Worker transitions from temporary to permanent employment: the Spanish case*

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March 2001

Abstract

This paper studies the duration pattern of fixed-term contracts in Spain and the determinants of their conversion into permanent ones. To address this issue we estimate a duration model for temporary employment, with competing risks of flowing into permanent employment versus alternative states, and flexible duration dependence. We find that the shape of the baseline hazard is suggestive of two possible uses of fixed-term contracts by employers. Spikes at durations around 1 year support the idea that such contracts are used as a screening device instrument: successful workers obtain a permanent renewal much before the legal duration limit of their contracts. There is also evidence of another pronounced spike at 3 years of duration, coinciding with the maximum duration of fixed-term contracts. This suggests that some employers only opt for permanent hirings when there is no other way to retain the worker. In other words, fixed-term contracts simply provide a cheaper option for adjusting their employment level.

Keywords: Fixed-term contracts, Duration models.
JEL Classification: C41, J41, J60.

*We wish to thank Manuel Arellano, Samuel Bentolila, Olimpia Bover, Alan Manning, Steve Nickell, Gilles Saint-Paul and Javier Suarez for very useful comments and Sergi Jimenez for providing the data. The usual disclaimer applies. Barbara Petrongolo acknowledges financial support from the Spanish Ministry of Education (Grant No DGES PB97-0091). E-mail addresses: maia.guell@econ.upf.es and b.petrongolo@lse.ac.uk.

1 Introduction

Spain is often thought of as an economy with highly regulated labour markets, and most aggregate indices of flexibility tend to rank the country at the bottom of the OECD list (see Grubb and Wells, 1993, and OECD, 1994). Spanish unemployment, at over 20% of the labour force, reinforced this belief among both policy-makers and employers. This situation was among the factors which triggered the implementation of the experiment of "flexibility at the margin", started in 1984 with the introduction of a new typology of labour contract, characterised by limited duration and negligible firing costs. Upon expiry of such contract, the firm can either offer the worker a contract of undetermined duration or dismiss her. The idea behind this policy was to introduce more flexible contracts than existing (permanent) ones, in order to fight high and persistent levels of unemployment.

These contracts have been massively used for nearly all types of jobs and sectors of the economy. Soon after their introduction - coinciding with the expansion of the late 1980s - 98% of newly registered contracts have been of this type (see Bentolila and Saint-Paul, 1992). But, at the same time, unemployment has remained as high as before the reform. Within a decade, the Spanish labour market had experienced record rates of gross job creation, but little permanent employment had been created because only a small proportion of fixed-term contracts (FTCs) has been converted on a permanent basis (PC).[1] The labour market had gradually evolved towards a dual structure,[2] with two thirds of employees retaining a permanent status and the rest working in a highly mobile market. Indeed, the share of temporary workers had increased to approximately one third by the early 1990s - which is more than three times the European average (see OECD 1987, 1993 and Toharia 1997). Interestingly enough, once these effects became evident Spanish policy makers tried to limit the applicability of FTCs (1994 reform) and created a new type of PC with lower firing costs (1997 reform).

There exists a growing literature which studies several aspects of the impact of FTCs on labour markets in OECD countries, with special reference to the Spanish case (see Dolado et al. 2001 for a recent survey).[3] Jimeno

[1] See Figure 4.

[2] See Bentolila and Dolado (1994), Castillo *et al* (1998), Saint-Paul (1996), Segura *et al* (1991), and Toharia (1997).

[3] In this context, Spain appears to be a striking case because it has the highest unemployment rate among OECD countries and has implemented one of the most radical

and Toharia (1993) and Bentolila and Dolado (1994) point at the perverse effects of FTCs on wage formation. They argue that, when wage setting is dominated by insider employees, protected by substantial turnover costs, permanent workers may be able to obtain higher wages, "since the presence of a buffer of flexible employees lowers the likelihood that insiders will lose their jobs". Jimeno and Toharia (1996) study the effects of FTCs on labour productivity. On the one hand, with shorter employment relationships, firms' and workers' incentive to invest in specific human capital are reduced. On the other hand, the dependence of severance payments on the tenure of the contract may lead temporary employees to exert higher effort than permanent ones. Wasmer (1999) provides a matching model to explain the rising share of temporary employment in Europe as a side-product of the slowdown in the growth of labour productivity. Cahuc and Also in a matching framework Cahuc and Postel-Vinay (1999) show that the increased turnover stemming from the introduction of FTCs may actually raise unemployment when firing costs on PCs are high enough. Aguirregabiria and Alonso (1999) estimate a dynamic labour demand model with PCs and FTCs on Spanish data. They find that the introduction of FTCs leads to an excess turnover but has negligible impact on output and the value of the firm. This contrasts with sizeable output and value effects under the hypothetical alternative reform consisting in a reduction in firing costs on all types of contracts. Finally, Güell (2000) endogenises firms' choice of contracts and the conversion of FTCs into PCs in an efficiency wage scenario. She concludes that employment is not necessarily higher in the two-tier system than in one with PCs only, unless wages on FTCs are flexible enough. Instead, a reform of the employment protection legislation would unambiguously increase employment at no expense of labour market segmentation.

There is an important aspect still missing in this literature, namely the study of the conversion of FTCs into PCs and its timing. This paper concentrates on these issues. We believe that these are important issues given that most employer-worker relationships in Spain start on a temporary basis, and have to be either promoted to a permanent status or destroyed within a legal limit of three years.

More specifically, we study the duration pattern of FTCs using micro data drawn from the Spanish Labour Force Survey (EPA). This should shed

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reforms.

some light on the kind of use that employers make of FTCs.[4]

Before 1984, the use of FTCs was only allowed in Spain for covering jobs whose underlying nature was seasonal, or linked to specific (temporary) activities. One key feature of the 1984 reform was to remove the seasonal requirement for the applicability of FTCs. And indeed they spread rapidly to non-seasonal jobs (see Figure 1). This leads us to analyse alternative reasons why firms opt for temporary hirings.

First, FTCs can be used as a screening device, that allows employers to observe the productivity of a job-worker pair during the maximum probation period of three years. In this perspective, job matches are interpreted as "experience goods", in the tradition of Jovanovic (1979, 1984). These models precisely assume that "the only way to determine the quality of a particular match is to form the match and experience it". In a high-firing-cost scenario, the introduction of FTCs would therefore provide employers with the adequate instrument for experiencing the quality of a match by means of hiring. Some matches are soon perceived to be productive enough to deserve an early upgrade of the correspondent FTC to a PC. Others are not, and therefore are destroyed. However, it can be noted that the screening period - while useful to both parties to find out about the value of a match - may also raise its initial value so long as the worker accumulates firm-specific human capital. Some matches that would not be successful ex-ante may therefore be made permanent after the accumulation of enough firm-specific human capital.

Second, even when the quality of a job-worker pair is observable before forming the match, employers can opt for FTCs as a more flexible option for adjusting employment in the face of adverse shocks to the firm, and as a cheaper factor of production. Jimeno and Toharia (1993) and De La Rica and Felgueroso (1999) find that temporary workers earn approximately 10% less than permanent ones, after controlling for observable personal and job characteristics.

We can in principle discriminate between different uses of FTCs by analysing the time pattern of the rate at which firms convert FTCs into PCs within the (legal) maximum spell of 3 years since their start. This reveals whether temporary employment is used as a genuine probation period that may end up in permanent employment at any time during the 3 years, or whether firms opt for permanent employment only when there is no other way to retain

[4]See also OECD (1993) for a discussion on this issue.

the worker. In the latter case we would observe that FTCs tend to be transformed into PCs towards the 3 years' duration limit. The technique that we use for this task is a duration model for temporary employment, with competing risks of flowing into permanent employment versus non-employment or a new spell of temporary employment, and sufficiently flexible duration dependence for the exit into permanent employment. This highlights the behaviour of the hazard during the whole duration of temporary employment.

The contribution of this paper to the existing literature is both methodological and related to the use of the EPA data base. We believe that the use of duration models best describes the dynamics of the transition process between temporary and permanent employment. Such models in fact exploit the potential strength of a cohort panel study, which is the possibility of being able to track individuals over time and observe exactly how long they take to make an employment change. Moreover, the use of individual information on individuals' characteristics that can be obtained from the EPA shows whether the prospect of permanent employment is shared among temporary workers, and to what extent there are some categories that are more likely than others to remain trapped in temporary jobs. The additional advantage related to the use of EPA data is the length of the period covered by the survey. We use data for the period 1987-1996, which allow us to assess how the 1994 reform has affected the conversion pattern of FTCs introduced ten years back.

The existing literature has not put both these things together. Existing contributions on renewal rates (see Toharia 1996 and Alba 1998) generally use logit models to analyse the determinants of the probability of receiving a PC, conditional on being initially hired on a temporary basis. Logit specifications may prove rather inflexible when applied to the analysis of the dynamic path of transition rates. Garcia-Fontes and Hopenhayn (1996) estimate a duration model of job tenure using the Social Security records. These data avoid the use of self-reported information on the duration of contracts, and therefore have the advantage of reducing measurement error, but on the other hand they provide very little information on worker characteristics, and do not allow to identify the temporary/permanent nature of the contract held.

The organisation of the paper is as follows. Section 2 describes our data set, extracted from the panel version of the EPA. Section 3 provides a discrete time duration model, that applies to worker transitions out of temporary employment. Section 4 presents our estimation results. Section 5 finally concludes. A detailed summary of the Spanish legislation on temporary employment is provided in the Appendix.

2 The data

The data used in this paper is drawn from the Spanish Labour Force Survey (Encuesta de la Población Activa), which is carried out every quarter on a sample of some 60,000 households. The EPA is designed to be representative of the total Spanish population, and contains very detailed information on labour force status of individuals. Each household can remain in the survey for a maximum of six consecutive quarters: each quarter a new cohort is selected, and one sixth of households leave the sample. Labour force transitions can be analysed by using the panel structure of the survey (*EPA enlazada*), available for all cohorts selected since 1987.

Our sample includes individuals belonging to cohorts that entered the survey between 1987:2 and 1996:3, covering (more than) a full cycle of the Spanish economy. We select all respondents who completed six quarterly interviews, and declared to hold a FTC in any of the interviews.

We disaggregate our observations in four broad sectors: agriculture, manufacturing, construction and services. Figure 2 shows that the share of seasonal contracts among all FTCs is only significant in agriculture.[5] An alternative way to check for any evident pattern of seasonality consists in looking at the absolute number of existing FTCs within each sector. This is illustrated in Figure 3: again, except in agriculture, seasonality does not play too strong a role in shaping the evolution of temporary employment.

In order to give a flavour of labour market transitions in our sample, Tables 1 and 2 report quarterly and yearly transition probabilities across three labour market states: non-employment, permanent employment, and temporary employment. Both tables display extremely strong persistence in the non-employment and the permanent employment states. As expected, the temporary employment category displays significant turnover, although most of such mobility represents reshuffling across FTCs, as shown in the bottom row of Table 2.

In our duration models, we concentrate on individual transitions out of the first FTC that is observed during the survey period. This leaves us with

[5] The plot refers to the sample period 1988:3-1996:3, in order to have 6 cohorts of workers present in the survey at all times.

118,197 temporary employment spells.

The duration of each contract is constructed using self-reported information from the various quarterly interviews. Given that no contract identifier is supplied, in order to follow each single FTC across interviews we rely on information concerning (i) the type of contract held; and (ii) the uncompleted duration of the present contract. The type of contract held can be permanent or fixed-term. The uncompleted duration of the present contract is expected to rise across interviews with calendar time, and to drop to zero whenever there is a contract switch. We therefore consider a spell of temporary employment as completed when either we observe a change in the type of contract or a drop in the uncompleted duration of the present contract.[6]

Roughly 65% of temporary employment spells that we observe started during the survey period. The remaining 35% started before the worker was selected for the survey, so that we need to condition on the length of temporary employment at the first interview date, using once more the information on the elapsed duration of the current contract that is reported at the first interview. The self-reported elapsed duration up to the interview date is measured in months if it is lower than one year, and in years otherwise. Whenever the reported elapsed duration is 1 year, this means anything between 4 and 7 quarters. Such data bunching problem could be eliminated by focusing only on entrants into temporary employment, that do not have any rounded measure of elapsed duration attached. However, this would only allow us to observe the time pattern of the conversion probability for at most six quarters of duration, and would leave us without any information on the behaviour of the hazard towards the legal duration limit of FTCs.

We therefore choose to exploit information on all spells, and correct for the data bunching problem in the following way. Any individual whose elapsed duration is 4 quarters or longer reports contract duration $\widetilde{j}$ which is a multiple of 4, and which implies a non-rounded duration $j \in \left\{\widetilde{j}, \widetilde{j}+1, \widetilde{j}+2, \widetilde{j}+3\right\}$. In our estimates we therefore replace each rounded elapsed duration $\widetilde{j}$ with a random draw from a uniform distribution with discrete support $\left\{\widetilde{j}, \widetilde{j}+1, \widetilde{j}+2, \widetilde{j}+3\right\}$. For comparison purposes, we also report estimates obtained by simply assign-

[6]We also computed the duration of fixed term contracts according to a more restrictive definition of a single spell. In particular, we considered a spell as completed when either (i) there is a change in the type of contract, or (ii) there is a drop in the uncompleted duration of the present contract, or (iii) there is a change in the sector where the worker is employed. No appreciable change was detected with respect to the definition given in the main text, which is the one we adopt in the empirical analysis reported here.

ing to each rounded duration (the integer of) its mean value, which means replacing $\widetilde{j} = 4, 8, 12$ with $j = 6, 10, 14$ respectively. As it will be illustrated below, the only difference between the two set of estimates is the presence of clear bunching spikes in the estimated baseline hazard obtained with the latter method.

Each spell of temporary employment can terminate with a new FTC, a PC, joblessness, or it can be censored if the worker is last observed holding the FTC at the sixth interview. The proportion of FTCs that terminated with a permanent renewal started at nearly 20% in 1988 and has declined monotonically until 1994 (7%), experiencing a very weak recovery thereafter, as depicted in Figure 4. These proportions look slightly lower than those computed in Toharia (1996, Table 4), although they follow exactly the same trend. It is worth noticing however that the renewal rates computed here refer to the proportion of workers that hold a FTC at some point in time and hold instead a permanent one at the next interview, *i.e.* direct transitions from temporary to permanent employment. Toharia (1996) computes instead the proportion of permanent workers that held a FTC one year back. We prefer to look at direct switches between two subsequent interviews because yearly renewals may conceal additional labour market transitions.

Given that we cannot use an employer identifier, we are not sure that new PCs observed in the survey are renewals of previous FTCs with the same employer, rather than newly-created jobs elsewhere in the economy. However, the fact that between 1986 and 1992 almost all (98%) new contracts registered at employment offices have been fixed-term would suggest that the vast majority of PCs that we observe in the survey are created through renewals of FTCs.

Table 3 reports the distribution of observed spells, according to their destination state. The figures reported suggest that, at relatively short durations, FTCs are more likely to end up into non-employment. As duration proceeds, the probability of non-employment decreases, while the chances of permanent employment increase. The table also shows evidence of some FTCs continuing beyond the legal limit of 3 years.

Explanatory variables included in our regressions are personal and family characteristics of the individuals such as gender, education, potential labour market experience,[7] marital status and number of dependent children. Year dummies (referring to the year of entry in the survey) are also included in

[7] Computed as age − years of schooling − 6.

order to capture any time pattern in renewal probabilities across the Spanish business cycle. Finally, industry dummies and the local unemployment rate (referring to the province of residence) should capture the effect of overall labour market performance (if any) on the renewal of contracts. Average sample values of these variables are reported in Table 4, for both the whole sample and each type of destination.

3 Econometric specification

The panel structure of the data set described requires a discrete time hazard function approach, as outlined in Narendranathan and Stewart (1993) and Jenkins (1995).

Suppose that the transition out of temporary employment is a continuous process with hazard

$$\theta_i(t) = \lambda(t) \exp\left(x_i{}'\beta\right), \tag{1}$$

where $\lambda(t)$ denotes the baseline hazard, x is a vector of time-invariant explanatory variables, and β is a vector of unknown coefficients. The discrete time hazard denotes the probability of a spell of temporary employment being completed by time $t+1$, given that it was still continuing at time t. The discrete time hazard is therefore given by

$$h_i\left(t\right) = 1 - \exp\left\{-\int_t^{t+1} \theta_i(u)du\right\} = 1 - \exp\left\{-\exp\left(x_i{}'\beta\right)\gamma\left(t\right)\right\} \tag{2}$$

where

$$\gamma\left(t\right) = \int_t^{t+1} \lambda(u)du \tag{3}$$

denotes the integrated baseline hazard. We do not specify any functional form for $\gamma\left(t\right)$, and estimate the model semiparametrically.

The (log) likelihood contribution of a spell of length d_i is

$$\begin{aligned} L_i &= c_i \ln h_i\left(d_i\right) + \sum_{t=1}^{d_i-1} \ln\left\{1 - h_i\left(t\right)\right\} \\ &= c_i \ln\Big(1 - \exp\Big[-\exp\Big\{x_i\left(d_i\right)'\beta + \gamma\left(d_i\right)\Big\}\Big]\Big) \\ &\quad - \sum_{t=1}^{d_i-1} \exp\left\{x_i{}'\beta + \gamma\left(t\right)\right\}, \end{aligned} \tag{4}$$

where c_i is a censoring indicator that takes the value 1 if d_i is uncensored and zero otherwise.

We assumed so far that we observe entrants into temporary employment. Assume instead from now on that we also observe spells of temporary employment that started before the survey period, and that we can use self-reported information to find out the quarter in which these spells begun. In order to avoid a stock sample bias, we need to condition transition rates on the length of temporary employment at the first interview date. Suppose that an individual i enters the survey after j_i quarters of temporary employment and holds the FTC for another k_i quarters, for a total duration $d_i = j_i + k_i$, that can be either censored or uncensored. The individual likelihood contribution is therefore

$$\begin{aligned} L_i &= c_i \ln h_i\left(j_i + k_i\right) + \sum_{t=j_i+1}^{j_i+k_i-1} \ln\left\{1 - h_i\left(t\right)\right\} \\ &= c_i \ln\left(1 - \exp\left[-\exp\left\{x_i'\beta + \gamma\left(j_i + k_i\right)\right\}\right]\right) \\ &\quad - \sum_{t=j_t+1}^{j_i+k_i-1} \exp\left\{x_i'\beta + \gamma\left(t\right)\right\}. \end{aligned} \tag{5}$$

The model outlined specifies the likelihood of a single risk: that of terminating fixed-term employment. As we will see below, FTCs can terminate with the conversion into a PC or alternative states. Given that we are interested in the first type of transition, we need to estimate a competing risk model, that distinguishes exit into permanent employment from exit into alternative states. It can be illustrated that the parameters of a given cause-specific hazard can be estimated by treating durations finishing for other reasons as censored at time of exit (see Narendranathan and Stewart, 1993). We therefore treat all temporary employment spells that end in a new FTC or in non-employment as censored at the time the first contract is terminated. Having said this, the semi-parametric hazard specification (5) used for the single-risk model can be applied for the permanent job hazard.

In what follows, the effect of possibly omitted regressors in the exit from fixed-term employment is controlled for by conditioning the hazard rate on an individual's unobserved characteristics, summarized into the variable v. The conditional (continuous time) hazard rate is then written as $\theta_i(t) = \lambda(t) \exp\left(x_i{}'\beta + v_i\right)$, with v_i independent of x_i and t. This specification therefore identifies the three sources of variation among individual hazard rates: the duration of the fixed-term contract (t), the observable differences among individuals (x) and the unobservable ones (v). However, in a competing risk framework, allowing for a random disturbance term in each of the cause-specific hazards requires an additional assumption, that imposes the independence of these disturbance terms across the cause-specific hazards.[8]

The unconditional hazard (that depends on observable regressors only) is obtained by integrating the conditional one over v, under the assumption that v is distributed as a Gamma variate of unit mean and variance σ^2.[9] Under these assumptions the likelihood is given by

$$\begin{aligned} L_i &= \ln \left[\left(1 + \sigma^2 \sum_{t=j_i+1}^{j_i+k_i-1} \exp \left\{ {x_i}' \beta + \gamma(t) \right\} \right)^{-1/\sigma^2} \right. \\ & \qquad \left. - c_i \left(1 + \sigma^2 \sum_{t=j_i+1}^{j_i+k_i} \exp \left\{ {x_i}' \beta + \gamma(t) \right\} \right)^{-1/\sigma^2} \right]. \end{aligned} \tag{6}$$

The baseline hazard can be estimated non-parametrically by maximising the log-likelihood $L = \sum_{i=1}^{n} L_i$ with respect to the $\gamma(t)$ terms, the vector β and the variance term σ^2. The vector of controls x_i includes a number of individual and job-related characteristics, that are treated as time invariant, and are measured at the start of the fixed-term contract (or at the time of the first interview if the contract had started previously).

4 Empirical results

We move on to estimating the econometric model outlined in Section 3, for the determinants of worker transitions from temporary to permanent employment. The results of our estimates are reported in Table 5. Two specifications of our regression equation are provided. In the first one we

[8] The alternative approach would be to assume perfect correlation (as opposed to zero correlation) between the cause-specific disturbance terms (see Narendranathan and Stewart, 1993, for a discussion of advantages and disadvantages of the two methods).

[9] See Lancaster (1979); see also Han and Hausman (1990) and Dolton and O'Neill (1996) for an application of Gamma-distributed unobserved heterogeneity to discrete time hazard models.

do not allow for unobserved heterogeneity among individuals. In the second one we control for the effect of possibly omitted regressors by allowing for a Gamma-distributed disturbance term.

The effect of several individual characteristics on renewal probabilities are fairly standard, and consistent with previous results obtained from logit estimates (see Alba, 1998). Column I of Table 5 shows that the probability of a permanent renewal is higher for males than females. This is probably explained by the fact that women tend to have weaker labour market attachment than men, and higher turnover, so that female employment may be perceived as relatively more risky from employers' point of view. Being married positively affects the probability of obtaining a permanent contract, while the number of children does not. It can also be noted that the probability of a permanent renewal increases monotonically with education but that only college education matters significantly. Also, it is enhanced by potential experience beyond 5 years of labour market attachment.

Industry dummies show that renewal rates are highest in services and lowest in construction. Time fixed-effects imply in turn a roughly monotonically decreasing trend in the proportion of FTCs being renewed on a permanent basis. This tendency is slightly reversed just after the 1994 reform. Finally, local unemployment has a positive and significant impact on conversion rates. It should be noted, however, that the unemployment coefficient switched sign when year dummies were dropped (results not reported), revealing a clear cyclical interaction between local unemployment and time fixed-effects.

The parallel estimation that controls for the effect of unobserved heterogeneity is represented in column II of Table 5. The positive and significant variance of the Gamma-distributed disturbance shows that there is some residual heterogeneity among individuals, which is not properly accounted for by included regressors. However, the partial effect of most regressors remains practically unchanged if compared with the case where no unobserved heterogeneity is accounted for, as does the global fit of the regression. In what follows we therefore do not allow for unobserved heterogeneity in our estimates.

The steps of the baseline hazard are reported in Table 6, and the corresponding predicted hazards are plotted in Figure 5 for our reference category (see notes to Table 5). Controlling for the presence of unobserved heterogeneity in regression II simply scales upward the whole hazard, as it is reasonable to expect, but hardly changes its overall time pattern. It can be noted that, with both specifications, the hazard has some spikes at durations around one, two and three years. We checked the significance of those spikes using a Wald test for the equality of adjacent baseline hazard steps. The resulting χ^2 statistics allowed to reject the hypothesis that the baseline hazard at 3 quarters of duration was equal to the one at 4 quarters[10]. All other adjacent steps were non-significantly different from one another.

This evidence on the baseline hazard may suggest that some FTCs are plausibly used as a screening device, and "successful" workers obtain a permanent renewal much before the legal limit. A spell of roughly one year seems in fact reasonable for adequately assessing the performance of a worker, and in order to retain those who pass the screening employers choose not to wait until the maximum legal limit of the contract. But there also seem to exist contracts that are only renewed upon expiry of the legal limit of three years: such contracts are probably used as a cheaper/more flexible option to adjust employment, and are only renewed when there is no other legal way to retain the worker. As we will see below, this use of FTCs is relatively more frequent for some categories of workers.

As we mentioned in Section 2, these results are obtained on a sample in which all our rounded elapsed durations $\widetilde{j}$ are replaced with random draws from a uniform distribution with discrete support $\left\{\widetilde{j}, \widetilde{j}+1, \widetilde{j}+2, \widetilde{j}+3\right\}$. For comparison purposes, we report in column I of Table 7 the results obtained by assigning to all rounded elapsed durations (the integer of) their mean values. This implies assigning to rounded previous duration of 4, 8 and 12 quarters the average value of 6, 10, 14 quarters respectively.

The effect of all regressors on conversion rates is almost identical to that found in Table 5. However, Figure 6 delivers additional spikes in the baseline hazard at durations of 7 and 11 quarters, respectively. These spikes are precisely the consequence of bunching in our reported durations. Among all the individuals who started their fixed-term employment spell before the first interview date, as much as 43% are observed to hold a FTC just in the first quarter they are interviewed, and to make a transition to a different contract - or to unemployment - in the following quarter. The total duration for these individuals is computed as 1 plus the "average" previous duration of the contract, which is itself 6, 10 or (in very few cases) 14. This finally implies that we are left with a considerable number of individuals that terminate a

[10] For the estimates without unobserved heterogeneity the wald test on H_0 : step3=step4 gave $\chi^2 = 29.5$, against the critical value $\chi^2(1, 0.05) = 3.84$. For the estimates with unobserved heterogeneity the correspondent value was $\chi^2 = 37.6$.

FTC at 7 and 11 quarters, these durations being the combination of 6 and 10 quarters of previous employment respectively, and one quarter of employment during the survey period. Such bunching phenomenon is clearly evident in our estimates.

In order to assess how serious the data bunching problem is, we report in the same Figure 6 the predicted hazard obtained on a subsample of individuals for whom the rounding problem does not apply, *i.e.* workers whose elapsed duration is equal to three quarters or lower.[11] This allows us to identify eight steps in the baseline hazard. As shown in Figure 6, the spike at 7 quarters disappears completely, while the one at 4 quarters remains. We should therefore interpret the spikes at 7 quarters (and, by the same token, at 11 quarters) as an effect of the rounding problem.

Given this bunching problem, all the estimates below are obtained on a sample in which the elapsed previous duration is corrected, as mentioned above.

We carry further tests in order to investigate two issues. First, we try to assess whether the 1994 reform has altered the renewal pattern of FTCs into PCs, and whether such an effect, if any, has affected some categories of workers more than others. The reform was in fact aimed at reducing the applicability of general FTCs and enhancing the renewal rates for labour market groups with supposedly poorer labour market prospects. We saw earlier that, despite the reform, the share of temporary employment did not fall after 1994 (see Figure 1). However, there was a slight increase in the proportion of FTCs being converted into permanent ones (see Figure 4). It is therefore interesting to document this trend, and check whether such overall tendency conceals diverging patterns for different labour market segments.

Second, we separately estimate renewal rates for different categories of workers over the whole sample period, in order to check for differences in the whole time pattern of renewals, and not simply in their levels.

We start, therefore, by splitting our sample in the following way. The first sub-sample includes cohorts that entered the survey between 1987:2 and 1992:3; the second includes cohorts that entered between 1995:1 and 1996:3. Cohorts entering between 1992:4 and 1994:4 are left out because it is not clear which legislation applies to their contracts. The results are presented in Tables 9 and 10, and the corresponding predicted hazards are plotted in Figure 7.

[11] The corresponding estimates are reported in Column II of Tables 7 and 8.

Table 9 clearly shows that permanent renewal prospects of women and less educated workers have improved after 1994: the female dummy switches sign in regression II, as do education dummies. Targeting subsidies to the renewal of contracts for women and the less-skilled seems in fact to have been effective in enhancing their prospects of accessing permanent employment. Also, conversion rates after 1994 have deteriorated in construction, falling below those in agriculture. The time pattern of renewals is also affected after the 1994 reform. Interestingly, before 1994 the most evident spike is the one at 3 years of durations, which becomes less important in the following period. After 1994, the most important spike becomes the one at 1 year. It seems therefore that the 1994 reform has successfully affected the use of FTCs in the sense of inducing employers to earlier renewals.

The existence of different spikes in the renewal hazard, and the consequent interpretation in terms of alternative uses of FTCs, leads us to estimate renewal probabilities for different categories of workers, defined over their gender or educational attainment. This should in fact reveal whether FTCs provide effective screening devices rather than simply cheaper hirings for some categories of workers more than for others.

Some gender differences in renewal rates are detected in Table 11. Human capital accumulation through formal education or work experience matters more for males than females, as do family variables like marital status and the number of dependent children. It seems moreover that, after 1994, renewal rates keep falling for males, while improving for females. Once more, we can detect the effects of the 1994 reform. Another interesting piece of information is delivered in Table 12 and Figure 8, which show that the one-year and two-year spikes in renewal rates are relatively more pronounced for females than males, and the opposite happens for the three-year spike. If anything, this suggests that the screening use of FTCs applies more to female than male employment. Given low participation rates and high turn-over of Spanish women, a temporary employment spell may in fact be used by employers in order to assess the degree of labour market attachment of their female employees.

No substantial differences across educational groups are instead detected, according to the estimates of Table 13. As expected, the baseline hazard at all durations is higher for educated workers than for the less-skilled, as depicted in Figure 9. Moreover, the one-year spike is relatively more important than the three-year spike for the more educated. Screening and early renewal for successful workers therefore plausibly applies to the skilled rather than the less-skilled.

5 Conclusions

This paper examined the determinants and the timing of the conversion of FTCs into PCs in Spain. This work was motivated by the observation of a massive use of general FTCs since their introduction in 1984, and by their relatively low conversion rates into PCs. The analysis was led in the context of a duration model for temporary employment, with flexible duration dependence for the permanent employment hazard, using longitudinal EPA data for the period 1987:2 and 1996:3.

The main focus of the paper was to investigate other reasons why firms opt for temporary hirings than for covering jobs whose underlying nature is temporary, as has been typically the case for Spain before the 1984 reform.

We find that the shape of the baseline hazard is suggestive of two possible uses of FTCs by employers. The fact that there are important spikes at durations around 1 year is supporting the idea that FTCs are used as a screening device. That is, "successful workers" obtain permanent renewals much before the legal limit of their contracts. In other words, good matches are retained with a permanent status as soon as their quality is revealed to employers. This use of FTCs seems to apply more to women than to men and to skilled workers rather than the less-skilled.

At the same time, there is evidence of another pronounced spike in the hazard at 3 years, coinciding with the maximum legal duration of FTCs. This suggests that some employers only opt for permanent hirings when there is no other way to retain the worker. In other words, FTCs just provide a cheaper option for adjusting their employment level.

Finally, we investigated the effects of the 1994 reform, aimed at limiting the applicability of general FTCs and enhancing their conversion into PCs. According to our results, this reform has been rather ineffective in reducing the incidence of temporary hirings. However, as far as the conversion process is concerned, we find that permanent renewal prospects of women and less educated workers have improved after 1994. Furthermore, after 1994, early spikes are more pronounced than the spike at the legal limit.

Table 1: Quarterly transitions across labour market states.

		quarter $t+1$			
		NE	PC	new TC	same TC
quarter t	NE	96.64	0.67	2.69	
	PC	1.94	95.12	2.94	
	TC	18.24	6.49	16.9	58.37

Notes. Transition rates are computed according to the distribution of individuals across labour market states at quarter $t+1$, conditional on their status at quarter t. Source: EPA.

Table 2: Yearly transitions across labour market states.

		year $t+1$			
		NE	PC	new TC	same TC
year t	NE	93.67	1.33	5.00	
	PC	7.35	89.47	3.18	
	TC	26.39	10.83	49.91	12.87

Notes. Transition rates are computed according to the distribution of individuals across labour market states at quarter $t+4$, conditional on their status at quarter t. Source: EPA.

Table 3: The duration distribution of fixed-term contracts, by state of exit.

duration (quarters)	NE	PC	new TC	same TC	Total No. of spells
1	52.56	12.10	12.85	22.49	34599
2	37.44	8.97	36.02	17.56	27790
3	28.85	9.54	45.38	16.24	18113
4	20.44	11.23	49.96	18.36	12079
5	17.29	10.86	49.53	22.32	7218
6	19.02	14.11	33.38	33.49	3628
7	15.78	13.11	30.70	40.41	2883
8	18.72	14.62	31.36	35.29	2318
9	20.17	20.36	31.65	27.82	1542
10	21.90	25.49	32.74	19.88	1283
11	20.69	24.49	33.11	21.71	1184
12	19.63	26.64	29.31	24.42	1085
13	15.23	25.77	28.98	30.01	873
14 and over	15.82	5.86	29.18	49.14	3602
Total No. of spells	40863	13434	38031	25869	118197

Notes. Each row sums to 100, with each entry giving the probability to exit into any of the four states, conditional on the contract duration. All our rounded elapsed durations $\widetilde{j}$ are replaced with random draws from a uniform distribution with discrete support $\left\{\widetilde{j}, \widetilde{j}+1, \widetilde{j}+2, \widetilde{j}+3\right\}$. Source: EPA.

Table 4: Sample characteristics of temporary workers.

	Total sample	NE	PC	new TC	same TC
female	39.77	43.11	39.31	35.18	41.47
primary ed. or below	38.06	43.75	38.05	34.54	34.31
secondary education	52.90	49.48	50.60	56.80	53.71
university education	9.94	6.77	11.35	8.66	11.98
pot. exp. 0-4 yrs	32.34	32.30	27.54	32.03	35.33
pot. exp. 5-15 yrs	31.28	28.42	35.01	34.61	28.96
pot. exp. 16+ yrs	36.38	39.28	37.45	33.36	35.71
married	41.28	41.79	44.89	40.42	39.89
Average No. of kids	0.81	0.86	0.84	0.76	0.78
agriculture	11.22	19.12	6.24	7.23	7.18
manufacturing	19.45	16.29	20.62	22.67	19.13
construction	17.33	17.00	14.70	18.23	17.88
services	51.99	47.59	58.42	51.86	55.81
Average unemp. rate	19.88	18.67	19.40	20.54	19.70
Total No. of spells	118197	40863	13434	38031	25869

Notes. All entries (except the average number of kids and the average unemployment rate) indicate the percentage of workers with a given characteristic in the sample. Source: EPA.

Table 5: Maximum likelihood estimates of the transition from temporary to permanent employment: Full sample.

	I		II	
	Coef.	(Std. err.)	Coef.	(Std. err.)
female	-0.030	(0.019)	-0.032	(0.023)
secondary education	0.019	(0.023)	0.022	(0.028)
university education	0.176	(0.033)	0.203	(0.041)
pot. exp. 5-15 yrs	0.186	(0.023)	0.212	(0.028)
pot. exp. 16+ yrs	0.242	(0.031)	0.290	(0.037)
married	0.089	(0.023)	0.093	(0.028)
number of kids	0.009	(0.009)	0.013	(0.011)
manufacturing	0.227	(0.040)	0.243	(0.047)
construction	-0.136	(0.041)	-0.172	(0.048)
services	0.368	(0.038)	0.415	(0.044)
year 1988	0.097	(0.043)	0.144	(0.054)
year 1989	-0.106	(0.041)	-0.093	(0.052)
year 1990	-0.297	(0.042)	-0.323	(0.052)
year 1991	-0.304	(0.044)	-0.382	(0.054)
year 1992	-0.562	(0.044)	-0.657	(0.054)
year 1993	-0.648	(0.047)	-0.759	(0.057)
year 1994	-0.760	(0.048)	-0.893	(0.058)
year 1995	-0.665	(0.046)	-0.781	(0.056)
year 1996	-0.787	(0.049)	-0.911	(0.059)
unemployment rate	0.127	(0.022)	0.155	(0.026)
σ^2	–		1.242	(0.096)
mean log-likelihood	-0.370		-0.370	
No. of cases	118197		118197	

Notes. Reference category: male, not married, with pot. exp.<5 yrs, less than secondary education, employed in agriculture, entered survey in 1987. Standard errors in brackets. Source: EPA.

Table 6: Baseline hazard estimates: Full sample.

	I		II	
quarters	Coef.	(Std. err.)	Coef.	(Std. err.)
1	0.125	(0.008)	0.138	(0.011)
2	0.105	(0.007)	0.125	(0.010)
3	0.102	(0.007)	0.132	(0.011)
4	0.127	(0.009)	0.174	(0.015)
5	0.124	(0.009)	0.183	(0.016)
6	0.119	(0.009)	0.178	(0.017)
7	0.116	(0.009)	0.170	(0.016)
8	0.119	(0.010)	0.167	(0.016)
9	0.130	(0.011)	0.180	(0.018)
10	0.120	(0.010)	0.174	(0.018)
11	0.129	(0.011)	0.186	(0.019)
12	0.139	(0.012)	0.202	(0.021)
13	0.124	(0.011)	0.190	(0.021)
14 and over	0.118	(0.011)	0.185	(0.021)

Notes. The estimates report the steps of the baseline hazard, according to regressions I and II of Table 5. Standard errors in brakets. Source: EPA.

Table 7: Maximum likelihood estimates of the transition from temporary to permanent employment: Full sample (with bunched elapsed duration) and subsample with elapsed duration ≤ 3 quarters.

	I		II	
	Full sample		Subsample with Elapsed dur. ≤ 3	
	Coef.	(Std. err.)	Coef.	(Std. err.)
female	-0.030	(0.019)	-0.015	(0.022)
secondary education	0.025	(0.023)	0.041	(0.026)
university education	0.187	(0.034)	0.214	(0.038)
pot. exp. 5-15 yrs	0.185	(0.024)	0.212	(0.026)
pot. exp. 16+ yrs	0.248	(0.031)	0.297	(0.034)
married	0.090	(0.023)	0.110	(0.026)
number of kids	0.009	(0.009)	0.001	(0.010)
manufacturing	0.218	(0.041)	0.252	(0.043)
construction	-0.143	(0.042)	-0.139	(0.045)
services	0.362	(0.038)	0.395	(0.041)
year 1988	0.099	(0.043)	0.196	(0.049)
year 1989	-0.104	(0.042)	-0.007	(0.048)
year 1990	-0.299	(0.042)	-0.208	(0.049)
year 1991	-0.311	(0.045)	-0.312	(0.052)
year 1992	-0.566	(0.044)	-0.519	(0.050)
year 1993	-0.647	(0.047)	-0.595	(0.054)
year 1994	-0.760	(0.048)	-0.723	(0.054)
year 1995	-0.663	(0.047)	-0.619	(0.052)
year 1996	-0.780	(0.050)	-0.746	(0.056)
unemployment rate	0.129	(0.022)	0.147	(0.025)
mean log-likelihood	-0.366		-0.339	
No. of cases	118197		104872	

Notes. Reference category: male, not married, with pot. exp.<5 yrs, less than secondary education, employed in agriculture, entered survey in 1987. Standard errors in brackets. Source: EPA.

Table 8: Baseline hazard estimates: Full sample (with bunched elapsed duration) and subsample with elapsed duration $\leq$ 3 quarters.

	I		II	
	Full sample		Subsample with Elapsed dur. $\leq$ 3	
quarters	Coef.	(Std. err.)	Coef.	(Std. err.)
1	0.125	(0.008)	0.113	(0.008)
2	0.103	(0.007)	0.093	(0.007)
3	0.102	(0.007)	0.093	(0.007)
4	0.127	(0.009)	0.115	(0.009)
5	0.120	(0.009)	0.108	(0.009)
6	0.100	(0.009)	0.090	(0.008)
7	0.143	(0.011)	0.075	(0.009)
8	0.113	(0.009)	0.098	(0.017)
9	0.093	(0.009)	–	
10	0.068	(0.007)	–	
11	0.167	(0.013)	–	
12	0.136	(0.012)	–	
13	0.100	(0.010)	–	
14 and over	0.066	(0.007)	–	

Notes. The estimates report the steps of the baseline hazard, according to regressions I and II of Table 7. Standard errors in brakets. Source: EPA.

Table 9: Maximum likelihood estimates of the transition from temporary to permanent employment: 1987-1993 and 1995-1996.

	I		II	
	1987-1993		1995-1996	
	Coef.	(Std. err.)	Coef.	(Std. err.)
female	-0.077	(0.023)	0.124	(0.050)
secondary education	0.055	(0.028)	-0.022	(0.061)
university education	0.315	(0.041)	-0.237	(0.086)
pot. exp. 5-15 yrs	0.185	(0.028)	0.210	(0.063)
pot. exp. 16+ yrs	0.261	(0.038)	0.116	(0.081)
married	0.105	(0.028)	0.105	(0.059)
number of kids	0.004	(0.011)	0.005	(0.027)
manufacturing	0.325	(0.049)	0.086	(0.113)
construction	0.083	(0.050)	-0.704	(0.119)
services	0.462	(0.046)	0.188	(0.105)
year 1988	-0.006	(0.042)	–	
year 1989	-0.211	(0.041)	–	
year 1990	-0.398	(0.041)	–	
year 1991	-0.415	(0.043)	–	
year 1992	-0.639	(0.044)	–	
year 1993	–		–	
year 1994	–		–	
year 1995	–		–	
year 1996	–		-0.064	(0.045)
unemployment rate	0.106	(0.025)	0.085	(0.067)
mean log-likelihood	-0.434		-0.277	
No. of cases	64235		24792	

Notes. Reference category: male, not married, with pot. exp.<5 yrs, less than secondary education, employed in agriculture, entered the survey in 1987 (regression I) or in 1995 (regression II). Standard errors in brackets. Source: EPA.

Table 10: Baseline hazard estimates: 1987-1993 and 1995-1996.

	I		II	
	1987-1993		1995-1996	
quarters	Coef.	(Std. err.)	Coef.	(Std. err.)
1	0.131	(0.009)	0.055	(0.009)
2	0.094	(0.007)	0.066	(0.010)
3	0.094	(0.007)	0.063	(0.010)
4	0.107	(0.008)	0.101	(0.017)
5	0.100	(0.008)	0.122	(0.022)
6	0.102	(0.009)	0.103	(0.020)
7	0.101	(0.009)	0.077	(0.017)
8	0.101	(0.010)	0.093	(0.020)
9	0.114	(0.011)	0.117	(0.027)
10	0.112	(0.011)	0.080	(0.019)
11	0.118	(0.012)	0.087	(0.020)
12	0.133	(0.013)	0.077	(0.019)
13	0.127	(0.013)	0.046	(0.013)
14 and over	0.121	(0.013)	0.049	(0.013)

Notes. The estimates report the steps of the baseline hazard, according to regressions I and II of Table 9. Standard errors in brakets. Source: EPA.

Table 11: Maximum likelihood estimates of the transition from temporary to permanent employment: Males and Females.

	I		II	
	Males		Females	
	Coef.	(Std. err.)	Coef.	(Std. err.)
secondary education	0.105	(0.029)	-0.129	(0.038)
university education	0.327	(0.046)	-0.016	(0.050)
pot. exp. 5-15 yrs	0.234	(0.031)	0.124	(0.036)
pot. exp. 16+ yrs	0.273	(0.042)	0.186	(0.047)
married	0.130	(0.031)	0.029	(0.035)
number of kids	0.021	(0.012)	-0.005	(0.015)
manufacturing	0.233	(0.045)	0.285	(0.098)
construction	-0.144	(0.044)	0.321	(0.147)
services	0.338	(0.043)	0.484	(0.093)
year 1988	0.173	(0.053)	-0.031	(0.072)
year 1989	-0.084	(0.052)	-0.143	(0.068)
year 1990	-0.284	(0.053)	-0.315	(0.068)
year 1991	-0.262	(0.056)	-0.359	(0.072)
year 1992	-0.617	(0.056)	-0.476	(0.070)
year 1993	-0.676	(0.061)	-0.599	(0.075)
year 1994	-0.781	(0.061)	-0.718	(0.077)
year 1995	-0.733	(0.059)	-0.555	(0.074)
year 1996	-0.867	(0.064)	-0.668	(0.078)
unemployment rate	0.118	(0.029)	0.134	(0.035)
mean log-likelihood	-0.377		-0.360	
No. of cases	71193		47004	

Notes. Reference category: not married, with pot. exp.$<$5 yrs, less than secondary education, employed in agriculture, entered survey in 1987. Standard errors in brackets. Source: EPA.

Table 12: Baseline hazard estimates: Males and Females.

	I		II	
	Males		Females	
quarters	Coef.	(Std. err.)	Coef.	(Std. err.)
1	0.112	(0.009)	0.130	(0.016)
2	0.094	(0.008)	0.104	(0.014)
3	0.091	(0.008)	0.107	(0.014)
4	0.109	(0.009)	0.142	(0.019)
5	0.107	(0.010)	0.139	(0.019)
6	0.101	(0.010)	0.137	(0.020)
7	0.102	(0.010)	0.124	(0.019)
8	0.110	(0.011)	0.118	(0.018)
9	0.110	(0.012)	0.147	(0.022)
10	0.108	(0.012)	0.122	(0.019)
11	0.116	(0.013)	0.132	(0.020)
12	0.132	(0.014)	0.131	(0.020)
13	0.114	(0.013)	0.125	(0.020)
14 and over	0.113	(0.013)	0.111	(0.018)

Notes. The estimates report the steps of the baseline hazard, according to regressions I and II of Table 11. Standard errors in brakets. Source: EPA.

Table 13: Maximum likelihood estimates of the transition from temporary to permanent employment: High and Low education.

	I		II	
	High education		Low education	
	Coef.	(Std. err.)	Coef.	(Std. err.)
female	-0.054	(0.023)	0.032	(0.034)
pot. exp. 5-15 yrs	0.161	(0.026)	0.158	(0.066)
pot. exp. 16+ yrs	0.237	(0.038)	0.193	(0.066)
married	0.125	(0.029)	0.074	(0.037)
number of kids	0.001	(0.013)	0.005	(0.012)
manufacturing	0.007	(0.066)	0.303	(0.054)
construction	-0.334	(0.071)	-0.043	(0.052)
services	0.151	(0.064)	0.481	(0.049)
year 1988	0.078	(0.057)	0.114	(0.064)
year 1989	-0.078	(0.054)	-0.154	(0.064)
year 1990	-0.292	(0.055)	-0.314	(0.065)
year 1991	-0.274	(0.057)	-0.353	(0.069)
year 1992	-0.540	(0.056)	-0.590	(0.070)
year 1993	-0.717	(0.061)	-0.546	(0.073)
year 1994	-0.755	(0.061)	-0.775	(0.077)
year 1995	-0.648	(0.059)	-0.707	(0.075)
year 1996	-0.769	(0.062)	-0.820	(0.082)
unemployment rate	0.160	(0.028)	0.079	(0.035)
mean log-likelihood	-0.373		-0.366	
No. of cases	73216		44981	

Notes. High education: with secondary education or above. Reference category: male, not married, with pot. exp.<5 yrs, employed in agriculture, entered survey in 1987. Standard errors in brackets. Source: EPA.

Table 14: Baseline hazard estimates: High and Low Education.

	I		II	
	High education		Low education	
quarters	Coef.	(Std. err.)	Coef.	(Std. err.)
1	0.164	(0.015)	0.118	(0.012)
2	0.140	(0.013)	0.092	(0.010)
3	0.146	(0.014)	0.085	(0.009)
4	0.192	(0.019)	0.095	(0.011)
5	0.169	(0.017)	0.113	(0.013)
6	0.164	(0.017)	0.107	(0.013)
7	0.153	(0.017)	0.111	(0.015)
8	0.177	(0.020)	0.091	(0.013)
9	0.181	(0.021)	0.113	(0.016)
10	0.173	(0.020)	0.099	(0.014)
11	0.183	(0.021)	0.111	(0.016)
12	0.189	(0.022)	0.128	(0.018)
13	0.176	(0.022)	0.108	(0.016)
14 and over	0.172	(0.022)	0.100	(0.015)

Notes. The estimates report the steps of the baseline hazard, according to regressions I and II of Table 13. Standard errors in brakets. Source: EPA.

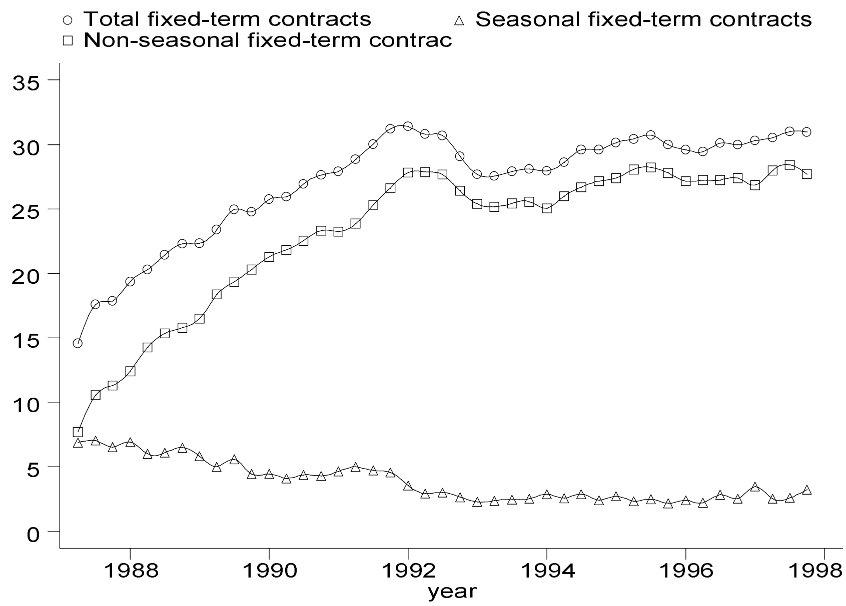

Figure 1: The share of fixed-term contracts (%) in total employment, 1987-1997. Source: EPA.

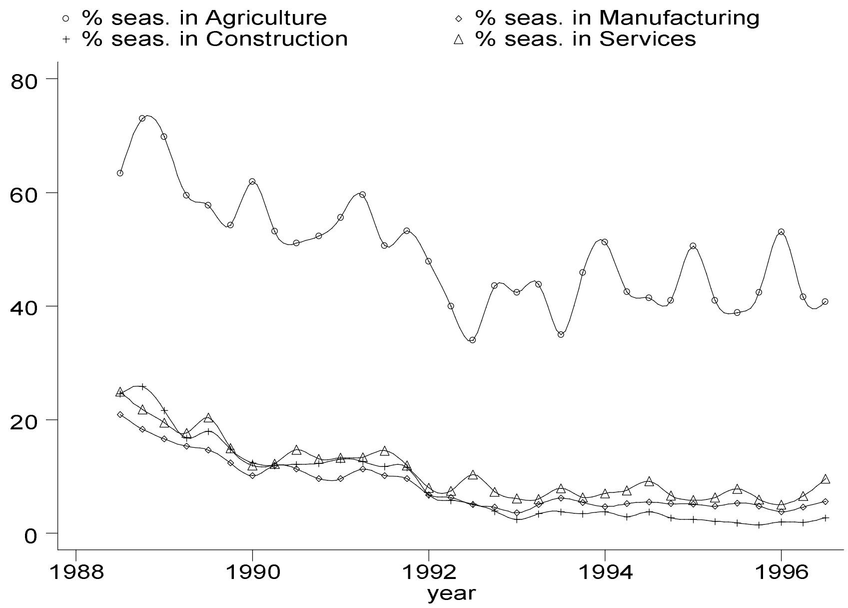

Figure 2: The share of fixed-term contracts in four broad sectors. Source: EPA.

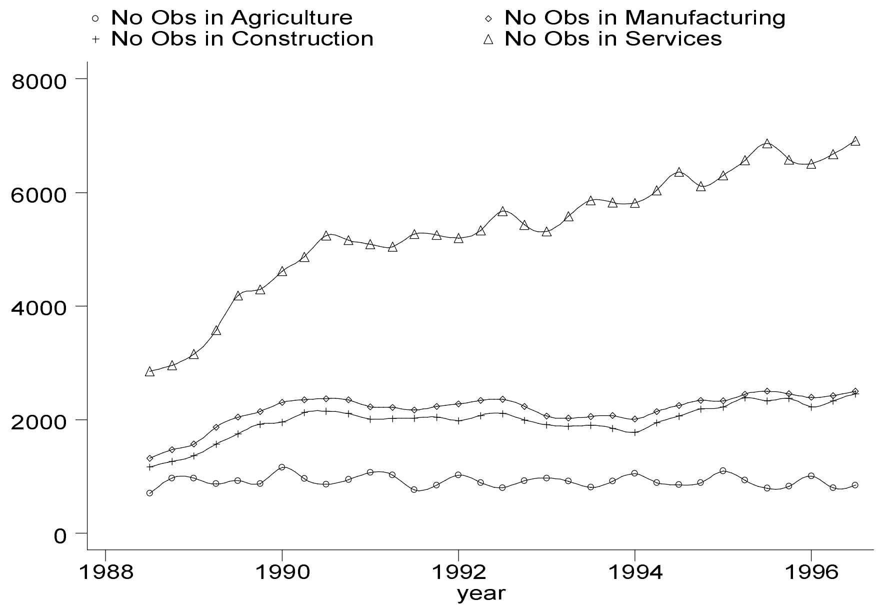

Figure 3: The number of existing fixed-term contracts, by sector. Source: EPA.

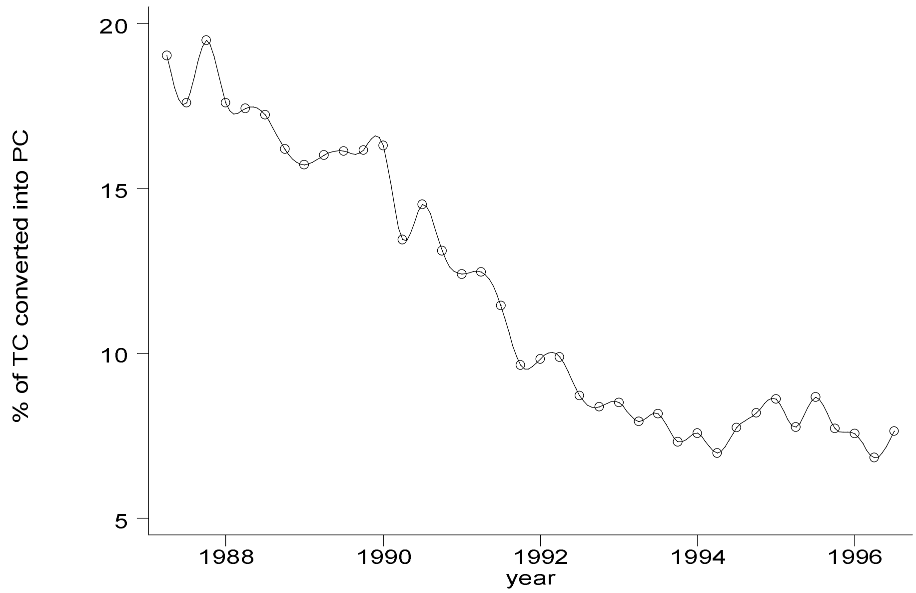

Figure 4: The proportion of fixed-term contracts being converted into permanent ones. Source: EPA.

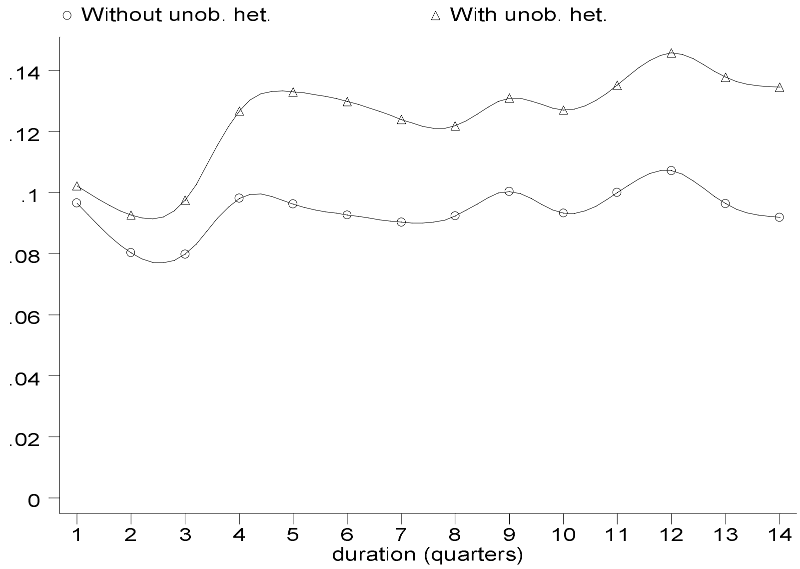

Figure 5: Predicted hazard for the reference worker in the full sample (See Tables 5 and 6).

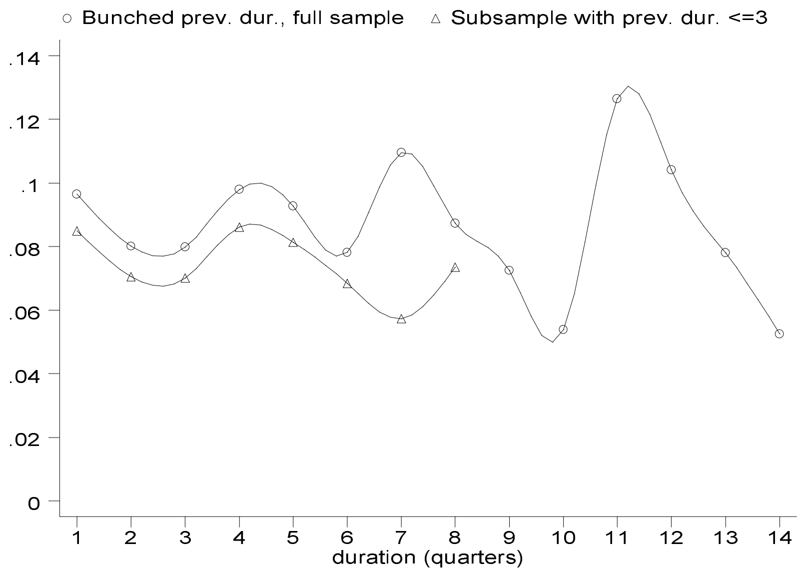

Figure 6: Predicted hazard for the reference worker in the full sample (with bunched elapsed duration) and in the subsample with elapsed duration ≤ 3 quarters (See Tables 7 and 8).

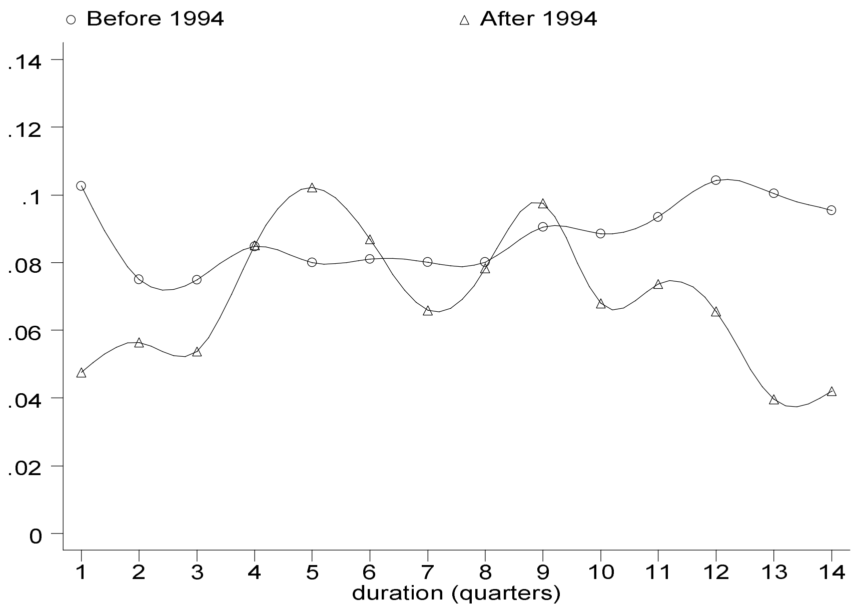

Figure 7: Predicted hazard for the reference worker: Before 1994 and after 1994 (See Tables 9 and 10).

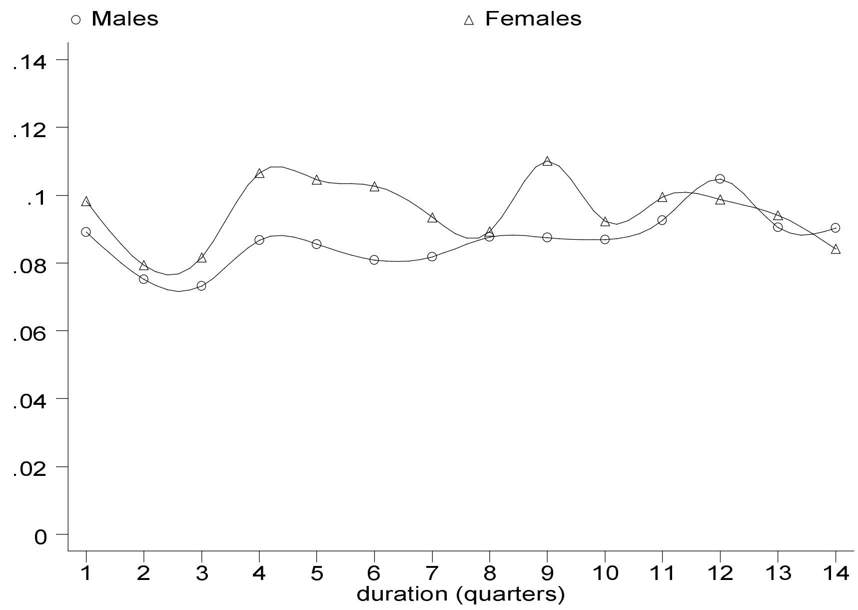

Figure 8: Predicted hazard for the reference worker: Males and Females (See Tables 11 and 12).

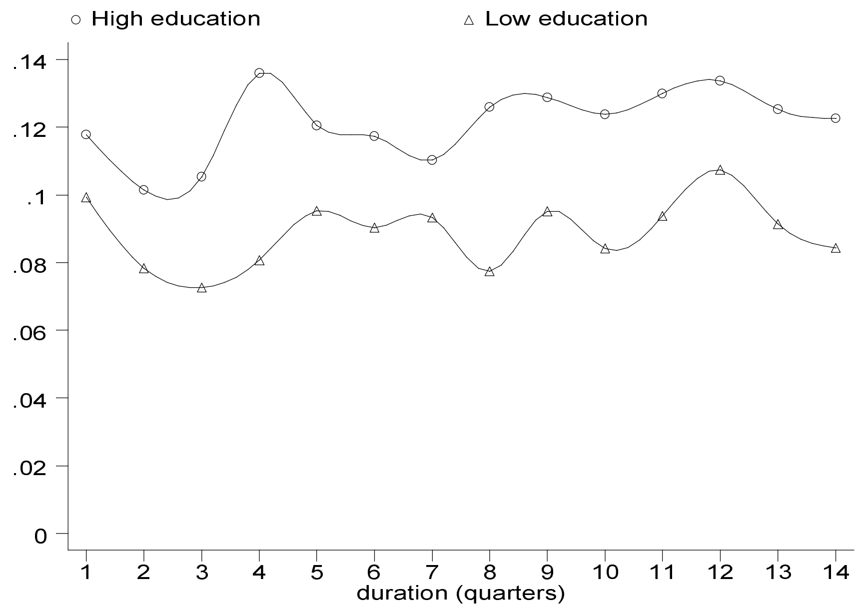

Figure 9: Predicted hazard for the reference worker: High and Low Education (See Tables 13 and 14).

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Appendix: The institutional background

Current legislation regarding labour contracts is contained in the Worker's Statute (*Estatuto de los Trabajadores*, ET) of 1980 which has since been modified on three occasions with the 1984, the 1994 and 1997 reforms. The ET of 1980 established priority to contracts of indefinite duration and allowed FTCs only for jobs which were temporary in their nature (like for particular projects, *eg* building, or seasonal jobs, *eg* tourism). Other situations in which FTCs were allowed was for eventual increases of demand or replacement of a permanent worker in case of absence or temporary suspension of contract. It also established the possibility for the Government to use FTCs as an incentive to promote employment. In other words, except in this last situation, a specific cause was generally required in order to sign a FTC ("causal" FTC).

The 1984 reform exploits this last possibility in an extreme way. It introduces flexibility by extending the applicability of FTCs. After the reform, any worker can be hired on a temporary basis without the requirement of a specific cause. This implies that for any job, employers can freely choose between a PC or a FTC.

There are two main types of "non-causal" FTCs: a general one (*contratos temporales de fomento del empleo*) and a specific one for youths. This second category includes training employment contracts (*contratos en prácticas*) and apprenticeship contract (*contratos para la formación*).

FTCs can be characterised according to: *i)* required conditions for workers and firms in order to sign them, *ii)* limits on their duration and *iii)* indemnities at their termination.

(i) Required conditions for workers and firms in order to sign the contract:

For general FTCs, workers that can sign it must not have exhausted the maximum limit of FTC duration (3 years) with one or several employers. If a worker has already been employed on a FTC for this limit period, she needs to wait 12 months in order to be eligible for a new one. Similarly, for firms this limit binds for a given vacancy, in the sense that they cannot fill the same vacancy for more than 3 years with one (or several) temporary worker(s). Also, firms cannot hire a temporary worker if they have reduced their workforce for objective reasons or dismissals declared unfair (see below in Section *iii)* in the previous 12 months[12].

[12] In practice, it is difficult to assess whether these rules have been effectively enforced.

The above principle also applies to FTCs for youths. In training employment contracts, workers are also required to have a secondary school qualification or higher, obtained within the previous 4 years. Apprenticeship contracts are designed for people between 16 and 20 years old, who do not hold the qualification required in the former contract.

(ii) Limits on duration

General FTCs can be signed for a minimum of 6 months, and FTCs for youths for a minimum of 3 months[13]. All of them have a maximum duration of 3 years. The contract cannot be renewed at the end of the duration limit with a new FTC for the same job, and it is not possible to transfer the worker to a different job within the firm without signing a PC. Upon expiry, the firm can therefore choose to retain the worker by offering her a regular contract of undetermined duration. Otherwise, the job-worker pair needs to be split and the position is destroyed.

(iii) Indemnities at termination

In this paragraph it is useful to introduce first the regulation of dismissals for PCs, in order to assess the change that the introduction of FTCs imply in this domain.

It is possible to distinguish three different types of (individual) dismissals within the ET regulation. First, there are disciplinary dismissals, in which the worker is fired without right to indemnities. Second, there are objective dismissals, for legally authorised reasons like lack of adjustment of the worker to the job, recurrent justified absence from work or technological changes. In this case the worker has the right to a severance payment of 10 days' wage per year of seniority, with a maximum of one year's wage. Last, there are redundancies, *ie* legally authorised dismissals for jobs eliminated for economic or technological reasons. In this last category, prior notice of 30 days is required and workers have the right to an indemnity of 20 days' wage per each year worked, with a maximum of 12 months' wage.

The worker always has the right to sue the employer if she disagrees with the dismissal case. Once the case is taken to court, it can be declared "fair" or "unfair". In the first case, the worker is fired without the right to any indemnity. In the second case, the worker has the right to indemnities of 45

[13]In 1992, this minimum was changed to 1 year for any FTC.

days' wage per year worked, with a maximum of 42 months' wage. She may also be recalled in the same job, but this hardly occurs.

If redundancy dismissals involve a high enough number of workers, it is considered a collective dismissal and requires advance approval by the Government's labour inspectorate[14]. These cases are negotiated between employers and workers' representatives. Their agreement is important for administrative approval. When an agreement is reached, administrative approval is automatic and indemnities on dismissal are similar to individual ones for "fair" cases. When there is no agreement, it is more difficult to obtain the approval, in which case indemnities are the same as on individual "unfair" cases.

The 1984 reform leaves the legal position of permanent workers unaffected, but makes it substantially easier to hire workers on a temporary basis. Upon expiry, employers have to pay an indemnity of 12 days' wage per year worked for general FTCs, while no severance payment is imposed upon expiry of training or apprenticeship contracts. Most importantly, in no case does the worker have the right to sue the employer for unfair dismissal.

Ten years after this major reform, the Spanish labour market had become highly segmented without any important reduction in unemployment. Unions and some political parties criticised the introduction of FTCs and their effects in terms of labour market segmentation. Consequently, there was a new reform in 1994 which put forward specific limits to the use of FTCs. The application of general FTCs was restricted to specific categories of workers (over 45 years of age, disabled, or long term unemployed). The minimum and maximum limits for FTCs for youth were changed to 6 months and 2 years, respectively. Also, subsidies and incentives to the creation of FTCs for youths were cut, and replaced by others that would promote the conversion of FTCs into PCs. Also, subsidies were introduced to promote the such conversion for workers older than 45 years old, women in professions or jobs where they are underepresented, and for disabled people.

Finally, the 1997 reform once more tried to implement new measures that would correct the excessive precarious employment situation created since 1984. As in the previous reform, subsidies to promote the transition from FTCs to PCs were agreed. And, more importantly, a new typology of PC was introduced, targeted at "protected categories" of workers (people younger

[14]For firms that employ less than 100 workers, it has to affect at least 10 workers; for firms that employ between 100 and 300 employers, it has to affect at least 10% and for bigger firms, at least 30 workers.

than 30, older than 45, long-term unemployed, and disabled workers), and carrying lower firing costs than existing ones. The effects of this last reform are not analysed in this work because there is no data available for the most recent years.