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UNEMPLOYMENT DURATION, BENEFIT DURATION AND THE BUSINESS CYCLE*

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In this paper we study the effects of unemployment benefit duration and the business cycle on unemployment duration. We construct durations for individuals entering unemployment from a longitudinal sample of Spanish men in 1987–94. Estimated discrete hazard models indicate that receipt of unemployment benefits significantly reduces the hazard of leaving unemployment. At durations of three months, when the largest effects occur, the hazard for workers without benefits is twice as large as that for workers with benefits. Favourable business conditions increase the hazard of leaving unemployment. At sample-period magnitudes, this effect is significantly smaller than that of benefit receipt.

Do unemployment benefits lead to longer unemployment spells? We would expect this to be the case, since individuals will be more selective concerning job offers the larger their out-of-work income. Moreover, under certain conditions, standard job search theory predicts that increases in either the amount or the length of unemployment benefit lengthens the duration of unemployment. Nevertheless, the effects of benefits on unemployment duration compound labour supply and demand forces, so that their magnitude is an empirical issue; indeed one which has been debated for long in the economics literature.

In this paper we investigate the effect of receiving unemployment benefits on the unemployment duration of male workers in Spain over the period 1987–94, using a longitudinal Labour Force Survey (LFS). We do so by comparing the exit rates of workers with and without benefits given unemployment duration, holding demographic and other variables constant. This is a meaningful exercise because our data have the fundamental virtue of providing exogenous variation across workers in the receipt of benefits. This is an important difference between our data and other data previously used in the empirical literature measuring effects of benefit entitlement. Often unemployed workers without benefits are a self-selected minority with special characteristics (e.g. seasonal workers) and the effects of benefits are captured through marginal variation in the length of benefit entitlement.

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On the contrary, in our data, almost one-half of workers enter unemployment without benefits. Moreover, the benefit–non-benefit division is close to a random assignment resulting from the Spanish labour market reform of 1984. This reform created a country-wide natural experiment by producing a new type of unemployed worker without any benefits at all, that co-existed with otherwise similar workers enjoying generous benefit entitlements.

Before 1984, the use of fixed-term contracts was restricted by law to certain activities (like seasonal jobs or other temporary activities) and the only contracts available to firms for regular jobs were open-ended contracts with high firing costs. After the reform of 1984, firms were allowed to hire workers on fixed-term (e.g. three-month) contracts, with low or no firing costs, for any kind of activity. The main restriction was that these contracts could only be renewed for a certain number of times, after which the temporary worker was either hired permanently on the old type of contract or dismissed.

At the time of the reform, the unemployment rate was over 20%. From then on, most new hirings were under low-cost fixed-term contracts, and temporary workers were typically dismissed at the end of the maximum contract length, to avoid transfer to the high-cost permanent contracts. Thus, the typical pre-sample job history of a prime-age male entering unemployment without benefits in our sample would be a sequence of short-term temporary contracts starting at some point after the 1984 reform, itself preceded by a permanent job that had ended, often due to a collective dismissal. The number of collective dismissals reached a peak in 1985, before the beginning of our sample, and a new peak in 1993, towards the end of our sample. So, both before and during our sample period, there were intervals of intense destruction of permanent jobs, that for the individuals concerned were followed by the start of a sequence of temporary jobs.

The jobs available to the unemployed in our sample were essentially fixed-term only, regardless of whether they were receiving benefits, or whether their previous job was temporary or permanent. Fig. 1 shows the gap between the pre- and post-reform shares of hirings under fixed-term contracts. Most prime-age workers in our sample must have held and lost a permanent job in their labour history. As a result, employers would not view workers with more unstable job histories as being less reliable than those with more stable histories. In a way, in this paper we are comparing the (benefit covered) spell following the loss of a permanent job with subsequent (non-covered) spells in between temporary jobs.[1]

Other relevant features of our data are as follows. First, the data set is large, which allows us to concentrate on unemployment entrants, whose information we expect to be more reliable than retrospective information. Second, we observe individuals' labour market status for up to six consecutive quarters, so that a large proportion of unemployment spells are not censored. Third, our sample period spans a full business cycle of the Spanish economy, enabling us to take into account changes in aggregate conditions. As a drawback, we observe unemployment

[1] However, if the absence of benefits were associated with particular characteristics that made workers less employable, we would expect this to cause a downward bias in the measured effect of benefits on exit rates. For this reason, we also consider a version of the estimated model which allows for unobserved heterogeneity that is correlated with benefits.

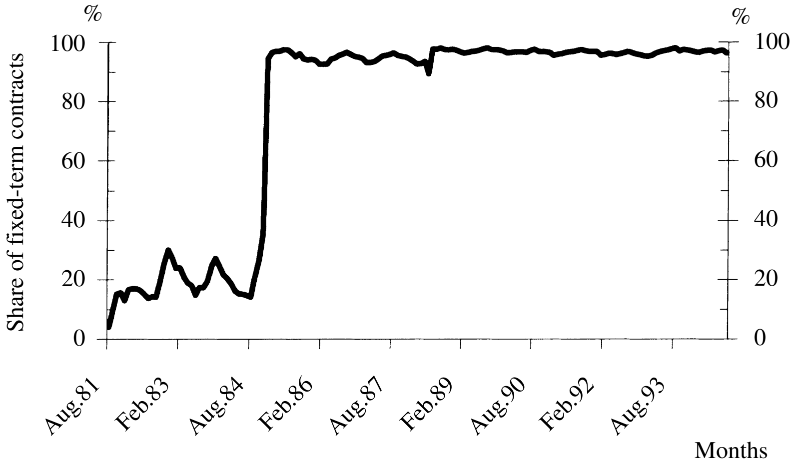

Fig. 1. *Share of Fixed-Term Contracts in Total Hiring in Spain*

benefit receipt but not the benefit amounts. Moreover, the length of benefit entitlement is a censored variable, since it is only observed if both benefits are exhausted before the end of the unemployment spell and the individual still remains in the sample at that time.

The existing empirical evidence from US and UK microeconomic data shows relatively small estimated effects of benefit amounts on average unemployment duration.[2] With regard to benefit length, the more telling evidence is the presence of spikes in the exit rate from unemployment around the time of benefit exhaustion; see, for example, Katz and Meyer (1990) for the United States.[3] While having a sample of entrants over the business cycle helps us overcome some of the problems often encountered in cross-sectional duration analysis (i.e. stock sampling and short time spans), those studies focus on different issues from ours and so their results are not readily comparable to ours. The key differences are as follows.

First, we observe monthly spells only, which implies that our data are uninformative on exit rates at the very short durations (e.g. weeks) that are often analysed. Monthly exit rates are nevertheless quite relevant for a country like Spain, where the unemployment rate rose from 16% of the labour force at the beginning of our sample period to a staggering 24% at the end, and durations

[2] Typical estimates for the United States imply that a 10% increase in the amount of benefits would lengthen average duration by 1–1.5 weeks (Moffit and Nicholson (1982) and Meyer (1990) respectively). For the United Kingdom the increase is estimated between 0.5 and 1 week (Narendranathan *et al.* (1985) and Lancaster and Nickell (1980) respectively). See Atkinson and Micklewright (1991) for a survey.

[3] For Spain, a number of studies using cross-section data from a 1985 Ministry of Finance survey have found positive effects of imputed benefit eligibility (actual receipt being unobserved) on duration: Alba-Ramirez and Freeman (1990), Ahn and Ugidos (1995), and Blanco (1995), while Andrés and García (1993) only find an effect when sectoral dummies are excluded. Also, Cebrián *et al.* (1995) find a spike in the exit rate in the last three months of benefit receipt – with data on recipients in 1987–92 – though it is steep only for workers with entitlements up to nine months. The latter three studies find small effects of the replacement ratio on the hazard of leaving unemployment.

were also extremely long: in 1994, 56% of the unemployed had been such for more than a year. Second, we cannot estimate the effects of benefit levels (or family income) on exit rates, because they are not observed. This omission may not be so crucial, however, since both Gritz and MaCurdy (1989) and Katz and Meyer (1990) find that benefit duration has significantly greater impact on unemployment duration than benefit levels. Third, while the data allow us to measure the effect of receiving benefits on exit rates, we cannot calculate the impact of a given benefit duration on average unemployment duration, without making very restrictive assumptions. This is due to our observing the presence of benefits while unemployed, but not entitlement length. We can make robust comparisons of exit rates for workers with and without benefits, but we cannot reconstruct the distribution of durations for a given entitlement without making further assumptions.[4] Last, a major objective of this paper is to study the effects of business cycle conditions on exit rates and to compare them with benefit effects, something we can afford owing to the time span of our dataset.[5]

Concerning econometric methods, we estimate logistic discrete hazard models by maximum likelihood. We specify both duration dependence and calendar time effects in a flexible way. Moreover, we treat benefits as a predetermined but not strictly exogenous variable in the hazard model. This is motivated by the fact that knowledge about benefit receipt at future durations can be expected to have an effect on current exit rates. We also consider an extended version of the model allowing for unobserved heterogeneity that is correlated with benefits. In doing so, we discuss the implications of introducing unobserved heterogeneity in discrete duration models with predetermined variables. We proceed by specifying a reduced form process for benefits and by maximising a joint mixture likelihood for the unemployment and benefit durations. The estimates of the model with unobserved heterogeneity do not alter our main empirical conclusions in any significant way.

The paper is structured as follows. Section 1 briefly presents the predictions of standard search theory about the effects of unemployment benefits on unemployment exit rates. Section 2 describes both the relevant features of Spanish labour market institutions and our database. Section 3 discusses the empirical models and econometric techniques, and Section 4 presents the empirical results. Section 5 contains a discussion of the results and our conclusions.

1. Theoretical Framework

1.1. *Unemployment Duration and Benefits*

Economic theory predicts that, under certain conditions, both higher levels and longer periods of unemployment benefits lower the hazard of leaving unemployment, and therefore result in higher unemployment duration.

[4] We do adopt one set of such assumptions at the end of the paper, so as to show estimated effects of benefits on median unemployment durations.

[5] A few papers, like Meyer (1990) or Imbens and Lynch (1993), also provide estimates of business cycle effects.

The standard framework for analysing this issue is well known, as contained for example in Mortensen (1977). The representative worker is assumed to maximise the present value of his lifetime utility, which depends on income and leisure. Income when employed is equal to the wage, and to benefits when unemployed. Benefits are received as long as the worker has been laid off from a job and has not reached the maximum benefit duration (which depends on past employment history). There is a stationary distribution of wage offers (jobs) and workers' search activity is represented as random draws from that distribution. The probability of leaving unemployment is the product of the probability of receiving an offer times the probability of accepting it. It is affected, among other things, by the worker's decision variables: search intensity and the reservation wage. On the one hand, the probability of receiving an offer is proportional to the intensity of search. On the other hand, the worker's optimal decision rule is to accept any wage offer above a certain reservation wage level.

Three key results concerning benefits emerge in this set-up. First, as exhaustion of benefits draws nearer, search intensity rises and the reservation wage falls, so that the hazard increases. Second, when benefits are exhausted, the hazard rate jumps to a higher level (as long as income and leisure are strict complements in utility), remaining constant thereafter. Third, an increase in the amount or the maximum duration of unemployment benefits raises the opportunity cost of search, thereby leading to a reduction in the hazard. This *disincentive effect* of benefits may be countered by an *entitlement effect:* an increase in benefits increases the expected utility from future, as opposed to current, unemployment spells with benefits. Thus, for a currently unemployed worker without benefits, an increase in the benefit level or duration raises the exit rate from unemployment (i.e. employment becomes more valuable because it gives right to now-enhanced future benefits). Since future events are discounted for both uncertainty and time preference reasons, we expect this to be a second-order effect for workers with benefits.

Later work has relaxed some of the assumptions in the standard model described above, leading to qualifications of the predictions regarding benefits (Atkinson and Micklewrigh, 1991). For example, receipt of unemployment benefits may permit an increase in the resources devoted to search by liquidity constrained individuals, thereby leading to increased hazards. Therefore the prediction of a disincentive effect of benefits may be partially or totally offset for certain individuals or periods by entitlement or other effects, and assessing this becomes an empirical question.

1.2. *Duration, the Business Cycle, and Hysteresis*

Search theory does not provide an unambiguous prediction on the sign of the relationship between the business cycle and unemployment duration. Higher growth raises the probability of receiving a job offer, but it also tends to increase reservation wages.[6] Empirical work has not resolved the issue either. For example,

[6] However, Burdett (1981) shows that a sufficient condition for higher job availability reducing expected unemployment duration is a 'log-concave' probability density function of wage offers.

with US data, Meyer (1990) finds that a higher state unemployment rate raises the hazard rates of unemployment benefit claimants, while Imbens and Lynch (1993) find that a higher local unemployment rate lowers the hazard rates of young unemployed workers. The latter paper is one of the few that uses a long period sample. Thus, firmer conclusions may be reached as more work is done on longer samples, like the one exploited in this paper.

Business cycle effects on individual unemployment duration are typically captured in empirical work by variables like GDP growth or the unemployment rate (in levels and/or rates of change). Recent research has pointed out a new channel through which the change in unemployment would affect unemployment duration, the so-called hysteresis effects. An increasing unemployment rate may reduce a worker's chances of re-employment more the longer his duration is if, as suggested by Layard *et al.* (1991, p. 365), it raises the share of recently unemployed workers in the total pool of the unemployed and these workers are more attractive to employers than the longer-term unemployed. This *ranking* behaviour of firms, proposed by Blanchard and Diamond (1994), could arise, for example, if human capital loss increases with unemployment duration. We explore these issues empirically for our sample of Spanish men.

2. Institutional Features and Data Description

2.1. *Institutional Features*

2.1.1. *The Unemployment Benefit System in Spain.* As in most European countries, unemployment benefits in Spain are of two types (details and a calendar of reforms appear in Appendix 1, Tables A1–3). The unemployment insurance system (UI, *Sistema contributivo*) pays benefits to workers who have previously contributed when employed. They must have been dismissed from a job held at least for one year. The replacement ratio is currently equal to 70% of the previous wage during the first six months of unemployment and 60% thereafter, subject to a floor of 75% of the minimum wage and to ceilings related to the number of dependants. Benefit duration is equal to one-third of the accumulated job tenure over some years prior to unemployment, with a maximum duration of two years. The system's generosity was reduced in 1992 and again in 1993.

The unemployment assistance system (UA, *Sistema asistencial*) grants supplementary income to workers who have exhausted UI benefits or who do not qualify for receiving them, with dependants, and whose average family income is below 75% of the minimum wage. It pays precisely that amount, for up to two years. From 1989 onwards, more generous conditions were granted to workers aged 45 or older, and benefits were extended until retirement age for workers aged 52 or older who qualify for retirement except for their age. The system was made more generous in 1992, but less generous in 1993. Last, there are special UA benefits for temporary agricultural workers in the Southern regions of Andalucía and Extremadura. Workers receive 75% of the minimum wage for 90–300 days within the year – depending on their age and number of dependants – provided they have been employed for at least 40 days (20 days if they were in the system already in 1983).

Going beyond the institutional setting, the actual coverage of unemployment benefits has increased in our sample period, from 35% of the unemployed in 1987 to 55% in 1993, with a trend decline in the share of UI in the total, from 54% to 50% over the same period (Toharia, 1997). For the population we analyse in this paper, men between 20 and 64 years old, the coverage is larger and the proportion of workers on UI is slightly lower (for instance around 67% and 48%, respectively, for the 20–59 year-old group).

We have no direct information on the level of income enjoyed by the unemployed. We can nevertheless provide some related, aggregate information from the Labour Force Survey. In 1987:II, the first period in the sample we use below, 21% of households had at least one member unemployed and, in 7% of them, all members were unemployed. As to heads of household, they represented 22% of the unemployed, 4.5% of the unemployed were heads of households in which no other member was employed, and 5.7% of the unemployed were heads of households in which no other member was receiving labour income (i.e. was either employed, receiving benefits or receiving a pension).

2.1.2. *Fixed-term Labour Contracts.* An important institutional change affected unemployment duration in Spain within our sample period. The Spanish labour market reform of 1984 allowed fixed-term contracts to be used for any kind of activity, temporary or otherwise. They could be signed for six months (one year, since April 1992) up to three years. They entailed lower severance pay than permanent contracts: nil, for some contract types, and 12 days of wages per year of service for other types, as opposed to 20 days if a permanent employee's dismissal is either not challenged or else ruled *fair* in court, and to 45 days if ruled *unfair.* This labour reform caused swift increases in the share of fixed-term contracts in hiring, as shown in Fig. 1, and in the temporary employment rate (as a share of employees), which rose from 15% in 1987 to 34% in 1994. The latter rate is slightly lower among men (32% in 1994), higher among youth (58% for those aged 20–29), and higher in agriculture and construction (around 58%) than in manufacturing and services (around 28%). A direct consequence of this change has been an increase in labour turnover rates. We estimate the impact of temporary employment on unemployment outflow rates in Section 4.

2.2. *The Data*

The data we use come from the rotating panel of the Spanish Labour Force Survey (*Encuesta de Población Activa: Estadística de Flujos,* EPA). The EPA is conducted every quarter on all members of around 60,000 households. One sixth of the sample is renewed quarterly; hence we can observe the labour market situation of an individual for up to six quarters. Some retrospective questions such as, for example, how long the individual has been in the current job or looking for a job, are also asked.

The EPA started in its current form in 1987:II and we use the waves up to 1994:III. These 30 quarters span a complete cycle of the Spanish economy. This data set therefore has two important features. First, we can observe *entrants* into unemployment, whose information we expect to be more reliable than retrospective information. Second, we observe them over an extended period of time. This allows us to study the influence of personal characteristics, in particular of benefit duration, taking into account changes in aggregate conditions, so that we can assess the relative importance of these factors.

The unemployed are asked each quarter whether they are receiving any unemployment benefits (without distinguishing between UI and UA). From their answers, we construct a duration of benefits variable, which is a censored entitlement to benefits variable since it only coincides with entitlement for workers with unemployment duration longer than benefit duration. We cannot construct accurate estimates of benefit entitlement duration from our data set. Contributory benefit entitlement depends on accumulated job tenure over the 6 years prior to becoming unemployed (4 years, before 1992), but the EPA provides information only about tenure in the latest job, which would be a poor guide to benefit entitlement. Morever, entitlement to UA benefits depends on a mix of tenure, age and family characteristics, so that estimates of benefit entitlement duration would be very noisy. There is no information on benefit amounts, and these cannot be imputed either, because no wage information is available in the EPA.

In contrast to the cross-sectional EPA, the rotating panel – as currently released – only includes individuals over 16 years of age and does not provide information on region of residence or family situation, except for marital and head-of-household status. Given this fact, we focus on men, since for understanding married women's behaviour it is particularly important to know the labour market situation of their husband and the number and age of their children. We also exclude from our sample men aged 16 to 19 years old, given the instability of their attachment to the labour market, and men aged 65 or older, due to the importance of transitions to retirement at those ages. This leaves us with men aged 20 to 64.[7]

Our initial sample included 1636,094 men. After filtering the sample (see Appendix 2), we obtain 60,036 unemployment spells of which 27,382 are for entrants into unemployment, i.e. people actually interviewed during the quarter in which their spell started. Of those entrants, only 1.37% are individuals without previous work experience. Since these are a tiny group for which sectoral variables are not available, they are excluded from the sample in the econometric estimation. Sample frequencies of individual variables are provided in Table A4. Given the focus of the paper, a cross-tabulation of certain individual characteristics and benefit receipt is provided in Table 1. In our sample, recipients are older, less educated, more often heads of household, and less often service sector workers than non-recipients.

We consider as unemployed a broader group than the one defined by the standard LFS definition. We exclude those individuals we take as being genuinely out of the labour force, namely those who declare themselves as either being out of the labour force throughout the observed period, being a full-time student, or having no work experience and not to be looking for a job. We include as being unemployed those classified to be out of the labour force during some quarters,

[7] The aggregate unemployment rate of men aged 20 years old or more, over the period 1987–94, was 14%.

Table 1

Frequencies of Individual Variables According to Benefit Receipt (%)

	Receiving benefits	Not receiving benefits
Age		
Age 20–29	37.26	45.19
Age 30–44	33.64	28.07
Age 45–64	29.10	26.74
Education		
Primary education or less	63.88	58.63
Secondary education	33.75	37.95
University education	2.37	3.42
Head of household status		
Head of household	57.24	47.71
Not head of household	42.76	52.29
Economic sector at previous job		
Agriculture	22.17	20.86
Construction	31.10	27.30
Industry	19.86	17.38
Services	26.88	34.45

which is not unreasonable having excluded women. Thus the transitions we look at are always from unemployment (or non-employment) to employment, rather than to non-participation.

2.3. *A First Look at Empirical Hazards, the Business Cycle, and Benefits*

We can gain a first impression of the influence of the business cycle on the probability of leaving unemployment by examining the evolution over time of the sample probability of finding a job. Namely, for each quarter, we evaluate the ratio of the number of individuals who find a job during that quarter to the total number of unemployed at the beginning of the quarter. This probability is displayed in Fig. 2, which shows that such probability mimics the pattern of Spanish economic activity, as captured by the quarterly growth rate of GDP, though it lags the movements in the aggregate unemployment rate.

Turning to the effect of benefits, for the reasons discussed above we now restrict the sample to include only individuals who are observed when entering unemployment. To examine this issue, we look at empirical hazards. The empirical hazard for a given number of months is the proportion of individuals unemployed for at least that number of months who find employment in exactly that number of months.

In Fig. 3, we represent the hazards for workers receiving and not receiving benefits. The latter includes workers who never received benefits and also those who received them at some point, but for a period shorter than the unemployment spell length under consideration.[8] Up to the ninth month of unemployment,

[8] Empirical hazard rates for workers who never received benefits only (not shown) are very similar to the no-benefits line in Fig. 3.

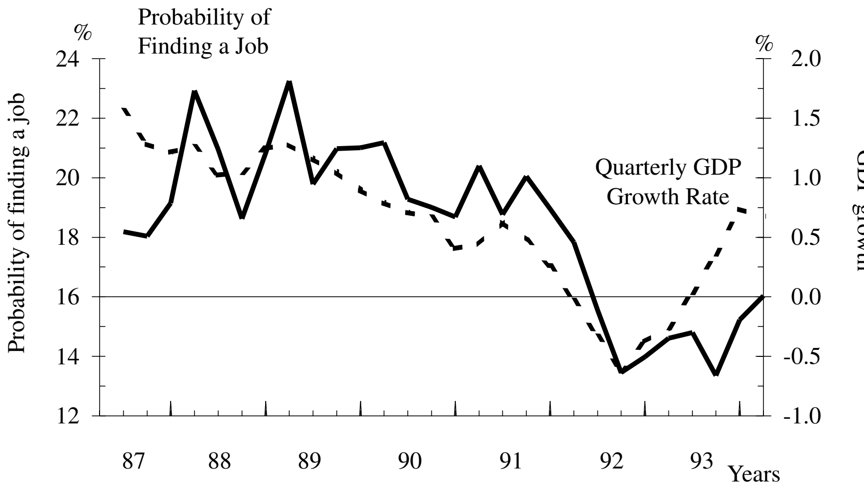

Fig. 2. *Probability of Finding a Job and GDP Growth*

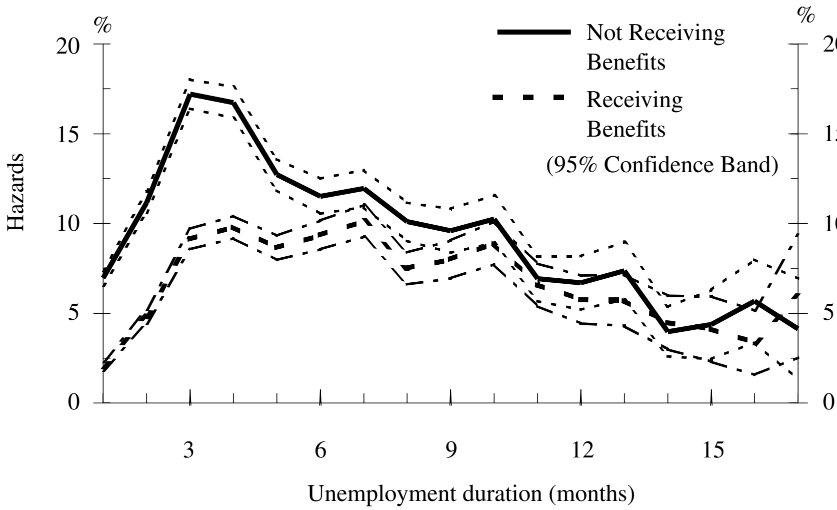

Fig. 3. *Empirical Hazards and Benefits*

individuals not receiving benefits have a significantly higher hazard than those receiving benefits, and markedly so during the first five months. In addition, we present in Fig. 4 the hazards for the group of men aged 30–44, without a university degree, and previously employed in the construction sector. This is a relatively homogeneous group, hence the comparison of the two hazard lines provides more robust evidence of the effect of benefits. As Fig. 4 shows, for the first six months of the unemployment spell, the difference between the hazards for workers with and without benefits is large. For example, an individual without benefits who has remained unemployed for at least three months has a probability of leaving

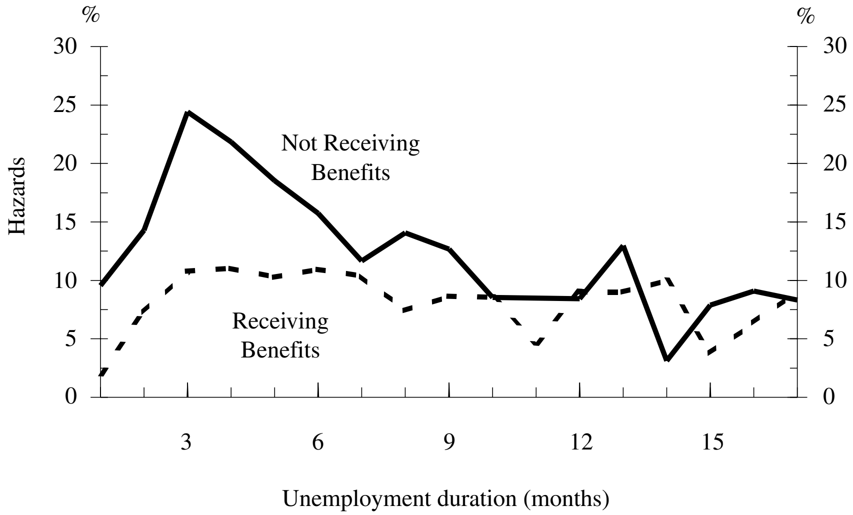

Fig. 4. *Empirical Hazards and Benefits*
(Age 30–44, Construction, Non-University Education)

unemployment during his third month of unemployment of 25%, as opposed to only 11% for a comparable individual receiving benefits.

A feature of the data revealed by Figs 3 and 4 is that the difference between the two empirical hazard lines (associated with a certain characteristic, in this case receiving *versus* not receiving benefits) is not constant. As a result, it will be advisable to allow for interactions between duration dependence and benefit status in the specification of the empirical models in the next section.

The observed decreasing pattern in aggregate hazards (as in Fig. 3) is partly due to the aggregation of groups of individuals with different exit rates. Once we estimate an econometric model controlling for personal characteristics, we shall be able to separate out effects on the hazards due to observed heterogeneity from those due to a combination of genuine state dependence and unobserved heterogeneity (such as variation in family income or unobserved human capital).

3. Empirical Models and Econometric Techniques

3.1. *Basic Models*

The individuals in our data set are asked for up to six consecutive quarters whether they are employed, and how many months they have been in the current state. They are also asked whether they are currently receiving unemployment benefits. From this information, we can construct complete or incomplete unemployment durations (in months) for individuals *entering* unemployment at the time of the first interview or later. Individuals who abandon the sample are supposed to do so at the end of the quarter covered by the interview. This allows us to calculate monthly empirical hazards on the basis of complete durations of entrants and surviving non-censored samples for up to 17 months. We can also construct the duration of benefit entitlement for individuals whose unemployment duration exceeds their benefit duration. Otherwise, we only observe the event that benefit entitlement is at least as long as unemployment duration. In our analysis, we treat unemployment duration (T) and benefit entitlement duration (B) as discrete random variables that are subject to censoring. Unemployment duration is right censored when the individual is still unemployed at the time of leaving the sample. Benefit entitlement duration has a different type of censoring since its observability depends on it being shorter than unemployment duration.

Let C be the number of periods the individual is in the sample after entering unemployment. In our database, C is at least 2 quarters but not larger than 6 quarters. We observe T if $T < C$; otherwise we only observe the event that $T \geq C$. Moreover, we observe B if $B < T < C$. We assume that T and B are independent of C, which is not an unreasonable assumption, given the rotating nature of our panel.

This observational plan motivates us to use, as the basis for our empirical analysis of the relationship between T and B, the hazard functions

$$\phi_0(t) = P(T = t \mid T \geq t, B < t)$$
$$\phi_1(t) = P(T = t \mid T \geq t, B \geq t)$$

The function $\phi_0(t)$ gives the probability of being unemployed for exactly t months relative to the group of individuals who have been unemployed for at least t months and do not receive benefits at t. On the other hand, $\phi_1(t)$ gives a similar probability for individuals who are unemployed for t periods or more, but are still receiving benefits at t.

The comparison between $\phi_0(t)$ and $\phi_1(t)$ provides a basis for studying a meaningful effect of B on T because both probabilities are conditional on being unemployed for t periods. In effect, regression or correlation analysis between T and B would be difficult to interpret. The reason is that the limitation in time of benefit entitlement creates an association between being on benefits and observing shorter unemployment durations which is unrelated to the effect of substantive interest.

To clarify the nature of our analysis, let us discuss how we would proceed if we could observe benefit entitlement for all workers. If entitlement were not a censored variable at $B \geq T$, the following conditional hazard functions would be identified for any entitlement s:

$$h(t, s) = P(T = t \mid T \geq t, B = s)$$

In our dataset, $h(t, s)$ is identified for $s < t$ but not for $s \geq t$. For example, with $B = 3$, $h(1, 3)$, $h(2, 3)$, and $h(3, 3)$ are not identified. So we cannot observe how the hazard rate for workers with benefits changes as the time of benefit exhaustion approaches.

To examine the relationship between the benefit effects that we can identify and the hazard rates conditional on benefit entitlement, notice that $\phi_0(t)$ and $\phi_1(t)$ are linear combinations of $h(t, s)$, such that we can write

$$\phi_0(t) - \phi_1(t) = \sum_{s=0}^{t-1} h(t, s) w_{ts} - \sum_{s=t}^{S} h(t, s) w'_{ts}$$

where S is the maximum value of B, and w_{ts} and w'_{ts} are proper weights given by

$$w_{ts} = \frac{P(B = s \mid T \geq t)}{P(B < t \mid T \geq t)}$$

and

$$w'_{ts} = \frac{P(B = s \mid T \geq t)}{P(B \geq t \mid T \geq t)}$$

Suppose that $h(t, s) = \phi_0(t)$ for $s < t$, but $h(t, s)$ increases monotonically as t approaches s for a given $s \geq t$ (i.e. the hazards for workers with and without benefits begin to approach each other before benefit exhaustion, as the former change their behaviour in anticipation of the arrival of the exhaustion date). In such a case,

$$\phi_0(t) - \phi_1(t) = \Sigma_{s=t}^{S}[\phi_0(t) - h(t, s)] w'_{ts}$$

can be regarded as a weighted average of the differences in hazards between those without benefits and those with benefit entitlements greater than t. Thus, our empirical difference will underestimate the difference in hazards for those whose benefit exhaustion is sufficiently ahead of t, and overestimate it for the rest. Moreover, as t increases, the observed differences will be closer to the actual differences for the relevant levels of benefit entitlement.

A simple but restrictive specification under which knowledge of $\phi_0(t)$ and $\phi_1(t)$ suffices to determine $h(t, s)$ is to assume that, at any t, there are only two possible hazard rates depending on whether individuals receive benefits, for example because there are only two search intensities. In other words:

$$h(t, s) = \begin{cases} \phi_1(t) & \text{for} \quad s \geq t \\ \phi_0(t) & \text{for} \quad s < t \end{cases}$$

This *two-regime* hazard model is a restricted version of the standard model described in Section 1. The latter predicts that, for two individuals with benefits at a given t, the one with shorter benefits has a greater hazard than the one with longer benefits, whereas the former model assumes that the two are equal. This assumption is not testable, though, because we do not observe B for individuals with $B \geq T$.

Given the two-regime model, it is possible to reconstruct the conditional distributions of unemployment durations for a given level of benefit entitlement. In effect, we have

$$P(T > t \mid B = s) = \prod_{k=1}^{t} [1 - h(k, s)] \quad t = 1, 2, \ldots$$

from which we can, for example, calculate the median unemployment duration for a given value of B, or changes in median duration from a change in benefit entitlement:

$$\Delta(s) = \text{med}(T \mid B = s + 1) - \text{med}(T \mid B = s)$$

However, the distributions $\{T \mid B = s\}$ are not identified nonparametrically in our data, and they can only be identified owing to a functional form assumption like the two-regime model. Therefore, we shall emphasise in our empirical analysis the modelling of $\phi_0(t)$ and $\phi_1(t)$, for which we have direct counterparts in the data. At the end of the paper, we shall, nevertheless, also present estimates of changes in median duration arising from changes in benefit entitlements.

A minor point is that, in our empirical analysis, we redefine $\phi_0(t)$ as

$$\phi_0(t) = P(T = t \mid T \geq t, B < t - 2)$$

to take into account that, while T is observed at monthly intervals, B is only observed at quarterly intervals (see Appendix 2). Obviously, this redefinition has no consequences for the relation of $\phi_0(t)$ and $\phi_1(t)$ to the two-regime model.

In addition to benefits, our analysis is also conditional on age, education, head of household status, sector and year variables. Alternatively, year and sectoral dummies are replaced by aggregate and sectoral economic variables. The parametric models that we consider are logistic hazards of the form

$$\begin{aligned} \phi[t, b(t), \mathbf{x}(t)] &\equiv P[T = t \mid T \geq t, b(t), \mathbf{x}(t)] \\ &= F[\theta_0(t) + \theta_1(t)b(t) + \theta_2(t)\mathbf{x}(t) + \theta_3(t)b(t)\mathbf{x}(t)], \end{aligned} \tag{1}$$

where the new symbols are as follows. $\mathbf{x}(t)$ is the vector of conditioning individual, sectoral and aggregate variables, some of which are time-invariant like education, while others, like the aggregate economic variables, are time-varying. The variable $b(t)$ is the binary indicator of whether the individual still has benefits in t:

$$b(t) = \mathbf{1}(B \geq t).$$

F denotes the logistic cumulative distribution function: $F(u) = e^u/(1 + e^u)$. In addition, $\theta_0(t)$ is an unrestricted parameter specific of each t that captures flexible additive duration dependence, and $\theta_1(t)$, $\theta_2(t)$ and $\theta_3(t)$ are polynomials in *log t* whose purpose is to capture interaction effects between duration and conditioning variables.[9]

In our model, $b(t)$ is a predetermined variable while the remaining time-varying variables in $\mathbf{x}(t)$ are strictly exogenous. This means that the probability in (1) should be understood as being conditional on the entire path of $\mathbf{x}(t)$ and the values of $b(t)$ up to t, but not on $b(t+1)$, $b(t+2)$, etc. Namely, we assume

$$P[T = t \mid T \geq t, b(1), \ldots, b(t), \mathbf{x}(1), \ldots, \mathbf{x}(\infty)] = P[T = t \mid T \geq t, b(t), \mathbf{x}(t)].$$

We treat $b(t)$ as predetermined as opposed to strictly exogenous because, as explained above, we would expect $P[T = t \mid T \geq t, b(t), \mathbf{x}(t)]$ to differ from the probability conditional on benefit entitlement, which is equivalent to

$$P[T = t \mid T \geq t, b(1), \ldots, b(\infty), \mathbf{x}(1), \ldots, \mathbf{x}(\infty)].$$

Note that $b(t)$ would only be exogenous if the two-regime model were to hold.

[9] Note that $\phi[t, b(t), \mathbf{x}(t)]$ is just a common notation for $\phi_0[t, \mathbf{x}(t)]$ and $\phi_1[t, \mathbf{x}(t)]$: $\phi[t, b(t), \mathbf{x}(t)] \equiv [1 - b(t)]\phi_0[t, \mathbf{x}(t)] + b(t)\phi_1[t, \mathbf{x}(t)]$, where we specify $\phi_0[t, \mathbf{x}(t)] = F[\theta_0(t) + \theta_2(t)\mathbf{x}(t)]$, and $\phi_1[t, \mathbf{x}(t)] = F[\theta_0(t) + \theta_1(t) + \theta_2(t)\mathbf{x}(t) + \theta_3(t)\mathbf{x}(t)]$.

Whether $b(t)$ is predetermined matters very little in the context of homogeneous models, but it will have the effect of rendering $b(t)$ an endogenous variable when we consider models with unobserved heterogeneity.

A hazard function in which all the conditioning variables $\mathbf{x}(t)$ are strictly exogenous corresponds to a conditional distribution of durations given the full stochastic process for $\mathbf{x}(t)$. By contrast, in the predetermined case, we are effectively considering a sequence of hazard functions corresponding to different conditional distributions of durations. However, in the absence of unobserved heterogeneity, conditional inference is still possible, and we can rely on the same likelihood estimation criterion under both assumptions. The interpretation of the criterion, however, differs in each case: while with strictly exogenous variables the criterion below is the actual conditional likelihood of the data, with predetermined variables it can only be regarded as a partial likelihood; see Lancaster (1990, pp. 23–31) for a discussion of these issues.

A discrete duration model can be regarded as a sequence of binary choice equations (with cross-equation restrictions) defined on the surviving population at each duration. This provides a useful perspective, for both statistical and computational reasons, that has been noted by a number of authors (Kiefer, 1987; Narendranathan and Stewart, 1993; Sueyoshi, 1995; Jenkins, 1995). It is also a straightforward way of motivating the estimation criterion for a duration model with predetermined variables.

To see this, let T_i^0 denote the observed censored duration variable, so that

$$T_i^0 = \begin{cases} T_i & \text{if } T_i < C_i \\ C_i & \text{otherwise} \end{cases}$$

and let c_i denote the indicator of lack of censoring: $c_i = \mathbf{1}(T_i < C_i)$. Moreover, let Y_{ti} be a (0, 1) variable indicating whether the observed duration equals t: $Y_{ti} = \mathbf{1}(T_i^0 = t)$. Then the conditional log-likelihood of the sample for Y_{ti} given $T_i^0 \geq t$ is of the form

$$L_t = \sum_{i=1}^{N} \mathbf{1}(T_i^0 \geq t)\{c_i Y_{ti} \log \phi_i(t) + (1 - c_i Y_{ti}) \log[1 - \phi_i(t)]\}$$

where N is the number of unemployment spells in the sample and

$$\phi_i(t) = \phi[t, b_i(t), x_i(t)]$$

Combining the L_t for all observed durations, we obtain our estimating criterion:

$$\begin{aligned} L(\boldsymbol{\theta}) &= \sum_{t=1}^{\tau} L_t \qquad (2) \\ &= \sum_{i=1}^{N} \left((1 - c_i) \sum_{t=1}^{T_i^0} \log[1 - \phi_i(t)] + c_i \left\{ \sum_{t=1}^{T_i^0 - 1} \log[1 - \phi_i(t)] + \log \phi_i(T_i^0) \right\} \right) \end{aligned}$$

where $\boldsymbol{\theta}$ is the vector of parameters to be estimated and τ is the largest observed duration.

We estimate $\boldsymbol{\theta}$ by maximising the partial likelihood $L(\boldsymbol{\theta})$. Notice that $L(\boldsymbol{\theta})$ is of the same form as a standard log-likelihood for censored discrete duration data with strictly exogenous variables, although with a different interpretation when conditioning on predetermined variables. In the absence of cross restrictions linking the parameters $\boldsymbol{\theta}$ with those in the benefit indicator process, the partial likelihood estimates of $\boldsymbol{\theta}$ will be asymptotically efficient.

3.2. *Models with Unobserved Heterogeneity*

The economic interpretation of the coefficients in model (1) is likely to be hampered by unobserved heterogeneity. Aside from the problem of censoring in the benefit entitlement variable discussed above, in our sample there are unobserved differences in family income and in the amount of benefits received. Moreover, individuals with and without benefits may differ in ways that we do not observe. For example, there may be correlation between benefits and unobserved human capital variables.

Such unobserved heterogeneity is likely to bias downwards the effect of benefits on the exit rates, and to introduce spurious negative duration dependence. In the absence of better data, it is unlikely that much more progress can be made on these issues. However, it is still possible to generalise the standard specification by making the analysis conditional on an unobserved variable u with a known distribution independent of the exogenous variables. Following the work of Heckman and Singer (1984), the recent econometric literature has emphasised the case where u is a discrete random variable with finite support, thus giving rise to a mixture model. This approach is attractive because it is flexible, and also because, by letting the support of u grow with sample size, it is possible to establish asymptotic properties for the estimators with respect to a model with an unspecified distribution for u.

Here we also follow this approach. In our case, the situation is fundamentally altered when unobserved heterogeneity is introduced, however, because we are conditioning on a predetermined variable. Unlike in the model with only strictly exogenous variables, we cannot just consider a mixture version of (2), since (2) is in our case a partial likelihood. In fact, by introducing unobserved heterogeneity, $b(t)$ becomes fully endogenous and we can no longer condition on it. We therefore proceed by specifying a reduced form process for $b(t)$ given u. In this way, we can allow for unobserved heterogeneity that is correlated with benefits but uncorrelated with the exogenous variables. This procedure is analogous to that used by Ham and LaLonde (1996), who specified a separate hazard and heterogeneity term for initial conditions in their evaluation of the effect of training on employment and unemployment spells; see also Meghir and Whitehouse (1997). A formalisation of these issues is presented in the following subsections.

3.2.1. *Unobserved Heterogeneity in Discrete Duration Models with Predetermined Variables.* The joint distribution of the complete paths of Y_t and $b_t = b(t)$ given the paths of the strictly exogenous variables (which are omitted for simplicity) can be factorised as follows:

$$f(Y_1, \ldots, Y_\tau, b_1, \ldots, b_\tau) = f_1 f_2$$

where

$$f_1 = f_{1\tau}(Y_\tau \mid Y^{\tau-1}, b^\tau) \ldots f_{11}(Y_1 \mid b_1)$$
$$f_2 = f_{2\tau}(b_\tau \mid Y^{\tau-1}, b^{\tau-1}) \ldots f_{22}(b_2 \mid Y_1, b_1) f_{21}(b_1)$$

and we use the notation $Y^t = (Y_1, \ldots, Y_t)$ and $b^t = (b_1, \ldots, b_t)$.

Under strict exogeneity, that is, given Granger non-causality,

$$f_2 = f(b_1, \ldots, b_\tau)$$

and f_1 becomes the conditional likelihood of Y^τ given b^τ. Otherwise, it is just a partial likelihood. But, in either case, we can conduct inferences on the parameters in f_1 disregarding f_2, provided those parameters are identified in f_1 alone.

With unobserved heterogeneity, we specify the hazard given u

$$g_{1t}(Y_t \mid Y^{t-1}, b^t, u)$$

which is the object of interest. In the absence of Granger non-causality, however, the observed hazard $f_{1t}(Y_t \mid Y^{t-1}, b^t)$ does not only depend on the sequence of hazards $g_{1s}(Y_s \mid Y^{s-1}, b^s, u)$ up to t, but also on the sequence of distributions $g_{2s}(b_s \mid Y^{s-1}, b^{s-1}, u)$ up to t. The link is made explicit by the following expression

$$f(Y^\tau, b^\tau) = \int g(Y^\tau, b^\tau \mid u) \mathrm{d}G(u)$$

or equivalently

$$\prod_{t=1}^{\tau} f_{1t}(Y_t \mid Y^{t-1}, b^t) \prod_{t=1}^{\tau} f_{2t}(b_t \mid Y^{t-1}, b^{t-1})$$
$$= \int \prod_{t=1}^{\tau} g_{1t}(Y_t \mid Y^{t-1}, b^t, u) \prod_{t=1}^{\tau} g_{2t}(b_t \mid Y^{t-1}, b^{t-1}, u) \mathrm{d}G(u)$$

where $G(u)$ is the cumulative distribution function of u.

3.2.2. *Our Log-Likelihood with Unobserved Heterogeneity.* A version of (1) allowing for unobserved heterogeneity is given by

$$\phi(t, u) = F[\theta_0(t) + \theta_1(t)b(t) + \theta_2(t)\mathbf{x}(t) + \theta_3(t)b(t)\mathbf{x}(t) + \theta_4(t)u]$$

In addition, we specify a logistic process for benefits as

$$\psi(t, u) = P(b(t) = 1 \mid b(t-1) = 1, T \geq t, \mathbf{x}(t), u)$$
$$= F[\gamma_0(t) + \gamma_1(t)\mathbf{x}(t) + \gamma_2(t)u]$$

The log-likelihood function takes the form

$$L_h = \sum_{i=1}^{N} \log \int \exp[\ell_{1i}(\theta, u) + \ell_{2i}(\gamma, u)] \mathrm{d}G(u) \qquad (3)$$

where

$$\ell_{1i}(\theta, u) = (1 - c_i)\sum_{t=1}^{T_i^0}\log[1 - \phi_i(t, u)] + c_i\left\{\sum_{t=1}^{T_i^0 - 1}\log[1 - \phi_i(t, u)] + \log\phi_i(T_i^0, u)\right\}$$

and

$$\ell_{2i}(\gamma, u) = \sum_{t=1}^{T_i^0} b_{i(t-1)}\{b_{it}\log\psi_i(t, u) + (1 - b_{it})\log[1 - \psi_i(t, u)]\}$$

with $b_{i0} = 1$ for all i.

Finally, the variable u is assumed to be independent of $\mathbf{x}(t)$ for all t, and to have a discrete distribution with finite support given by $\{m_1, m_2, \ldots, m_j\}$ and associated probabilities $p_1, \ldots, p_j$. This adds $2(j - 1)$ parameters to the likelihood since the probabilities add up to one, and we assume that $\mathrm{E}(u) = 0$ given the presence of constant terms in the model. The probabilities are also constrained to lie in the (0, 1) interval. Hessian-based asymptotic standard errors are obtained under the assumption that the number of points of support of u is known, so that the estimation is fully parametric.

Note that the unobservable variable u is assumed to be independent of the observables in the entire population of entrants, which implies that, in general, there will be correlation between u and x within surviving subpopulations at different durations or for different benefit status groups. For instance, we assume that $\Pr(u = r \mid x) = \Pr(u = r)$ but in general $\Pr(u = r \mid T \geq t,\ x) \neq \Pr(u = r \mid T \geq t)$ since $\Pr(u = r \mid T \geq t,\ x) = \Pr(u = r)\Pr(T \geq t \mid x,\ u = r)/\Pr(T \geq t \mid x)$, which will depend on x as long as there is unobserved heterogeneity. In the absence of multi-spell data, there is little we can do to relax the assumption of independence between u and the strictly exogenous variables. The assumption, nevertheless, introduces potentially relevant limitations to the type of heterogeneity that can be allowed. For example, if entry rates varied substantially through time, we would expect the distribution of u to alter through time as varying proportions of the labour force enter unemployment.

3.2.3. *An Extended Model with a Bivariate Heterogeneity Distribution.* In the previous model, there is a single unobservable variable that is allowed to affect in different ways the exit rate from unemployment and the conditional probability of receiving benefits. As a way of checking the specification, we now consider a model which relaxes the assumption that the heterogeneity terms in the benefits and exit rate equations are perfectly correlated. Thus, we specify a bivariate heterogeneity distribution. The motivation for such model could arise from regarding unobserved heterogeneity as a compound of various unobservable factors that may affect the two probabilities in different ways.

We proceed by rewriting the process for benefits as a function of a second unobserved variable v:

$$\psi(t, v) = F[\gamma_0(t) + \gamma_1(t)\mathbf{x}(t) + v]$$

so that the log-likelihood function takes the form

$$L_{bh} = \sum_{i=1}^{N} \log \int \int \exp[\ell_{1i}(\theta, u) + \ell_{2i}(\gamma, v)] \mathrm{d}G(u, v)$$

where the expression for $\ell_{2i}(\gamma, v)$ is similar to the one for $\ell_{2i}(\gamma, u)$ above after replacing $\psi_i(t, u)$ with $\psi_i(t, v)$.

The variables u and v are assumed to have a joint discrete distribution with finite support given by $\{m_\ell, m_k^*\}$ $(\ell, k = 1, \ldots, j)$ and associated probabilities $p_{\ell k} = \Pr(u = m_\ell, v = m_k^*)$. Relative to the homogeneous likelihood, this scheme adds $(j^2 - 1) + 2(j - 1) = (j + 3)(j - 1)$ parameters, since the probabilities add up to one, and u and v have zero means given the inclusion of constant terms in the equations.

4. Empirical Results

We now estimate the influence on the hazard of leaving unemployment of individual characteristics, including whether the worker receives benefits, and of the business cycle, while controlling for duration dependence. We discuss the latter first, then take in turn individual and business cycle variables, and follow with the results allowing for unobserved heterogeneity.

The estimation results are reported in Table 2. To check the robustness of the results, we estimate two alternative specifications of the hazard equation (1). In the first, economy-wide and sectoral determinants are captured by including dummy variables while, in the second, macroeconomic variables appear directly. The qualitative impacts of the variables on the hazards are discussed in terms of the sign and statistical significance of the estimated coefficients. The size of those impacts – discussed in Section 5 – is measured instead by the predicted effects of changes in the variables on the hazards, which is the appropriate metric in view of both the nonlinearity of the specification and the presence of terms of interaction between variables.

4.1. *Duration Dependence*

As already mentioned, instead of imposing a given functional form, we capture duration dependence in a very flexible way by introducing an additive dummy variable for each monthly duration. Thus, a variable labelled *Dur t* in Table 2 is equal to 1 if the hazard corresponds to a duration of unemployment of *t* months, and 0 otherwise. Durations of more than 14 months are treated as censored at 14 months, due to their relatively small number of observations. Additional effects of duration are captured by introducing as regressors the interactions of certain variables with logged duration.

The results indicate a non-monotonic duration dependence. The typical pattern of the predicted hazard is shown in Fig. 5, for a given reference group.[10] For workers without benefits, the predicted hazard is increasing up to the third month

[10] Heads of household aged 30 to 44, with primary education, keeping aggregate variables at their sample means, and using the estimated coefficients of the specification with economic variables in Table 2.

Table 2

Estimates of Logistic Hazard of Leaving Unemployment

Variable	With dummies Coeff.	With dummies *t*-ratio	With economic variables Coeff.	With economic variables *t*-ratio
Individual characteristics				
Benefits	−1.244	25.32	−1.262	25.57
Benefits × log *Dur*	0.572	18.44	0.581	18.73
Benefits × Age 30–44	−0.183	4.42	−0.185	4.45
Age 30–44	0.030	0.94	0.030	0.92
Age 45–64	−0.434	7.20	−0.479	8.00
Age 45–64 × log *Dur*	−0.210	5.47	−0.168	4.42
Secondary education	0.035	1.46	0.022	0.92
University education	0.286	2.29	0.320	2.60
Univ. education × log *Dur*	−0.218	2.45	−0.266	3.05
Head of household	0.496	9.91	0.505	10.13
Head of household × log *Dur*	−0.153	4.67	−0.164	5.03
Sectoral and time dummies				
Construction	0.308	5.22	–	–
Construction × log *Dur*	−0.393	9.99	–	–
Industry	0.149	2.17	–	–
Industry × log *Dur*	−0.475	10.34	–	–
Services	−0.053	0.85	–	–
Services × log *Dur*	−0.333	8.13	–	–
1988	0.124	2.59	–	–
1989	0.126	2.65	–	–
1990	0.184	3.87	–	–
1991	0.136	2.85	–	–
1992	−0.151	3.17	–	–
1993	−0.292	6.18	–	–
1994	−0.184	3.62	–	–
Economic variables				
Δ*GDP*	–	–	9.784	6.26
Δ*GDP* × log *Dur*	–	–	−2.528	2.40
Sectoral unemployment rate	–	–	−2.366	9.72
Δ *Sectoral unemployment rate*	–	–	0.557	2.65
Δ *Sectoral unempl. rate* × *Benefits*	–	–	−0.667	5.79
Δ *Sectoral unempl. rate* × log *Dur*	–	–	−0.296	2.08
Temporary employment rate	–	–	1.844	20.33
Second quarter	0.135	5.04	0.136	5.08
Third quarter	0.106	3.84	0.120	4.40
Fourth quarter	0.021	0.72	0.053	1.91
Duration dummies				
Dur 1	−2.936	40.37	−2.874	61.42
Dur 2	−2.124	35.79	−2.280	58.89
Dur 3	−1.500	27.35	−1.773	50.06
Dur 4	−1.412	25.65	−1.768	48.73
Dur 5	−1.587	26.73	−2.013	49.41
Dur 6	−1.627	25.78	−2.104	46.76
Dur 7	−1.486	22.89	−2.008	43.32
Dur 8	−1.690	23.34	−2.258	41.53
Dur 9	−1.689	21.57	−2.285	37.50
Dur 10	−1.545	19.25	−2.172	34.82
Dur 11	−1.877	19.86	−2.548	32.40

Table 2

(Continued)

	With dummies		With economic variables	
Variable	Coeff.	*t*-ratio	Coeff.	*t*-ratio
Dur 12	−2.002	18.27	−2.695	28.23
Dur 13	−1.884	16.88	−2.597	26.73
Dur 14	−2.322	15.95	−3.059	22.74

Notes: No. of spells: 27,006. Log-likelihood: First specification, −39,506.77; second specification, −39,581.02.

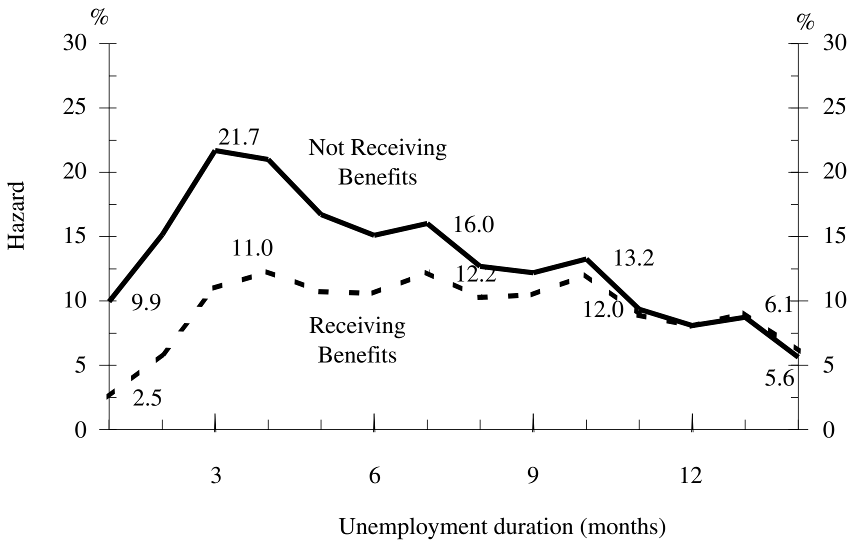

Fig. 5. *Predicted Hazards and Benefits.* GDP growth rate, 2.3%; sectoral unemployment rate, 14.87%; rate of change of sectoral unemployment rate, 8.9%; and temporary employment rate, 39.6%

and decreasing thereafter. This shape results from the combined effects of the duration dummies and the interactions of duration with other variables. We discuss these interactions below. Here we simply note that duration dependence is much less evident for workers receiving benefits: as shown in the graph, after the third month, the hazard levels off, or falls mildly.

It is worth pointing out that the shape depicted by the 14 coefficients of the duration dummies can be accurately reproduced by a second-order polynomial on logged duration, together with a dummy that controls for spurious accumulation points at durations 4, 7, 10 and 13, due to within-quarter rounding errors. Fitting such model by OLS to the estimated coefficients for the duration dummies in the second column of Table 2, and using the notation of (1), we obtain

$$\widehat{\theta}_0(t) = -2.91 + 1.54(\log\ t) - 0.59(\log\ t)^2 + 0.10(\log\ t) \times r(t)$$
$$R^2 = 0.954$$

where $r(t)$ equals one if $t \in \{4, 7, 10, 13\}$, and zero otherwise. A likelihood ratio test statistic for these restrictions is $LR = 102.62$, which is a large number for a chi-square with 10 degrees of freedom. The result is not surprising given the large sample size involved, but all the other coefficients in the two specifications remain virtually unchanged.

4.2. *Individual Characteristics*

4.2.1. *Unemployment Benefits.* It is quite evident from Fig. 5 that receipt of unemployment benefits reduces the hazard of leaving unemployment. This is in agreement with the theoretical prediction of the models introduced in Section 2. Moreover, the coefficient on the benefit variable is the single most significant estimated effect and the one that produces the largest change in the hazards. The reduction in the hazard falls as duration increases (note the positive coefficient on *Benefits* × log *Dur* in Table 2), closing up after one year of unemployment.

There is an additional negative effect of benefits on the hazards of workers aged 30–44 years old (captured by *Benefits* × *Age* 30–44), relative to those in the two other age groups. Although it would be natural to interpret this finding as the result of a particularly negative impact of benefit receipt on the search intensity of prime age workers, several points should be kept in mind. First, in the comparison with young workers (20–29 years old), this benefit effect is likely to be capturing as well the fact that prime age workers are usually entitled to higher amounts of benefits, given their higher employment seniority and number of dependants. Second, in the comparison with older workers (45–64 years old), the expected relative amount of benefits is not obvious, since older workers are likely to claim higher seniority but also a lower number of dependants (children are more likely to have left home).[11] Also, since older workers have lower hazards than prime age workers when not receiving benefits, it turns out that benefit receipt lowers the hazards in similar proportions for the two groups (e.g. at 3-month duration, by 49% for prime age workers and 42% for older workers, c.f. Fig. 6 and Table A6).

4.2.2. *Other Characteristics.* The estimated effects of other personal characteristics are quite intuitive. Starting with age, Fig. 6 shows that – among benefit non-recipients – the hazards of prime age workers are practically identical to those of the young but considerably higher than those of older workers. As a result of the effect noted in the previous paragraph, prime age workers show lower hazards than the young, among benefit recipients (Table A6). There is also evidence of negative duration dependence for older workers (captured by *Age* 45–64 × log *Dur*), which seems natural for workers near retirement, though the effect is minor (presumably due to the youngest workers in this age band).

[11] We chose the starting age for the older group at 45 because the conditions for eligibility to unemployment benefits are significantly relaxed at this age.

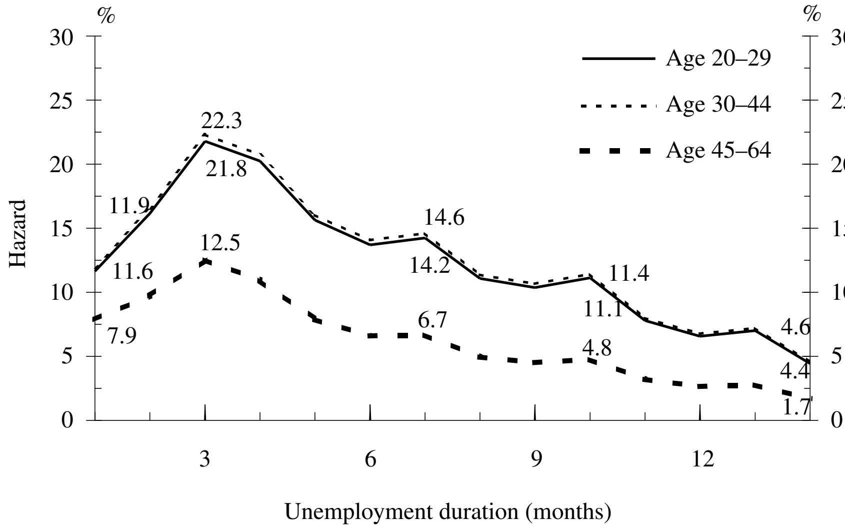

Fig. 6. *Predicted Hazards and Age in 1989*
(Not Receiving Benefits) Primary education, Manufacturing, Head of household

As to education, holding a university degree increases the hazard only at the beginning of a spell. After the third month, the presence of negative duration dependence (captured by *University education* × log *Dur*) reduces the hazards of college graduates below those of less educated workers, which presumably reflects the former's higher reservation wages. A secondary education degree does not raise the hazards significantly. Last, being a head of household does increase the chances of re-employment, with the effect diminishing over time (see Table A6).

4.3. *Business Cycle*

As explained in Section 1, search theory provides ambiguous predictions on the sign of the relationship between the business cycle and re-employment hazards, and the existing empirical results have also gone either way. All the same, Fig. 2 suggests a positive relationship in our data.

Aggregate effects are measured alternatively by dummies and macroeconomic variables. In the first specification in Table 2, they are captured by sectoral, yearly, and seasonal dummies.[12] The yearly dummies are significant – the reference year being 1987 – and indicate that hazards are higher for expansion years (1988–91) than for recession years (1992–94). These dummies, however, are probably also capturing the changes in the legislation in 1992–93 which made unemployment benefits less generous overall. Additionally, the hazards appear to be higher in the second and third quarters of the year.

There also appear to be significant differences in hazards across sectors. Table A6 shows, for workers without benefits, that the time pattern of hazards is

[12] An alternative specification with quarterly dummies produces virtually identical results (Bover *et al.*, 1996).

similar across sectors – slightly flatter in agriculture, but the levels are quite different. The ordering of sectors in terms of the hazard of finding a job, from highest to lowest, is: agriculture, construction, services and manufacturing. This order does not match the ranking of the sectoral unemployment rates in Spain well, which over the sample period was: services (10.4%), manufacturing (11.5%), agriculture (13.4%) and construction (20.4%). In particular, the two sectors with the lowest unemployment rates show the lowest hazards of leaving unemployment, and *vice versa*. The puzzle is resolved once we realise that we are only analysing unemployment outflows and ignoring inflows. The outflow ordering we have obtained is, on the other hand, correlated with the sectoral ranking in terms of the proportion of temporary employment, as described in Section 2. Thus we shall include temporary employment rates by sector as explanatory variables below.

The last two columns in Table 2 contain the results obtained when we include macroeconomic variables rather than dummies.[13] These variables are measured as quarterly levels (e.g. sectoral unemployment rate in 1988:II) and as rates of change from same quarter of the previous year (e.g. $\Delta GDP_{1988:II} = GDP_{1988:II} - GDP_{1987:II}$).[14] The only economy-wide variable included is the rate of growth of GDP. Fig. 7 depicts the hazards for workers without benefits, evaluated at the sample means of the macroeconomic variables and for the same individual characteristics as in the previous figures. For comparison, the hazards are also plotted for the maximum and minimum second-quarter GDP growth rates in the period: 5.4% in 1988:II and −1.6% in 1993:II (the corresponding hazards for workers receiving benefits appear in Table A6). The positive effect of GDP growth on the hazards is evident, although it dies out as time passes (note the negative coefficient on $\Delta GDP \times \log Dur$).

We also introduce the following sectoral variables, which refer to the job the worker held right before becoming unemployed: the unemployment rate, in levels and rates of change, and the temporary employment rate. The level and the rate of change of the unemployment rate are intended to measure sector-specific effects, while the interaction of the latter with individual duration should capture hysteresis mechanisms, as discussed in Section 1.

In Table 2, the sectoral unemployment rate shows the expected negative sign. Fig. 8 gives an idea of size, by plotting the hazards for the average, maximum and minimum second-quarter sectoral unemployment rates in the sample period, for benefit non-recipients. The coefficient on the change in the sectoral unemployment rate is a composite one. The constant term should be considered jointly with the other two which capture the business cycle: GDP growth and the level of unemployment. The interaction with benefits is significant, suggesting a reduction of benefit recipients' search effort when the employment outlook becomes

[13] Note that to control for sector in the previous job seems reasonable since mobility between sectors is low in Spain. For example, according to the LFS, the percentage of male employees working in the same sector in 1988:II and 1989:II was: agriculture, 91%; construction, 85%; manufacturing, 84%; and services, 88%. Figures would obviously be larger for shorter periods.

[14] An alternative specification in which all quarters in a given year are assigned the same yearly average level and rate of change, respectively, produces the same results (Bover *et al.*, 1996). Sample statistics of aggregate variables are shown in Table A5.

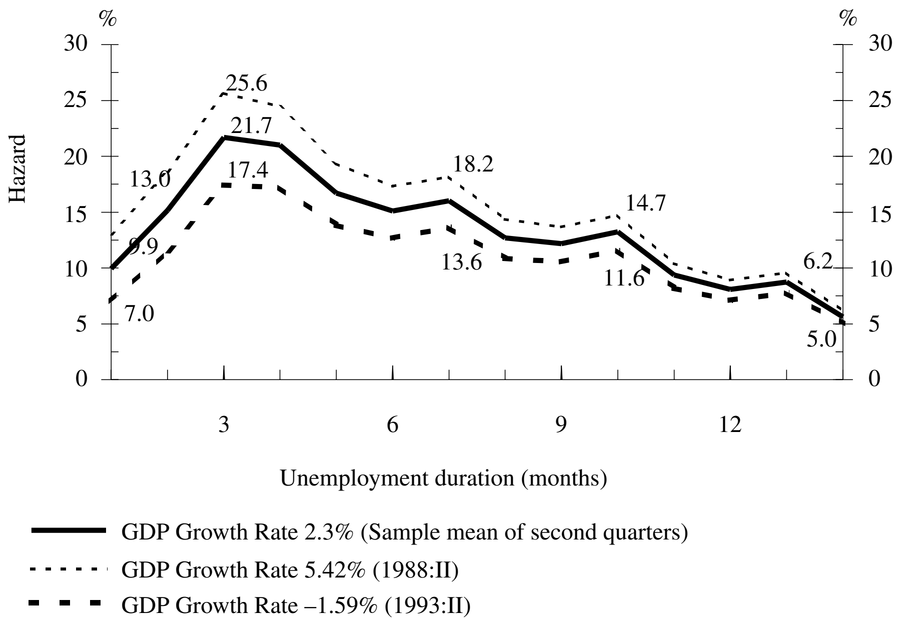

Fig. 7. *Predicted Hazards and GDP Growth* (Not Receiving Benefits) Sectoral unemployment rate; 14.9%; rate of change of sectoral unemployment rate, 8.9%; and temporary employment rate, 39.6%

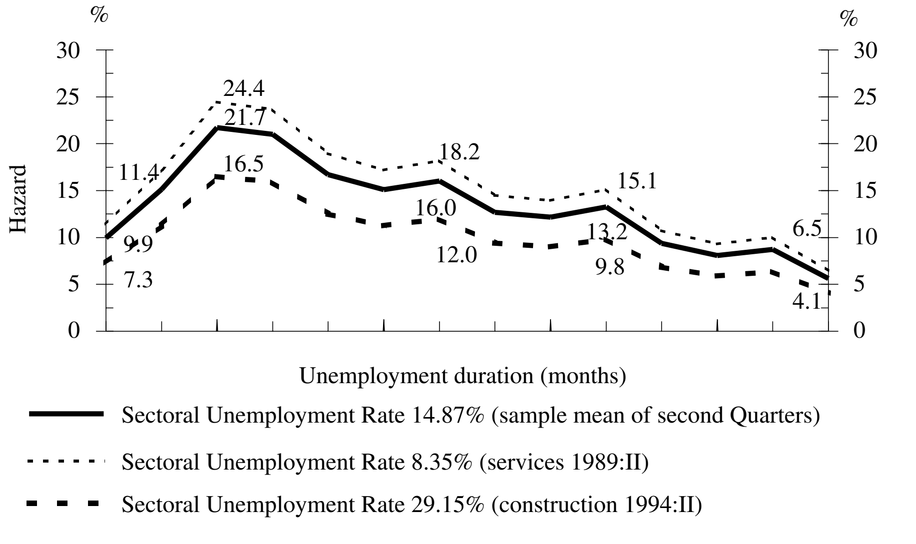

Fig. 8. *Predicted Hazards and Sectoral Unemployment* (Not Receiving Benefits) GDP growth rate, 2.3%; rate of change of sectoral unemployment rate, 8.9%; and temporary employment rate

gloomier. The interaction with individual duration is negative and significant, which can be interpreted as favourable evidence for the idea that, when hiring, firms favour workers with lower duration. The separate effect of this interacted term is shown in Fig. 9, which reveals that hysteresis effects are not large.[15]

The sectoral temporary employment rate attracts the expected positive sign and it is the most significant estimated aggregate effect. Its impact, plotted in Fig. 10, is shown to be relatively large.[16] Let us note, however, that we also estimated an encompassing specification including both sectoral dummies and macroeconomic variables, so as to ascertain whether the effects of sector-specific skills could be confounded with the effects of macro variables. The estimates (not shown) were virtually identical to those in the last two columns of Table 2, except for the coefficient on the temporary employment rate, which fell to 1.094 (with a *t*-ratio of 4.06). In such specification, sectoral dummies are likely to be simply capturing the different incidence of temporary jobs by sector, rather than, for example, skills, so we do not use it as a benchmark.

The estimated effects of the macro variables would be biased (and possibly those of the individual variables) if there were significant omitted aggregate variables;

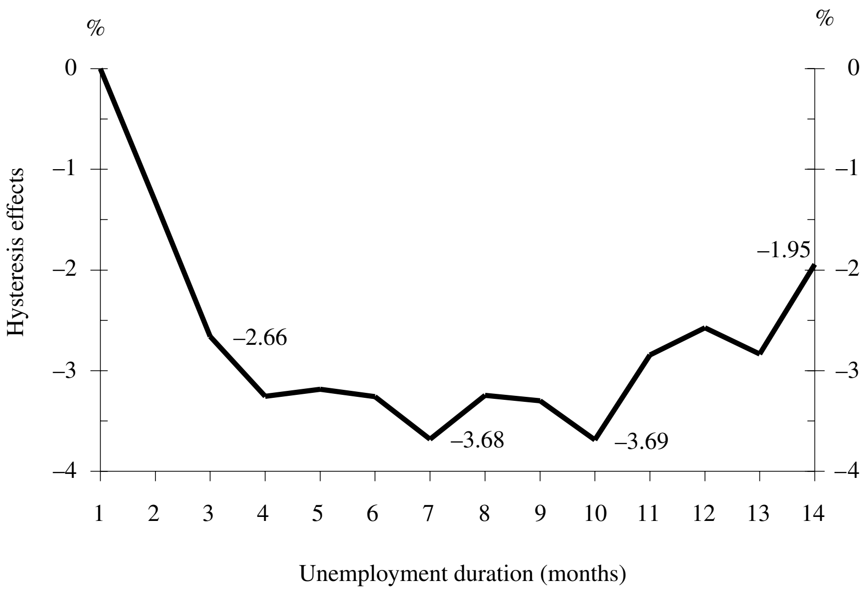

Fig. 9. *Hysteresis Effects of the Change in Sectoral Unemployment Rate on Predicted Hazards* (Not Receiving Benefits) Difference with Hazard for reference individual due to interaction of duration with rate of change of sectoral unemployment rate of 58.2%. GDP growth rate, 2.3%; sectoral unemployment rate, 14.9%; rate of change of sectoral unemployment rate, 8.9%; and temporary employment rate, 39.6%

[15] Significant but small hysteresis effects were also found, in the context of wage setting in Spanish manufacturing firms, by Bentolila and Dolado (1994).

[16] To capture the potential effect of a change of the legislation in 1992 increasing the minimum length of fixed-term labour contracts, which may have made them less attractive for employers, we included the interaction of the temporary employment rate with a dummy variable taking the value of 1 from 1992:II on. Its coefficient was hardly significant, so we have left it out.

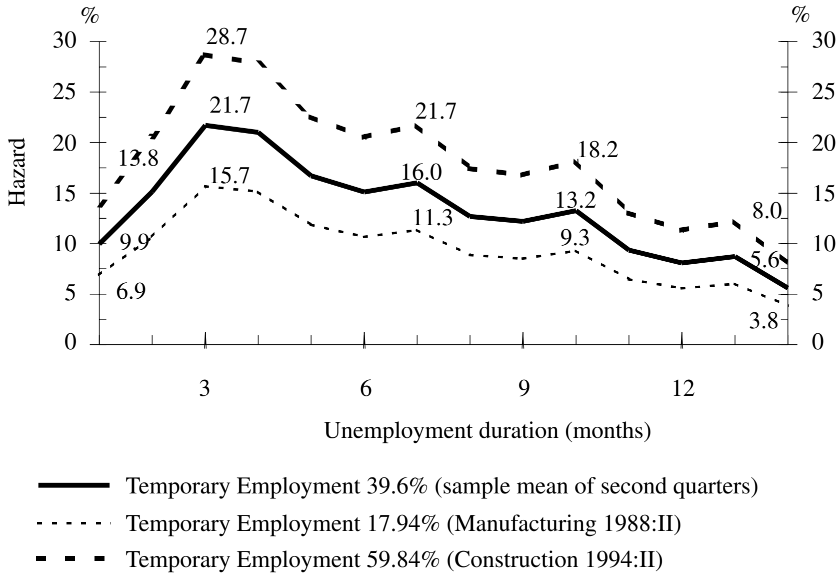

Fig. 10. *Predicted Hazards and Temporary Employment*
(Not Receiving Benefits) Sectoral unemployment rate 14.87%, GDP rate of growth 2.3% and sectoral unemployment rate of change 8.9%

see Manski (1993) for a discussion of identification in models with aggregate effects. We have already mentioned changes in the legislation in 1992–93 reducing the generosity of unemployment benefits as potentially having aggregate effects. There might also be time series changes in the composition of the pool of the unemployed that are not captured by the individual variables on which we are conditioning. However, we could not find evidence in our data that significant effects of this kind are at work. In particular, both the estimated effects of benefits and other individual variables and the overall fit are very similar in the model with time and sectoral dummies and the model with macro variables.

4.4. *Goodness of Fit*

To evaluate how well the model fits the data, we computed sample averages of predicted exit rates from the model along several dimensions. Comparing the average estimated hazard rates with the actual empirical hazards provides a diagnostic of the specification of the estimated model. Given the paper's focus, we obtained average exit rates by benefit status and for high and low periods of the business cycle (the second quarters of 1988 and 1993, respectively). Moreover, we also computed average hazards broken down by age, and each of those by benefit status as well.

Table 3 provides a summary of results for short, intermediate and long durations. The fit is, in general, quite good along the dimensions that we explored. Qualitative differences in exit rates are small, and mean fitted hazard rates are almost always well inside the 95% asymptotic confidence intervals for the empirical hazards.

Table 3

Goodness of Fit: Empirical and Estimated Average Exit Rates

		Benefits		No benefits		All	
	Months	Empirical	Fitted	Empirical	Fitted	Empirical	Fitted*
All	3	0.091	0.091	0.159	0.160	0.123	
		(0.086, 0.097)		(0.151, 0.166)		(0.118, 0.128)	
	7	0.102	0.098	0.110	0.115	0.106	
		(0.093, 0.111)		(0.101, 0.120)		(0.099, 0.113)	
	11	0.065	0.066	0.067	0.066	0.066	
		(0.053, 0.078)		(0.055, 0.079)		(0.058, 0.075)	
Age 30-44	3	0.106	0.101	0.201	0.196	0.145	0.140
		(0.095, 0.116)		(0.184, 0.218)		(0.135, 0.154)	
	7	0.113	0.111	0.129	0.146	0.120	0.124
		(0.097, 0.130)		(0.107, 0.151)		(0.106, 0.133)	
	11	0.058	0.079	0.083	0.086	0.067	0.082
		(0.038, 0.077)		(0.054, 0.113)		(0.051, 0.084)	
1988: II	3	0.138	0.108	0.171	0.173	0.154	0.140
		(0.093, 0.183)		(0.121, 0.221)		(0.121, 0.188)	
	7	0.121	0.117	0.180	0.117	0.153	0.117
		(0.052, 0.190)		(0.107, 0.253)		(0.103, 0.204)	
	11	0	0.086	0	0.062	0	0.074
		(39)†		(37)		(76)	
1993: II	3	0.065	0.064	0.175	0.128	0.112	0.091
		(0.042, 0.089)		(0.133, 0.218)		(0.089, 0.134)	
	7	0.083	0.073	0.079	0.092	0.081	0.080
		(0.046, 0.119)		(0.037, 0.121)		(0.054, 0.109)	
	11	0.121	0.062	0.063	0.066	0.094	0.063
		(0.052, 0.190)		(0.008, 0.118)		(0.049, 0.139)	

Notes:
* Since the model contains unrestricted duration dummies, empirical and average fitted logit exit rates for the whole sample coincide.
† 95% asymptotic confidence intervals for the empirical exit rates appear in parentheses except when the exit rate is zero, where we indicate the size of the relevant subsample.

On a closer look, the fit is specially good for the first five months. At 6–7 months, we overpredict the exit rate for those without benefits and underpredict it for those with benefits. From eight months onwards, the reverse is true, as the model underestimates the effect of benefits on aggregate exit rates. Such a pattern of discrepancy is particularly clear for those in the 30–44 age band. After six months, there may be a change in composition in the surviving population that the model fails to take into account. This would be the case for individuals who entered unemployment after a one-year temporary contract with a six-month benefit entitlement (prior to the 1993 reform).

As for business cycle effects, in a boom year (1988), the model overestimates the effect of benefits at 3–5 months and underestimates it at 11–12 months. In a recession year (1993), however, the discrepancy turns out to be the opposite: underestimation at short durations and overestimation at longer durations. Overall, there seems to be little distortion in estimated business cycle effects, although aggregate estimated hazards for 1988 and 1993 underpredict slightly their empirical counterparts at short durations and also at 10–12 months.

4.5. *Unobserved Heterogeneity*

We now turn to the estimation of the model for the hazard of leaving unemployment with unobserved heterogeneity presented in Section 3.2, which entails endogenising benefit receipt. Estimates of the joint mixture log-likelihood for unemployment duration and benefit receipt, as specified in (3), are contained in Table 4. We do not allow any interaction of the effect of the unobserved variable u with duration. Thus, in terms of the notation of Section 3.2.2, the coefficients associated with u in the unemployment and benefits hazards are, respectively, $\theta_4(t) = 1$ and $\gamma_2(t) = \gamma_2$. Moreover, we specify a distribution for u with two mass points, m_1 and m_2, with probabilities p_1 and p_2. However, since $\mathrm{E}(u) = 0$, we are effectively introducing three additional free parameters in the model: m_1, p_1, and γ_2, which, together with the 35 parameters in the unemployment hazard and the 32 parameters in the benefits process, gives a total of 70 parameters in the mixture log-likelihood.

We need not devote much effort to interpreting the estimates on benefit receipt, since this is just an auxiliary reduced-form equation. Notice that we are concerned, for the first month of unemployment, with the probability that the worker is entitled to benefits on becoming unemployed while, in subsequent periods, we have the probability that the worker is entitled to benefits given that he has remained unemployed until the current month and was entitled to benefits in the previous month. The first probability depends on eligibility rules and the remaining ones on benefit duration rules. Both types of rules, however, depend on the type of benefits received. Eligibility to unemployment insurance depends only on tenure in the previous job while, for unemployment assistance, it depends on the number of dependants, family income and age. Some regressors are correlated with both rules in the same way. For example, the worker's age or being a head of household should be positively correlated with eligibility to both UI and UA. But, for other variables, the signs may differ. For example, the correlation between higher education and eligibility should be positive for UI (through longer employment tenure) but negative for UA (through higher family income).

The last two columns in Table 4 show the results for a very general specification including interactions of the regressors with unemployment duration (retaining only the significant coefficients).[17] We include as a regressor a step dummy starting in April 1992, to capture the legal change raising the stringency of UI eligibility.[18] The results are quite intuitive and we do indeed find, in two instances, differences between the results for the first month and thereafter. According to our estimates, the conditional probability of receiving benefits:

(a) increases with age (after the first month for workers aged 45–64), university education (after the second month), and head of household status,
(b) falls with the sectoral proportion of temporary employment,

[17] The results from estimating the reduced form process for the benefit receipt indicator separately are very close to the ones shown in Table 4 (Bover *et al.*, 1996).

[18] A dummy starting in April 1989 interacted with *Age* 45–64, meant to capture an extension of UA eligibility for that group of workers, was not significant. This was expected, since the change mostly affected workers after having received UA benefits for at least 18 months, a duration which is absent in our data. Legislative changes in 1993 affected benefit amounts but not eligibility rules.

Table 4

Joint Estimates of Logistic Hazards for Leaving Unemployment and for Benefit Receipt, with Unobserved Heterogeneity

	Leaving unemployment		Benefits process	
Variable	Coeff.	*t*-ratio	Coeff.	*t*-ratio
Individual characteristics				
Benefits	−1.288	15.93	–	–
Benefits × log *Dur*	0.594	12.43	–	–
Benefits × *Age* 30–44	−0.199	4.50	–	–
Age 30–44	0.022	0.62	0.161	4.60
Age 30–44 × log *Dur*	–	–	0.110	2.52
Age 45–64	−0.711	7.46	−0.028	0.68
Age 45–64 × log *Dur*	−0.043	0.77	0.185	3.68
Secondary education	0.023	0.91	−0.037	1.37
University education	0.475	2.62	−0.301	3.99
Univ. education × log *Dur*	−0.350	2.92	0.236	2.09
Head of household	0.680	8.86	0.348	10.63
Head of household × log *Dur*	−0.260	5.60	0.099	2.35
Economic variables				
Δ*GDP*	11.415	5.29	−2.314	2.07
Δ*GDP* × log *Dur*	−3.468	2.53	–	–
Dummy 1992:II–1994:III	–	–	−0.299	6.77
Sectoral unemployment rate	−2.823	10.26	1.267	4.27
Δ *Sectoral unemployment rate*	0.480	1.62	0.674	6.30
Δ *Sectoral unempl. rate* × *Benefits*	−0.724	5.84	–	–
Δ *Sectoral unempl. rate* × log *Dur*	−0.222	1.18	–	–
Temporary employment rate	2.097	19.67	0.226	2.07
Temporary empl. rate × log *Dur*	–	–	−0.401	3.60
Seasonal dummies				
Second quarter	0.136	4.83	0.045	1.44
Third quarter	0.130	4.49	−0.022	0.71
Fourth quarter	0.052	1.76	−0.014	0.44
Duration dummies				
Dur 1	−3.931	13.07	−0.069	1.91
Dur 2	−2.202	36.91	3.347	52.25
Dur 3	−1.566	27.15	2.778	47.11
Dur 4	−1.547	26.21	4.509	35.90
Dur 5	−1.787	28.74	2.811	38.85
Dur 6	−1.874	28.63	2.426	33.57
Dur 7	−1.775	26.56	4.755	23.19
Dur 8	−2.025	27.77	2.863	27.78
Dur 9	−2.050	26.18	2.361	24.05
Dur 10	−1.937	24.26	3.905	19.20
Dur 11	−2.312	24.78	2.552	19.42
Dur 12	−2.460	22.75	2.083	16.20
Dur 13	−2.362	21.50	3.824	12.93
Dur 14	−2.823	19.58	2.521	13.06
Heterogeneity coefficients				
m_1	−0.230	5.06		
m_2	5.486			
p_1	0.960	131.10		
γ_2	−0.174	7.82		

Notes: Number of spells: 27,006. Log-likelihood: −66,312.69.

(c) is countercyclical, and
(d) fell in April 1992 for all workers.

The observed counter-cyclicality probably arises from the fact that the recession period in our sample was characterised by a shake-out of older, long-tenure workers which firms intended to replace by younger workers on fixed-term contracts in the subsequent expansion.

Regarding the hazard of leaving unemployment, the results with and without unobserved heterogeneity are quite consistent. All coefficients in Table 4 have the same sign as the corresponding ones in Table 2 and they are of a similar magnitude. The only exception is the interaction of *Age* 45–64 with duration, whose coefficient becomes insignificant and very close to zero. Thus, in both Table 2 and Table 4, benefit receipt reduces the hazard significantly, while GDP growth and temporary employment raise it.

The last panel in Table 4 shows that, of the two unobserved types of workers we have allowed for, one is much more frequent (its probability being 0.96) while the other, less frequent type has a much higher constant hazard. More specifically, the estimate for m_1 is -0.23 and the implied estimate for m_2 is 5.49.

4.6. *Checking the Specification with the Two-Error Model*

As explained in Section 3, we checked the unobserved heterogeneity specification by considering a model with different errors in the unemployment exit rate and benefit processes. We specified a distribution for u and v with two mass points in each case, m_1, m_2 and m_1^*, m_2^*, respectively, and parameterised the likelihood function using the marginal distribution of u and the conditional distribution of v given u. Therefore, there were five mixing parameters given by m_1, m_1^* (since m_2 and m_2^* are determined by the zero mean conditions) and the probabilities:

$$\begin{aligned} p_1 &= \Pr(u = m_1) \\ \pi_1 &= \Pr(v = m_1^* \mid u = m_1) \\ \pi_2 &= \Pr(v = m_1^* \mid u = m_2) \end{aligned}$$

Due to computing limitations, we report estimates for the two-error model based on a 10% random subsample of 2700 spells. Results are in Table A7, which also shows estimates of the one-error model on the same subsample for comparison. The latter are similar to those based on the full sample but, as expected, standard errors are larger, by a factor of about 3.

The main finding is that the estimated effects from the two-error model are very similar to those shown for the one-error model. Thus, we can conclude that the previous estimates were not hampered by a too restrictive specification of unobserved heterogeneity, at least relative to the direction that we have relaxed here.

As before, one of the values of u occurs with a much higher frequency than the other but, given u, the conditional probabilities for v are not very different. Indeed, a standard normal Wald test statistic of the null hypothesis $\pi_1 = \pi_2$, takes the value 0.64, so that the hypothesis of independence between u and v cannot be rejected at conventional significance levels. This result is not surprising, given the similarity between the estimates from the models that treat benefits as either predetermined or endogenous variables.

5. Discussion of the Results and Concluding Remarks

We finish by discussing the relative size of the empirical effects we have found. Among all the variables, we essentially focus on the most meaningful from an economic point of view: unemployment benefits and macroeconomic variables. Impact sizes for the remaining personal characteristics are easily read off the corresponding graphs and tables. Comparisons of size are not straightforward, because the exact magnitudes of the effects depend on the reference group of individuals and the values of the macroeconomic variables chosen for the evaluation. We discuss the results obtained for the particular values underlying the previous graphs, which are broadly representative of our results, starting with hazard rates and ending with median unemployment durations.

5.1. *Effects on Hazard Rates*

The relative importance of the effects on hazard rates of benefit receipt and GDP growth can be gauged as follows. Take the estimated hazards for individuals not receiving benefits as the benchmark, keeping the growth rate of GDP at its sample period mean (2.3%). Now consider two departures from this benchmark. The effect of benefit receipt can be measured by comparing the benchmark with the hazards for individuals receiving benefits, keeping the GDP growth rate at its mean. The effect of GDP growth can be measured by comparing the benchmark with the hazards for individuals not receiving benefits, setting the GDP growth rate at the sample period minimum (−1.6%). These comparisons were respectively shown in Figs 5 and 7. Then, according to our estimates, within the first six months of unemployment, receiving benefits implies a reduction of the monthly hazard rate ranging from 4.5 percentage points (at 6 months' duration) to 10.7 points (at 3 months). By contrast, reducing the rate of growth of GDP from the mean to the minimum reduces the predicted hazard by at most 4.3 percentage points (at 3 months). After the first six months of unemployment, the two effects are quite similar.

Since the effect of hazard rates on unemployment duration is cumulative, in Fig. 11 we depict the impact of benefits and the cycle in terms of rates of survival in unemployment. The figure highlights how the accumulated impact of receiving benefits is larger than that of changing GDP growth. For instance, at the end of the fourth month, the chances of remaining in unemployment are less than one-half (47.3%) in the benchmark case, 56.2% with the lowest GDP growth rate, and 71.6% for workers receiving benefits. Or, put in a slightly different way, the survival rate reaches the value of one-half in about 4 months in the first case, 5 months in the second, and 7 months in the last case.

The *ceteris paribus* clause may seem too strong for this comparison, and so we have repeated the exercise for the case when the change in the GDP growth rate comes along with the actual weighted average sectoral unemployment rate and its (yearly) rate of change that were observed in that same quarter. Table A6 shows that moving from the average to the minimum GDP growth rate does not reduce the hazards by more than 5 percentage points, still below the effect of benefit receipt. Furthermore, we are measuring these differences taking a worker not claiming benefits as the benchmark. The differences would be still larger if we took

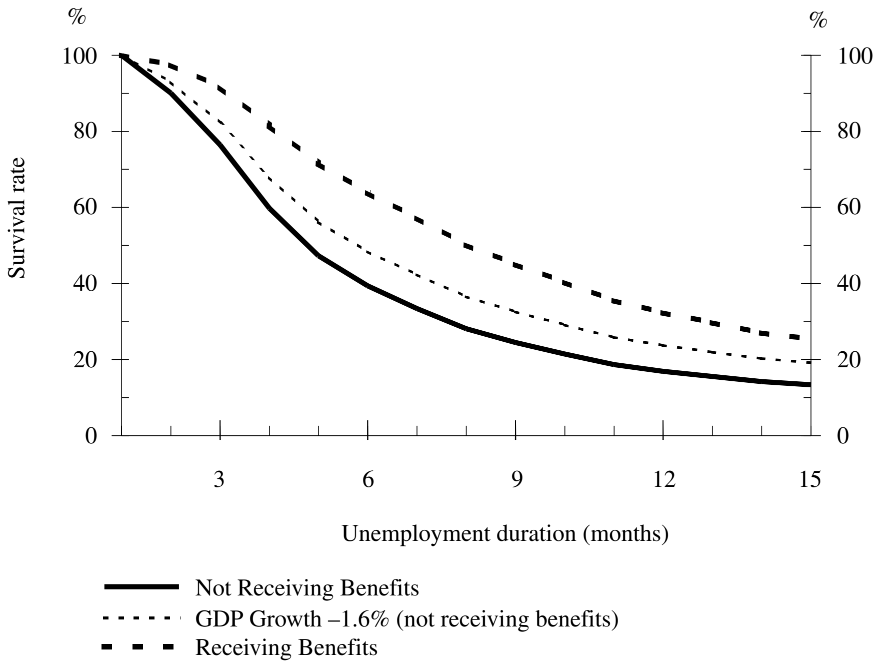

Fig. 11. *Survival Rate in Unemployment*
GDP growth rate, 2.3%, except for the middle line. Other parameters as in Fig. 5.

a benefit recipient as the benchmark since, in absolute terms, recipients' hazards are less affected by GDP growth than those of non-recipients (see Table A6). We therefore conclude that, for assessing the chances of re-employment of a given individual, it appears to be much more important to know whether he is receiving benefits or not than the state of the business cycle.

Another interesting exercise refers to the effects of fixed-term contracts. Fig. 10 indicates that the predicted monthly hazard rates for the same reference worker, who was previously working in a sector with a temporary employment rate of 40%, are 2–6 percentage points higher than if he had been working in a sector with a temporary employment rate of 18%. The effect is not negligible at all.

An important caveat applies to the interpretation of the results concerning duration dependence. In spite of controlling for observed worker heterogeneity, we cannot be sure of the extent to which the pattern found reflects true duration dependence. In general, we expect part of the estimated duration dependence to stem from unobserved heterogeneity; in our case, for example, from differences in family income or in the amount of benefits received and its time pattern. As is well known, spurious duration dependence may arise from changes in the composition of the stock of unemployed as time passes.[19] We have already shown that, when unobserved heterogeneity of the type considered in Section 3.2 is allowed for, the

[19] Suppose, for instance, that there were two types of workers with different, but constant, hazards. As the high-hazard workers disproportionately leave unemployment, the proportion of the low-hazard ones in the remaining stock would increase, and this would show up as negative duration dependence.

estimated effects of the key variables of interest do not vary much. Nevertheless, the basic identification problem remains. As a result, more attention should be paid to the exit rates in the first few months, since they are based on a more representative sample. For the same reason, we prefer not to put much emphasis on the disparity between the shapes of duration dependence found in the data and those predicted by the standard search model.

5.2. *Effects on Median Unemployment Duration*

What policy implications can be drawn from our results? Surely, the policy goal should be to reduce the unemployment rate, rather than increasing jobfinding rates *per se*. However, Spanish unemployment is, as in many other European countries, chiefly an outflow problem. This has manifested itself in a large share of long-term unemployment. These facts make a *prima facie* case for policy measures aimed at increasing re-employment probabilities.

A way to show the implications from our results is to assess the differential impact over the business cycle of policies aimed at reducing unemployment duration through reductions in benefit entitlements. We have already shown that, at three or four months into the spell, the exit rates of workers without benefits are twice as large as those of workers with benefits, and the difference is statistically very significant.

Now, under the restrictive assumptions of the *two-regime* model introduced in Section 3.1, we can integrate these hazard rates to obtain approximate median durations of unemployment spells. The results are summarised in Table 5. Our data are, in fact, best suited to providing information on the effects of experimenting with the most extreme policy of all: eliminating benefits altogether. The table indicates that such an extreme measure would have sizable effects on median unemployment duration.[20] It also shows that, for long durations, there are larger effects of benefits in an expansion than in a recession. So, for example, reducing the benefit entitlement from 12 to 8 months has no effect in a recession but it implies a 4-month reduction in median unemployment in an expansion.

Policy decisions, however, should be based on welfare assessments, and it is not obvious that reducing benefit duration would necessarily increase welfare. Unemployment benefits create both gains and losses. The former come in the form of smoother consumption of households with unemployed members (in the presence of risk aversion and incomplete private insurance against the unemployment risk) and of more efficient worker-firm matches.[21] The losses, apart from longer unemployment duration and the potential resulting loss of human capital, may arise from lower precautionary saving, leading to lower capital stock and output. As a result, the net welfare impact of a change in benefit duration would be difficult to assess, and no established evidence is yet available (Valdivia, 1995; Hopenhayn and Nicolini, 1997). What our results show is that the desirable effects of benefits

[20] Note that lowering benefit duration may not only raise hazard rates from unemployment but may also lower hazard rates into unemployment (not analysed here), although the international empirical evidence suggests that this effect is relatively small (Atkinson and Micklewright, 1991).

[21] Gruber (1997) provides empirical evidence on the consumption smoothing role of unemployment benefits in the USA.

Table 5

*Estimated Median Unemployment Duration for Given Benefit Entitlement (in months)**

Year	GDP growth†	Benefit entitlement duration			
		$B = 0$	$B = 4$	$B = 8$	$B = 12$
1991	2.3	3	6	10	14
1993	−1.6	5	9	14	14

Notes:
* Reference individual: household head, aged 30–44, with primary education.
† In our data, −1.6, 2.3 and 5.4 are the smallest, average and largest GDP growth, but the results for the last two values coincide.

have to be traded off against the undesirable outcome of significantly larger unemployment durations.

Last, the difference between affecting unemployment rates and unemployment outflow probabilities mentioned before becomes especially relevant in the case of fixed-term labour contracts. We have found that these contracts have a sizable positive effect on the hazard of leaving unemployment. On the other hand, they are sure to raise the hazard of entering unemployment as well, thus raising the unemployment rate. By providing workers with work habits and experience, and by mitigating adverse duration dependence effects (especially for the long-term unemployed), we might expect that the net effect of fixed-term contracts on the unemployment rate would be positive. Establishing this conjecture, however, would require an empirical assessment of the dynamics of employment and unemployment spells, which is outside the scope of this paper. Ultimately, any policy recommendation about temporary contracts cannot be dissociated from those concerning the firing costs of the alternative permanent labour contracts.

5.3. *Concluding Remarks*

In this paper, we have investigated empirically the influence of individual characteristics and the business cycle on the probability of finding a job, with special emphasis on the effects of unemployment benefits. For this purpose, we have estimated monthly discrete hazard models using duration data constructed from a rotating panel sample of unemployed men in the Spanish Labour Force Survey, for the period 1987:II–1994:III.

Our main empirical results can be summarised as follows.

(a) Receiving unemployment benefits reduces the hazard of leaving unemployment. For example, at an unemployment duration of three months – when the largest effects occur – the hazard rate for workers without benefits doubles the rate for those with benefits.
(b) Hazard rates are procyclical.
(c) At sample-period magnitudes, receipt of unemployment benefits affects an individual's hazard of leaving unemployment to a significantly higher degree than changes in the state of the business cycle. More specifically, again

at 3-month duration, the fall in the hazard caused by the receipt of benefits is 2.5 times larger than that due to a 4-point drop in GDP growth.

(d) measures which increase labour market flexibility – the introduction of fixed-term contracts in the Spanish case – raise hazard rates from unemployment into employment.

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Appendix 1. Unemployment Benefits in Spain

Table A1
Unemployment Insurance

Maximum length		Amount		Maximum amount	
Tenure	Length	Length	% Wage*	Dependants	% min w
1987–91					
1–5 m	0	1–6 m	80	None	170
6–48 m	Tenure/2†	7–12 m	70	1 child	195
>48 m	24 months	13–24 m	60	>1 child	220
1992–94					
1–11 m	0	1–6 m	70		
12–72 m	Tenure/3‡	7–12 m	60	Same as above	
>72 m	24 m	13–24 m	60		

Notes: m = months.
* Previous wage (average of last 6 months).
† Lengths have to be multiples of 3, so the actual formula is: 3 × integer(tenure/6).
‡ The actual formula is: 2 × integer(tenure/6), so that the length is an even number.

Table A2
Unemployment Assistance

Maximum length			Amount		
Tenure	Length				
1987–88					
1–2 m	0				
3–5 m	Tenure		75% of the minimum wage		
>5 m	18 months				
1989–94					
1–2 m	0		Age < 45		75% of the min. wage
3–5 m	Tenure				
6–11 m	Age < 45	18 m		1 dep	75% of the min. wage
	Age ≥ 45	24 m	Age ≥ 45	2 deps	100% of the min. wage
>12 m	Age < 45	24 m		>2 deps	125% of the min. wage
	Age ≥ 45	30 m*			

Notes: deps. = dependants, min. wage = minimum wage.
* Plus 6 additional months if they have received contributory benefits for 24 months.

Table A3

Calendar of Unemployment Benefit Reforms in Spain, 1987–94

• *March 1989:* More generous assistance system established for the unemployed aged 45 or older in terms of the duration and the amount of benefits. Unemployed workers aged at least 52 years old who have fulfilled all requirements for obtain a retirement pension except for the minimum age, may draw benefits until retirement age.

• *November 1990:* Reform to make the special unemployment benefit system for underemployed workers in agriculture in two regions (Andalucía and Extremadura) more similar to the general assistance system.

• *April 1992:* Reduction in the generosity of contributory benefits in terms of the the duration and the amount of benefits. The option of receiving the full amount of unemployment benefit entitlement in one instalment so as to set up a business is severely limited.

• *February 1993:* Unemployment benefits become taxable income (as from January 1994). The minimum amount for contributory benefits is lowered from 100% ot 75% of the minimum wage. Assistance benefits:

(a) The amount of assistance benefits becomes proportional to hours worked, in the case of part-time workers.
(b) Explicit definition of dependants: spouse, children below 26 years old, and disabled children, as long as their income is below 75% of the minimum wage.
(c) Income per family member cannot be above 75% of the minimum wage (before it was 100%).

The unemployed worker drawing benefits must, within a 5-day period, show proof of having visited firms offering him/her a job through the employment office.

Appendix 2. Database description

A2.1. *Individual Data*

Source. Rotating panel from the Spanish Labour Force Surveys (*Encuesta de Población Activa: Estadística de Flujos*) from 1987:II to 1994:III, provided by the National Statistical Office (Instituto Nacional de Estadística, INE).

Sample. From a sample of men of 20–64 years of age, we exclude those

- in the military or the substitute civil service
- always employed during the observed period
- never in the labour force during the observed period
- observed only once
- with a missing interview between two valid interviews
- who have never worked and are not looking for work
- who are full-time students (from the moment they become so)
- employed who do not answer the question about how long they have been in their current job
- unemployed (and those not in the labour force) who answer neither the question 'How long has it been since your last job?' nor the question 'How long have you been looking for a job?'
- unemployed who do not answer the question about their relation with the public employment office (INEM)
- unemployed for over eight years.

60,036 unemployment spells satisfy these restrictions. Restricting the sample to those unemployed observed when entering unemployment leaves 27,382 spells of unemployment. Finally, at the estimation stage, we drop 376 spells (1.37%) for which the information on economic sector at the previous job is lacking.

Unemployment duration. Both the unemployment and the benefit duration variables are measured in months, the smallest unit allowed by the data. The length of unemployment spells is determined using quarterly observations on the individual's labour market status. We start from the information provided the first time he answers the question 'How long has it been since your last job?' or the question 'How long have you been looking for a job?'. For subsequent quarters, unemployment duration is computed as initial duration plus three months, instead of taking the actual reply because sometimes it led to inconsistent sequences. Although these inconsistencies may arise from very short-term employment spells, detailed analysis of the data reveals that they are much more likely to be due to measurement error (note that sometimes a single person answers the survey for all household members). To determine the end of the unemployment spell, we use the answer to the question 'How long have you been in the current job?' given by those who are unemployed at one interview and employed at the next.

Benefit duration. Benefit duration is constructed assuming that benefits are received throughout, up to the last time the individual declares to be receiving them (from a question about his relation with the employment office). Alternatively, we could have accepted the raw quarterly information on benefit receipt. An advantage of the former, smoother measure is that it overcomes the measurement error arising from the fact that individuals often start receiving benefits with some (varying) delay due to administrative reasons.[22] In any case, for 87% of our sample of entrants into unemployment, the difference between the two measures is non-existent and, for over 97%, the difference is of three months at most. If an individual is unemployed and receiving benefits at one interview and employed at the next, we assume his benefits duration to be at least as large as his unemployment duration.

The following dummy variables used in the estimation are taken at their values at the beginning of the unemployment spell:

- *Economic sector at the previous job* Grouped as agriculture (including farming and fishing), manufacturing (including mining as well), construction and services
- *Education* Three groups: illiterate, no schooling, and primary education; secondary education and vocational training; and university education.
- *Age* The available five-year age bands are grouped further into three categories: 20–29 years old, 30–44 years old, and 45–64 years old.
- *Head of household.* The variable takes the value of 1 for heads of households and 0 otherwise.

Table A4 provides the frequencies of the individual variables for the sample of 27,006 entrants into unemployment that is used in the estimation. Monthly frequencies show troughs at multiples of 3, in both unemployment and benefit duration. The reason is that at the first interview after becoming unemployed, most workers reply having been such for 1 or 2 months. Few reply 3 months and hardly anybody replies 0 months. These troughs naturally translate to our estimated hazards.

A2.2. *Aggregate and Sectoral Variables*

- *Proportion of temporary workers* Percentage of employees on fixed-term contracts. *Source*: *Encuesta de Población Activa* (EPA), INE.
- *Share of fixed-term contracts in total hiring* *Source*: Instituto Nacional de Empleo (from 1981 to 1983, total contracts extrapolated backwards with growth rates of placements).
- *Unemployment rate* *Source*: EPA and *Series Revisadas EPA* (*1977–1987*), INE.
- *Gross domestic product* Constant prices. *Source*: *Cuentas Financieras de la Economía Española* (1985–1994), Banco de España.

Descriptive statistics are provided in Table A4.

[22] An official document reports that this delay was of 18 days as of May 1993, and that it had been longer in previous years (Ministerio de Trabajo y Seguridad Social, 1993).

Table A4

Frequencies of Individual Variables (Sample of entrants into unemployment)

	Spells	%		Spells	%
Total number of spells	27,006	100.00			
Censored	14,625	54.15			
Non-censored	12,381	45.85			
Duration of the unemployment spell			*Censored duration of benefits*		
			No benefits	13,464	49.86
1 month	4,255	15.76	1 month	1,594	5.90
2 months	3,986	14.76	2 months	1,988	7.36
3 months	2,764	10.23	3 months	1,229	4.55
4 months	3,540	13.11	4 months	1,988	7.36
5 months	2,831	10.48	5 months	1,650	6.11
6 months	1,199	4.44	6 months	644	2.38
7 months	1,923	7.12	7 months	1,072	3.97
8 months	1,595	5.91	8 months	860	3.18
9 months	580	2.15	9 months	305	1.13
10 months	1,072	3.97	10 months	563	2.08
11 months	924	3.42	11 months	492	1.82
12 months	256	0.95	12 months	131	0.49
13 months	578	2.14	13 months	292	1.08
14 months	589	2.18	14 months	275	1.02
15 months	144	0.53	15 months	73	0.27
16 months	407	1.51	16 months	201	0.74
17 months	363	1.34	17 months	185	0.69
Head-of-household status			*Economic sector at previous job*		
Head of household	14,175	52.49	Agriculture	5,811	21.52
Not head of household	12,831	47.51	Construction	7,887	29.20
			Industry	5,029	18.62
Age			Services	8,279	30.66
20–29 years old	11,131	41.22	*Year**		
30–44 years old	8,334	30.86			
45–64 years old	7,541	27.92	1987	2,282	
			1988	3,824	
Education			1989	4,112	
			1990	4,364	
Primary education			1991	4,423	
or less	16,545	61.26	1992	4,941	
Secondary education	9,680	35.84	1993	5,975	
University education	781	2.89	1994	4,503	

* Number of people who are unemployed in at least one month of the corresponding year (percentages not shown due to overlap among years).

Table A5

Sample Statistics of Economic Variables Across Spells (%)

	Mean	St. dev.	Minimum	Maximum
Sectoral variables				
Temporary employment rate	39.28	14.50	10.98	60.49
Unemployment rate (level)	14.70	5.93	7.99	31.50
Unemployment rate (rate of change)	8.26	18.14	−36.30	60.00
National variables				
Gross domestic product (rate of change)	2.31	2.38	−1.59	6.13

Appendix 3. Additional Empirical Results

Table A6

*Predicted Hazards for Different Population Groups and Aggregate Variables' Values**

Variable	Group	Unemployment duration (months) 1	3	7	10	14
Age	20–29	3.7	13.1	12.7	11.9	5.7
(with benefits)	30–44	3.2	11.4	11.1	10.3	4.9
	45–64	2.4	7.2	5.9	5.1	2.2
Education	Primary	11.9	22.3	14.6	11.4	4.6
(without benefits)	Secondary	12.3	22.9	15.0	11.8	4.7
	University	15.3	23.1	13.0	9.4	3.5
Head of household	Not h. of h.	7.6	17.1	12.3	10.0	4.2
(without benefits)	Head of h.	11.9	22.3	14.6	11.4	4.6
Sector	Agriculture	10.4	29.4	27.1	24.9	12.6
(without benefits)	Construction	13.7	26.9	19.0	15.4	6.5
	Industry	11.9	22.3	14.6	11.4	4.4
	Services	10.0	21.5	15.6	12.7	5.4
GDP growth	−1.6%	1.7	8.6	10.3	10.5	5.4
(with benefits)	2.3%	2.5	11.0	12.2	12.0	6.1
	5.4%	3.4	13.3	13.9	13.4	6.7
Cycle†	Recession	7.0	16.8	12.4	10.3	4.3
(without benefits)	Average	9.9	21.7	16.0	13.2	5.6
	Expansion	13.1	26.3	19.1	15.6	6.6

Notes:
* Source: Table 2, second specification.
† Definitions (u = sectoral unemployment, all variables in percentages):

	ΔGDP	u	Δu
Recession	−1.6	19.2	35.0
Average	2.3	14.9	8.9
Expansion	5.4	12.4	−1.2

Table A7

Estimates of Logistic Hazards for Leaving Unemployment and for Benefit Receipt, in One- and Two-error Unobserved Heterogeneity Models (10% sample)

	One-error model				Two-error model			
	Leaving unemployment		Benefits process		Leaving unemployment		Benefits process	
Variable	Coeff.	*t*-ratio	Coeff.	*t*-ratio	Coeff.	*t*-ratio	Coeff.	*t*-ratio
Individual characteristics								
Benefits	−1.634	5.57	–	–	−1.635	5.56	–	–
Benefits × log *Dur*	0.813	4.84	–	–	0.813	4.84	–	–
Benefits × Age 30–44	−0.282	1.91	–	–	−0.282	1.91	–	–

Table A7

(Continued)

	One-error model				Two-error model			
	Leaving unemployment		Benefits process		Leaving unemployment		Benefits process	
Variable	Coeff.	*t*-ratio	Coeff.	*t*-ratio	Coeff.	*t*-ratio	Coeff.	*t*-ratio
Age 30–44	−0.025	0.22	0.143	1.33	0.025	0.22	0.204	1.25
Age 30–44 × log *Dur*	–		0.019	0.14	–	–	0.049	0.29
Age 45–64	−1.652	5.15	−0.009	0.07	−1.652	5.15	−0.021	0.11
Age 45–64 × log *Dur*	0.441	2.36	0.027	0.18	−0.441	2.36	0.025	0.13
Secondary education	−0.021	0.25	−0.135	1.57	−0.020	0.24	−0.216	1.43
University education	−1.056	0.98	−0.499	2.23	−1.037	0.96	−0.694	1.97
Univ. ed. × log *Dur*	0.352	0.60	0.133	0.48	0.343	0.58	0.116	0.36
Head of household	1.321	5.17	0.328	3.24	1.321	5.17	0.478	2.59
H. of h. × log *Dur*	−0.547	3.51	0.232	1.78	−0.547	3.51	0.384	1.74
Economic vars.								
Δ*GDP*	12.766	1.57	−5.624	1.59	12.960	1.60	−7.177	1.38
Δ*GDP* × log *Dur*	−4.492	0.90	–	–	−4.623	0.93	–	–
D. 1992:II–1994:III	–		−0.470	3.36	–	–	−0.686	2.81
Sectoral unempl. rate	−4.116	4.87	1.075	1.16	−4.118	4.88	1.980	1.26
Δ *Sect. unempl. rate*	1.188	1.16	0.528	1.57	1.213	1.19	0.861	1.53
Δ *Sect. u.* × *Benefits*	−0.517	1.30	–		−0.524	1.31	–	–
Δ *Sect. u.* × log *Dur*	−0.688	1.09	–		−0.702	1.11	–	–
Temporary empl. rate	2.521	7.80	0.249	0.72	2.521	7.80	0.282	0.56
T. e. rate × log *Dur*	–	–	−0.543	1.52	–	–	−0.534	1.21
Seasonal dummies								
Second quarter	0.029	0.31	−0.153	1.55	0.029	0.31	−0.199	1.53
Third quarter	0.094	0.99	−0.145	1.45	0.094	0.16	−0.190	1.43
Fourth quarter	0.123	1.29	−0.174	1.79	0.124	0.10	−0.254	1.85
Duration dummies								
Dur 1	−4.801	4.22	0.171	1.50	−4.798	4.23	0.209	1.00
Dur 2	−2.185	12.08	3.855	17.36	−2.185	12.09	3.781	13.91
Dur 3	−1.333	7.12	2.950	16.07	−1.334	7.14	2.702	9.86
Dur 4	−1.372	7.44	4.940	11.56	−1.373	7.46	4.645	9.54
Dur 5	−1.583	8.23	2.912	13.22	−1.584	8.25	2.462	6.32
Dur 6	−1.642	8.19	2.733	11.69	−1.642	8.21	2.128	4.47
Dur 7	−1.431	7.07	4.845	8.12	−1.432	7.09	4.205	5.63
Dur 8	−1.974	8.54	3.649	8.87	−1.975	8.56	2.951	4.61
Dur 9	−1.849	7.72	2.574	8.30	−1.850	7.73	1.753	2.77
Dur 10	−1.771	7.21	5.177	5.10	−1.771	7.23	4.345	3.76
Dur 11	−2.190	7.51	2.842	6.72	−2.191	7.52	1.936	2.67
Dur 12	−2.204	6.73	2.489	5.83	−2.205	6.74	1.470	1.85
Dur 13	−2.241	6.47	3.603	4.90	−2.242	6.48	2.548	2.52
Dur 14	−3.200	5.88	2.309	4.57	−3.199	5.89	1.185	1.32
Heterogeneity coefficients								
m_1	−0.409	3.26			−0.408	3.01		
m_2	6.177				6.177			
p_1	0.938	80.60			0.938	80.68		
γ_2	−0.030	0.70						
m_1^*					−1.464	3.44		
m_2^*					1.184			
π_1					0.442	2.61		
π_2					0.526	2.62		

Notes: No. of spells: 2,700. Log-likelihood: One-error model, −6545.96; Two-error model, −6545.39.

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