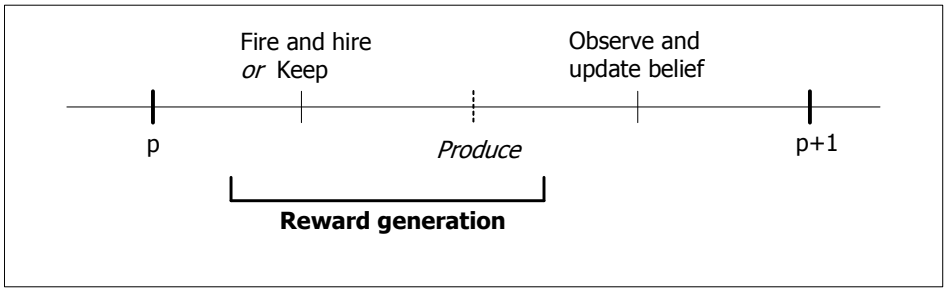
In what follows, I use a model based on dynamic programming developed in Marinescu [2006] to form testable hypotheses regarding the

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 E.

A. Assumptions

When a firm and a worker begin their employment relationship they do not perfectly know their match quality[6] 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:



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, in this simple version of the model, unemployment or the overall level of labor demand are not modeled. Instead, the focus is on the efficiency and timing of the matching process.

[6] 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.E, 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 *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. The tenure variable is perfectly observed by the firm. Moreover, tenure cannot be higher than some tenure $t_{\max}$, which is to be conceived of as the retirement tenure. Match quality can be either good or bad[7]: a good match means that the worker is adequate for the job, whereas a bad match means that the worker is inadequate. I assume that a proportion *q* of the matches is good whereas a proportion *1-q* is bad.

Match quality is not perfectly observed. Instead, at each period, the firm observes a normally distributed[8] 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. Because there are only two values of match quality, the belief that the match is bad is $1-b(s,t)$. 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).

Using the Bellman 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

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[7] This simplifying assumption allows me to keep the model intuitive. However, I also extended the model so that the match quality can be drawn from an arbitrary continuous distribution. For example, assuming a normal distribution of match quality - as in Jovanovic[1979] - leads to the same qualitative results (see Marinescu[2006]) as those described here.

[8] Again, the signal does not have to be normally distributed. One could specify any probability distribution conditional on quality.

match is 1 whereas the per period return to a bad match is 0. Moreover, the wage is fixed and set to 0[9]. Setting the wage to 0 rather than another constant does not entail any loss of generality given that labor demand is fixed in this economy and firms all pay the same wage. 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. I assume $c(t_{\max}) = c(1)$, i.e. when the worker retires, the firing cost is the same as the one incurred at tenure 1. This is because, at tenure 1 as at retirement, the separation cost consists mainly of the hiring cost of a new 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:

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

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

[9] 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.

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.

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:

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

Using dynamic programming and the appropriate Matlab code, the optimal policy of the firm is computed (see Marinescu [2006] for the technical details). 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 using the appropriate Matlab code. At tenure 1, the distribution of possible beliefs is computed given the assumed distributions. Then the hazard of firing at tenure 1 is the integral of the belief distribution from 0 to the firing threshold $\tau(1)$. At tenure 2, a set of possible signals is observed, which leads to a new distribution of possible beliefs, and the firing hazard is again the integral of the belief distribution from 0 to $\tau(2)$. And so on for each subsequent tenure (see Appendix 1 for the equation). Note that the computation does not rely on simulation, i.e. the technique used does not involve drawing a large number of matches in conformity with the distribution and then averaging over the results. Instead, equations directly use the definitions of probability distributions, and computations rely on an approximation of the normal distribution of the signal by a finite number of points.

B. Parameters

I now proceed to examine the effects on the hazard rate of termination of a discontinuity in firing costs (with higher firing costs after a given tenure) and how the hazard rate changes when the length of the probationary period changes. I thus model the potential effects of the 1999 reform within the framework of this model.

I choose a benchmark case 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 (shown in Figure VIII). Moreover, in this benchmark case, I pay attention to choosing parameters so that the variations in these parameters show sufficiently large effects to be clearly visible on graphs. When analyzing actual data, I will directly fit the theoretical hazard curve to the empirical one and derive the underlying parameters.

The parameters of the benchmark case are as displayed in Table I. A firing (and hiring) cost of 7 corresponds to 7 months of output. Note that an increase in the maximal tenure does not change the hazard of firing for tenures 1 to 50, tenures on which I will be focusing.

I introduce a tenure-dependant firing cost in the following form. The firing cost is 7 before the end of the probationary period, and 9 thereafter. I start with analyzing the effects of different lengths of the probationary period.

C. Variation in length of the probationary period

The hazard of firing is determined by two factors: the firing threshold $\tau(t)$ expressed in terms of belief, and the distribution of the firms' belief. The latter distribution is itself determined by two factors: the distribution of match quality embodied in the q parameter giving the proportion of good matches, and the distribution of signals engendered by the variance σ^2. 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]. In Figure II, I plot the distribution of firms' beliefs at different tenures, after they have observed the

signal at that tenure and before they fire. First, note that at tenure 1, right before firms have their first opportunity to fire, the distribution of beliefs about match quality is roughly normal with a mean of 0.5, corresponding to the q I specified. Now, up to tenure 48[10], the firing threshold is constant at .22, i.e. if the probability that the match is good is 22% or more, the firm keeps the worker, and otherwise it fires. The firing range of belief is shaded in Figure II. As already mentioned, the firing hazard is the integral of the belief distribution below the threshold[11], i.e. in the shaded area. This explains why, as tenure increases, the distribution moves away from normality: indeed, as firms fire the worst-performing workers, they truncate the lower tail of the distribution. Thus, as tenure increases, the distribution of belief in the neighborhood of .5 flattens and its mean moves towards 1: this is because in the long run, firms only keep workers whose match quality is almost certainly high. The shape of the belief distribution by tenure also helps to understand why the firing hazard first increases and then decreases with tenure. Indeed, if firms never fired anyone, the belief distribution would be more and more concentrated at 0 and 1 with increasing tenure. This is because as firms observe more and more signals, they improve their inference about whether a match is good or bad, and thus after an infinite number of observations, the belief distribution would be two-peaked with a density of .5 (because of the parameter q=.5) at 0, .5 at 1, and 0 everywhere else. Thus, in the absence of firing there would be more

[10] The firing threshold changes slightly thereafter because the firm anticipates that the worker is going to retire after 200 months.

[11] The reader may have noticed that these integrals at different tenures do not perfectly square with the firing hazard plotted in Figure I. This is because the computation of the hazard is based on the "sum of observations" statistic. While this latter statistic translates unequivocally into a given belief (see the formula for the belief in appendix 1), converting a distribution in terms of sum of observations to a distribution in terms of belief entails a certain degree of approximation because of the discretization used for the "sum of observations" statistic.

and more workers below the firing threshold with increasing tenure, so that the potential firing hazard would monotonically increase with tenure. This explains why the firing hazard first increases with tenure: more and more matches are discovered to be of bad quality as tenure increases. But as firms always dissolve the worst quality matches, eventually a large proportion of 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.

What is the effect of the introduction of a probationary period? To illustrate this effect, I assume that at tenure 24, the firing cost goes from 7 to 9. This only affects the firing hazard through the threshold and not through the parameters determining the belief distributions since σ^2 and q remain unchanged by assumption. With a higher firing cost after 24 months, the threshold will obviously decrease for tenures greater than 24 months, i.e. as firing is more expensive, firms keep workers with lower believed match quality. So the threshold after the end of the probationary period will be lower with a probationary period than without. 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 exactly 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, less workers get fired because the threshold is lower *and* those who were most likely to fall below it have been fired preventively. While it is the case that the firing threshold is lower in the post-probationary period, the firing hazard at tenures higher than 35 is almost the same as in the absence of a probationary period: this is because at that point very few workers get fired. For example, when one looks at the distribution of beliefs at tenure 35 in Figure II, one can see that the area between .22 and .2 (that is, the threshold at tenure 35 with a 24 months probationary period) is fairly small, and so moving the threshold down to .2 has a relatively small effect compared to, say, the same downward move of the threshold occurring at tenure 5.

What is the effect of a shortening of the probationary period? The firing cost is assumed to increase from 7 to 9 at tenure 12. This implies that the firing threshold will decrease earlier due to higher firing costs setting in earlier, and so the increase in the firing threshold before the probationary period will also occur earlier. For the shape of the firing hazard, this implies that while the firing hazard will remain exactly the same at very low tenure, the spike and trough will occur earlier, while there will be little effect on the firing hazard at high tenures, which is what can be seen in Figure I.

[12] Note that the existence of two peaks in the firing hazard in Figure I is an artifact of discretization. In reality, there is only one peak before the end of the probationary period, and the hazard increases smoothly before that.

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 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.

D. Endogenous response: modification of the quality of recruitment or monitoring

To probe whether changing the quality of recruitment or monitoring after the reform 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 reform.

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 III.

An increase in recruitment quality results in a decrease in firing at all tenures. This effect can be decomposed in two elements (which are in fact jointly determined, and only separated for the purpose of exposition). First, the increase in q increases the firing threshold from .22 to .34 in the 8 first months of tenure, which, for the belief distributions with q=.5, would imply more firing. But second, the increase in q changes the shape of the belief

distribution by tenure as shown in Figure IV, i.e. it changes how *likely* it is that the firm holds a belief below the threshold. Figure IV shows that the means of the distributions are shifted rightwards, so that the lower tails of the belief distributions are thinner, which implies less firing. To understand why, as can be seen from the hazard curves in Figure III, the effect on the belief distribution dominates the effect on the threshold, we must consider the following. First, note that the firm's belief exclusively depends on the sum of observations for a worker. Therefore, the threshold may also be equivalently expressed in terms of sum of observations. The threshold expressed in terms of sum of observations goes from -10 to -12 when recruitment effort increases (remember that a good match generates on average an observation of 1 per period whereas a bad match generates an observation of -1). In other terms, firms wait for more negative observations before they fire someone. This is intuitive because now they have a higher prior: they know that 70% instead of 50% of matches are good. Therefore any bad observation is more likely to be just noise. The fact that the threshold goes down in terms of sum of observations also implies that it is less likely that someone gets fired in general, because it is less likely that the sum of observations be below -12 rather than below -10: indeed with half good matches and half bad matches as in the reference case, the average observation will be 0; moreover, with an increase in recruitment efforts there are not 50% but 70% of good matches so it is even less likely that the sum of observations for a worker shall fall below -12. Therefore, the hazard of firing should fall. So why is the threshold expressed in terms of belief higher? This is because while firms wait for more negative observations before they fire someone, at the same time they know

that there are more good matches in the population of potential employees: this therefore makes them slightly more demanding on the current employees.

By contrast, an increase in monitoring results in an increase in firing at low tenures and a decrease in firing at high tenures (Figure III). This results again from two effects. First, the firing threshold decreases from .22 to .12 in the first 8 months, which for the belief distributions with $\sigma^2 = 16$, would imply less firing. Second, the shape of the belief distributions changes, as shown in Figure V: the distributions are flatter than before in the neighborhood of .5. To understand why this is the case, let's take the distribution at tenure 1, before the firm has had any chance to fire. This distribution is flatter because the signals are more informative than before: so, instead of having the belief distribution highly concentrated around .5, which is the prior over the population of hired workers, the belief distribution has more weight on its tails, because even after one signal firms are already quite certain that some matches are bad while others are good. This change in the shape of the distribution entails more firing at low tenures, because now for any threshold below .5, there are more workers below this threshold at low tenures. But eventually, because firms can quickly get rid of bad matches the hazard of firing gets lower. In this case, when expressing the threshold in terms of sum of observations, this threshold goes up to from -10 to -4. With an unchanged proportion of good matches (50%), it is more likely that a random worker has a sum of observations below -4 instead of below -10. Therefore, at low tenure, when firms did not yet get to fire many people so that the population of employed workers is still similar to the population of employable workers, it is more likely that someone gets fired. The firing

hazard is thus higher at low tenure. So then why does the belief threshold go down? This is because as firms get more precise information every period, they can afford to wait a little bit longer to be really sure that a match is indeed likely to be bad and therefore worth terminating.

Thus, both increasing the recruitment effort and the monitoring intensity indeed 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.

E. Limits to the model

The first limit to the model developed above is that match quality can only take two values, good or bad. However, in Marinescu [2006], I show that the qualitative implications of the model are preserved if one uses a normal distribution. Second, the model 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. Moreover, even if these costs are not known, examining the marginal benefits of increasing recruitment efforts or monitoring can inform us about firm's optimal decisions for a given cost.

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 get higher 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.

F. Main conclusions drawn from the model

The main conclusions drawn from the model are summarized in Table II. 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 the purpose of empirical analysis, the most important lesson from the theory is that it is by looking at workers with low tenure that one can hope to distinguish among the different scenarios summarized in Table II. It is moreover important to note that while the absolute size of the effects of large changes in recruitment and monitoring efforts on the hazard of firing for workers with 0 to 24 months

tenure is large, effects are very limited for workers with more than 24 months tenure (see Figure III). This implies that workers with more than 24 months tenure should form a reasonable if imperfect control group.

IV. The impact of the reform on the firing hazard

Before moving on to the description of the micro dataset used in this paper, it is useful to first have an idea of the macroeconomic context in which the reform takes place. I thus plot in Figure VI the evolution of the employment-to-population ratio in the United Kingdom in the long run. The focus of this paper, the 1999 reform, occurs during a phase of steadily growing employment in the UK, and the reform 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 reform. 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 reform 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 covers women from 15 to 59 years old, and men from 15 to 64 years old at the

date of the first interview. 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.

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 reform for this group would not be as instructive as for permanent workers. I therefore perform the analysis on the latter group only[16].

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.

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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.
[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).

Having defined the relevant group of workers, I also have to compute their tenure. 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, which allows for the computation of the tenure in months. 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 present. If however workers have found a new job, the date when they left their last job is not present, so it has to be imputed. 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 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.

What are the potential tenure sampling problems? The sample of jobs is what is traditionally called in the duration literature a stock sample with follow-up: one observes the tenure of workers in employment at the date of the first interview (stock sample), and then whether they separate by the

second interview (follow-up). This causes two problems. First, long tenures are overrepresented in the sense that one observes a higher proportion of high tenure workers in the sample than would be observed in a flow sample, i.e. in a sample where one can follow workers from day one of their job. 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[17]. 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. 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 latter cases.

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

[17] To be precise, I use as date of entry in the study the date of interview minus one month. This is because Stata drops all observations for which a failure is observed at the date of entry, and I just mentioned how some workers either lost their job during the month of their first interview or otherwise were assumed to have done so. The whole small subtlety occurs because we have discrete time steps that are long enough (one month) to contain both the interview and the job loss.

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 3 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 21.7%, 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. 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.

I now focus on workers who were fired.

B. A first look at firing rates by tenure

Assuming, consistent with the model, that workers with more than 24 months tenure are a reasonable control group, I plot the raw monthly job loss rate by tenure range in Figure VII. The raw job loss rate is defined as the

number of employees who lost their job through dismissal or redundancy over the total number of employees in the sample. Although there is a lot of month-to-month variation, one observes that globally the job loss rate of the control group (the more than 24 months tenure) is stable during the period observed, with some minor decrease in mid-2001, and some slightly higher values after the world economic downturn following September 2001. On the other hand, the treated group, i.e. the employees with less than 24 months tenure, has a decreasing trend in its firing rate starting after the June 1999 reform, so that at the end of the observation period the job loss rates for the treated group are smaller on average than at the beginning of the observation period, and they are also almost undistinguishable from the job loss rates of the control group.

This preliminary graphical analysis thus seems to indicate that the job loss probability of the treated group is negatively affected by the reform. In other terms, the reform seems to have decreased the separation probability for employees with less than 2 years tenure. I now investigate how the reform affected the hazard of firing for all tenures.

C. 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 VIII). 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 XII). 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 and in Marinescu [2006] 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.

Figure VIII 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 III: in particular, one very clearly observes a trough in the firing hazard around 24 months. With respect to the change introduced by the reform, one observes that from 24 months on, the hazard function is essentially identical before and after the reform. This confirms that employees with more than 24 months of tenure form a good control group. The hazard of termination after the reform is significantly lower on the interval [0,24]. It is thus lower not only 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 (see model's predictions in Table II). Note that while there is no observable change in the firing hazard for the 24 to 48 months tenure group, this does not 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 III).

In order to estimate how big a role increases in recruitment and/or monitoring efforts play in explaining the change in the shape of the hazard function after the reform, it is informative to perform a model calibration exercise: what are the parameters of the model that best correspond to the

Kaplan-Meier empirical hazard? While imperfect due to the limitations of the model and the calibration procedure, this exercise is useful to build quantitative intuition about the effects of the reform on the firing hazard. 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[18]. The fixed parameters in the model are the same as in section III. The results of the calibration exercise are shown in Table IV. I begin with fitting the hazard in the pre-reform period. I find that the best fit implies a total firing and hiring cost of 6.6 during the first 24 months, and 6.8 thereafter. To judge how big these costs are, the reader is reminded that a good match produces a value of 1 per month. Thus, firing and hiring costs are somewhat higher than 6 months of output. The proportion of good matches is 41%, and the standard error of the observation is 5.7. The calibration thus implies that the matching technology is not too efficient and that firing and hiring costs are high. Would it be optimal for firms to increase recruitment quality or monitoring intensity after the reform? 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 pre-reform hazard. I find that decreasing the standard

[18] 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[2006] for a model that includes this feature).

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, which is an effect that is 5 times larger than the effect of decreasing σ. I conclude that, for given marginal costs of recruitment and monitoring, firms should rather seek to increase recruitment quality rather than monitoring intensity after the reform.

I now evaluate the impact of the reform on the parameters using the post-reform hazard. I first set the length of the probationary period to 12 months, and I look for the best q and σ parameters to fit the post-reform empirical firing hazard, leaving all other parameters unchanged. I find that the quality of recruitment has increased a lot from 42% of good matches to 63% of good matches, which is a 52% increase. Moreover, monitoring intensity must also have slightly increased as the standard error decreased. These results are fully consistent with the prediction that increasing recruitment quality leads to higher marginal gains for firms than increasing monitoring intensity. In the third column of Table IV, I use an alternative calibration procedure where I also allow the firing and hiring cost during the probationary period to vary. The reason for doing so is that if the recruitment efforts have increased, then hiring costs must have increased as well. The calibration results shown in the third column imply a substantially higher firing and hiring cost during the probationary period: indeed, the latter is now almost as high as the cost incurred after the probationary period (6.782 versus 6.8).

The calibration thus confirms the increase in recruitment effort in the post-reform period – an inference which could already be made by observing

the empirical hazard function and using the model's predictions – and quantifies that increase. The calibration also shows a small increase in monitoring intensity, which could not be inferred by looking at the shape of the empirical hazard function but is consistent with what could have been expected ex ante. Thus, the reform seems to have encouraged firms to increase the quality of their recruitment and the intensity of monitoring. One way to check for the plausibility of this prediction is to rely on the fact that increasing monitoring or recruitment effort is likely to take some time while reducing the firing hazard of workers with 12 to 23 months tenure can be done more quickly. In this case, over time, one should observe that the hazard of firing first diminishes for the 12 to 23 months tenure, and then for the 0 to 11. This is indeed what I find when I plot the hazards using one year of data at a time (results not reproduced here). The way the hazard of firing changes through time is thus consistent with firms first directly reacting to the reform by firing fewer workers with 12 to 23 month tenure, and then increasing recruitment and monitoring efforts.

Another way of checking for the plausibility of the model's predictions is to 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 reform: 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.

D. 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[19], 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].

[19] 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.

To test for the effect of the 1999 reform, I use two related procedures. First I plot the baseline hazard of firing before and after the reform in Figure IX. The method used here is to run a stratified Cox model and compute the baseline hazards for the strata "before" and for the strata "after". The stratified Cox model assumes that the coefficients on the control variables are the same before and after the reform. Figure IX is almost identical to the Kaplan-Meier plot in Figure VIII implying that controls for covariates do not change the main conclusions.

I then proceed to run a Cox regression with the following specification for the hazard of termination:

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

Z is a set of controls, including the regional monthly unemployment rate and a full set of year dummies[20]. *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 treatment group systematically differs from the hazard of termination for the control group. A test of the negative effect of the reform on the hazard of termination is that the coefficients in the γ_1' vector are negative and significant.

[20] 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.

Panel A of Table V 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 reform dummy, I find that the reform significantly reduced the firing hazard by 18% for workers with 0 to 11 months tenure and by 20% for workers with 12 to 23 months tenure relative to those workers having more than 24 months tenure. I can also use as a control the workers with 24 to 48 months tenure, as they are likely to be more similar to the 0 to 23 months tenure group than workers who have tenures above 48 months. Using this control group does not change the results: if anything, the effect of the reform is now stronger. A problem with using "after June 1999" as the post-reform period is that firms may have anticipated the reform 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, however, affected by this change in the definition of the reform period[21].

In panel B, I use detailed tenure categories to examine the effects on different tenure subgroups. Again, the choice of control group or post-reform period does not change the results. I therefore concentrate on the more demanding specification, i.e. taking the 24 to 48 months group as a control and using "after June 1999, excluding May 1998 to May 2000" as the post-reform period. This is also the specification I adopt in the rest of the paper, unless otherwise specified. Concerning the effect of the reform on different tenure categories, I find that the negative effect of the reform on the firing hazard is

[21] 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 V are however unaffected by these alternative definitions.

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 III (compare the "24 months prob. period" curve with the "12 months prob. period, q=.7" curve) and Figure VIII. The reduction in the firing hazard is largest for the 18 to 21 months tenure, likely due to the fact that before the reform there used to be a spike at about 21 months tenure (Figure VIII).

In general, the reform is found to be effective in lowering the hazard of firing for the group newly protected by the right to claim unfair dismissal, i.e. the 12 to 23 months tenure group. Moreover, it also significantly lowers the hazard of termination for workers with 0 to 11 months tenure, which is consistent with the employers having increased their recruitment efforts in reaction to the reform.

E. Impact on different groups

In this section, I test whether the reform 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] or Blanchard and Landier [2002]). It is therefore interesting to ask if this tightening in workers' protection against unfair dismissal affected differentially these latter groups. Moreover, analyzing the effects by sub-groups allows for better estimation in

as much as fewer constraints on the parameters have to be imposed. Indeed, different worker types have different underlying parameters affecting the shape of their firing hazard, while the Cox specification only allows for proportional shift with covariates. If a sub-group has an altogether different pattern of firing hazard by tenure, then the Cox specification does not properly take that into account. This means that it is useful for identification purposes to separate the sample in more homogenous sub-groups.

Table VI examines the effects of the reform by gender and age, while Table VII looks at education. Panel A of Table VI shows the break-down by gender. While females see a somewhat higher decrease in their firing hazard than men, this difference is not significant. Thus, reforms in the areas of dependent care and sex discrimination, which intervened at the same time as the reform 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 basically the same for old and young workers, whereas the effect for 12 to 23 months tenure group is more pronounced for younger workers.

Table VII shows the impact of the reform 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 reform (figure not reproduced here), it appears that the

positive insignificant effect of the reform (Table VII) on workers with 0 to 11 months tenure is due to the fact that after the reform the peak in the firing hazard occurs at 7 months, while it occurred at 12 months before the reform. Moreover, while the trough in the firing hazard at 24 months was much bigger for university educated workers than for the whole population before the reform, it completely disappears after the reform. The model in section III explains these results. First, the peak in the firing hazard occurs later for higher educated workers than for others because these workers are more costly to fire and hire and/or harder to monitor. Both assumptions seem realistic in the case of university educated workers. However, after the reform, firms can no longer wait so long before they fire because with the new 12-months probationary period they would incur too high a firing cost; thus the peak in the firing hazard occurs before 12 months after the reform, consistent with an increase in monitoring effort. Moreover, the model tells us that the hazard of firing will only decrease at low tenures, i.e. here for workers with 0 to a few months tenure, if the quality of recruitment increases. It is likely that university educated workers were already recruited with care, so that there was not much room for efficient improvement there, which provides an explanation for the absence of a negative effect for the 0 to 11 months tenure group. To get a better understanding of the impact of the reform on the university/college educated workers, I fit the model to the Kaplan-Meier estimates of the firing hazard using only the sample of college educated workers. Table VIII shows the results of this exercise. First, note that these results confirm that hiring and firing costs, and the uncertainty surrounding match quality, are higher than average for college educated workers (compare

to the results in Table IV). Concerning the impact of the reform, we see that after the reform, recruitment efforts remain roughly the same with about 63% of good matches. Note that this number is higher than for the whole sample before the reform (41%) and roughly equal to the sample mean after the reform. In other terms, university/college educated workers were indeed already recruited with much more care before the reform. After the reform, the recruitment effort for other employees catches up. The other salient finding of Table VIII is that employers have significantly increased monitoring efforts after the reform, with a standard error of the observation process going from 7.6 to 6.9. Lastly, allowing firing and hiring costs during the probationary period to change does not yield in this case a higher cost after the reform, but the cost seems to have slightly decreased. 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 reform. 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 reform has a different impact on the most educated workers when compared to other educational groups.

F. 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 reform (Figure X). One can see that while all separations significantly decrease after the reform, 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 reform, 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 reform.

While the firing hazard has decreased after the reform, it is possible that firms have forced some workers to quit in order to avoid firing costs. These quits would then be disguised firings.

Figure XII in Appendix 2 shows that the quit hazard did not increase after the reform. Because firms have increased their efforts towards higher match quality, one might expect to see a lower quit hazard, and so the fact that the latter only slightly diminishes may indicate that indeed some firms push the least productive workers to quit. Making the extreme assumption that all quits are in fact firings, I reproduce the analysis of Table V. While I still find that the firing hazard has decreased, the decrease is now of lower magnitude,

and it is only statistically significant for workers with 12 to 23 months of tenure (results not reproduced here). The assumption that all quits are disguised firings being extreme, I take the results of this analysis as showing that my findings are robust to shifts from firings to quits. I next look at the impact of the reform on other key labor market outcomes such as training, wages or unemployment.

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

The analysis of the impact of the reform on other labor market outcomes is enlightening for two reasons. First, it allows for further investigation of the plausibility that firms have indeed increased their recruitment and monitoring efforts. Second, to better evaluate the overall welfare effect of the reform, one should look, beyond the effect on firing, at other positive or negative effects of the reform. 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 reform 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 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]. The relevant comparison in this case is workers with 12 to 23 months tenure versus workers with 24 to 48 months tenure. Indeed, workers with 12 to 23 months tenure are more expensive to fire after the reform, so 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.

However, before I can test this effect on wages, I have to take into account the introduction of a National Minimum Wage, which came into force April 1st 1999. Studies of the effect of the minimum wage in the UK show that spillovers may have taken place on the wage distribution up to the first decile at most [Low Pay Commission, 2003]. In order to eliminate the effects of the minimum wage, I look only at workers above the first decile of the wage distribution. Panel A of Table IX shows the effect of the reform on wages of workers with different tenures: while if I use all workers, wages seem to have increased, and even significantly so for workers with 0 to 11 months tenure, when using only workers who were not affected by the minimum wage, this

effect disappears. I therefore conclude that the reform had no significant effect on wages[22].

Training can be affected in two ways by a probationary period. First, higher firing costs after the probationary period can increase training in as much as it can be cheaper to train current marginal employees than to fire them and try to hire more productive employees. Thus, empirically firms who perceive higher firing costs are also more likely to train their workers [Pierre and Scarpetta, 2004]. Another related theory is that firing costs increase implicit screening costs for all firms, which increases the value of the informational advantage of the current employer. Therefore the latter is more likely to provide training [Acemoglu and Pischke, 1998]. These two theories would imply in my setting that employees newly protected by the 1999 reform, that is employees with 12 to 23 months tenure, should receive more training after the reform. 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 reform, the *expected* firing cost does increase as the probationary period is now shorter. A second way in which firing costs could affect training is through the interaction between training and recruitment and monitoring efforts. First, training can select for a more productive and stable workforce: thus, Cappelli [2002] shows that employers who offer tuition assistance for their employees to go to college manage to select better quality employees who stay longer on the job.

[22] 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.

This would imply that training increases across the board after the reform as a strategy used by employers to generate better quality matches. Second, firms face a trade-off when deciding on the timing of training. On the one hand, training can be particularly beneficial at the beginning of the employment relationship because the worker can get better adapted to the job from the very beginning. On the other hand, the firm may not be willing to invest in workers whose quality is uncertain and whom it would be likely to fire later on. If, however, recruitment quality increases, training can take place earlier in the employment relationship. This predicts that workers with low tenure should receive more training. Third, training in the very beginning of the employment relationship may also be used as a screening and monitoring device, i.e. by training workers, firms may learn more about their ability than otherwise. From fitting the model in section III, we know that employers have likely increased their monitoring efforts. This implies that training should have increased after the reform for the 0 to 11 months tenure group.

The proportion of workers who get training[23] has increased across the board after the reform (results not reproduced here), consistently with the idea that employers are trying to select for better matches, 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 IX documents the effect of the reform on firms' propensity to train their workers at different tenures. Workers with 12 to 23 months tenure do not get more training after

[23] 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".

the reform compared to workers with 24 to 48 months tenure, but workers with 0 to 11 months tenure do. The impact of the reform on training[24] is thus consistent with firms having increased their recruitment and monitoring efforts.

B. Impact on unemployment duration

There are three reasons why unemployment duration may increase after the reform. First, if expected firing costs increase, then labor demand may decrease, leading to higher unemployment duration. Second, even if labor demand does not decrease, 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 reform should all other things equal have higher unemployment durations than workers fired between 1 and 2 years tenure before the reform[25].

Table XI tries to identify the effects of the reform on unemployment duration. To perform this analysis, 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 Table X. In order to shed light on the impact of a reform such as the abandoned French CPE, I allow the impact on workers who are below 26

[24] 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.
[25] Unfortunately, for lack of a long enough follow up period, it is not possible to properly test this specific hypothesis.

to be different. To identify the effect of the reform on the duration of unemployment, I use two strategies. First, in panel A of Table XI, I look at the probability of finding a permanent job with more than 16 hours (i.e. a treated job) a week after the reform. Overall, the reform actually seems to have a positive effect on the probability of exiting unemployment towards a treated job. This is not to say that the reform has actually increased this probability by 14%, but it seems that at least any negative impact of the reform has been overpowered by otherwise positive trends. Moreover, increased match quality may create positive externalities so that the supply of treated jobs may increase despite the cost to individual firms of increasing match quality. Consistent with this finding, I observe 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 reform (Figure XI). Therefore, it does not seem that the reform incited employers to substitute away from full-time permanent jobs. On the other hand, when we look at the impact on workers below 26, the reform seems to have had a negative impact: young workers below 26 are 10% less likely than older workers to exit unemployment after the reform. The overall impact on the less than 26 years old is however still positive: the probability of their exiting unemployment has still increased by 4% after the reform.

A second strategy I use in panel B of Table XI 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 full-time jobs take longer to find a job relative to other unemployed workers. Note that 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.

Overall, the table shows no negative effect of the reform on the duration of unemployment for workers looking for full-time jobs relative to the others[26]. Quite to the contrary, the reform seems to have a positive effect implying that workers looking for a full-time job are 10% more likely to exit unemployment after the reform. Once again, this is not to say that the reform had a causal effect, but that any negative effects have been overrun by stronger positive effects. On the other hand, consistent with the results from Panel A, we find that workers below 26 have been negatively affected by the reform relative to older workers. Still, as previously, even workers under 26 who were looking for a full-time job are about 4% more likely to exit unemployment after the reform.

Thus, while all workers are more likely to exit unemployment after the reform, there is some evidence of a negative effect of the reform for workers under 26 compared to older workers. In conclusion, the reform 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.

[26] 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.

VI. Conclusion and possible extensions

Using a learning model, I have shown that the existence of a probationary period influences firms' firing pattern so that, all else equal, there is a peak in the firing hazard just before the end of the probationary period and a trough right after the end of the probationary period. This effect is smaller the smaller the difference between firing costs before and after the end of the probationary period. The empirical analysis 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, reflecting firms' increase in recruitment efforts. Firms have also increased their monitoring efforts and their investment in training after the reform.

These results are only partially consistent with the predictions of the British labor government about the impact of the reform (section II). 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 are more careful about whom they hire, they monitor their workers better, and they train them more. 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 reform prompted firms to change their human resource management policies, precisely to limit the need for firing past one year of tenure.

These results on the British reform are of particular interest for the evaluation new French and German employment policies. Concerning the now abandoned French CPE, which would have allowed firms to hire workers under 26 with a two years probationary period, this paper provides some evidence consistent with the idea that such a reform may have improved the employment prospects of this group. The evidence is however weak as we were not able to develop an entirely convincing strategy to test for the effects of the reform on unemployment duration. Relying on the evidence for all groups, we can predict the likely effects of the new CNE contract in France and the longer probationary period proposed in Germany by the Merkel government: thus, lengthening the probationary period should increase firing, decrease match quality, and have a limited impact on employment. However, these predictions are based on direct extrapolation of the British results, and do not take into account the specificities of the French or German economies. I therefore plan to evaluate these reforms directly as data becomes available.

In the debate about the effects of firing costs, this work has shown that the British reduction in the probationary period, and the associated increase in expected firing costs, did not have any discernable negative effect on employment or the duration of unemployment, while it likely increased productivity via better matches and more training.

This paper could be extended along several lines. First, 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. Second, the model used here could be applied to other questions. For example, I have shown how the distributions of firms' beliefs about workers' productivity evolve with tenure: this may have important implications for the wage distribution by tenure.

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. While I have shown some ways in which a probationary period can affect economic efficiency, i.e. for example by influencing firms' investments in match quality and human capital, this paper sheds little light on how this institution affects labor demand or interacts with the business cycle. The analysis of general equilibrium and business cycle effects of tenure-dependant job protection is thus a promising avenue for future research.

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Table I : 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

Figure I : the effect of a probationary period on the firing hazard

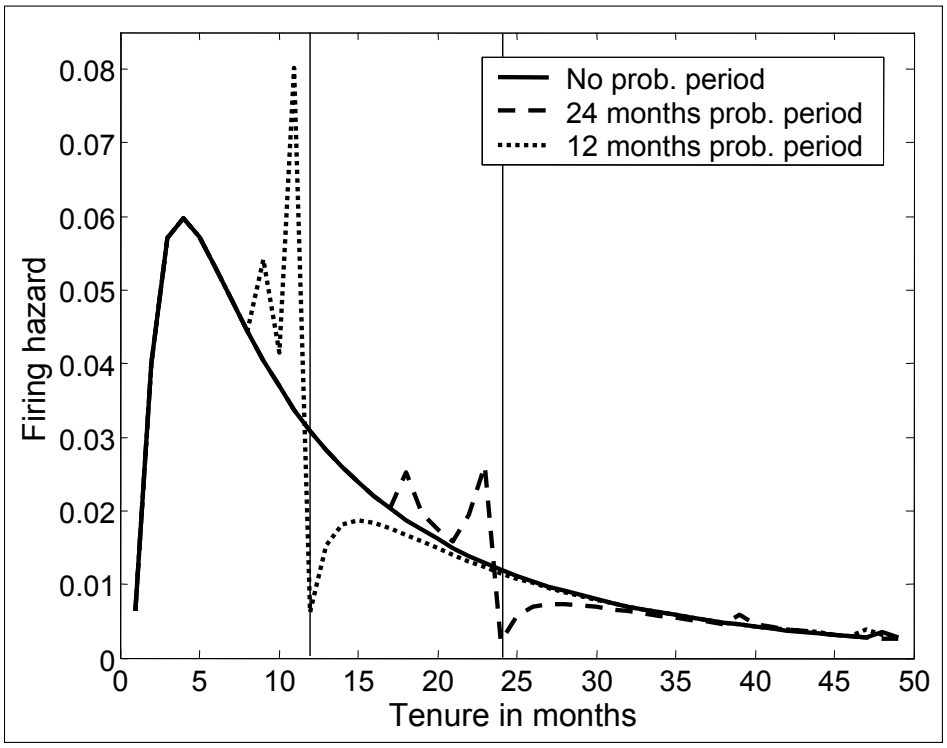


Figure II: belief distributions in the reference case

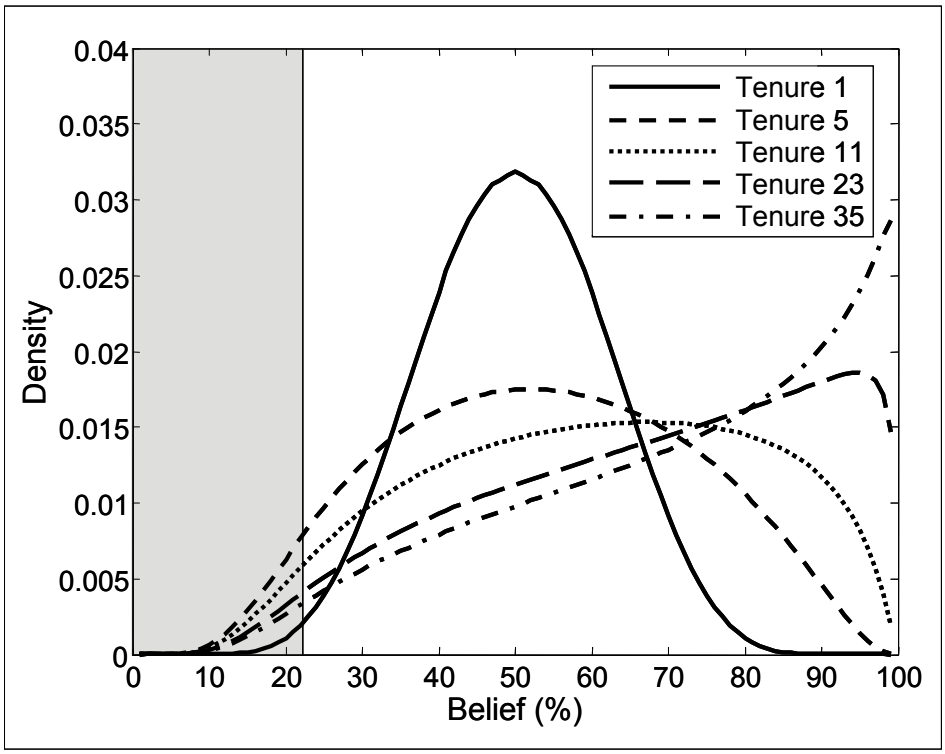


Figure III: the effect of an increase in recruitment effort or monitoring intensity after the reform

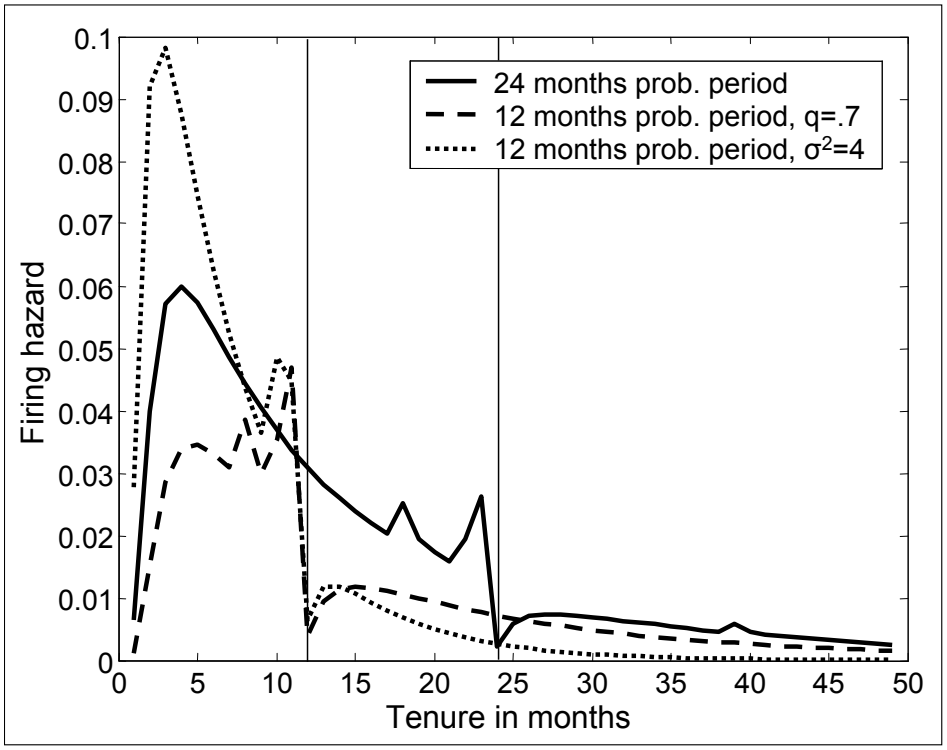


Figure IV: belief distributions in the case of an increase in recruitment effort after the reform

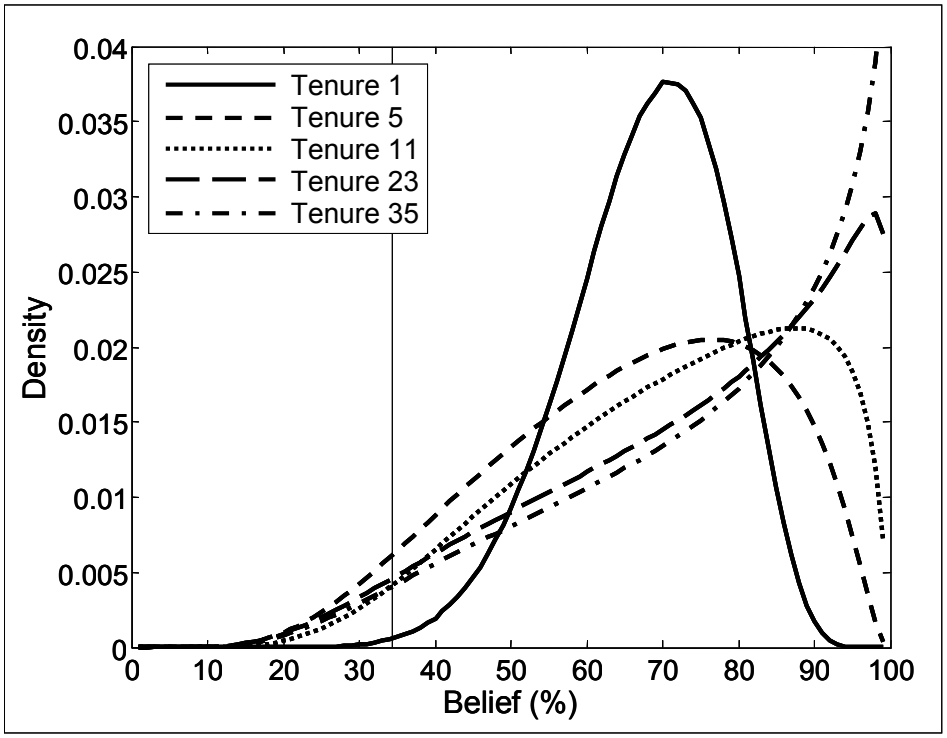


Figure V: distributions of belief in the case of an increase in monitoring intensity after the reform

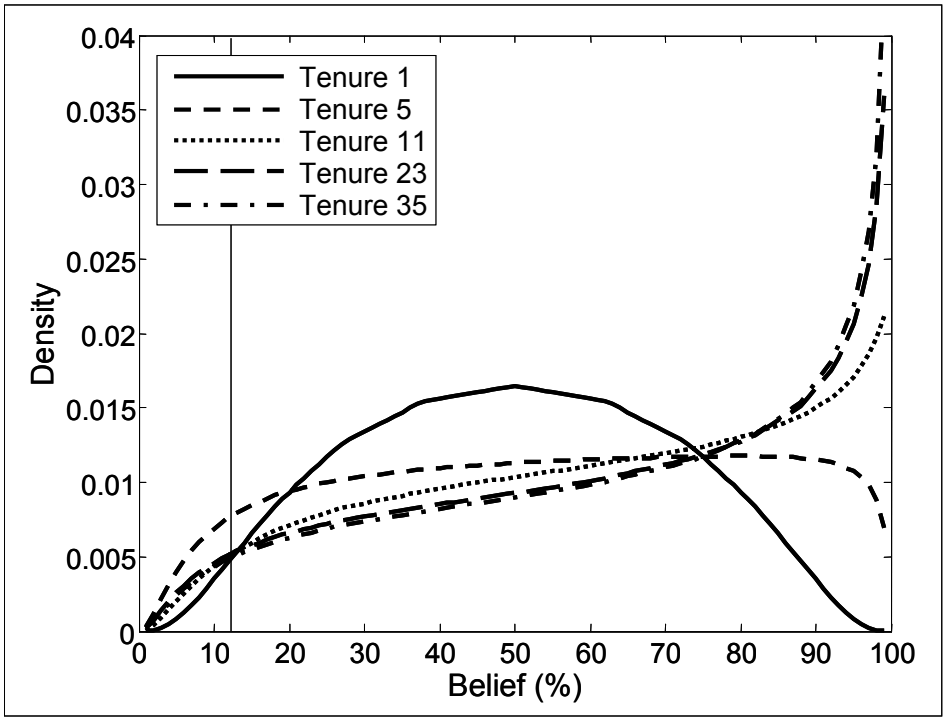
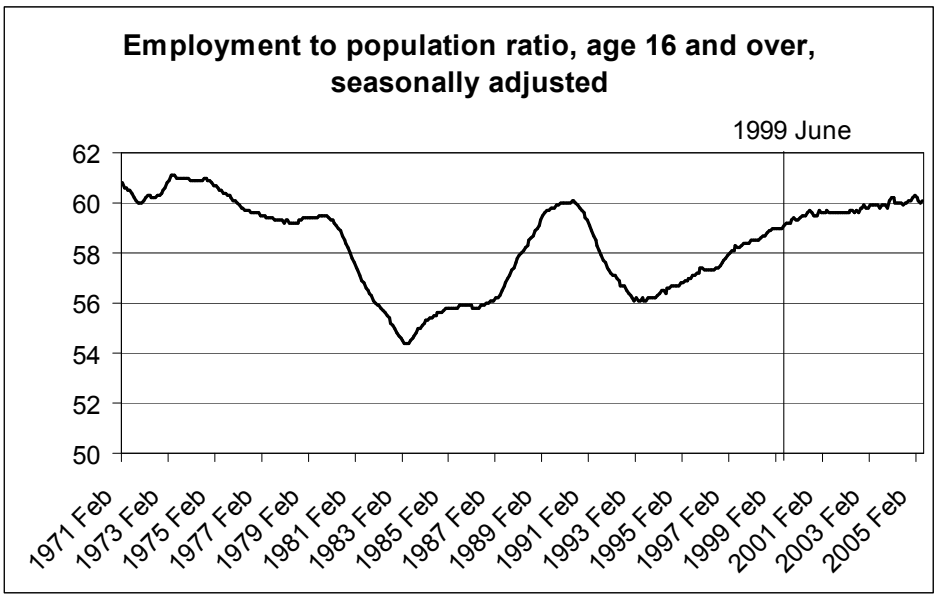


Table II: Effects predicted by the model for a reduction in the probation period from 24 to 12 months

	No change in recruitment effort or monitoring	Increase in recruitment effort	Increase in monitoring
0 to a few months tenure	NONE	**---**	**+++**
12 to 24 months tenure	**---**	**---**	**---**
24 months tenure and more	~NONE	**---**	**---**

Figure VI: The evolution of the employment to population ratio



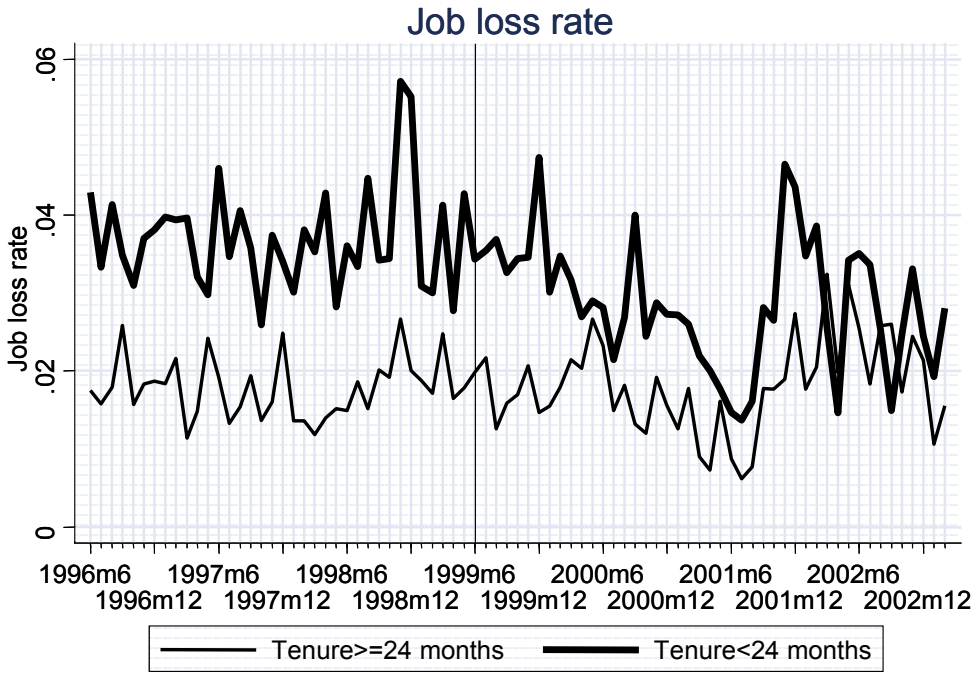
Source: UK National Statistics, MGSR series, computed from the Labour Force Survey.

Table III: 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

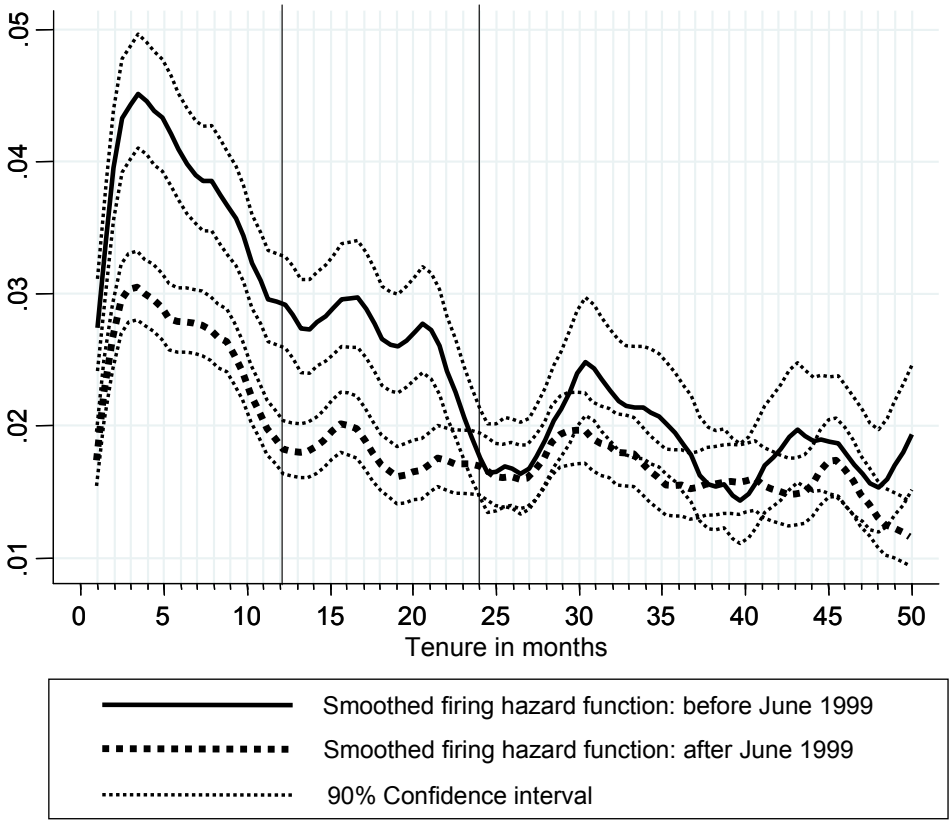
Notes: The sample is restricted to persons who are employed in the first quarter, in a permanent job, and usually working 16 or more hours a week. Only the first observation for each job (as defined by the hiring date) is kept.
Source: Labour Force Survey Two-Quarter Longitudinal Dataset (www.data-archive.ac.uk). For the unemployment rate, UK National Statistics, Time Series data [NS TSD], Regional claimant count rate, non seasonally adjusted, series code EGU4.

Figure VII: Job loss rate



Notes: the job loss rate is calculated as the number of workers who were dismissed or made redundant between the first and the second interview quarter over the total number of workers employed in the first quarter. 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 VIII: Kaplan-Meier estimates of the firing hazard before and after the reform



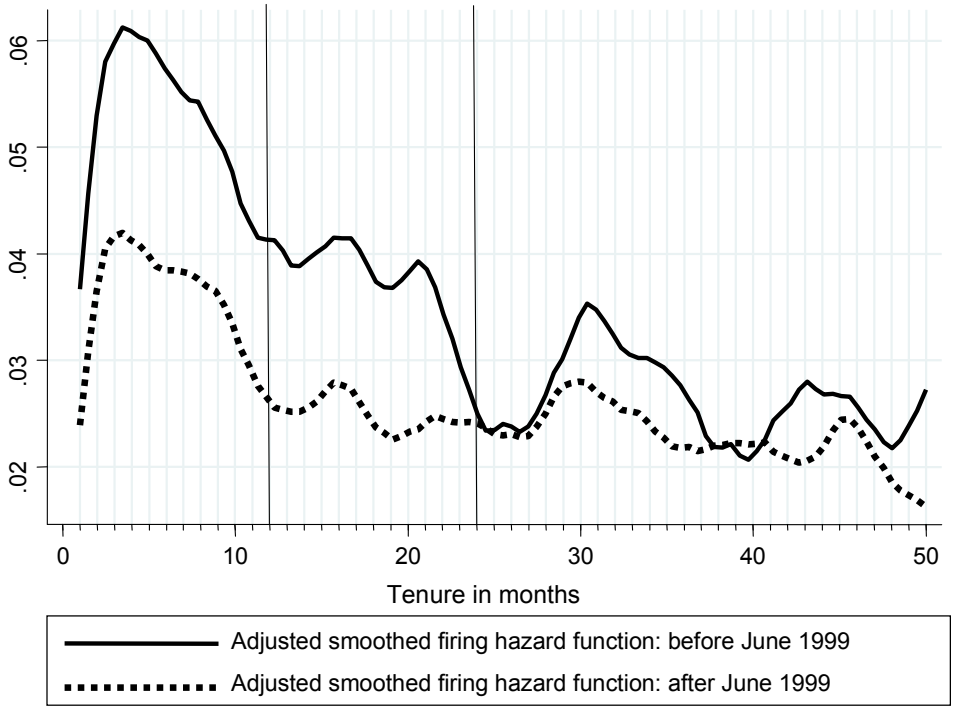
Notes: The figure plots smoothed non-parametric Kaplan-Meier firing hazard estimates. Firing is defined as dismissing or making redundant a worker. The sample is restricted to persons who are employed in the first quarter, in a permanent job, and usually working 16 or more hours a week. Only the first observation for each person is kept.
Source: Labour Force Survey Two-Quarter Longitudinal Dataset (www.data-archive.ac.uk).

Table IV: 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.

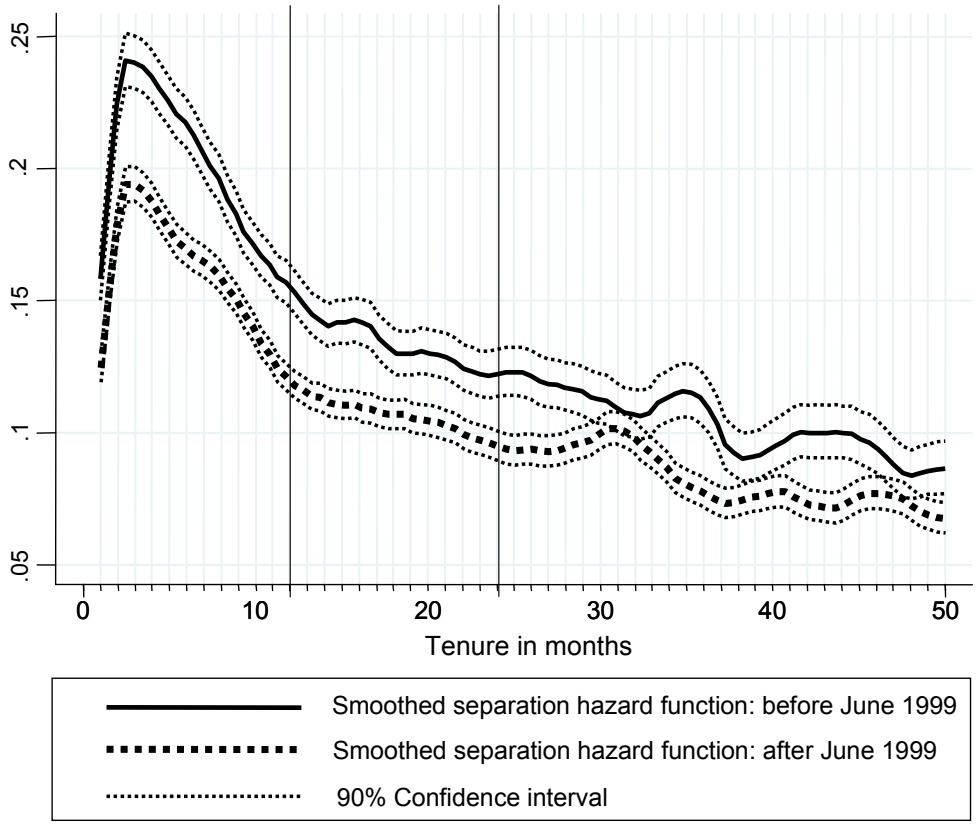
Figure IX: Adjusted estimates of the firing hazard before and after the reform



Notes: The plots are the smoothed baseline firing hazards after a stratified Cox regression model where "before" and "after" are the two strata. Firing is defined as dismissing or making redundant a worker. The control variables included in the Cox regression are: the regional unemployment rate in the month under consideration, age, gender, education, occupation, sector (public or private), industry. The graph is then plotted at the median values of these variables (when the latter are categorical and cover more than one category, the most frequent category is used). 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). For the unemployment rate: UK National Statistics, Time Series data [NS TSD], Regional claimant count rate, non seasonally adjusted, series code EGU4.

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



Notes: same as Figure 8, except that the failure event here is any job separation, instead of dismissals or redundancies only.

Table V: 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	
	Control group: 24 months and more tenure	Control group: 24-48 months tenure	Control group: 24 months and more tenure	Control group: 24-48 months tenure
	A. Basic tenure categories			
0 to 11 months tenure	-0.182	-0.267	-0.158	-0.282
	(0.050)***	(0.072)***	(0.060)***	(0.087)***
12 to 23 months tenure	-0.205	-0.290	-0.202	-0.326
	(0.065)***	(0.083)***	(0.078)**	(0.100)***
	B. Detailed tenure categories			
	Control group: 26 months and more tenure	Control group: 26-48 months tenure	Control group: 26 months and more tenure	Control group: 26-48 months tenure
0 to 4 months tenure	-0.135	-0.223	-0.071	-0.195
	(0.069)*	(0.088)**	(0.085)	(0.108)*
5 to 11 months tenure	-0.203	-0.292	-0.190	-0.313
	(0.060)***	(0.082)***	(0.073)***	(0.099)***
12 to 17 months tenure	-0.203	-0.292	-0.188	-0.311
	(0.081)**	(0.098)***	(0.099)*	(0.120)***
18 to 21 months tenure	-0.304	-0.393	-0.300	-0.423
	(0.113)***	(0.125)***	(0.134)**	(0.149)***
22 to 23 months tenure	0.048	-0.041	0.002	-0.122
	(0.173)	(0.181)	(0.206)	(0.216)
24 to 25 months tenure	0.052	-0.036	0.154	0.032
	(0.194)	(0.201)	(0.231)	(0.240)
Number of observations	430604	430604	335782	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%.
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). When using workers with tenure 24 to 48 months or 26 to 48 months as a control group, regressions also include a dummy for tenure greater than 48 months, and the interaction between this latter dummy and the "after" 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 firing hazard by gender and age

	A. Gender	
	Males	Females
0 to 11 months tenure	-0.223	-0.392
	(0.106)**	(0.153)**
12 to 23 months tenure	-0.307	-0.348
	(0.122)**	(0.177)**
Number of observations	180899	154883
	B. Age	
	Age<40	Age>=40
0 to 11 months tenure	-0.299	-0.263
	(0.110)***	(0.146)*
12 to 23 months tenure	-0.421	-0.150
	(0.127)***	(0.167)
Number of observations	174762	204265

Notes: The coefficients reported are the interactions between tenure categories and the "after June 1999, excluding May 1998 to May 2000" 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, 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). Regressions in panel B 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 VII: Impact of the reform on the firing hazard by education level

	Less than high school	High school but less than college	University/College educated
0 to 11 months tenure	-0.358	-0.319	0.145
	(0.153)**	(0.125)**	(0.215)
12 to 23 months tenure	-0.297	-0.429	-0.166
	(0.177)*	(0.148)***	(0.220)
Number of observations	81712	159224	94846

Notes: The coefficients reported are the interactions between tenure categories and the "after June 1999, excluding May 1998 to May 2000" 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, female dummy, 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).
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 VIII: 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 IX: Impact of the reform on wages and training

	A.Log real hourly wage		B. Training
	All workers	Workers above the 1st decile	
0 to 11 months tenure	0.016	-0.002	0.017
	(0.009)*	(0.008)	(0.007)**
12 to 23 months tenure	0.012	0.009	-0.008
	(0.008)	(0.008)	(0.007)
R-squared	0.45	0.44	
Number of observations	126855	106934	338238

Notes: The coefficients reported are the interactions between tenure categories and the "after June 1999, excluding May 1998 to May 2000" dummy. Panels A reports results from OLS regressions. 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.

Table X: 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

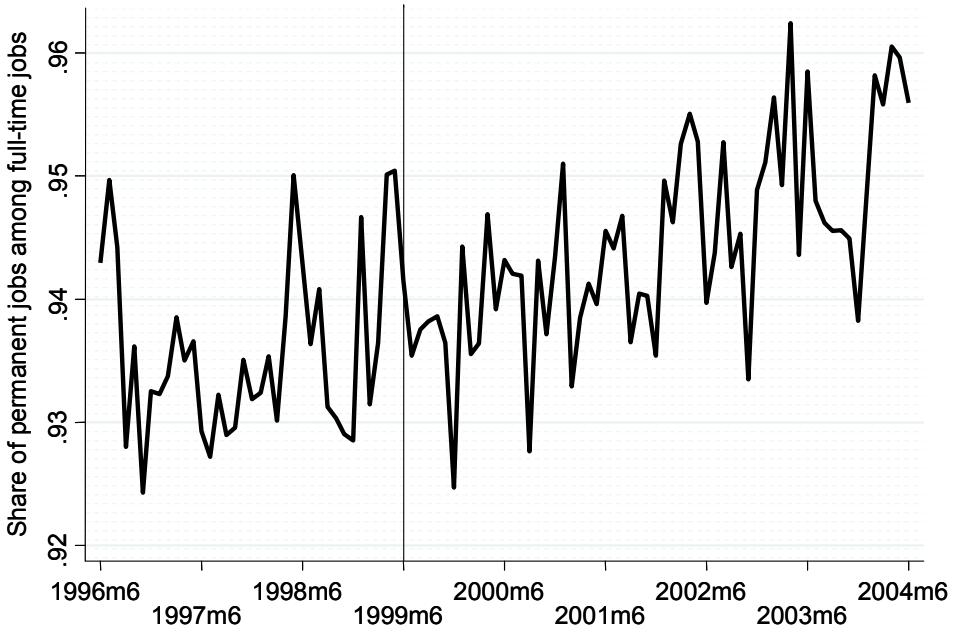
Notes: The sample is restricted to persons who are ILO unemployed in the first quarter and whose date of leaving their previous job is known. Only the first observation for each unemployment spell (as defined by the date when the last job was left) is kept.
Source: Labour Force Survey Two-Quarter Longitudinal 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.

Table XI: 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	0.138	
	(0.034)***	
After*Young26	-0.097	
	(0.046)**	
Looking preferably for full-time employee job *After		0.104
		(0.032)***
Looking preferably for full-time employee job*After*Young26		-0.067
		(0.031)**
Number of observations	27966	27956

Notes: Cox proportional hazard models are used. After is defined as "after June 1999, excluding May 1998 to May 2000".
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.

Figure XI: Evolution of the proportion of permanent jobs among full-time jobs



Notes: The sample is restricted to persons who are employed in the first quarter 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).

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*:

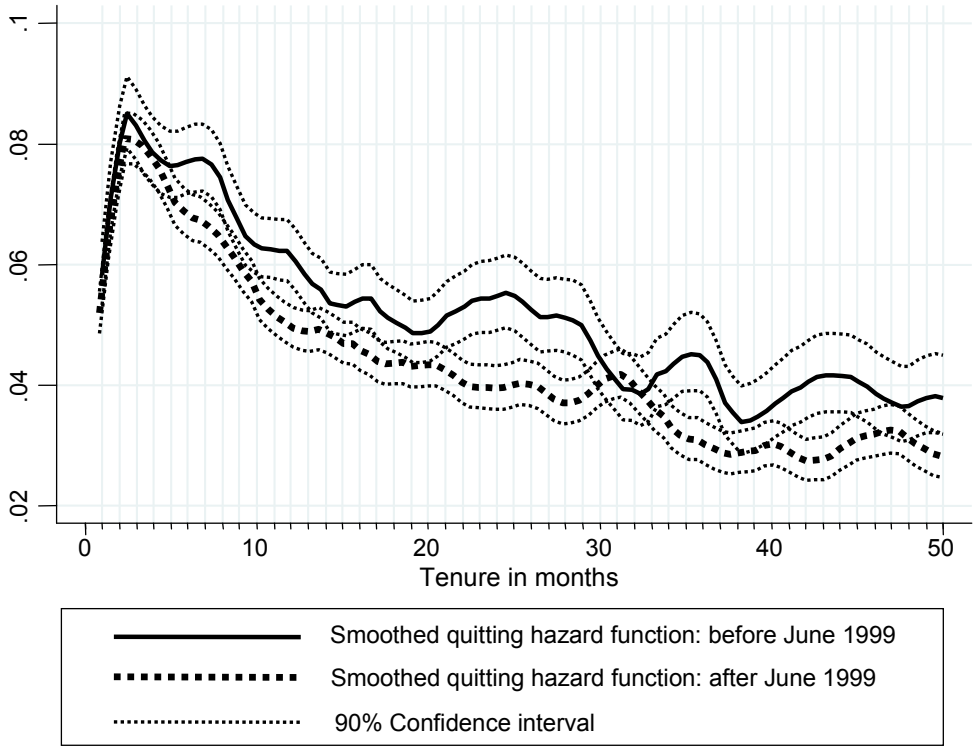
$$f_t(s) = \int_{s(\tau(t))}^{+\infty} f_{t-1}(s_1).p(s \mid s_1)ds_1$$

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: robustness checks

Figure XII: Kaplan-Meier estimates of the quit hazard before and after the reform



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