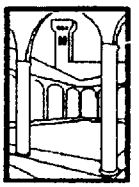


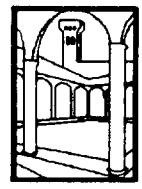
EUROPEAN UNIVERSITY INSTITUTE
Department of Economics

The Probability of Leaving Unemployment: some new evidence for Great Britain

Elena Stancanelli

Thesis submitted for assessment with a view to obtaining the Degree of Doctor of the European University Institute

Florence
August 1994



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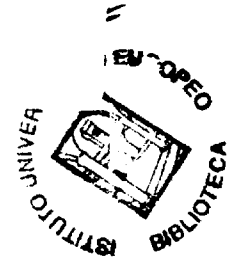

Florence
August 1994

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Acknowledgements

I am indebted to my supervisor John Micklewright for his valuable advice. He has guided my understanding of the issues that are dealt with here and has always been willing to discuss them and comment on my work. Not least, I should thank him for having made available to me the data that I analyse. Without exaggeration, this thesis would have not been possible had he not been my supervisor. Next, I owe many thanks to my co-supervisor Robert Waldmann for his helpful suggestions and comments and in particular for having always encouraged my work. I would also like to thank Götz Rohwer, now at Bremen University, for useful discussions and for having made available a program for duration analysis, "Transition Data Analysis" or TDA. Patrick Heady of OPCS is to be thanked for having replied to my queries on the data that are here used.

Among the people that provided me with comments or encouragement at different stages, I would especially like to thank Amedeo Amato, Gerard van den Berg, Isabella Consigliere, Christian Dustmann, Pieter Hasekamp, Kristina Kostial, Maarten Lindeboom, Lavan Mahadeva, Jan van Ours, Arthur van Soest, Geert Ridder, Gilles Teyssiere, Ugo Trivellato. I have also benefited from comments of the participants to seminars that I presented: at the Prime Quality Workshop of the European University Institute; at the 1992 SPES Workshop on the Microeconometrics of European Labour Markets in Dublin; at the annual meeting of AIEL of 1992 in Reggio Emilia; at the 1993 ESEM meeting in Uppsala; at the 1994 5th Conference on Panel Data in Paris; and at the University of Tilburg, the University of Leiden and the Free University of Amsterdam, in the Netherlands.

Many thanks go to the EUI departmental secretaries Jacqueline Bourgonje, Marcia Gastaldo and Jessica Spataro for their amazing helpfulness and their friendliness. I should then thank the staff of the EUI computer centre for having improved more and more the Institute facilities, although this has demanded more time on my side to be able to follow all the changes. In particular, I would like to thank Enrico Spinelli, who has always been very helpful. I would also like to thank all the people working at the EUI, in the canteen, the bar or simply cleaning who have made this work environment more pleasant. I should, finally, thank my closest friends and relatives for having encouraged me to finish my work.

Some changes to the first draft of this thesis were made while I was a Research Fellow at the Free University of Amsterdam. Therefore, I would also like to thank the Free University for financial support and Tinbergen Institute, to which I was then attached for hospitality.

Any errors are obviously mine.

Preface

An earlier version of Chapter 4 has been published on "Quaderni di Economia e Lavoro" (1993). Chapter 7 in part draws on joint work in progress with Stephen Jones and John Micklewright.

Chapter 1

Introduction

The purpose of this work is to shed further light on the determinants of the duration of unemployment. The methodology employed for the analysis in the thesis is applied econometrics. The data used relate to Great Britain. They are taken from the Survey of Living Standards during Unemployment (LSUS) collected by the Office of Population Censuses and Surveys on behalf of the Department of Social Security in 1983/84.

Why is it important to study unemployment duration? The main reason is for economic policy purposes. Unemployment is currently considered as one of the biggest structural problems of most Western European economies. The analysis of the determinants of individual unemployment duration should help to identify economic policies that could be implemented in order to reduce unemployment. If, for instance, the unemployed with certain socio-economic characteristics are found to be less likely to find a job then economic policy can be used to attempt to correct at least in part for this disadvantage. For instance, examples of such policies are training courses organized or sponsored by government agencies. Another importance issue is whether the probability of leaving unemployment diminishes with the lengthening of the unemployment spell, all things equal.

The rate of unemployment at a certain point in time can be decomposed into the proportion of the labour force that enter unemployment at that point in time (inflow rate into unemployment) multiplied by the average duration of unemployment. Jackman et al. (1991, pp. 219–224) look at this decomposition for a number of countries in 1988. The authors find that unemployment durations are very low in North-America and inflow rates quite high. In the EC instead inflow rates are rather low but durations are "huge", to use their words. The authors conclude by looking at time series evidence that the change in the rate of unemployment in Britain is mainly explained by changes in average unemployment duration rather than by changes in the inflow rate. By contrast, in the USA the answer is about half and half. These findings highlight the importance of analyzing unemployment duration, especially in the case of Britain.

The rate of unemployment has recently attained high levels in most West-

ern European economies and it appears to be increasing. In the UK it has gone up from about 8% in 1990 to over 12% in 1992–93 [1]. The data that I employ cover a sample of the unemployed in Britain in 1983–84 when the rate of unemployment was also high and close to the current levels, equal to about 13%. It has been pointed out that the behaviour of the unemployed in search of a job may differ in times of high and rising unemployment rates. For instance, awareness of the fact that there is probably going to be severe competition for the jobs available should make the unemployed less likely to refuse any job offer. This is at least a good reason to analyse the LSUS data given that most published applied UK studies on unemployment duration relate to the seventies when the rate of unemployment was much lower and less than 10%.

The literature on unemployment duration is huge as reviewed, for instance, by Atkinson and Micklewright (1991) and Devine and Kiefer (1991). There are, however, some issues that present in my opinion scope for original contributions. The originality of the analysis carried out here stems from the elements that are briefly reviewed below.

A first element of originality lies in the data used for the analysis. These data are a longitudinal sample of the inflow into registered unemployment of household heads in Great Britain. They contain interesting and useful information for the analysis of individual unemployment duration as will be discussed in Chapter 2. For instance, detailed information is collected on the level of savings and debt of the unemployed and their spouses. These data have not yet been object of any study except for the descriptive analysis carried out by the survey planners (Heady and Smith, 1989). It seems interesting to see whether the predictions of previous studies of unemployment duration for Britain are confirmed. It is also important that the public money spent on the survey sees a return in terms of economic analysis of the data collected. Not least, it seems fair to the unemployed that participated in the survey replying to several questions to exploit at best the information they provided.

A second element of originality is to be found in the methodology of analysis. Although already established techniques will be used, an attempt is made to compare different ways of specifying the (conditional) probability of leaving unemployment using the LSUS data. The importance of using a flexible baseline when modelling the hazard rate and of distinguishing the destination states entered upon leaving unemployment will be assessed with respect to the LSUS data, by means of intuitive arguments and formal testing. This is the object of Chapter 4. The specification of single risk models and the use of a monotonic baseline hazard rate has been the practise until quite recently. It seems interesting to gather more evidence on the possible implications of these alternative specifications of the hazard rate, especially in

[1] The level of the rate of unemployment may vary slightly depending on the source adopted. The figures quoted here were extracted from Table 2.2 of different issues of the Employment Gazette.

light of the fact that there is only one previous applied study for Britain that has taken these two methodological issues into account, Narendranathan and Stewart (1993). I should anticipate that their conclusions are quite different from those of the present study.

These problems are important for the purpose of economic policy planning. The baseline hazard rate is used to model the variation in the individual (conditional) probability of leaving unemployment which is due to the elapsing of the time spent in the state of unemployment —the so-called time dependency effect. For instance, findings of negative time dependency would indicate that the unemployed that experience longer unemployment spells stand less chances to leave unemployment all things equal. The assumption of a monotonic functional form for the baseline hazard rate does not allow the sign of the dependency on time of the (conditional) probability of leaving unemployment to vary over the course of the unemployment spell. However, this assumption is not backed up by theoretical arguments. On the other hand, the use of a competing risks specification allows one to take into account the economic states entered upon leaving unemployment. This is particularly important since the impact of the explanatory variables —and also the sign of the dependency on time modelled by means of the baseline hazard rate— may vary with the economic states exited to. For instance, the impact of age might differ if one distinguishes the probability of leaving unemployment to take up a full-time job from the probability of leaving unemployment to withdraw from the labour force.

A third element of originality is to be found in the analysis of the influence of socio-economic factors on the individual (conditional) probability of leaving unemployment to take up a full-time job. Previous applied UK studies have focused on estimating the impact of the level of unemployment benefit on the duration of unemployment. The present study considers a large range of explanatory variables, some of which proxy individual characteristics which normally are not observed such as psychological feelings about financial constraints or leisure. The focus of interest is the re-employment probability, which is modelled using a competing risks model. Previous UK studies have estimated single risk models except for Narendranathan and Stewart (1993). I anticipate that while most of the variables considered are found to have the expected impact on the re-employment probability, the estimated impact of the level of unemployment benefit turns out not significantly different from zero. The robustness of this finding is discussed in Chapter 5 together with the estimated impact of the other explanatory variables considered.

The fourth element of originality stems from the analysis of the relationship between the duration of entitlement to unemployment benefit and the individual probability of leaving unemployment in the case of the UK. This issue is especially important since the November 1993 Budget in Britain announced that the duration of the national insurance unemployment benefit will be cut down to six months in 1995. There are no previous studies that use individual data for the UK to look at this issue. There is, however,

some evidence from cross-countries time series studies that the duration of the unemployment benefit contributes substantially to explain the rate of unemployment and the duration of individual unemployment in the UK and other OECD countries. This topic is developed in Chapter 6.

A final element of originality is to be found in the analysis of the relationship between the unemployed's savings and debt and the individual duration of unemployment. Previous work in this area is very limited. There are almost no other applied studies on the relationship between financial resources and unemployment duration, at least to my knowledge. Job search models, to be introduced in the next section, normally assume that individuals are risk neutral with the consequence that the unemployed's levels of savings and debt are not expected to affect the individual duration of unemployment. If the unemployed are risk averse, the level of the unemployed's financial resources might instead influence the individual probability of leaving unemployment. The level of financial resources will also affect the probability of leaving unemployment if those without financial resources are liquidity constrained. In general, one would expect that on average a higher level of financial resources results in a higher reservation wage and a longer unemployment duration. This hypothesis will be discussed and tested using the LSUS data in Chapter 7.

Some background information on the topics that are dealt with in this thesis is provided below. This is meant to be for the reader that has no previous knowledge in the field. In Section 1.1, the main features of job search models which normally provide the theoretical underpinnings for the analysis of unemployment duration are outlined[2]. In Section 1.2, the expexted impact of the level of unemployment benefit on the duration of unemployment, which has been the focus of most previous UK studies, is briefly discussed and some of the most relevant findings reviewed. In the same section, the expexted impact of other variables such as the intensity of search and the value attributed to leisure relative to labour is discussed. The expected impact of the duration of entitlement to unemployment benefit on the individual probability of leaving unemployment is the subject of Section 1.3. Some description of the methodology used for the analysis follows. The approach adopted to modelling unemployment duration is presented together with the basic statistical concepts in Section 1.4. The use of competing risks models is illustrated in the same section. In Section 1.5, some insights into the issue of the dependency on time of the individual probability of leaving unemployment are provided. A description of the structure of the thesis concludes the chapter.

[2]Some authors, as for instance Moffit and Nicholson (1982) use the standard labour supply model of individual choice between leisure and labour to model unemployment duration.

1.1 The theoretical framework of job search theory

The theoretical framework of models of unemployment duration is the theory of job search, which has the advantage over the more conventional neoclassical labour supply models of explicitly allowing for the dynamics of the labour market arising from uncertainty and imperfect information. The pioneering work is Stigler (1962).

Job search models assume that the unemployed —and in more recent developments also workers that might be dissatisfied with their current job— look for jobs in that segment of the labour market which is most suited to their qualifications. The job searchers are rational individuals that maximize their present discounted value of lifetime income net of job search costs and subject to information constraints.

In the simplest job search model, job offers are characterized in terms of the associated wage and job seekers are assumed to to know the distribution of wages which characterize their potential job offers but not which offer they will receive at which in point in time. The job seekers are assumed to receive a job offer each period of time and jobs once accepted are assumed to last for ever. The offer distribution, the individual's income, the costs of search and the the discount rate are also assumed constant over time. Under these restrictive assumptions the job seeker's optimal policy turns out to be a *reservation wage policy*: a reservation wage is formed and the first offer that exceeds the reservation wage is accepted. The reservation wage is that wage that equates the marginal costs and the marginal benefits of search.

In recent developments of job search models, the restrictive assumptions of the earliest models are relaxed. The reader is referred to Devine and Kiefer (1991) for a review of the job search literature. Some simple facts are described below.

Within a job search framework, the probability of leaving unemployment at a certain point in time, $\theta(t)$, can be expressed as follows:

$$\theta(t) = \alpha(t)\eta(t)(1 - \gamma(t)), \tag{1.1}$$

where α is the probability of coming across a suitable or desirable job vacancy (and of applying for it), η is the probability of being offered the job, γ is the probability of rejecting the offer and $1-\gamma$ is the acceptance probability. The probability of leaving unemployment is allowed to vary over time. Some analysis of non-stationarities in job search models is developed for instance in van den Berg (1990). The above probabilities are affected by individual characteristics and by the conditions on the demand side of the labour market, such as the local unemployment rate or the vacancy rate in that segment of the labour market in which the job seeker is searching.

The key element of job search models is the *reservation wage*, defined as that wage at which the job seeker is indifferent between continuing to search

and accepting the job offer. In the traditional model of job search, the reservation wage is assumed to affect the probability of leaving unemployment via the acceptance (rejection) probability. It is also assumed to remain constant over the unemployment spell. By allowing the probabilities to vary over time in 1.1, the reservation wage has been allowed to vary over time. Devine and Kiefer (1993) in their review of applied studies point out that that the reservation wage might be less important than previously thought in explaining unemployment duration. This follows from two types of argument. The first is that if the job seeker applies only for "desirable" jobs then the rejection probability should be almost zero. The second is that in times of high and rising unemployment rates the offer rejection probability is probably close to zero. In both cases, the reservation wage fails to affect the probability θ via the acceptance probability. One of the consequences is that the level of unemployment benefit would then fail to affect the probability of leaving unemployment, at least in the conventional models, as it is discussed below. However, the the reservation wage might still affect the probability of leaving unemployment by contributing to determine which job offers are to considered as "desirable" or "acceptable".

One of the most recent developments of the job search model is due to van den Berg and Ridder (1993), who take into account at the same time the behaviour of firms and job seekers. One of the particular feature of the model is that the distribution of wage offers is endogeneous to the model since firms take into account the job seeker's reservation wage when making their wage offers.

1.2 The impact of unemployment benefit, search intensity and leisure

The disincentive effect of the level of unemployment benefit on the probability of leaving unemployment has been the focus of most previous applied studies of unemployment duration. In the traditional job search model, higher levels of unemployment benefit relative to the expected earnings from work are shown to influence the probability of leaving unemployment by raising the reservation wage and with it the rejection probability. If the role played by the reservation wage in explaining unemployment duration is minor then also the impact of unemployment benefit becomes less important.

The UK literature — from the first pioneering studies of Nickell (1979), Lancaster and Nickell (1980), Atkinson et al. (1984) to the most recent studies of Narendranathan and Stewart (1993, 1993a)— has paid considerable attention to the impact of the level of benefit on the duration of unemployment. The first studies of Nickell (1979) and Lancaster and Nickell (1980) found a significantly negative impact of higher levels of unemployment benefits on the (conditional) probability of leaving unemployment. Atkinson et al. (1984) argued that these findings were not robust, showing that they were

sensitive to the measures of unemployment benefit and expected income used. In particular, the authors found that the estimated impact differed from zero only when an unrealistically overgenerous pattern of benefit receipts was assumed. Narendranathan et al. (1985), used administrative data on unemployment benefit receipts and unemployment spell durations (the DSS Cohort Study of the Unemployed 1978-79) and concluded that the findings of a significantly negative impact of higher levels of benefit on the probability of leaving unemployment were robust. Narendranathan et al. (1985) were the first to use a longitudinal dataset (to analyse unemployment duration in the UK) and to have access to administrative information on benefit receipts — the previous studies used cross-section data such as the Family Expenditure Survey and had incomplete information on benefit payments. However, Narendranathan et al. (1985) and Narendranathan and Stewart (1993a) — who used the same data— found that the benefit effect becomes statistically insignificant after the first five/six months of unemployment.

In other countries such as the US higher levels of unemployment benefits have generally been found to affect negatively the probability of leaving unemployment as, for instance, in Katz and Meyer (1988, 1990).

The effect of unemployment insurance might, however, be more complex than that expexted by the traditional job search model. Mortensen (1977), for instance, pointed out that the effect of an increase in the benefit paid by unemployment insurance schemes on the probability of leaving unemployment is ambiguous. The author's argument was that in addition to the disincentive effect of the level of benefit on the probability of leaving unemployment there is also a positive impact which applies to the unemployed not entitled to unemployment insurance and to benefit recipients who have used up their unemployment insurance entitlement. According to the author, this positive impact arises from employment been more attractive relatively to unemployment precisely because it gives entitlement to unemployment insurance. This hypothesis might for instance apply, in the presence of a dual labour market, to workers in the secondary sector of the market, typically characterized by less work protection, who are looking for a job in the primary sector of the market.

It has also been argued that higher levels of unemployment benefit may influence the unemployed's intensity of search and with it the probability of getting to know about a desirable job offer, "α". There is, however, no agreement on the sign of this impact. Following Barron and Mellow (1979), search effort, S, can be expressed as a function of time, t, and money, c, spent searching and of individual characteristics, x, $S = S(t, c, x)$. Barron and Mellow (1979) show that higher benefits will augment the money spent searching but reduce the time of search. Ben-Horim and Zuckerman (1987) show that the positive effect of the benefit level on search intensity might offset the reservation wage effect and result in an overall positive effect of the benefit level on the probability of leaving unemployment. Wadsworth (1991), who analysed a sample of the unemployed extracted from the UK 1984 Labour

Force Survey, concluded that recipients of unemployment benefit search more intensively than the unemployed that do not receive any unemployment benefit. On the other hand, Lindeboom and Theeuwes (1993) find for a sample of Dutch unemployed in the area of Leiden that the level of unemployment benefit has a significantly negative impact on search activity.

An important feature of search intensity is that it might vary during the course of the unemployment spell. For example, Lindeboom and Theeuwes (1993) find that the effect of search intensity on the probability of leaving unemployment is significantly positive and varies over time. The authors find that search intensity is most effective in the first weeks of the unemployment spell. Yoon (1981), looking at variable search intensity finds that unemployment duration is in large part explained by the time the unemployed spends searching for jobs rather than by the rejection of received offers, at least for the US PSID sample considered. This would support the argument that the probabilities of coming across a desirable job offer, α, and of being offered the job, η may, after all, play a more important role in explaining unemployment duration than the offer rejection probability, γ.

The sign of the overall impact of increased search intensity on the probability of leaving unemployment depends on the relative importance of the negative effect on the acceptance probability —which works through the reservation wage—, and of the positive effect on the probability of coming across a "desirable" job offer, α. Van den Berg (1994) looking at the impact of an increase in the offer probability on the conditional probability of leaving unemployment shows that for reasonable assumptions about the form of the wage offer distribution the positive effect dominates the negative effect. It seems plausible to assume that increased search intensity will raise the conditional probability of leaving unemployment. Of course, it should be taken into account that the intensity of job search might vary during the course of the spell of unemployment and therefore be endogeneous to the model.

The basic ingredient of any labour supply equation is the relation between the wage obtained from labour and the value attributed to leisure. In the job search model, the assumption of income maximization on the side of the job seeker is equivalent to the assumption of risk neutrality. Assuming risk neutrality, allows one to dispense with the possible effects of risk aversion and non-labour income on the individual re-employment probability. This also implies that the impact of different valuations of leisure (L) relative to labour is not taken into account. However, it seems plausible that job seekers will differ in the value they attribute to the time spent not working. One way to allow for the impact of leisure valuation on the probability of leaving unemployment is to assume that the job seeker maximize their utility (rather than income) defined as a function of leisure and income. One would expect the reservation wage to increase with higher values of leisure relative to work, since the opportunity cost of working will be higher for higher valuations of leisure. The impact of a higher leisure valuation on the probability of leaving unemployment is therefore negative. Some evidence for the UK on the

impact of individual valuation of leisure on the reservation wage is gathered by Jones (1989), who has access to information on individual feelings and psychological attitudes during unemployment. The author finds that psychological variables affect significantly and negatively (as expected) the level of the reservation wage.

Once, the risk neutrality assumption is relaxed, also the financial resources of the unemployed might affect the probability of leaving unemployment. For a given degree of risk aversion, higher levels of savings are expected to raise the unemployed's reservation wage, as discussed, for instance, in Jones et al. (1993). Intuitively, one would expect that wealthier persons have *ceteris paribus* higher reservation wages and longer unemployment durations. On the other hand, increasing risk aversion is likely to result in a lower reservation wage (Kohn and Shavell, 1974, and Pissarides, 1974), all things equal.

1.3 The duration of entitlement to unemployment benefit

The impact of the potential duration of entitlement to unemployment benefit on the individual probability of leaving unemployment has recently received considerable attention. This issue is exteremely relevant for the purpose of economic policy planning. Is it the case that reducing the duration of the entitlement period to unemployment benefit, will lead to lower unemployment duration on average?

Interest in this question is heightened by the announcement of the UK November 1993 Budget that the duration of the national insurance unemployment benefit will be reduced to six months in 1995. Entitlement to this benefit lasts currently for a year.

It has been argued, for example by Mortensen (1977) and van den Berg (1990), that the unemployed will tend to lower their reservation wage and to increase their search intensity as the time of the expected reduction in the level (or of the exhaustion) of the benefit approaches. This would result in an increase in the individual chances of leaving unemployment near the time of the expected reduction in the level (or of the exhaustion) of the benefit. The majority of previous applied studies have found evidence in favour of this hypothesis. In particular, Katz (1986), Ham and Rea (1987), Katz and Meyer (1988, 1990), Meyer (1990), looked at this issue for North America. Similar findings were obtained for the Netherlands by Van den Berg (1990) and Lindeboom and Theeuwes (1993).

There are no previous studies that look at this issue using microdata for the UK, at least to my knowledge. However, some cross-countries time series studies have found evidence in favour of a positive relationship between the duration of entitlement to unemployment benefit and the duration of individual unemployment spells in the UK, among other countries (Burda,

1988, and Jackman et al. , 1989).

In order to analyse the relationship between the duration of entitlement to unemployment benefit and the individual probability of leaving unempoyment for the case of the UK, the workings of the UK unemployment benefit schemes must be taken into account. Two types of unemployment benefits are available in the UK. The first is the National Insurance benefit, Unemployment Benefit (UB), which is paid conditional on having paid sufficient work contributions and (currently) has a maximum duration of 52 weeks. The second is the social assistance benefit, Supplementary Benefit (SB), now called Income Support, which is is means-tested and unlimited in time. SB is means-tested on the resources and the needs of the unemployed (and their partner if any) and subject to other conditions such as demonstrating availability to work.

Both benefits are flat rate[3] with additions for dependent spouse and children. The two benefits can be received simultaneously —at any time, even at the commencement of the unemployment spell— if the unemployed's resources including UB fall below their needs. The benefit received by the unemployed can vary over time both in type and amount. For instance, the unemployed that initially receive only UB might gain entitlement to SB upon UB exhaustion; the amount of benefit received might be reduced because the partner has taken up a full-time job; or augmented due to the birth of a child; etc. etc. .

In Chapter 6, I estimate the impact of the expected exhaustion of unemployment insurance on the duration of unemployment. Estimating the relationship between the potential duration of entitlement to unemployment benefit and the probability of leaving unemployment is, however, a controversial issue. The problems with detecting the entitlement effect are discussed in Chapter 6.

1.4 A reduced form approach: the hazard rate

The traditional models of job search are not easily reduced to an estimating equation. They are neither of much help in guiding the choice of a specific functional form for the probability of leaving unemployment. A reduced form approach to modelling the probability of leaving unemployment is adopted throughout this work.

Structural models derive their estimating equations directly from the theoretical models of job search. In the reduced form models, instead, the probability of leaving unemployment is modelled as a conditional probability by means of the *hazard rate*, —drawing on the biomedical and statistical liter-

[3]The Earnings Related Supplement which used to link the amount of UB received to the level of previous earnings was abolished in January 1982.

ature on the duration of life— and job search theory is used to guide the choice of the explanatory variables and the interpretation of the results.

Reduced form models present the advantage over structural models of a simpler specification. Moreover, reduced form models do not impose any restrictive functional form on the data and their results are easy to interpret. For these reasons, reduced form models of the probability of leaving unemployment have become popular. However, a major disadvantage of such models is normally that the parameters of the underlying structural model cannot be recovered from the estimated reduced form parameters.

The hazard rate is a conditional probability. It allows one to specify the individual probability of leaving unemployment at a given point in time conditional on the individual being still unemployed up to that time. Some useful statistics textbooks are Kalbfleish and Prentice (1980) and Cox and Oakes (1984). One good econometric textbook is Lancaster (1990).

Let us define the duration of the unemployment spell, T, as a random variable. The probability of leaving unemployment in a certain time interval, $(t, t+dt)$, given equation 1.1 above, can be written:

$$\theta(t)dt = (\alpha(t) * \eta(t) * (1 - \gamma(t))dt, \tag{1.2}$$

where $\theta(t)$ is the hazard function. This equation gives the probability that unemployment will be exited between t and $t+dt$, conditional on the unemployment spell having lasted longer than t; i. e. formally:

$$\theta(t)dt = P(t \leq T < t+dt | T \geq t), \tag{1.3}$$

where P stands for probability. The instantaneous conditional probability of leaving unemployment is obtained from 1.3, dividing by dt —which gives the average conditional probability for each instant of time in the interval $(t, t+dt)$— and taking the limit:

$$\theta(t) = \lim_{dt \to 0} \frac{P(t \leq T < t+dt | T \geq t)}{dt}. \tag{1.4}$$

The expression above describes the probability of leaving unemployment in any instant of time, t, conditional on being still unemployed an infinitesimal amount of time to the left of t.

The hazard rate is normally allowed to depend on time, t, and on a vector $x(t)$ of socio-economic variables which are individual characteristics and labour market conditions. Spells of unemployment end at the time of exit out of unemployment (completed spells), if this is observed. Often some unemployment spells are still in progress at the ending time of the observation period —right-censored spells[4]. The dependency on time of the hazard rate is captured by what is normally called the *baseline hazard*. The importance of

[4]Right-censoring is a common feature of data on unemployment duration and is easily dealt with in the applied econometric analysis.

allowing for time dependency is discussed in the next section. The functional form adopted for the baseline hazard is presented in Chapter 3.

Multiple destinations out of unemployment are modelled by means of a *competing risks* model. Competing risks specifications model unemployment duration together with the states exited into upon leaving unemployment. Examples of such states are employment, retirement, education, sickness. Instead, *single risk* models, focus only on the duration of unemployment spells without distinguishing the destination states entered upon leaving unemployment. In a single risk framework exit from unemployment is defined as exit to any destination state.

In a competing risks context, the probability of leaving unemployment at a certain point in time, t, and of exiting to a specific destination state k, given the set D of possible exit states, can written as:

$$\theta_k(t) = \lim_{dt \to 0} \frac{P(t \leq T < t + dt, D = k, |T \geq t)}{dt}, \tag{1.5}$$

which is the so-called *cause-specific* hazard. The destinations k —also called failure types or simply exits/exit states– are assumed to be mutually exclusive, as plausible. It follows that the overall hazard rate, modelling exit to any state, can be thought of as the sum of the hazard rates of exiting into the states:

$$\theta(t_i, x_i(t)) = \sum_{k \epsilon D} \theta_k(t_i, x_i(t)), \tag{1.6}$$

where D is the set of destination states considered. More details on the econometric background are given in Chapter 3. The estimating econometric model is also presented in Chapter 3.

It is plausible to assume that the impact of socio-economic factors on the individual probability of leaving unemployment will vary with the states exited to upon leaving unemployment. Previous studies such as, for instance, Katz and Meyer (1988, 1990) and Meyer (1990) have confirmed this view. Previous applied UK studies on individual unemployment duration were conducted in a single risk framework, except for Narendranathan and Stewart (1993), who used a competing risks model to distinguish the probability of exiting from unemployment into employment from the probability of exiting from unemployment into other states. The authors compared the results of estimation of their preferred competing risks model with those of the corresponding single risk model and concluded that the estimated effects of most of the explanatory variables were similar in the two models although some effects were larger for the competing risks exit into employment.

The importance of using competing risks models of the probability of leaving unemployment is assessed in Chapter 4, by means of intuitive arguments and formal tests. I anticipate that the results obtained lead to different conclusions than Narendranathan and Stewart (1993).

1.5 Some insights into the problem of time dependency

The probability of leaving unemployment may vary purely as a function of the time spent in the state of unemployment. This issue has attracted considerable attention in the previous studies of individual unemployment duration given its implications for the purpose of economic policy.

It is possible that longer unemployment durations render the unemployed less "choosy" about accepting job offers, lowering their reservation wage. Consequently, the probability of leaving unemployment increases as a function of time, *ceteris paribus* —positive time dependency. It is, however, also possible that longer unemployment durations attach some kind of "stigma" to the unemployed which makes them less attractive as employees. The potential employers might, for instance, fear that the long term unemployed are less active or less motivated to work or have lost their previous work experience. This would lead *ceteris paribus* to decreasing chances of leaving unemployment with increasing unemployment duration —negative time dependency. Decreasing search intensity due to unemployed's discouragement might also result in negative duration dependency.

It is important for economic policy purposes to investigate whether and how the probability of leaving unemployment varies over time purely as a function of the elapsing of the time spent in the state of unemployment. Negative time dependency implies that unemployment may be a "vicious circle" for the long-term unemployed: the longer the unemployed stays unemployed, the smaller are their chances to exit from unemployment.

Time dependency has often been modelled as a monotonic function, using a Weibull functional form for the baseline hazard rate (see Chapter 3). However, this choice is not supported by theoretical arguments. The sign of the dependency on time of the probability of leaving unemployment (if any) may vary over the unemployment spell. Only recently flexible specifications of the baseline hazard rate have been employed, where the sign of time dependency is allowed to vary during the course of the unemployment spell. An interesting specification was for instance put forward by Meyer (1986). It represents a development of the model proposed by Prentice and Gloeckler (1978).

The only previous work on individual unemployment duration in Great Britain that modelled the baseline hazard rate using a flexible specification is due to Narendranathan and Stewart (1993). The authors adopted the same specification as Meyer (1986). The implications of using a flexible or a (more restrictive) monotonic specification of the baseline hazard rate are investigated in Chapter 4.

The issue of time dependency is closely related to that of unobserved heterogeneity, as pointed out by Lancaster (1979). The author argued that the unemployed with the "best" unobservable characteristics will be more

likely to leave unemployment first, so that negative time dependency might spuriously result. Findings of negative time dependency might simply reflect the fact that unobserved heterogeneity has not properly been controlled for. The issue becomes more complicated in the case of competing risks models. It is possible that unobserved individual heterogeneity might result in positive time dependency. For instance, if one looks at the probability of leaving unemployment to exit out of the labour force, the individual with the "best" unobserved characteristics with respect to the re-employment probability, will now be among the last to withdraw from the labour force. For instance, Katz and Meyer (1990, pp. 56) argue:

> *Although uncontrolled heterogeneity biases estimates of duration dependence in the total hazard towards spurious findings of negative duration dependence, a bias in the opposite direction is possible for an individual escape route hazard in a competing risks model. If uncontrolled factors that raise the recall rate also reduce the new job finding rate, then one can, at least in theory, generate spurious positive duration dependence in the new job finding hazard.*

Katz and Meyer,(1988, 1990) allow for two destinations out of unemployment: the recall into the previous job and the finding of a new job. They claim that unobserved heterogeneity might lead to spurious positive time dependency even in the case of exit into employment.

1.6 The structure of the thesis

The structure of this thesis is the following.

In Chapter 2, the LSUS survey is described and some preliminary descriptive analysis of the data is carried out. This is a useful exercize since these data have not been employed before in applied econometrics studies. The main features of the survey are first illustrated. The advantages and disadvantages of the LSUS data with respect to the analysis of unemployment duration are discussed and the LSUS data are compared to other UK and US datasets. The selection of the sample for the econometric analysis and the construction of some variables is reviewed next. I carry out some descriptive analysis of variables that deserve particular attention, such as the economic states occupied before or after experiencing unemployment and the amounts and types of unemployment benefit received during the course of the unemployment spell.

In Chapter 3, the econometric framework of analysis is laid out and the preferred econometric model is presented. This is a competing risks model where the baseline hazard is specified as a piecewise linear exponential and allowed to vary weekly. Two exit states out of unemployment are distinguished: full-time work and other states.

In Chapter 4, the results of estimation of single and competing risks models are compared. The models are first estimated by the non-parametric Kaplan-Meier method. The appropriateness of the competing risks specification is tested formally following the approach suggested by Narendranathan and Stewart (1991). The specification of a flexible baseline hazard rate is compared with a monotonic specification.

In Chapter 5, the impact of socio-economic factors on the conditional probability of leaving unemployment to take up a full-time job is investigated. The individual characteristics considered are for instance search activity, leisure valuation, spouse work activity, financial hardship and replacement-ratio variables. The impact of the unemployment benefit on the conditional probability of leaving unemployment is modelled not only in terms of the amount paid but also distinguishing the different types of benefit. The unemployed that receive UB at the time of commencement of their unemployment spell are expected to leave unemployment first relative to the recipients of SB. The rationale of this assumption is the different duration of entitlement to to UB and to SB. The estimating model is a competing risks model where exit into a full-time job is distinguished from exit into other states. Sensitivity of the results to the specification of a competing risks model where part-time work is considered together with full-time work is tested. The robustness of the estimated coefficient on the level of unemployment benefit with respect to the choice of regressors is also tested. The predicted mean duration is computed under different sets of assumptions on the observed characteristics of the unemployed.

In Chapter 6, the relationship between the potential duration of entitlement to unemployment benefit and the conditional probability of leaving unemployment is investigated. The problems that might hinder the estimation of this relationship are reviewed. The duration model is first estimated for the full sample of benefit recipients. The impact of benefit exhaustion is modelled with some timevarying dichotomous variables that take value one in given time intervals for the recipients of UB. The robustness of the estimated coefficients on these dummies is tested for. Next, the duration model is estimated separately for the the unemployed that receive UB and the unemployed that receive SB (either by itself or together with UB). The two estimated hazard rates are compared.

The object of Chapter 7 is estimating the impact of the level of financial resources of the unemployed on the re-employment probability. First, the theoretical background is laid out. Next, some descriptive analysis of the financial resources of the unemployed is presented. A competing risks model of the re-employment probability is estimated. The robustness of the results is tested for. The last Section concludes.

Conclusions to the thesis are drawn in Chapter 8.

Chapter 2

Description of the data and preliminary analysis

2.1 Introduction

The data employed are taken from the survey of the Living Standards during Unemployment (LSUS). This survey was carried out by the Office of Population Censuses and Surveys on behalf of the Department of Health and Social Security (DHSS)[1] in 1983/1984.

The LSUS data are largely unexploited. Only the survey planners have analysed them, at least to my knowledge, and mostly from a descriptive point of view (Heady and Smith, 1989). Therefore, I spend some time describing those features that are of most interest for the analysis of unemployment duration.

In particular, I focus on the economic states that the "LSUS" unemployed reported to occupy before and after entering unemployment. Too often labour economists implicitly assume that the only alternative to unemployment is employment. The importance of explicitly allowing for the many economic states that exist in the real world in models of unemployment duration has been stressed for instance by Atkinson and Micklewright (1991). It is shown here that even within each broad state of employment, unemployment and out of the labour force many other states can be distinguished. For example, unemployment covers both "registered" and "non-registered" unemployment. Registered unemployment relates to the unemployed that are registered with the Unemployment Benefit Offices (UBO) to receive unemployment benefit. Non-registered unemployment refers to the unemployed that do not receive unemployment benefit.

The other set of variables that I analyse are unemployment benefit receipts. In spite of the attention paid in the literature to the disincentive effects of the unemployment benefit level on the probability of exiting from unemployment, the actual workings of the benefit have not been paid much

[1] Now called simply DSS.

attention. As Atkinson and Micklewright (1991, pp. 1680) argue

> *we are concerned with the fact that —with some notable exceptions— labour market economists have paid little attention to how unemployment benefits actually work.*

The LSUS survey contains detailed information on the savings and debt of the unemployed and their spouses at different points in time. Access to such information is not common in previous studies. However, it might be the case that the financial resources of the unemployed influence their unemployment duration. The reader is referred to Chapter 7 for a descriptive analysis of the financial resources of the unemployed.

The structure of this Chapter is the following. In the next Section 2.2, the main features of the LSUS data are presented. Advantages and disadvantages of this survey are reviewed in relation to other datasets that have been used for the analysis of unemployment duration, in Section 2.3. In Section 2.4, I describe how I use the LSUS data. First, I discuss the selection of the sample for analysis. I describe the economic states occupied in the week before the commencement of the observed spells of registered unemployment. The construction of some of the variables that are used in the econometric analysis, such as the duration of the unemployment spells and the expected earnings of the unemployed, is illustrated next. Some descriptive analysis of the economic states occupied before and after the commencement of the registered unemployment spell follows. The types and amounts of unemployment benefit received at the two interviews are analysed in Section 2.5. Given the limited information on the possible variation in the types and amounts of unemployment benefit received during the course of the unemployment spell, I have imputed some of these changes, which are then described. Conclusions follow.

2.2 A description of the LSUS data

The LSUS survey was planned with the aim of gathering information on the change in the standards of living of the unemployed. The survey sample is drawn from the population of the unemployed that started to register at mainland Great Britain Unemployment Benefit Offices (UBOs) in the Summer of 1983, between 21st June and 20th August 1983[2]. The unemployed sampled were interviewed twice. The data can therefore be classified as a longitudinal sample of the inflow into registered unemployment.

The sample was selected adopting a two stage procedure. The first stage sample units were Great Britain Unemployment Benefit Offices, the second stage units were the unemployed that began their spells of registered unem-

[2]Both unemployment benefits UB and SB described in the introduction are paid at UBOs.

ployment in the Summer of 1983[3]. It has to be pointed out that the survey relates to Great Britain and not the UK given that Northern Ireland UBOs are not sampled[4].

The sample units

The unemployed with the following characteristics were sampled:

1. *they started to register at unemployment benefit offices in the summer of 1983, between 21st June and 20th August 1983;*
2. *they were either married men or single people of either gender living on their own or with their children;*
3. *they were aged between 20 and 58;*
4. *they had been signing on[5] continuously for three months following the commencement of their registered unemployment spell, in the summer of 1983.*

The first characteristic defines the unemployment covered by the survey as registered unemployment. Unemployment is defined on the basis of administrative records rather than according to individual's self-perception or intensity of search. An inflow sample of the unemployed is drawn: those unemployed that started their unemployment spell within a given interval of time (the summer of 1983) are sampled. The opposite concept is that of a stock sample, which is a sample of the population of the unemployed at a given point in time. In a stock sample, the unemployed will have began their unemployment spells at very different points in times.

The second criterion defines "household heads". As a consequence of this criterion, those living as single people in shared accomodation or living at home with their parents were excluded from the sample. Also married women were not sampled[6]. For the survey purposes, individuals are defined as married if they are either actually married or cohabiting.

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[3]The reader is referred to Heady and Smith (1989) for more details of the sampling procedure.

[4]The same applies to the DHSS Cohort Study of the unemployed analysed by Narendranathan and Stewart (1993, 1993a). The United Kingdom includes Northern Ireland, Wales, Scotland, England; Great Britain includes England, Wales and Scotland, but it excludes Northern Ireland.

[5]"Signing on" means in the British jargon going to social security offices to confirm that one is unemployed in order to get state benefits.

[6]This criterion for sample selection might have followed from the rules governing married women's entitlement to UB and SB in 1983/84. Until 1984 married women were not entitled to claim SB and they also had the possibility of opting out of the UB scheme by choosing to pay reduced rate contributions.

The third criterion excludes young people aged less than 20 and people close to their retirement age. Male unemployed aged less 20 represented about 15% of the population of the unemployed at July 1983[7].

The fourth criterion was such that the unemployed for less than three months were not interviewed. Following the actual sampling of the inflow into unemployment, interviews were conducted only with those persons that had unemployment spells longer than three months. The rationale for this sample selection criterion was that the survey planners aimed at gathering information on changes in the unemployed's standards of living due to the experience of unemployment and therefore they were not interested in short-term unemployment. The first interview took actually place three months after the inflow sample was drawn. The second interview was conducted a year after the first interview.

The informational content and the structure of the survey

The householders sampled and their spouses were interviewed twice. The participants in the first interview were 2929 unemployed persons of whom 212 (7.2%) were single women (there were 396, 13.5%, single men among the participants to the first interview). The respondents to the second interview were 2299, of whom 168 (7.3%) were single women and 279 (12.1%) were single men, where marital status is defined with respect to the first interview.

The first interview took place about three months after the commencement of the unemployment spells. Detailed information was collected on the demographic, sociological, economic and financial characteristics of the sample informants and their spouses at the first interview and one month before the commencement of the observed spells of registered unemployment. At the time of the first interview retrospective questions were also asked on the economic activity week-by-week in the year before the commencement of the observed spell of registered unemployment.

The second interview was carried out about a year after the first interview, i. e. about 15 months after the commencement of the unemployment spells. Detailed information on the socio-economic characteristics of the unemployed and their spouses was collected. At the time of the second interview, questions were asked retrospectively on the economic activity week-by week during the year falling between the two interviews. This information allows one to compute the duration of the unemployment spells and to observe the economic state entered upon leaving unemployment, if any.

The structure of the information collected by the survey is illustrated best by Table 2.1[8]. The longitudinal structure of the data enables one to gather some limited information on the change if any in the types and amounts of unemployment benefits received. This information is exploited in the later Sections.

[7]Source: Employment Gazette, September 1983.

[8]This table is extracted from Heady and Smith, 1989, Vol. I, pag. 2.

Table 2.1: *The structure of the dataset*

Longitudinal structure	
Content of first interview	
History:	*description of the sampled person's last permanent job; week by week data on economic status in the year before signing on.*
Situation one month before signing on:	*detailed cross-sectional dataset with limitation imposed by the practicability of collecting information retrospectively.*
Situation three months after signing on:	*cross-sectional dataset.*
Content of second interview	
Events between 3 and 15 months after signing on:	*week by week data on economic status of sampled person and of spouse.*
Situation 15 months after signing on:	*cross-sectional dataset*
Contents of cross-sectional datasets	
Economic status of sampled person and spouse; income, savings and debt; financial commitments; material living standards; informants' opinions; measures of psychological welfare; domestic, social and leisure activities; sources of help.	

All the information collected by the survey is based on self-reports by the unemployed to the interviewers and not on administrative records. The drawback of self-reported information is the possible presence of recall error or misreporting, as will be discussed later on.

The response rate and the problem of attrition

Attrition is defined as the loss of sample participants across different waves of a panel survey. Attrition is a problem when non-participation in successive interviews is related to the object of the analysis. In the analysis of unemployment duration this would be the case if non-response to successive interviews were associated with the sample informants having gone back to work. This would imply for the LSUS data that the estimates of the conditional probability of leaving unemployment obtained considering only the subsample of second interview participants underestimate the rate of exit from unemployment. Some analysis of attrition will be carried out below.

Heady and Smith (1989, Vol. 1 p. 4) report that the overall response rate to the first interview was between 67.9% and 71.4% , taking into account non-response to the sift questionnaires and to the main questionnaire[9]. The response rate to the second interview was 79%. among first interview

[9]A range is given because computing non-response to the sift questionnaires is not straightforward.

respondents[10].

Heady and Smith (1989) observe that the response rate to the second interview was about 79% for all age classes and family types of first interview respondents but that a much lower proportion (67%) of younger single people participated. Non-respondents at second interview can be classified as follows, using information gathered by the interviewers:

- *respondents (8) died between the two interviews;*
- *respondents (25) had already said at the time of the first interview that they were not willing to participate in the successive interview;*
- *respondents (297) refused to participate in the second interview, when they were actually contacted in order to be interviewed;*
- *respondents (293) could not be contacted.*

For 56% of the last two categories (590 people), some information on the economic activity at the second interview was also collected by the interviewers[11]. It turned out that, at the time of the second interview, 53.6% were in full-time work, 37.5% were in registered unemployment, 9% had left unemployment and were in states other than full-time work[12]. The same figures for the male subsample to be defined in Section 2.4.1 are 46%, 41% and 13%. It appears that the proportion of persons that have gone back to work is slightly higher among non-respondents than among respondents to the second interview. However, these estimates relate to only 56% (590) of the non-respondents to the second interview. It is not possible to exclude that the proportion of persons that took full-time employment was larger for those unemployed for whom no information of any sort on their later economic activity could be recovered.

To gain more insights into the extent of possible problems arising from attrition I have estimated a binary probability model of the non-response to the second interview[13]. The results of estimation of a probit model of the

[10]For comparison purposes, I give here the corresponding figures for another UK longitudinal survey of the unemployed. The response rate for the DSS Cohort Study of 1987/88 was 70% to the first interview and 71% to the second interview, as reported in Garman et al. (1992).

[11]Some information concerning the last category of people namely those that could not be contacted was gathered by asking their relatives or neighbours.

[12]This information will not be used for duration analysis, since not much is known about the economic activity of these persons during the time in between the two interviews. This information does not tell us when the spell of unemployment, started in the summer of 1983, was actually terminated nor the state to which the person had exited to, if any. For instance, somebody might have concluded their "first" observed unemployment spell to take up a full-time job and then gone back into unemployment by the time of the second interview. However, one could make the assumption that the unemployment spell ended some time between the first and the second interview for the non-respondents that were back to work at the time of the second interview.

[13]See for instance Maddala (1988) for an introduction to binary probability models.

probability of participating in the second survey interview are presented in Table 2.2. The dependent variable is the probability of participating in the second survey interview. The explanatory variables considered are those of the preferred duration model of Chapter 5, to which the reader is referred for their description. The analysis is carried out only for the male unemployed.

Table 2.2: *The probability of participating in the second survey interview: a probit model*

Variable	*Coeff*	*SE*
F/t work most of time in the year before	*0.2634**	*0.0954*
Unemployed most of the time in year before	*0.1332*	*0.1018*
Sick out of work most of the time in year before	*0.1177*	*0.1653*
Profess. /Interm. Occupation class	*-0.0891*	*0.0769*
Unskilled Occupation	*-0.0235*	*0.1177*
Age 25-34	*-0.1508*	*0.0969*
Age 35-44	*-0.1924*	*0.1038*
Age 45-54	*-0.1419*	*0.1128*
Age 55-58	*-0.0891*	*0.1316*
Any child aged < 5	*0.1226*	*0.0756*
Married	*0.2578**	*0.0862*
Spouse working month before	*-0.1029*	*0.0761*
Searches less than before	*0.1587**	*0.0787*
Values Leisure more	*0.0089*	*0.0822*
Experiences money shortage	*-0.0977*	*0.0680*
House owner	*0.0630*	*0.0648*
County unemployment rate	*2.9410**	*0.8718*
Receives only UB at t1	*-0.0123*	*0.0665*
Benefit amount (£) logs.	*-0.0548*	*0.0352*
Expected earnings (£) logs.	*0.0530*	*0.0485*
Expected earnings not available	*0.2284*	*0.3288*
*Max. lik. : -1383.78. A * indicates statistical significance at the two sided 5% level. Descriptive statistics of the explanatory variables are given in the Chapter 5. The model is estimated only for male unemployed.*		

From Table 2.2, it emerges that most explanatory variables considered do not affect significantly the probability of participating in the second survey interview. These variables are instead found to affect significantly the probability of leaving unemployment, as shown in Table 5.2 of Chapter 5. This result is quite encouraging. The probability of participating in the second survey interview is significantly higher for persons with the following characteristics: married people; that were in full-time work most time in the year before becoming unemployed; residing in an area with (relatively) higher unemployment rates; demonstrating little search activity. These last two variables affect the probability of staying in the sample in an opposite direction than the re-employment probability. The first two variables listed above affect instead both probabilities in the same direction. To sum up there is no strong indication that non-response might be associated with the non-respondent having gone back to work. Only two of the variables considered are found to affect significantly and in the same direction the probability of participating in the second survey interview and the re-employment probability. However, it is not possible to exclude that unobserved individual characteristics might affect the two probabilities in the same direction. I have considered only a very simple model of non participation. More sofisticated models would involve estimating the two probabilities simultaneously and testing for any correlation across the errors in the model. A good reference in this respect for the analysis of attrition is van de Berg et al. (1991)[14]. Given the large number of explanatory variables here considered, some of which capture individual characteristics that are normally unobserved, it seems unlikely that attrition might constitute a serious problem.

2.3 An assessment of the LSUS data and a comparison with other datasets

The advantages and the disadvantages of the LSUS survey with respect to the analysis of unemployment duration are discussed below. At the same time, the LSUS data are compared with other data that have been used for the analysis of unemployment duration in the UK and in the USA. These two countries are selected for comparison since the majority of the literature I refer to relates to them.

The other surveys considered for the UK are: the DSS Cohort Study of the unemployed of 1978/79, analysed by Narendranathan et al. (1985) and by Narendranathan and Stewart (1993, 1993a); the DSS Cohort Study of the unemployed 1987/88; the national survey for Political and Economic Planning (PEP) of 1974 employed, for instance, by Lancaster (1979), the General Household Survey (GHS) 1972 analysed by Nickell (1979) and the

[14]The authors estimated simultaneously the duration of participation in the sample and the duration of unemployment[15], for a sample of Dutch unemployed. Their results point to independency of survey participation duration and unemployment duration.

Family Expenditure Survey (FES) 1972-1977 used by Atkinson et al. (1984). The surveys considered for the USA are: the Continuous Wage and Benefit History (CWBH) data employed, among others, by Moffit (1985), Katz and Meyer (1988) and Meyer (1990); Panel Study of Income Dynamics (PSID) analysed, for instance, by Katz (1986) and Katz and Meyer (1990).

Ideally in order to conduct some microeconometric analysis of the determinants of the duration of unemployment one would need information about the local labour market conditions, the duration of the unemployment spells, the destination states out of unemployment, the amounts of unemployment benefit and other sources of income received while unemployed, the expected income from work and other individual characteristics, such as age, gender, family composition, previous work history, search activity. Some of these variables such as, for instance, the local labour market conditions and the amount of unemployment benefit received, may vary over the course of the unemployment spell. Information on any changes in the values of these variables intervened during the course of the unemployment spell should also be available. There is no need to say that all information should be as detailed and accurate as possible.

A first advantage of the LSUS data stems from its longitudinal structure which allows one to observe the ending time of the unemployment spells and the economic states occupied upon leaving unemployment, at least for those unemployment spells that are completed by the time of the second interview. Normally cross-section surveys such as the GHS and the FES do not contain such information. As a result, the likelihood functions for the data in any analysis of unemployment duration with such cross-section samples are very complicated and require an assumption about the inflow rate. Moreover, such cross-section samples do not usually contain any information on the economic states entered upon leaving unemployment by the sample informants.

A second advantage is that the period of time covered by the survey is one of high and rising unemployment rates, while the majority of previous applied UK studies relate to the seventies when the rate of unemployment was much lower. Indeed, the re-employment probability is likely to be influenced by the overall level of unemployment.

A third advantage of the survey is that it collects detailed information on the socio-economic characteristics of the unemployed. There is for instance information on the states occupied before unemployment, on the individual valuation of leisure, search activity, financial constraints and on the participation of the spouse in the labour force. Moreover, most of these variables are observed a short time (one month) before the commencement of the observed unemployment spells. This allows one to avoid potential endogeneity problems. For instance, search intensity may vary as a function of the duration of the unemployment spell. Furthermore, the LSUS survey contains detailed information on the savings and debt of the unemployed. This information is unique in the sense that there are, to my knowledge, no other datasets that contain such information.

A fourth advantage is that the survey covers the situation of the unemployed for longer than a year. This is particularly important given that in Britain unemployment is concentrated in durations longer than a year, as shown in Figure 2.1. Moreover a year coincides with the maximum entitlement to UB[16]. Therefore, it is possible to observe the behaviour of the unemployed beyond UB exhaustion. The two DSS Cohort studies do not follow the unemployed for longer than a year. Similarly, the US CWBH does not follow the unemployed beyond unemployment benefit exhaustion. The CWBH data used by Katz and Meyer (1988) were instead supplemented by telephone interviews which collected also information on the unemployed behaviour after entitlement to unemployment benefit had expired.

The main disadvantage of the LSUS data is that they contain little information on any change in the amount or the type of unemployment benefit received during the course of the unemployment spell. This problem is discussed in more detail in the later sections, where also some imputations of the changes in benefit receipts are made. A similar problem affects the FES, PEP and GHS but not the DHSS Cohort Studies nor the CWBH. This problem can hinder the estimation of the unemployment benefit effect. However, using imputed benefit receipts rather than the actual values might introduce additional problems into the regression.

Another disadvantage of the LSUS survey stems from the administrative definition of unemployment adopted. Also the DSS Cohort studies and the CWBH adopt an administrative definition of unemployment. As Atkinson and Micklewright (1991) point out different definitions of unemployment may have different implications for the analysis of the duration of unemployment. On the basis of the administrative definition adopted here any person receiving unemployment benefit (UB or SB) is considered as unemployed regardless of their actual search activity. Instead, there might be persons not entitled to unemployment benefit that are actively searching for jobs in the labour market. These individuals will not be considered as unemployed, if an administrative definition of unemployment is adopted. Another drawback of defining unemployment on the basis of administrative records is that administrative law may be used to control artificially the figures on unemployment, for instance by tightening the conditions for entitlement to unemployment benefit.

A further disadvantage of the data is due to the self-reported nature of all information contained in the LSUS survey. However, most surveys are based on individual self-reported information. Exceptions are for instance the DHSS Cohort Studies and the CWBH which contain administrative information on the duration of the unemployment spells and on the amounts of unemployment benefits paid. Self-reported information might be biased by recall error or misreporting. For instance, the retrospective information on

[16]The reader is referred to Section 1.3 of Chapter 1 for a brief description of the UK system of unemployment benefits.

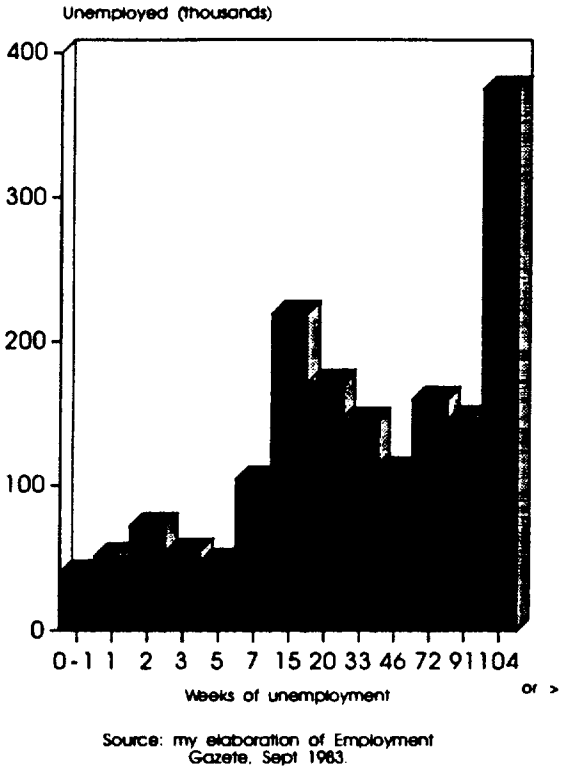

Figure 2.1: *The distribution of unemployment durations*

the economic activity week-by-week in the year between the two interviews, which is used to construct the duration of the unemployment spells and to define the exit state out of unemployment (if any), might be biased by recall error or misreporting. However, this does not seem a major problem in the case of the LSUS data. Some analysis of the accuracy of the information on the amounts and types of benefit reported is provided in the next sections.

Another drawback of the LSUS data is the non-coverage of unemployment spells that lasted for less than three months. However, this problem is not very serious since one can condition each unemployment spell contribution to the likelihood for the LSUS data on the probability that the spell lasted for longer than three months. One could also argue that the real problem for the economy is not short-term unemployment but long-term unemployment. Figure 2.1 shows the distribution of unemployment spell by their durations for the population of the unemployed at July 1983 (not for the LSUS sample). The Figure relates to the male unemployed aged between 20 and 59[17] at July 14th 1983. I have considered only males since it was not possible to distinguish between married or single women in the published figures. The bulk of unemployment is concentrated in durations longer than three months. At July 1983, the male unemployed for longer than 12 weeks were 78% of the population of male unemployed aged more than 20 or less than 59.

[17]The limit 59 rather than 58 was chosen because of the structure of all information in the Employment Gazette which groups together age 58 and 59.

To conclude other problems of the LSUS survey are that no information on race nor on the educational attainments of the unemployed is collected.

None of the above drawbacks is however so serious that one should restrain from using the LSUS data to investigate the determinants of individual unemployment duration. It seems instead worth the effort to carry out some analysis of unemployment duration using the LSUS data given the attractive features of the data described above.

2.4 Using the LSUS data

The preparation of the LSUS data for econometric analysis is reviewed below. The criteria that lead to the selection of a subsample of the unemployed are reported first. The construction of the duration of the unemployment spells, of the exit states and of the expected earnings from work is presented next. Some descriptive analysis of the states occupied before and after entering unemployment; of the type and the amount of unemployment benefit received; and of how it may vary over course of the unemployment spell follows. A section on social security benefits other than "UB" and "SB" concludes.

2.4.1 The subsample for the econometric analysis

Information on the week-by-week economic activity of the sample informants in the year before the commencement of the the unemployment spell and in the year falling between the two sample interviews is collected retrospectively.

The LSUS survey allows one to distinguish the following economic states:

- *full-time work;*
- *registered unemployed and entitled to benefit;*
- *registered unemployed and not entitled to benefit;*
- *full-time education;*
- *government scheme;*
- *part-time work of over 10 hours per week;*
- *part-time work of 10 hours or less per week;*
- *sick and out of work;*
- *in prison;*
- *looking after family;*
- *other.*

The survey allows for four possible states within the labour force: full-time work; part-time work of more than 10 hours per week; part-time work of 10 hours or less per week; registered unemployment.

"Work" is defined, for the survey purposes, as (ESRC, 1990(a), pag. 3.): *"regular work regardless of hours and excluding odd jobs, mail order agent, baby sitting, but including people away sick with work to return to"*. Full-time work is defined as (ESRC,1990,a,pag. 3.): regular work of *"more than 30 hours per week, except teachers and lecturers, and including people working short-time if job is normally full-time"*. Registered unemployment is defined as registration with Unemployment Benefit Offices, on the basis of administrative records. Some unemployed , although registered at UBO, are "not entitled to benefit" because they are for instance awaiting to receive benefit or temporarily suspended from benefit receipt.

Five "out of the labour force" states are distinguished within the OPCS survey: full-time education; government training scheme; care of famlly; sickness; prison.

The category "sick and out of work" is defined as *"being out of work and having medical or doctor's certificate exempting one from signing on"* (ESRC, 1990a, pag. 12).

The state "other" includes any other state not previously considered. It includes for instance *"on strike at the end of an employment spell"* (ESRC, 1990a, pag. 12). It includes also *"answers which imply rather than a state that the person was not entitled to benefit. For instance: 'paid up till here'; 'finished work, waiting to sign on'; 'pay in lieu of notice'"*[18]. Unfortunately it is not possible to distinguish these cases from the others. I have then recoded the state "registered unemployment but not entitled to benefit" under "other states". Retirement is not considered because of the age range (20-58) covered by the survey. However, there is no mention of whether early retirement schemes might be coded under the category "other states".

There is a question of whether or not the state "in care of family" should be considered as a "marginal job" and, therefore, a state within the labour market, as suggested by Atkinson and Micklewright (1991). The same question applies to the state "government training scheme". However, trainees that are receiving a proper wage rather than a small allowance are already coded by the interviewers as in full-time or part-time work, as appropriate.

The economic state occupied in the week before entering unemployment are summarized in Table 2.3 for the subsample of second interview respondents[19]. The proportion of first interview participants in the different entry states is actually very similar and can be looked up in the published reports (Heady

[18]The unemployed are not entitled to unemployment benefit while receiving pay in lieu of notice.

[19]As will be explained below, unemployment duration analysis will be carried out only for this subsample of the unemployed. Second interview participants are 2299. However, for two of them the information on the economic activity undertaken in the week before the commencement of their registered unemployment spell is not available; i. e. missing.

and Smith, 1989, Vol. 1, p. 5).

As shown in Table 2.3, almost 55% of the unemployed were in full-time work in the week before registering as unemployed at UBO. This confirms that it is not enough to describe the possible economic states as either employment or unemployment. However, it is also true that the majority of the sampled unemployed were in full-time work before becoming unemployed. A considerable number (7.8%) were sick and out of work in the week before registering at UBOs. A large fraction (20.2%) were classified in the state "other". Of those people whose entry state is classified as "other", 28% reported to have received some pay in lieu of notice.

Some unemployed —129, as shown in the Table 2.3— turned out to be already in the state of registered unemployment in the week before the time when, according to the survey, they began their registered unemployment spell. Heady and Smith (1989, pag. 5) comment upon this by observing that: *"The fact that 6% of the sample were actually signing on for most of the previous week is perhaps particularly surprising, but can happen because of technical interruptions in the process of signing on and claiming, which may lead to being counted as starting a new spell"*. Another possible explanation is that some unemployed had just moved to different geographical areas and they were erroneously counted as starting a new spell of unemployment.

Table 2.3: *The entry states into unemployment*

Economic states in the week before	*2nd interview participants*		*Subsample used for analysis*	
	No.	*%*	*No.*	*%*
f/t work	*1236*	*53.8*	*1252*	*57.0*
signing on	*129*	*5.6*	*0*	*0*
f/t education	*53*	*2.3*	*54*	*2.6*
govt scheme	*62*	*2.7*	*63*	*2.9*
p/t work over 10 hrs	*49*	*2.1*	*49*	*2.2*
p/t work 10 hrs or less	*11*	*.5*	*11*	*.5*
sick and out of work	*196*	*8.5*	*201*	*9.1*
in prison	*43*	*1.9*	*44*	*2.0*
looking after family	*41*	*1.8*	*41*	*1.9*
other	*465*	*20.2*	*469*	*21.3*
not entitled to benefit	*12*	*.5*	*12*	*.5*
Total Sum	*2297*	*100.0*	*2196*	*100.0*

The majority of those people whose entry state turned out to be registered unemployment were deleted from the sample for econometric analysis. For few of them (28) it turned out that they were in states other than registered unemployment two weeks before the recorded commencement of their

registered unemployment spell. These persons were kept in the sample for analysis[20]. Most of them were in full-time work at that time, as shown in Table 2.3 by the difference between the numbers in the second and fourth column of the first row.

The unemployed that were "not entitled to benefit" in the week before starting their registered unemployment spell were in the same position as the people in the category "other", which were receiving pay in lieu of notice. Therefore, these people were kept in the sample for analysis.

The subsample for analysis is restricted to male unemployed only. This makes the results of the analysis comparable with those obtained in previous UK studies of unemployment duration, which relate to men only. Including the subsample of single women unemployed may, indeed, throw considerable unobserved heterogeneity into the sample. Recent literature, as, for instance, Jenkins (1992) and Blundell et al. (1992) has focused on the peculiar situation of "lone mothers" with respect to the decision to work, given the special social security treatment they enjoy[21]. Comparing the duration of unemployment of the female unemployed with that of the male unemployed is the subject of a separate piece of work (Stancanelli, 1994).

2.4.2 The duration of the unemployment spells

The duration of the unemployment spells was constructed (by myself) as described below.

To start with, three types of unemployment spells can be distinguished, as illustrated in Figure 2.2. Let us define "t0" as the starting time of the unemployment spell, "t1" the time of the first interview (about three months later) and "t2" the time of the second interview (after a further 12 months). The times t0, t1 and t2 differ for each individual, although each of them falls in a given interval of time. For instance, t0 lies somewhere between 20th June and 21 August 1983, t1 falls in the Autumn of 1983 and t2 in the Autumn of 1984.

Unemployment spells of type "a" last less than three months and are not observed given the sample design. In the econometric analysis of unemployment duration, I allow for the fact that only spells that last longer than three months are observed; i. e. for left truncation of the sample at the three months point. Spells of type "b" are completed spells, they last longer than three months and end before the time of the second interview, t2. For these spells, exit from unemployment is observed to take place between t1

[20]Their registered spell of unemployment was amended and set one week longer than it would have otherwise been. Similarly their entry state was corrected and set equal to the state actually occupied two weeks before.

[21]It is interesting to know that while the social security legislation the authors refer to in their work applies to lone parents of either gender, they are considering only lone mothers as having a disincentive to work, the reason being that 90% of lone parents are women.

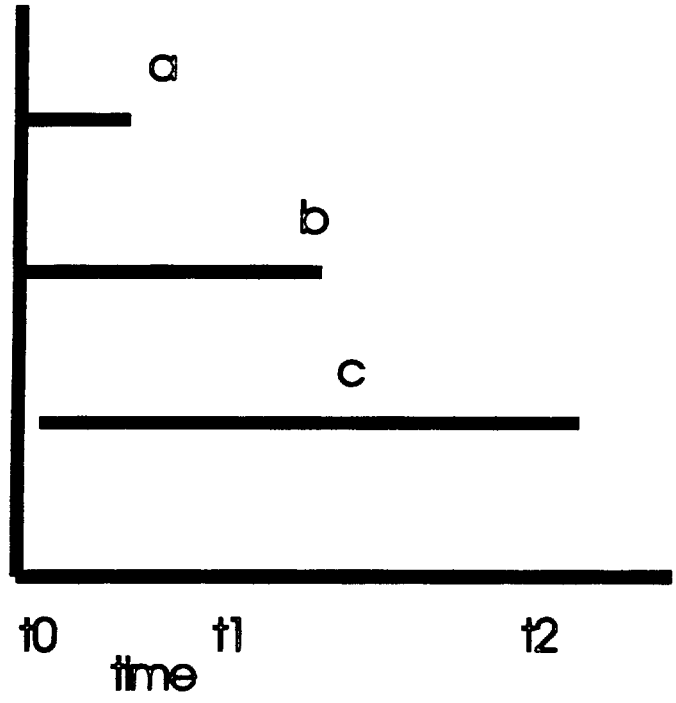

Figure 2.2: *Types of unemployment spells*

and t2. Spells of type "c" are right-censored since they are still in progress at the second interview, t2. Right-censoring is a typical feature of duration or transition data. Often some proportion of the sample units have not exited to a new state by the end of the observation period. This is easily allowed for in the econometric model.

The duration of the unemployment spells was constructed for spells of type "b" and "c" using the information on the week-by-week economic activity in the year falling between the two interviews. The unit of time considered throughout the analysis is the week and time is assumed to be continuous.

Unemployment duration can be computed only for the participants to the second interview, since the information on the economic activity week-by-week after the commencement of the unemployment spell was collected at the second interview[22]. This is one of the reasons for restricting attention to this subsample of the unemployed.

The duration of the unemployment spells was computed as follows.

1. The survey weekly calendar time was transformed in "homogenous" non-calendar units of time, setting the first week of registered unemployment equal to one for each individual— rather than for instance to the second week of July or the third week of August— and counting

[22]Non-participants to the second interview could be treated as right-censored at the first interview. However, given of the sample design they should also be treated as left-truncated at the first interview, which implies that their contribution to the sample likelihood would be irrelevant.

up[23].

2. The individual left truncation period was set equal to the lapse of time (in weeks) between the date of registration at benefit offices and the date of the first interview. The resulting variable varies between 11 and 17 weeks, since the first interview took place in the Autumn of 1983 and the unemployed began their spells in the Summer of 1983.

3. The time the individuals had been unemployed between the two interviews was computed on the basis of the replies to the retrospective questions on the economic activity in each week in the year falling between the first and the second interview. The exit state variable was constructed using the same information, for completed spells. Spells still in progress at the second interview are right-censored.

4. The duration of the first spell of unemployment was set equal to the sum of the two durations constructed at points 2 and 3.

Table 2.4 illustrates the variation in the individual left truncation points. Most spells, about 38%, present a left truncation point of 13 weeks. The left truncation point falls between 12 and 14 weeks for 85.5% of the sample.

Table 2.4: *Left truncation points*

truncation	*No. of spells*	*Percentage*
11	*23*	*1.1*
12	*363*	*17.8*
13	*770*	*37.8*
14	*609*	*29.9*
15	*195*	*9.6*
16	*62*	*3.0*
17	*13*	*.6*
Sum	*2035*	*100.0*

The truncation intervals are measured in weeks. The table refers to the male subsample.

Table 2.5 shows the frequency of the duration of the unemployment spells. All right-censoring takes place from week 63 onwards. The completed spells in Table 2.5 relate to exit from unemployment into any economic state. There is no indication that the reported durations might be biased by "rounding recall error"; i. e. there is no evidence of durations been bunched at regular intervals of time. This is instead often the case when retrospective questions are asked on the duration of the unemployment spell (Torelli and Trivellato,

[23]It should be remembered that unemployment spells that started in the summer of 1983 were sampled. The time of commencement of the unemployment spells differs therefore across the sampled unemployed.

1993a and 1993b). However, in the case of the LSUS data a separate question is asked about the respondent's economic activity in each given calendar week. One might conclude that this type of question probably avoids the occurrence of rounding recall errors.

I do not use the information on any subsequent spells of unemployment recorded in the data. Multiple spells of unemployment are therefore ignored. For instance, a person exiting from unemployment at week 21 to enter full-time work and then going back into unemployment or perhaps sickness in week 30, is recorded as having unemployment duration of 20 weeks and exit state full-time work.

Table 2.5: *Unemployment spells*

unemployment duration	*censored spells no.*	*complete spells no.*	*unemployment duration*	*complete spells no.*	*censored spells no.*
12		*4*	*43*		*18*
13		*25*	*44*		*28*
14		*44*	*45*		*21*
15		*42*	*46*		*18*
16		*43*	*47*		*22*
17		*39*	*48*		*15*
18		*33*	*49*		*23*
19		*31*	*50*		*24*
20		*26*	*51*		*10*
21		*27*	*52*		*19*
22		*18*	*53*		*25*
23		*21*	*54*		*19*
24		*27*	*55*		*15*
25		*27*	*56*		*15*
26		*31*	*57*		*11*
27		*27*	*58*		*16*
28		*31*	*59*		*15*
29		*25*	*60*		*14*
30		*26*	*61*		*13*
31		*36*	*62*		*12*
32		*22*	*63*	*9*	*11*
33		*38*	*64*	*135*	*10*
34		*29*	*65*	*300*	*7*
35		*21*	*66*	*236*	*2*
36		*12*	*67*	*103*	
37		*27*	*68*	*34*	
38		*24*	*69*	*7*	
39		*23*	*71*	*1*	
40		*18*	*Column*	*825*	*1210*
41		*18*	*Total*	*40.5*	*59.5*
42		*12*			

2.4.3 The exit states out of unemployment

The exit states that can be distinguished using the information contained in the LSUS survey are the same economic states which were described in Section 2.4.1: full-time work; part-time work of more than ten hours per week; part-time work of less than ten hours per week; full-time education; government scheme; sickness; looking after family; prison; other states. Information on the exit states is used in the specification of the competing risks models in the later chapters, although at a much less disaggregated level given the small number of exits to most of these states.

Table 2.6 compares the exit states with the entry states for the male unemployed selected as described in Section 2.4.1. This table gives a "snapshot" of individual history with respect to the economic states occupied throughout one's life. The cell frequencies rather than the percentages are shown in

Table 2.6: *The entry and exit states of the male subsample*

entry state	*exit states out of unemployment*										
	censored	*f/t*	*ed.*	*gov.*	*p/t >*	*p/t ≤*	*sick*	*pris.*	*fam.*	*oth.*	*total*
f/t work	453	612	4	17	29	5	47	1	1	25	1194
f/t education	9	22	1	1	4	1	1	1		1	40
gov. scheme	24	26	1	6			3			1	61
p/t work > 10h	13	16		4	3	1	1				38
p/t work ≤ 10h	1	1		1	1	1	1				6
sick no work	100	48	1	6	3		16			5	179
prison	30	7		1			3	3			44
care of family	11	2		1	1		2				17
other	183	198	2	7	16	3	23	1	3	18	442
total	824	932	9	44	57	11	97	5	4	50	2033

The table relates to the subsample of males participating in the second interview, constructed as in the paragraphs above. There are two "missing cases", for whom such information is not available.

order to retain full information, given the very small numbers found in some of the matrix cells. The state of "not being entitled to benefit" has been merged with "other states". The number of persons whose entry state was "not entitled to benefit" were 12; those whose exit state was not "entitled to unemployment benefit" were 7.

Two points emerge. First, a quick glance at this table should persuade the reader that reducing the economic world to the two standard states of employment and unemployment is too strong a simplification of reality. However, it is true that about 59% of the unemployed were in full-time work before becoming unemployed. Furthermore, almost 46% of the unemployed exited to full-time work. About 77% of the unemployed that exited from unemployment exited to full-time work. Second, the table reveals how people move across states. Having been in a certain state before becoming unemployed does not necessarily imply going back to the same state upon leaving unemployment. People from all different "backgrounds" of economic entry

states into unemployment are observed to leave unemployment to take up a "regular" full-time job. For instance, almost 30% of those that were sick before entering unemployment find a full-time job before the time of the second interview. The same percentage for those that were in full-time work before unemployment is about 66%. The corresponding figure for the individuals entering unemployment from "other" states is about 45%. Instead, about 4% of those that were in full-time work before becoming unemployed end their unemployment spell in sickness.

For the purpose of the later econometric analysis the unemployed exiting into prison (11 observations) or into "not entitled to unemployment benefit" (7 observations) will be treated as if right-censored at the time of exit. The rationale for this choice is that the number of people exiting into prison is too small to allow one to consider this state separately. Furthermore, it seemed preferable not to consider "prison" together with the "other states" given the already considerable heterogeneity of the "other states" exit. Similarly, the few cases that reported to be not "entitled to unemployment benefit" were treated as if right-censored since it was not possible to know whether they were only temporarily suspended from benefit receipt or they had actually exited the state of registered unemployment.

2.4.4 The expected earnings from work

The expected earnings from work contribute to determine the level of the unemployed's reservation wage and search intensity. However, individual expected earnings are not normally observed. One would actually like to observe the mean of the distribution of expected earnings. Earnings in the last job are often used as a measure of expected earnings from work, in the analysis of unemployment duration. One rationale for this choice is its computational convenience, given that earnings in the last job are most of the time observed. It is also plausible that the unemployed form expectations of their future earnings looking at their past earnings.

Past earnings may however be *"endogenous"* to the model since *"the individuals who are more selective about accepting jobs may well have had higher than average earnings"* (Narendranathan and Stewart, 1993a, p. 4). Past earnings might also proxy unobserved individual characteristics which might affect the offer probability. Narendranathan et al. (1985) argue that expected earnings should be measured by the average earnings of the individuals with similar qualifications rather than by the level of past earnings. The authors suggest that one could for instance use a "smoothed" earnings variable constructed using the fitted earnings from an earnings regression where past earnings are the dependent variable.

In addition to these reasons, there is an important practical reason for not using past earnings. In the LSUS survey past earnings are available only for about 52% of the subsample considered and for about 53% of the participants in the first interview. Using the procedure described below I have imputed

expected earnings if past earnings were not reported.

However, the drawbacks of using imputed earnings instead of actual earnings might be severe. It is well known that instrumenting variables might introduce unobserved heterogeneity in non-linear models. There is however no reasons to believe that imputed earnings might be less close to the true expected earnings distribution than earnings in the last job. The sensitivity of the estimates of the preferred model of Chapter 4 to the use of either earnings variables will be tested for (in Chapter 4).

I have computed a "smoothed" earnings variable for the LSUS sample as follows.

1. I have estimated an earnings regression for the unemployed that reported net earnings in the last job (one month before unemployment, see Table 2.1) and whose last job was full-time, excluding the self-employed in professional occupations. The units of analysis were 52% (1407 out of 2717) of the male respondents to the first survey interview.

2. The dependent variable is usual net earnings in the last job (in £) and it is entered in logarithms. The explanatory variables considered are industry, occupational group, the type of position occupied (i. e. manager or foreman or employee), marital status and a quadratic in age.

3. The estimated earnings from this regression are used as a measure of expected earnings. The expected earnings of the unemployed that had reported past earnings are set equal to the fitted values from this regression.

4. Using the estimated coefficients on the explanatory variables from this regression, expected earnings are imputed for the unemployed that provided information on the values taken by (at least some of) the explanatory variables but did not report the level of their last earnings. When information on some of the explanatory variables was not available, the person was treated as if being in the base group for these variables.

Results of estimation are given below. Descriptive statistics of the explanatory variables are given in the Appendix. Unfortunately, no information is available on the educational levels of the sample participants nor on their race.

Table 2.7: *Results of estimation of the earnings equation.*

Variable	Variable description	Coefficient	SE
Constant		8.51	.13
age/100		3.55	.71
$(age/100)^2$		-4.05	.90
married		.05	.03
manager		.16	.03
foreman		.14	.03
Ind2	Minerals & Chemicals	-.10	.04
Ind3	Engineering	-.13	.03
Ind4	Other Manufacturing	-.12	.03
Ind6	Hotels & Catering	-.33	.03
Ind8	Finance	-.13	.05
Ind9	Other Services	-.19	.04
Noind	Information not available	-.25	.09
G1	Employers/managers large firm	.31	.06
G2	Employers/managers small firm	.25	.07
G3	Professional/self-employed	.62	.18
G4	Professional/employees	.15	.06
G6	Junior non-manual	-.25	.06
G9	Skilled manual workers	-.25	.04
G10	Semi-skilled manual	-.26	.08
G11	Unskilled manual	-.10	.04
G13	Farmers employers/managers	-.14	.05
G14	Farmers own account	-.22	.03
G15	Agricultural workers	-.18	.03
G16	Members of armed forces	-.12	.06

Multiple R=.515 rsquare=.265 ; adjusted rsquare=.251; Number of observations 1407. The dependent variable is log. weekly earnings (measured in pence). The average reported earnings were £98.50 (SD £51.90) per week. Descriptive statistics of the explanatory variables are given in Table 8.1 in the Appendix. The base for the dummies "manager" and "foreman" is "employee". The base for the occupational groups is "group 12"; i. e. own account workers ingaged in any occupation other than one requiring a University degree. The occupational groups are defined in the Appendix. The base for the industry dummies is the construction industry.

The results relate to the restricted model. I shall not stop to comment in detail these results since the purpose of this exercise is to construct a smoothed earnings variable and not to explain individual earnings. A discussion of the earnings of the unemployed that participated in the survey is provided by the survey planners (Heady and Smith (1989), Vol. 1 pp. 16–18, Vol. 2 pp. 18–20).

The results of estimation indicate that earnings are maximum at about age forty-four. According to the estimates managers and foreman turn out to have average earnings above employees, as reasonable. Employers or managers in large firms have slightly higher earnings (on average) that employers or managers in small firms. It emerges that average earnings of workers in the construction industry (the base for the industry dummies) are higher than average earnings in other industries. The average reported earnings were £98.50 (SD £51.90) per week. The average estimated earnings are £83 (SD £20.65) per week. Restricting attention to the subsample selected for the later econometric analysis, 1082 unemployed out of 2035 reported usual earnings in the last job while expected earnings were estimated for 2012 of them. The mean reported earnings was £97.29 (SD £51.90) and the mean expected earnings was £92.02 (SD £20.49).

2.5 An analysis of unemployment benefit receipts

I present below some descriptive analysis of unemployment benefit receipts. The type of unemployment benefit received is discussed first. Next, I look at the amounts reported. Then, I explain how I imputed part of the changes in the pattern of benefit receipts over the course of the unemployment spell. A discussion of other types of benefits that might be received by the unemployed, such as, for instance, housing benefit, concludes this Section.

2.5.1 The type of unemployment benefit

Four categories of the unemployed can be distinguished with respect to unemployment benefit receipt:

- recipients of only Unemployment Benefit (UB);
- recipients of only Supplementary Benefit (SB);
- recipients of both UB and SB;
- recipients of no unemployment benefit at all. (The data contain information only on registered unemployed that are not entitled to unemployment benefit).

The reader is referred to Section 1.3 of Chapter 1 for a description of the workings of the unemployment benefit scheme in the UK. UB is paid to the unemployed that show a satisfactory contribution record while SB is awarded on the basis of means-testing. Both UB and SB are flat rate with additions for dependent spouse and children. UB lasts for maximum a year while SB is unlimited in time. SB can be received in addition to UB if the unemployed's resources including UB fall below their needs, at any time.

Table 2.8: *The type of unemployment benefit*

Benefit Receipt at 1st interview	*Number of recipients*	*Percent*
Unemployment Benefit	*736*	*36.2*
Joint UB and SB	*717*	*35.2*
Supplementary Benefit	*488*	*24.0*
No UB nor SB	*94*	*4.6*
Total	*2035*	*100.0*
The table relates to the subsample of male unemployed.		

Frequencies of the type of benefit reported at the first interview — about three months after the commencement of the unemployment spell— are given in Table 2.8.

Overall, 71.4% of the unemployed report to receive UB, either by itself or together with SB. There are also some 4.6% of the unemployed that report no unemployment benefit receipts. This might appear unreasonable, at least at first sight, given that the sample is extracted from the registers of unemployment benefit offices (UBO). However, these unemployed persons might be temporarily suspended from benefit receipt or they might have already exhausted their entitlement to UB[24].

To gain more insights, I compare the LSUS unemployed with the stock of male unemployed. Of the population of male unemployed at November 1983[25], respectively about 29% were UB recipients, either by itself or together with SB, and about 10% were non-recipients of benefit (DSS, Social Security Statistics 1989). The percentage of UB recipients in the LSUS inflow sample (71%) is much higher than in the stock of the unemployed. The main reason for this difference is the limited duration of entitlement to UB. It is likely that the unemployed in the stock experience on average longer unemployment durations than the unemployed in the LSUS flows sample. Shorter spells of unemployment are probably under-represented in the stock

[24]Entitlement to UB lasts 52 weeks. Those unemployed that leave unemployment before fully exhausting their UB entitlement, will be able to use up their residual entitlement in future spells of unemployment. Instead, those unemployed that fully exhaust their UB entitlement must work for at least 13 weeks in order to gain new entitlement to UB.

[25]November 1983 is chosen as a reference period because it is close to an average date of the first LSUS interview, which took place in the Autumn of 1983.

of the unemployed. Another reason might be that the LSUS unemployed are all household heads. Household heads are probably more likely to have hold a full-time job, on average. Those sharing accomodation with others are excluded from the LSUS survey. They are instead included in the stock of the unemployed.

The type of benefit received may vary over time. For instance, the unemployed may exhaust their entitlement to UB or they may be disqualified from benefit receipt or they may become poorer. In Table 2.9, I compare the type of benefit reported at the two survey interviews. The Table covers only those unemployed with right-censored spells[26]. One should keep this in mind when interpreting this table since it is one of the claims of this thesis that the type of unemployment benefit affects the probability of leaving unemployment.

Table 2.9: *The type of benefit reported at the two interviews*

Benefit receipt at 1st interview	*Benefit receipt at 2nd interview*				*Total*
	UB	*joint UB/SB*	*SB*	*no UB nor SB*	*Unemployed*
Unemployment Benefit (UB)	*13*	*4*	*149*	*108*	*274*
percentage	*4.7%*	*1.5%*	*54.4%*	*39.4%*	*100%*
Joint UB and SB	*1*	*21*	*264*	*8*	*294*
percentage	*0.3%*	*7.1%*	*89.8%*	*2.7%*	*100%*
Supplementary Benefit (SB)	*3*	*11*	*212*	*6*	*232*
percentage	*1.3%*	*5.2%*	*91.4%*	*2.6%*	*100%*
No UB nor SB	*2*	*0*	*9*	*14*	*25*
percentage	*8%*	*0*	*36%*	*56%*	*100%*
Total	*19*	*36*	*630*	*140*	*825*
percentage	*2.3%*	*4.3%*	*76.4%*	*17%*	*100%*

The table relates to the subsample of male unemployed that were right-censored at the time of the second interview, so that the benefits received at the second interview were recorded.

It emerges that the type of benefit varies considerably over the course of the unemployment spell. About 76% of the (right-censored) unemployed reported to receive only SB payments at the second interview. The corresponding figure at the the first interview was about 28% (232 out of 825, in the last column). About 17% reported no UB nor SB at the second interview. The corresponding figure at the first interview was about 3% (25 out of 825, in the last column). A few of the unemployed reported payments of UB by itself (about 2%) or UB together with SB (about 4%) at the second

[26]The unemployed that exited from unemployment before the second interview but were again unemployed by the time of the second interview are excluded from this table. Their inclusion would be misleading since for instance they might have gained new entitlement to UB.

interview. However, entitlement to UB lasts for 52 weeks while the second survey interview took place about 65 weeks after the commencement of the unemployment spell. One possible explanation for this apparent inconsistency are temporary suspensions from UB or perhaps misreporting of the type of benefit.

Of those (right-censored) unemployed receiving only UB at the first interview, almost 55% reported to receive only SB at the second interview. Almost 40% of them reported no benefit receipts at the second interview. The corresponding figure for the full sample —this table covers only right-censored spells— is likely to be higher since the receipt of unemployment benefit may actually influence the duration of the unemployment spell.

Of those (right-censored) unemployed that reported UB together with SB at the first interview, almost 90% reported only SB payments at the second interview. This supports the view that upon exhaustion of entitlement to UB the foregone UB payment is replaced by some SB payment, for those unemployed receiving some SB on top of UB at the commencement of their unemployment spell. Almost 3% of them reported no unemployment benefit at the second interview. However, about 7% of them reported joint payments of UB and SB at the second interview.

About 91% of those (right-censored) unemployed receiving only SB at the first interview, reported only SB at the second interview. This finding suggests that the unemployed receiving only SB at the commencement of their unemployment spell are likely to continue to receive SB throughout their unemployment spell.

To conclude, let us remark that most (right-censored) unemployed reporting no benefit payments at the second interview (108 out of 140, in the 5th column), reported receipts of UB by itself at the first interview. Instead, 96% of the (right-censored) unemployed receiving SB, either by itself or together with UB, at the first interview (264 plus 212 out of 294 plus 232 persons) reported only SB at the second interview. The rationale for this finding is that the unemployed that pass the means-test at the commencement of their unemployment spell continue to pass it thereafter, unless there is a significant change in their circumstances. Instead, the type of benefit varies considerably over time for the unemployed receiving UB by itself at the commencement of their unemployment spell. The evidence on the changes in benefit receipts is important in order to justify the assumptions I make in Chapter 6 concerning the relation between the potential duration of entitlement to unemployment benefit and the duration of unemployment.

However, one has to acknowledge that the type of benefit may be misreported by the unemployed. For the DSS survey of 1978/79, it was found by Moylan et al. (1984, pp. 19–20) —who had access to both administrative and self-reported information— that as many as two-fifths of the unemployed had misreported the type of benefit. Indeed, it is possible that some unemployed may be unaware of the type of benefit they receive. It is typically found that the unemployed tend to misreport the type of benefit received more than the

actual amounts. I test for misreporting of the type of benefit in one of the next sections.

2.5.2 The potential duration of UB

I have computed the potential duration of entitlement to UB for the recipients of UB by itself at the commencement of their unemployment spell. The maximum duration of UB is of 52 weeks. However, UB entitlement might sometimes last for less than 52 weeks because of the so called "linked spell" rule, which says that unemployment spells separated by less than eight weeks of employment will be counted together as a single spell for the purposes of benefit entitlement. I have made an attempt to allow for linked unemployment spells by using the information on the economic activity week-by-week in the year before the commencement of the observed spell of registered unemployment. Given that this information was collected retrospectively at the time of the first interview (see Table 2.1) the estimated potential duration of entitlement to UB may not be precise.

The estimated potential durations of entitlement to UB are reported in Table 2.10 for the recipients of UB by itself. Upon UB exhaustion the type of benefit received will definitely change for recipients of UB by itself, at least if they have not yet exited from unemployment. This is why I am interested in computing the maximum potential duration of benefit for this group of people.

Table 2.10: *The potential duration of entitlement to UB*

maximum entitlement		*maximum entitlement*		*maximum entitlement*	
weeks	*freq.*	*weeks*	*freq.*	*weeks*	*freq.*
1	1	21	3	39	2
2	2	22	1	40	1
7	1	23	4	41	1
8	1	25	1	42	3
9	1	27	1	43	3
10	1	29	2	44	3
11	2	30	2	45	1
12	2	31	1	46	1
14	3	33	3	47	2
15	3	33	3	48	1
17	1	34	3	50	3
18	1	37	1	52	672
20	1	38	1	TOTAL	736

The table relates to the recipients of UB by itself within the sub-sample of male unemployed.

About 91% of these unemployed enjoy full maximum duration of UB of 52 weeks. The remaining 8.7% of them shows a potential duration of UB entitlement that differs considerably, ranging from only one week to 50

weeks. This information will be used to impute benefits over the course of the unemployment spell.

2.5.3 Benefit amount and changes

Summary descriptive statistics of the amount of unemployment benefit reported at the first interview are presented in Table 2.11. These amounts correspond to the November 1982 rates —unemployment benefits are regularly updated in November of each year; the amounts asked for at the first survey interview, which took place in the Autumn of 1983, were therefore the 1982 rates.

The 1982 UB flat rate was £25 per week with 30 pence addition for each dependent child and £15.45 addition for dependent spouse. The dependent spouse addition is payable if the spouse's earnings are less than the level of the addition. Two reduced rates were also payable: the 2/3 rate was £20.23, £30.34 with reduced rate spouse addition; the 1/2 rate was £12.50, £18.75 with spouse addition. Reduced rates were mainly paid to people whose contribution record would not give entitlement to the full rate or that were partially disqualified from benefit receipt. In practise few unemployed received reduced rates as shown in Atkinson and Micklewright (1989). (Reduced rate unemployment benefit payments have since been abolished.)

The SB flat rates were £26.80 for single persons and £43.50 for married persons with dependent spouse. The SB additions for children varied from £9.15 to £13.70 depending on the age of the children. The amounts of SB payable are computed on the basis of the difference between the nuclear family resources and the flat rate amounts which should measure family needs. Therefore, there is no particular reason to find exactly the flat rate amounts recorded in the data, in contrast to the UB amounts which should in principle be observed.

Table 2.11: *Descriptive statistics of benefit amounts*

Benefit received	*Mean*	*Std Dev*	*Minimum*	*Maximum*	*Recipients Number*
UB	*31.2*	*10.2*	*6.8*	*130.2*	*274*
UB and SB	*51.8*	*15.4*	*9.2*	*150.0*	*294*
SB	*43.8*	*16.8*	*0.95*	*124.0*	*232*

The amounts are in pounds. The table relates to the male subsample and to the benefits received at the 1st interview.

From Table 2.11, it emerges that the mean benefit amount is the lowest for UB recipients while it is the highest for recipients of both UB and SB. This finding is probably due to the the means-tested nature of SB, which

implies that those unemployed receiving SB on top of UB are quite poor and therefore receive a substantial amount of unemployment benefit.

The minimum and maximum amounts of UB received by itself might perhaps represent evidence of misreporting since the flat rate UB may vary from a minimum of £12.5 (1/2 reduced rate) to a maximum of about £42 (the standard rate plus dependent spouse and children additions). Another possibility is that the local Unemployment Benefit Office made an error in computing the amount of benefit to be paid to the unemployed. This seems however unlikely given the flat rate nature of UB.

Unemployment benefit receipts were self-reported by the unemployed. As already stressed, they might therefore be affected by recall error or misreporting. Moylan et al. (1984) who had access to both self-reported and administrative information for the DSS Cohort Study of the unemployed of 1978/79 found that three-quarters of the reported amounts fell within 5 pound sterling of the recorded amounts with the UBOs. Their analysis relates to the unemployed whose spells lasted for longer than 13 weeks. They concluded (Moylan et al. 1984, pp. 19–20) that *"despite the fact that the recorded measures referred to an average while the interview data referred to a single week, there was a fairly high degree of agreement between the records and the reports as to the amount of benefit received"*. These findings are in agreement with those of Katz and Meyer (1988) who also had access to both individual self-reports and administrative records on unemployment benefits for the US CWBH. They found that 67.5% of the unemployed reported exactly the same benefit amount as that registered by administrative sources and that 85% reported amounts within $ 10 of the true (administrative) amount.

I have looked at the accuracy of the unemployment benefit reported by the recipients of UB by itself at the first interview (736 of the subsample for analysis), by comparing the UB amounts reported with the 1982 flat rates. The results are summarized below. I have not carried out similar analysis for the recipients of SB, either by itself or together with UB. In principle one could compute SB amounts and compare them with the reported amounts. However, given the complexity of the means-test calculations, the scope for errors would be rather too large to allow one to test the accuracy of the reported amounts. The following points emerge with respect to the unemployed that reported to be in receipt of UB by itself at the first interview[27]:

- 41.4% reported precisely the flat rate amount (£25), 6.7% the flat rate plus one child addition (£25.30), 3.7% the flat rate plus two children additions (£25.60), 0.8% plus three children (£25.90);

- 9.6% reported the flat rate plus addition for dependant spouse (£40.45), 3.5% the flat rate plus dependent spouse and one child addition (£40.75),

[27]The amounts of UB by itself reported at the first interview are shown in the Table 8.2 in the Appendix.

2.3% plus dependent spouse and two children additions (£41) and 1.1% with three children addition (£41.35),

- 0.4% report precisely the 2/3 reduced rate (£18.75) and 0.7% the 1/2 reduced rate (£12), another 1% the reduced 1/2 rate with additions for spouse and children.

It appears that overall 71.2% of the subsample considered reported UB amounts which are exactly equal to the official rates. Moreover there are:

- 8.4% that reported amounts between £25 and £26, and that were not considered above;

- 8.7% that reported amounts which fell between £40 and £42 and that were not counted above.

If these 17.1% are treated as true UB amounts, lying within one pound from the precise amount, it is possible to conclude that 88.3% of the unemployed that reported UB by itself at the first interview provided correct information on the payment received.

The amounts of benefit payable may vary over the unemployment spell for different reasons, such as, for instance, the exhaustion of entitlement to UB or the birth of a new child or the spouse's earnings rising above the benefit addition. Some descriptive statistics of the change in the amounts of benefit reported at the two interviews are given in Table 2.12, for the right-censored individuals. I have distinguished two groups of unemployed: recipients of UB by itself and recipients of SB, either by itself or together with UB. Some "only UB" unemployed are excluded from Table 2.12 since the benefit amounts they report at the first interview might be evidence of misreporting of the type of benefit received, as discussed above.

As already mentioned, unemployment benefits are regularly increased in November of each year to allow for the rising cost of living. The benefit amounts reported at the first interview relate to the November 1982 rate. The amounts reported at the second interview relate normally to the November 1983 rate. However, in a few cases the amounts reported are probably the 1984 rates, since the second interview took place in November 1984, for a few persons. To give an idea of the order of magnitude of the benefit increases, the average of the increases in the 1983 rates was about 5%.

Table 2.12: *The percentage change in benefit receipts*

Benefit loss or gain at the 2nd interview in percentage	*Percentage that loses or gains Type of benefit received at 1st interview*	
	SB by itself or with UB	*only UB*
loses 100%	*2.7*	*43.7*
loses 50% or more (but less than 100%)	*4.0*	*4.8*
loses 10% or more (but less than 50%)	*11.9*	*7.0*
loses more than zero (but less than 10%)	*13*	*3.5*
loses nothing, gain nothing	*0.0*	*0.0*
gains more than zero (but less than 10%)	*40.6*	*8.7*
gains 10% or more (but less than 50%)	*21.3*	*18.8*
gains more than 50% (but less than 100%)	*4.2*	*8.7*
gains 100% or more	*0.3*	*4.8*
total percentage	*100.0*	*100.0*
mean benefit amount at t1 (£)	*49.7 (SD 17.6)*	*31.7 (SD 7.8)*
mean benefit amount at t2 (£)	*49.6 (SD 19.7)*	*21.9 (SD 23.1)*
total number of unemployed	*526*	*229*

The table relates to the right-censored unemployed. They report benefit amounts also at the 2nd interview. The unemployed that reported at the first interview amounts of UB by itself less than £12.50 or greater than £43 are excluded from this table (these are about 7% of the "only UB" unemployed at the first interview). The November benefit update was not allowed for.

The proportion of the unemployed that lose completely their benefit entitlement is much higher among recipients of only UB at the commencement of their unemployment spell. This feature of the data had already emerged from Table 2.9. In Table 2.12 the percentage of recipients of only UB (at the time of the first interview) that lose their benefit entitlement is slightly higher than in Table 2.9. This happens because some "only UB" unemployed are excluded from Table 2.12, as discussed above.

On the other side, the "only UB" persons are found on average to both lose more and gain more than the unemployed receiving SB (with or without UB) near the commencement of their unemployment spell. It is quite surprising that the proportion of the "only UB" persons that gain more than 10% (32.3 %) is higher than that of the "SB" recipients (25.2%). It is instead not surprising that the percentage of "only UB" unemployed that lose more than 50% of their benefit entitlement by the time of the second interview is higher than the corresponding percentage for 'SB" unemployed.

Most of the gains in benefit receipts of less than 10% are probably due to the regular November update of benefit payments. About 42% of the recipients of SB (with or without UB) at the first interview that are still unemployed by the time of the second interview report benefit amounts that are no more than 10% higher than before. The corresponding figure for the "only UB" unemployed is about 9%.

However, the evidence presented in Tables 2.12 and 2.9 relates to the right-censored unemployed. Since the behaviour of the unemployed with respect to exit from unemployment is likely to be influenced by their benefit receipts, one has to be careful in interpreting the above evidence. One would expect that the "only UB" unemployed that lose more in terms of benefit receipts will have a stronger incentive to leave unemployment possibly before the time of the second interview (or right-censoring time), which corresponds to about one and half year after the commencement of their unemployment spell. In particular, about 77% of the unemployed receiving only UB near the commencement of their unemployment spell are observed to exit from unemployment before the time of the second interview. The corresponding figure for the unemployed receiving SB (with or without UB) at the time of the first interview is about 56%.

2.5.4 Imputing some changes in benefit receipts

The type and amount of unemployment benefit may vary over the course of the unemployment spell, as illustrated in the preceding sections. Therefore, i is necessary to allow for this variation in the econometric analysis. However, as we have seen, the LSUS data contain very limited information on any change in the pattern of benefit receipts. Some of these changes are known only for the unemployed with right-censored spells. Moreover, the time at which any change took place is not normally known.

I have constructed a time-varying variable for the receipt of unemploy-

ment benefit as follows. The main criterion adopted is to avoid imputing any change except for the recipients of only UB at the commencement of their unemployment spell. This choice is supported by the evidence gathered in the preceding sections. The type and the amount of the unemployment benefit is likely to vary at the time of UB exhaustion for those unemployed that received only UB at the commencement of their unemployment spell, unless their spell lasted less than the maximum duration of UB. Very little is instead known about the timing or the reasons for any change in the pattern of benefit receipts of the other unemployed. Actual changes are observed only for the unemployed with right-censored spells. If the level of benefit were allowed to vary only for the unemployed with right-censored spells, the level of benefit would vary with the dependent variable. This would clearly result in enormous bias.

1. For recipients of SB (either by itself or with UB) at the first interview, I have imputed no change in the type nor the amount of benefit received, independently of whether the benefits received at the second interview were observed —right-censored spells. The idea is that even when the change in the amount or the type of benefit received is observed, the time of and the reasons for this change were not observed. It seems therefore more reasonable to assume no change for any of the SB (or SB and UB) recipients, given that no sensible imputation is possible.

2. For the right-censored unemployed that reported to receive UB (by itself) at the first interview, the type and amount of benefit received at the second interview are known. I have therefore imputed the first interview amount up to the time of UB exhaustion and the second interview amount (reduced by 5% to remove the effect of uprating) thereafter.

3. For those unemployed receiving only UB at the first interview who completed their unemployment spell by the second interview, I have first of all compared their unemployment duration with their potential UB duration. If their unemployment spell were shorter than their potential UB duration, I have assumed the same amount of benefit receipt throughout the spell. If, instead, their spell were longer than the potential UB duration, I have looked at their savings. If the total savings reported at the first interview (including the spouse savings) were larger than £3500, I have assumed zero amount of benefit receipt at the second interview[28]. If instead the unemployed savings fell below £3500 the amounts of UB reported at the first interview were imputed

[28]The unemployed with savings of over about £3000 were not entitled to the means-tested benefits in 1983. I have used as a reference the savings at the first interview since the savings at the second interview may vary depending on whether the unemployed were entitled to any benefit after UB exhaustion or not.

throughout the spell. It seems plausible to assume that if the UB recipients were entitled to larger amounts, they would have received SB together with UB at the first interview. It is instead possible that these persons might have received lower amounts than the UB amounts reported at the time of the first interview. However, the amounts were set equal to the first interview levels to remain in line with the assumptions made concerning the pattern of benefit receipts by the recipients of SB (with or without UB). The rationale for this choice is that I preferred to impute no change in the benefit amounts received whenever not enough precise information on the changes was available.

One more justification for the procedure adopted is that about 95% of the SB recipients (at the first interview) reported SB payments also at the second interview (if they had not yet exited from unemployment) and about 42% reported small variations in benefit receipts (if they had not yet exited from unemployment).

Of course, the assumptions made are very much open to criticism. Moreover, using imputed benefit values instead of actual values might introduce errors into the econometric model. The sensitivity of the estimated coefficient of the model to a different specification of the benefit level variable is tested in Chapter5. There, it is shown that employing this timevarying benefit level rather than the benefit level at the first interview does not actually affect the estimated coefficient on the benefit variable. This confirms perhaps that with the above assumptions no major error was introduced. It might however also imply that not enough variation in the pattern of benefit receipts was allowed for.

2.5.5 Housing benefit and other social security benefits

In addition to unemployment benefit (UB or SB), the unemployed have also access to other subsidies, such as children benefit, sickness benefit[29], Family Income Supplement (FIS) and housing benefit. These benefits are taken into account in the computation of total unemployment income, a variable that is used in the econometric analysis of Chapter 5. However, I shall refer throughout this thesis to unemployment benefit as UB and SB unless otherwise specified, since normally not only the unemployed but also the low wage earners can apply for other benefits such as FIS, sickness benefit and housing benefit.

[29]Sickness benefit is payable to people that have satisfied the same work contribution conditions as for the contributory unemployment benefit. Since April 1983 workers sick for less than eight weeks are not entitled to sickness benefit but only to statutory sick pay to be paid by their employer. Statutory sick pay is flat rate without additions for dependants.

The "housing benefit" scheme deserves particular attention. A new housing benefit scheme was introduced in April 1983. Two schemes of financial help with domestic rents and rates were available before April 1983: rent and rates subsidies were either paid (and administered) together with the Supplementary Benefit (SB) or they were paid (and administered) separately by the local authorities. After the reform, the benefit is administered only by the local authorities.

This change in the system of rent and rates subsidies took place right in between the first and the second wave of the LSUS survey. In spite of the fact that the survey planners did their best to get round this problem by designing particular questions aimed at capturing the amount of rent and rates subsidies before and after the change in rules, I am sceptical about the completeness of the information collected. The new housing benefit scheme was introduced in April 1983 and the first survey interview took place in the Autumn of 1983. However, many unemployed appear to me as unaware of the workings of the new scheme.

Only 12% (240 out of 2035) of the subsample of males considered reported to receive some rent or rates subsidies at the first interview. The amount of monthly subsidy reported varied from 79 pence to £103.8. The mean reported subsidy was £15.34 (SD £9.36). About 28% (67 out of 240) of those reporting some housing benefit at the first interview, report help with their rent and rates also one month before the commencement of their unemployment spell. For 45% of these unemployed the same amount is reported at both dates.

The number of the unemployed that reported rent and rates subsidies is quite low. It is quite possible that most unemployed were confused about the new housing benefit scheme and perhaps reported these subsidies together with the SB payments. They might not have been able to spell out the housing subsidy from the SB payment. Since in the case of SB recipients it is not always clear whether the amounts of rent and rates subsidies reported had already been reported as part of SB payments, I resolved not to add the amounts of housing benefit (or rent and rates subsidies) reported separately to the benefit payments in order to avoid possible double counting. Of course, this might also introduce errors since some SB payments might include housing benefit[30]. Sensitivity of the model estimates to this choice is tested when considering the reported amounts of housing benefit in the total unemployment income variable[31].

[30]The mean value of UB and SB receipts at the first interview is £41.59 (SD 18.99). If the reported amounts of housing benefit are summed to the UB and SB amounts, the mean benefit amount becomes £43.40 (SD 21.14).

[31]This is done in Section 5.4. I anticipate that none of the total unemployment income variable or the "other unemployment income" variable (which is equal to the total unemployment income minus UB and SB payments) turn out to have a significant impact on the hazard rate. However, also the coefficient on benefit level variable is found statistically insignificant.

It is worth mentioning that while the self-employed are not entitled to contributory benefit scheme, UB, they are instead entitled on condition of having paid enough contributions to sickness benefit. The self-employed are, though, entitled to means-tested unemployment benefit.

Children benefit is paid to anybody, not only to the poor or the needy, and it is of fixed amount. It is payable to the mother and it is tax free. The person entitled, must, however, make a claim in order to receive it. The average amount of child benefit reported at the first interview was about £12 (SD about £6).

FIS can only be claimed by families with children when household head is in full-time work. Eligibility is conditional on gross income being below a certain prescribed level and the benefit paid is equal to half the difference between gross income and the prescribed level. However, this benefit is paid for fifty-two weeks irrespective of changes in the claimant situation, which implies that sometimes an unemployed person may be in receipt of FIS. The FIS benefit has been object of much debate. It was introduced as a temporary measure to reduce poverty among working people but it is still in force. It is now called "Family Credit" since the 1988 reform.

Total benefit receipts were computed for the unemployed, his spouse and the nuclear family by adding to the reported amounts of UB and SB any other social security payment reported; i. e. child benefit, sickness benefit, housing benefit, Family Income Supplement and any other benefit. The mean value of total benefit receipts for the unemployed is £42.18 (SD 19.14), for the spouse is £8.03 (SD 9.37) and for the nuclear family altogether (husband plus spouse) is £50.21 (SD 23.67).

2.6 Summary

In this chapter, I have presented the main features of the LSUS data; I have described how some variables were constructed; and I have carried out some descriptive analyis.

It seems to me that the LSUS data are well worth using for the analysis of unemployment duration. The main drawback of these data is the limited information on any variation in the pattern of unemployment benefit receipts over time. This has made necessary some crude imputation of the changes in benefit receipts over the course of the unemployment spell. This problem may hinder the estimation of the benefit impact on the re-employment probability.

It was found that about 6% of the respondents to the second interview reported to be already in registered unemployment in the week before they were actually sampled as starting their spell of registered unemployment. These persons were dropped from the sample for analysis except for a few of them for whom this inconsistency could be amended.

Non-respondents to the second interview were not included in the final sample for analysis. Information on the duration of the unemployment spells

was available only for second interview participants. Some discussion of attrition and non-response has been provided. Also female unemployed were excluded from the final sample for the following reasons: only single women entered the definition of household heads; few women were sampled; most previous UK studies have looked at male unemployed only; the behaviour of British single women is generally believed to differ from that of married or single men.

From the analysis of the economic states occupied by the unemployed before and after the observed unemployment spell, it emerged that about 30% of them were in full-time work in the week before entering registered unemployment. Almost 46% of them found a full-time job by the time of the second survey interview. About 77% of those that exited from unemployment exited into full-time work.

The type and the amount of unemployment benefit has been shown to vary across the unemployed and throughout the observed unemployment spell. At the first interview, about 35% of the unemployed reported only UB; about 25% of them reported only SB; and about 35% reported both UB and SB. About 5% of the unemployed reported no benefit receipts.

It was possible to conclude that probably more than 70% of the unemployed reporting only UB at the first survey interview reported accurate amounts of benefit (to the pence) and almost 90% of them probably reported correct amounts (to the pound). Almost 95% of those unemployed receiving SB (either by itself or together with UB) at the first interview, reported SB also at the second interview. Instead, about 55% of those unemployed receiving (only) UB at the first interview, reported SB at the second interview. These last two figures relate to the unemployed with right-censored spell, for whom the information on benefit receipts at the second interview is available.

Chapter 3

The econometric framework

3.1 Introduction

The purpose of this chapter is to to provide the reader with some econometric background and to introduce the preferred econometric model. I adopt a reduced form approach to modelling the individual probability of leaving unemployment, as anticipated in Section 1.4. The probability of leaving unemployment is modelled as a conditional probability by means of the hazard rate and job search theory is used to guide the choice of explanatory variables and the interpretation of the results of estimation.

In Section 3.2, the statistical underpinnings of the model are laid out. Some of the basic statistical concepts were already introduced in Chapter 1. The preferred specification for the baseline hazard rate, a piecewise linear functional form, is presented together with alternative specifications that have been often used in the literature in Section 3.3. The advantages of such a flexible specification of the dependency on time of hazard rate are discussed. Modelling the impact of the explanatory variables on the hazard rate is the subject of Section 3.4. The competing risks model is presented in Section 3.5. A brief description of the non-parametric Kaplan-Meier method of estimation is provided in Section 3.7. Finally, in Section 3.8 the likelihood function for the LSUS data is specified for the single and the competing risks models. A brief summary concludes the Chapter.

Good references for a more detailed treatment of the material presented in this chapter are for instance Kalbfleish and Prentice (1980), Cox and Oakes (1984) and Lancaster (1990).

3.2 The hazard rate and the survivor function

The probability of leaving unemployment is expressed with the hazard rate. The hazard rate defines a conditional probability as follows:

$$\theta(t) = \lim_{dt \to 0} \frac{P(t \leq T < t + dt | T \geq t)}{dt}. \tag{3.1}$$

This expression describes the probability of leaving unemployment in any instant of time, t, conditional on being still unemployed an infinitesimal amount of time to the left of t. By the law of conditional probability:

$$P(t \leq T < t + dt | T \geq t) = \frac{P(t \leq T < t + dt, T \geq t)}{P(T \geq t)} \tag{3.2}$$

$$= \frac{P(t \leq T < t + dt)}{P(T \geq t)}.$$

Defining time since the commencement of the unemployment spell as a continuous random variable T —where T does not need to be calendar time— with cumulative distribution function $F(t) = P(T < t)$ and density function $f(t) = dF/dt$, Equation 3.1 can be rewritten as follows:

$$\theta(t) = lim_{dt \to 0} \frac{F(t + dt) - F(t)}{dt} \frac{1}{1 - F(t)} = \frac{f(t)}{1 - F(t)}, \tag{3.3}$$

where $1 - F(t)$ is a recurring function in statistical duration analysis, the so called *"survivor function"*. From now onwards, I shall define the survivor function as $G(t) = 1 - F(t)$. Equation 3.3 defines the hazard rate in terms of the probability density function, $f(t)$, and of the survivor function, $G(t)$. The hazard rate is also called *"transition rate"*, to emphasize the fact that it can be employed to describe the transition from a given economic state to a new economic state.

It is easy to notice that $\theta(t) = -dlogG(t)/dt$, from which assuming $G(0) = 1$, it is possible to express the survivor function in terms of the hazard rate, as below:

$$G(t) = exp\{-\int_0^t \theta(u)du\}, \tag{3.4}$$

and, consequently, the density function in terms of the hazard rate:

$$f(t) = \theta(t)exp\{-\int_0^t \theta(u)du\}. \tag{3.5}$$

The integral above is the so-called *"integrated hazard"*. These relations will be used to specify the likelihood function for the data.

Since the hazard rate expresses a probability it is constrained to take on positive values. Assuming that exit out of unemployment will definitely take place at some point in time, than $\lim_{t \to \infty} G(t) = 0$, and it follows that:

$$\lim_{t \to \infty} \int_0^t \theta(u)du = \infty, \tag{3.6}$$

which means that the integral must diverge, otherwise $\lim_{t\to\infty} G(t) > 0$ and the distribution will be "defective".

Two related concepts are the *"expected total duration"* of the unemployment spell at a certain time s from commencement of the spell, conditional upon being still unemployed at that time, $e(s)$, and the *"expected remaining duration"*, $r(s)$, which gives the expected duration at time s; i. e. $r(s) = e(s) - s$. It can be shown (Lancaster, 1990, pp. 13) that the expected remaining duration at the commencement of the unemployment spell is equal to the integral of the survivor function, as below:

$$e(0) = r(0) = E(T) = \int_0^\infty G(t)dt. \tag{3.7}$$

This concept can be used in the empirical analysis to compare the actual mean expected duration with the estimated mean expected duration under different assumptions.

3.3 The specification of the baseline hazard rate

The hazard rate is normally specified as a function of time. The dependency on time of the (conditional) probability of leaving unemployment is captured by the baseline hazard rate, , $\psi_1(t)$. The reader is referred to Section 1.5 for a more intuitive discussion.

The simplest functional form for the baseline hazard rate is the *exponential*:

$$G(t) = e^{-at}; \qquad f(t) = ae^{-at}; \qquad \theta(t) = a, \tag{3.8}$$

where $a > 0$ is the parameter that defines the exponential function. The hazard rate, as a pure a function of time, is constant over time and equal to a. This is a particularly unattractive feature for the purpose of modelling unemployment duration. It is not possible to exclude on economic grounds that the sign of the dependency on time of the probability of leaving unemployment may vary over time.

The most commonly used functional form for the baseline hazard rate —at least until quite recently— is the *Weibull*:

$$G(t) = exp[(-at)^c]; \qquad \theta(t) = ac(t)^{c-1}; \tag{3.9}$$

$$f(t) = ac(t)^{c-1}exp[(-at)^c]; \tag{3.10}$$

where $c > 0$ is the Weibull parameter, which describes the sign of the dependency on time of the hazard rate. When $c = 1$, the Weibull reduces to an exponential distribution and time dependency is constant; if $c > 1$, the probability of leaving unemployment augments with the length of the

unemployment spell —positive time dependency—; if, instead, $c < 1$, the probability of leaving unemployment decreases with time —negative time dependency. The Weibull model has been adopted extensively in the literature because not only it has a very simple formulation (only one parameter), but it also allows the hazard rate to vary as a function of time.

However, the Weibull model only allows for monotonic time dependency, while the sign of time dependency may in fact vary during the course of the unemployment spell. Using a Weibull specification for the baseline hazard constrains the data to indicate either positive or negative or no time dependency. This restriction has no economic rationale. It is for instance plausible to assume that the hazard rate may first rise and then decline, as a function of time. The literature has approached this issue only recently.

Flexible formulations of the baseline hazard rate can be generalized to the *piecewise-constant hazard*:

$$\theta(t) = \begin{cases} \theta_1, & 0 \leq t \leq m_1 \\ \theta_2, & m_1 < t \leq m_2 \\ \cdots & \\ \cdots & \\ \theta_Q, & m_q < t < \infty \end{cases} \tag{3.11}$$

where the hazard rate remains constant in each of the $q-1$ time intervals defined in 3.11 but varies across intervals. Flexibility is only ensured if enough "pieces" or time intervals are specified. A functional form must be assumed for θ. One possibility is to specify an exponential functional form. The resulting baseline hazard is a *piecewise linear exponential*:

$$\theta(t) = \begin{cases} exp(a_1), & 0 \leq t \leq m_1 \\ exp(a_2), & m_1 < t \leq m_2 \\ \cdots & \\ \cdots & \\ exp(a_q), & m_q < t < \infty \end{cases} \tag{3.12}$$

which can be rewritten as:

$$\theta(t) = exp(a_l), \qquad ,t \subset I_l \tag{3.13}$$

$$I_l = \{t | \tau_l \leq t < \tau_{l+1}\}, \qquad l = 1, 2, \cdots, m.$$

The advantage of this formulation is that it does not impose severe restrictions on the behaviour of the hazard rate over time, especially if several time intervals are specified. Moreover, the sign of the dependency on time of the hazard rate is allowed to vary over time (if enough time intervals are specified). This is a particularly desirable property since economic theory is inconclusive as far as the pattern over time of the probability of leaving unemployment goes. Another advantage of the piecewise linear exponential is

that it is easy to estimate given its simple functional form. This is the specification that I shall adopt in the later Chapters. I also estimate a Weibull baseline in Chapter 4, for comparison purposes.

Another flexible specification of the hazard rate which is a piecewise-constant but relies on a non-parametric[1] approach, is due to Meyer (1986, 1990), and it is an extension of Prentice and Gloecker (1978):

$$P(T \geq t+1|T \geq t) = exp[-\int_t^{t+1} \theta(u)du], \tag{3.14}$$

where $P(.)$ is the probability that the spell lasts until $t+1$ given that it has lasted until t. This corresponds to the ratio $G(t+1)/G(t)$, where $G(t)$ is the discrete time survivor function. This specification was adopted for instance by Narendranathan and Stewart (1993, 1993a). In practise, this approach does not differ from the one that I adopt, i. e. a piecewise linear with a weekly baseline.

3.4 Allowing for the impact of observed explanatory variables

The next step is to introduce dependency of the hazard rate on a vector of (observed) explanatory variables, defined as x, which take different values for each unemployed i. The hazard rate can now be written as:

$$\theta(x_i, t). \tag{3.15}$$

It is convenient to assume what has been called a *proportional hazard* specification, which distinguishes the influence of the elapsing of time from the influence of other observable on the hazard rate as follows:

$$\theta(t_i, x_i) = \psi_1(t_i)\psi_2(x_i), \tag{3.16}$$

where ψ_1 and ψ_2 do not vary across individuals, but the explanatory variables and the duration of the spell do. In the case of the proportional hazard, the explanatory variables have a proportional multiplicative impact on the hazard rate over time. For instance, the hazard rates for two individuals with regressors vectors, x_1, x_2, are in the same ratio $\psi_1(x_1)/\psi_2(x_2)$ for all t. The proportional hazard specification has been widely adopted in the literature.

The impact of the covariates on the hazard rate is conventionally modelled exponentially:

$$\theta(t_i, x_i) = \psi_1(t_i)exp(\beta x_i), \tag{3.17}$$

[1]Where non-parametric methods are generally characterized by not imposing a specific functional form on the data. So that the observed data, rather than a given functional form, shape the estimates of the hazard rate. For instance, the hazard rate is also estimated non-parametrically in the Cox partial likelihood (Cox 1972).

where β is the vector of coefficients associated with the vector of explanatory variables (x).

Adopting a piecewise exponential functional form for the baseline hazard rate, the overall hazard rate can be written as follows:

$$\theta(t) = exp(a_l)exp(\beta x_i), \qquad t \subset I_l, \tag{3.18}$$

$$I_l = \{t|\tau_l \leq t < \tau_{l+1}\}, \qquad l = 1, 2, \cdots, m.$$

One further step is to allow covariates to vary over time:

$$\theta(t) = exp(a_l)exp(\beta x_i(t)), \qquad , t \subset I_l, \tag{3.19}$$

$$I_l = \{t|\tau_l \leq t < \tau_{l+1}\}, \qquad l = 1, 2, \cdots, m.$$

Examples of explanatory variables that may vary over time are the local unemployment rate and the unemployment benefit level. As Lancaster (1990) points out, when the explanatory variables are allowed to vary over time the proportionality property may not hold any longer. Equation 3.19 constitutes the basis for the model that is laid out in the next sections.

3.5 A competing risks model of the probability of leaving unemployment

The attractiveness of using a competing risks model of the probability of leaving unemployment has been stressed in Chapter 1. Competing risks specifications allow one to model the duration of unemployment together with the economic states entered upon leaving unemployment. The opposite concept is that of single risk models which do not distinguish the economic states to which the unemployed exit.

Assuming that the exit states are mutually exclusive, the conditional probability of leaving unemployment at a certain point in time, t, and of exiting to a specific destination state k, given the set D of possible exit states, can be written as:

$$\theta_k(t) = \lim_{dt \to 0} \frac{P(t \leq T < t + dt, D = k, |T \geq t)}{dt}, \tag{3.20}$$

which is the so-called *cause-specific* hazard.

Given the assumption that the destinations k —also called failure types or simply exits/exit states– are mutually exclusive, the overall hazard rate can be expressed as the sum of the cause-specific hazards of exiting into the states considered:

$$\theta(t_i, x_i(t)) = \sum_{k \epsilon D} \theta_k(t_i, x_i(t)), \tag{3.21}$$

where D is the set of destination states.

Given the relation between the hazard rate and the survivor function, illustrated by the expression 3.4 above, the survivor function can be written in the competing risks case as:

$$G(t) = \prod_{k \epsilon D} exp\{- \int_0^t \theta_k(u)du\}, \tag{3.22}$$

where G is, as before, the survivor function and D is the set of destination states.

Adopting a piecewise exponential functional form for the baseline hazard and an exponential function for the impact of covariates on the hazard rate, the competing risks hazard rate is the following:

$$\theta(t_i, x_i(t)) = \sum_{k \epsilon D} exp\{a_l^k + \beta^k x_i^k(t)\} \qquad t \epsilon I_l, \tag{3.23}$$

$$I_l = \{t | \tau_l \leq t < \tau_{l+1}\}, \qquad l = 1, 2, \cdots, m,$$

where I allow for two destination states k[2]:

$$\begin{cases} \text{full-time work, } k = 1 \\ \text{other economic states, } k = 2. \end{cases}$$

3.6 Time dependency and unobserved heterogeneity

The concept of the dependency on time of the probability of leaving unemployment was introduced in Section 1.5. The effect of the elapsed unemployment duration on the conditional probability of leaving unemployment is captured by the baseline hazard rate. I adopt a functional form adopted, a piecewise linear exponential, whi is a flexible specification and does not impose severe constraints on the form of time dependency.

However, the estimates of the time dependency of the hazard rate might be affected by unobserved individual heterogeneity (see Section 1.5). Whatever the sign of the bias, it seems therefore important to allow for unobserved individual heterogeneity. However, this is not straightforward because of two sets of problems. The first is that in order to allow for unobserved heterogeneity, assumptions on the form it takes must be made, which might not be the most appropriate. The second has to do with the specification of competing risks models since unobserved heterogeneity may be correlated across the cause-specific hazards. Assumptions must be made on the degree of correlation of unobserved heterogeneity across the cause-specific hazards, which will impose additional constraints on the data.

[2]The reason for this choice of destination states is given in Section 2.4.3 and in the later Chapters.

The literature has conventionally allowed for unobserved heterogeneity by introducing a multiplicative random error term in the specification of the hazard rate:

$$\theta(t_i, x_i, v) = v\psi_1(t_i)exp(\beta x_i), \tag{3.24}$$

where ψ_1 is the baseline hazard, as defined above and v is the random error term. The error term v has typically been modelled, by means of a Gamma distribution (Lancaster, 1979) or using mass point techniques that approximate the continuous error distribution by a discrete distribution (Heckman and Singer, 1984). In the case of competing risks models the additional assumptions of cross-hazards correlation of the error terms of 1 (Katz and Meyer, 1988) or 0 (Flinn and Heckman, 1982) have been made.

Narendranathan and Stewart (1993, p. 71) argue that *"severe distortions are likely to be imposed by the use of such techniques to allow for omitted heterogeneity and there is no reason to expect these distortions to be less serious than those resulting from ignoring omitted heterogeneity in the first place"*. They therefore resolve not to allow for unobserved heterogeneity.

The main finding of previous studies that have allowed for unobserved heterogeneity (by means of a Gamma distribution) in a competing risks framework and that have specified a flexible baseline hazard rate is that allowing for unobserved heterogeneity tends to raise the absolute value of the coefficients (Katz and Meyer, 1988). However, in a single risk framework (allowing for a flexible baseline hazard rate) Katz and Meyer (1990) find that when unobserved heterogeneity is allowed for the sign and the statistical significance of the main coefficients does not change much except for the benefit coefficients (benefit level and duration) which rose by 25%. Meyer (1990) (using a flexible specification of the baseline hazard) finds in a single risk framework that the inclusion of unobserved heterogeneity tends to raise the absolute value of the coefficients. It would seem to be the case that unobserved heterogeneity is more of a problem when the functional form adopted for the baseline hazard is rather restrictive, such as, for instance, the monotonic Weibull, than if a flexible baseline is allowed for. Following these arguments, I do not allow in waht follow for unobserved heterogeneity. However, the wide number of the covariates considered, some of which relate to normally unobserved characteristics, such as for instance, a measure of the individual valuation of leisure, together with the extremely flexible specification of the hazard rate adopted, may reduce the extent of unobserved heterogeneity in the model. Some sensitivity analysis will be carried out in the later chapters to test for the robustness of the parameters of the estimated models[3]. A good reference for the consequences of misspecifying unobserved heterogeneity is Ridder (1987).

[3]It would also have been possible to test explicitely for the presence of unobserved heterogeneity, as suggested by Chesher, 1984.

3.7 The non parametric Kaplan-Meier method

The survivor function and the hazard rate for any sample are often estimated by non-parametric methods before carrying out any estimation by parametric methods. The main advantage of non-parametric methods is that they do not impose any specific or restrictive functional form on the data. Given that economic theory is inconclusive as to the functional form of the conditional probability of leaving unemployment, non-parametric methods are very attractive, at least as a preliminary tool of analysis. Non-parametric estimation of the hazard rate (or of the survivor function) may be useful to gain insights into the behaviour over time of the probability of leaving unemployment.

Non-parametric methods are also useful to approach specific issues such as the detection of spikes in the hazard rate near the time of benefit exhaustion (when the duration of entitlement to unemployment benefit is the same for most people in the sample), which might be preliminary to more sofisticated analysis. Also, some interesting information on the (conditional) probability of leaving unemployment of some exclusive categories of the unemployed may be be gathered by comparing non-parametric estimates their survivor functions. For instance, one could compare the estimated survivor functions for the male and the female unemployed (see Stancanelli, 1994).

The drawback of non-parametric models of unemployment duration is that they do not allow for individual heterogeneity. In practise, homogeneity of the sample with respect to observed and unobserved individual heterogeneity is assumed (Kalbfleish and Prentice, 1980, Meyer, 1990). Non-parametric analysis is normally only useful for preliminary inspection of the data. In order to draw some conclusions, individual heterogeneity must be allowed for.

Non-parametric estimates of the survivor function can be obtained by means of the Kaplan-Meier or Product Limit method[4], which will be briefly described below.

The probability of leaving unemployment, at a given point in time, is set equal to the ratio of the number of completed unemployment spells over the "risk set"; i. e. the number of the observations that are "at risk" of exiting from unemployment. The risk set at a given point in time is equal to the number of the observations that have not yet exited from unemployment plus those observations that are right-censored at the same point in time.

Formally, let us define E_m as the number of observations with completed spells of unemployment at τ_m, Z_m as the number of observations that are right-censored in the time interval (τ_{m-1}, τ_m) and R_m as the number of observations in the risk set at τ_m. The risk set R_m is defined as follows:

$$R_m = (E_m + Z_m) + (E_{m+p} + Z_{m+p}) + \cdots + (E_q + Z_q), \tag{3.25}$$

[4]Other non-parametric methods are reviewed for instance in Kalbfleish and Prentice (1980). They will not be used here.

where τ_m are the discrete points in time at which at least one spell is completed and m varies from 1 to q. The Kaplan-Meier estimate of the survivor function is defined, at any point in time t, as follows:

$$\hat{G}(t) = \prod_{m|\tau_m<t} (1 - \frac{E_m}{R_m}). \tag{3.26}$$

This estimate of the survivor function is based on the observed frequency of exits from unemployment over the risk set at any point in time. The standard error for the Kaplan-Meier survivor function is conventionally defined as follows:

$$SE(\hat{G}(t)) = \hat{G}(t)\sqrt{\sum_{m|\tau_m<t} \frac{E_m}{R_m(R_m - E_m)}}. \tag{3.27}$$

The expression for the integrated hazard, defined in the Section 3.2 above, is straightforward and equal to:

$$\hat{H}(t) = -log(\hat{G}(t)). \tag{3.28}$$

It is possible to derive estimates of the so-called "empirical hazard rate" from the above expression by means of numerical differentiation. The following linear approximation will be used: $\theta = \Delta H / \Delta t$.

The generalization to the competing risk case is straightforward, given the definitions of section 3.5. Let us define D as the set of the k mutually exclusive exits or destination states out of unemployment and $\tau_{k,m}$ as any point in time at which at least one unemployment spell is completed, with $m = 1, \cdots, q_k$. Accordingly, $E_{k,m}$ is now defined as the number of observations that exit to destination k at time $\tau_{k,m}$ and R_m as the risk set at the same point in time. The risk set at any time does not depend on the destination states, but it is equal to the risk set for the single risk case; i. e. to the number of observations with starting time less than $\tau_{k,m}$ and ending time greater than $\tau_{k,m}$. The Kaplan-Meier estimates of the destination specific "pseudo-survivor" functions are given by:

$$\hat{G}(T) = \prod_{m|\tau_{k,m}<t} (1 - \frac{E_{k,m}}{R_m}). \tag{3.29}$$

There is no direct interpretation of the destination specific or pseudo-survivor functions in terms of survivor functions, since they do not properly sum up to one. The overall single risk survivor function is equal to the product of the destination specific pseudo-survivor functions, as illustrated in the preceding section 3.5. In practise, the pseudo-survivor function relative to a given destination is computed treating observations that exit to destinations other than the one under consideration as if they were right-censored.

3.8 The likelihood function for the model

It is conventional to use maximum likelihood techniques to estimate hazard rate models. One of the main reasons is that maximum likelihood techniques allow one to deal easily with right-censoring of the unemployment spells.

The contribution to the likelihood of completed spells of unemployment is the density function, evaluated at the exit time, while the contribution of right-censored spells is the survivor function, evaluated at the time of censoring. The single risk likelihood for a random sample of the inflow can be written as follows:

$$L = \prod_{i \epsilon A} f_i(x_i(t), t_i) \prod_{i \epsilon B} G_i(x_i(t), t_i), \tag{3.30}$$

where i indicates an unemployed individual, A is the set of completed spells and B is the set of right-censored spells. The time t_i is the observed end of individual "i" spell of unemployment, ending with exit from unemployment or right-censoring.

Given the relation between the density function and the hazard rate and between the hazard rate and the survivor function (provided in 3.4 and 3.5), the likelihood function for the data can be rewritten as:

$$L = \prod_{i \epsilon A} \theta_i(x_i(t), t_i) \prod_{i} exp\{- \int_0^{t_i} \theta_i(x_i(u), u) du\}, \tag{3.31}$$

where the second product is now over all spells.

In the case of the LSUS data, it is necessary to allow for the the left truncation of the sample at about three months from the commencement of the unemployment spell. As explained in Section 2.4.2, the left truncation period is set equal to the lapse of time between the commencement of the unemployment spell and the time of the first interview, which varies for each unemployed between 11 and 17 weeks. Allowing for left truncation, the likelihood function for the LSUS sample can be written as:

$$L = \prod_{i \epsilon A} \theta_i(x_i(t), t_i) \prod_{i} exp\{- \int_{t_s}^{t_i} \theta_i(x_i(u), u) du\}, \tag{3.32}$$

where t_s is the individual left truncation time, which varies between 11 and 17 weeks. For estimation purposes, it is convenient to define the corresponding log-likelihood function:

$$LogL = \sum_{i \epsilon A} log\theta_i(x_i(t), t_i) + \sum_{i} \{- \int_{t_s}^{t_i} \theta_i(x_i(u), u) du\}, \tag{3.33}$$

and adopting a piecewise exponential specification of the hazard rate, as in Equation 3.19:

$$LogL = \sum_{i \epsilon A}\{(a_l)\beta x_i(t)\} + \sum_i\{-\int_{t_s}^{t_i}[exp(a_l)\beta x_i(u)du], \qquad (3.34)$$

$$l \subset I_l, \qquad I_l = \{t|\tau_l \leq t < \tau_{l+1}\}, \qquad l = 1, 2, \cdots, m,$$

which is the single risk model I use in the following Chapters.

In the competing risks case, given the definition of the hazard rate provided in 3.21, the likelihood function can be written as follows:

$$LogL = \prod_{k \epsilon D}\prod_{i \epsilon A_k}\theta_{ki}(x_i(t), t_{ki})\prod_{k \epsilon D}\prod_i exp\{-\int_0^{t_i}\theta_{ki}(x_i(u), u)du\}, \qquad (3.35)$$

where A_k is the set of completed spells ending into destination state k. From Equation 3.35 it is possible to see that each cause-specific hazard of the competing risks model can be estimated by simply applying single risk techniques and treating the observations exiting to states other than the one under consideration as if right-censored. Taking the logarithms, allowing for left truncation and specifying the hazard as a piecewise exponential[5], the following expression for the competing risks likelihood is obtained:

$$LogL = \sum_{k \epsilon D}\sum_{i \epsilon A_k}\{a_l^k + \beta^k x_i^k(t)\} + \sum_{k \epsilon D}\sum_i\{-\int_{t_s}^{t_i} exp\{a_l^k + \beta^k x_i^k(u)du\} \qquad (3.36)$$

$$l \epsilon I_l, \qquad I_l = \{t|\tau_l \leq t < \tau_{l+1}\}, \qquad l = 1, 2, \cdots, m,$$

which is the competing risks model I use in the later Chapters, allowing for two destination states k,

$$\begin{cases} \text{full-time work, } k = 1 \\ \text{other economic states, } k = 2. \end{cases}$$

3.9 Summary

In this Chapter, I have laid out the econometric model that I use in the next chapters. Some background information on the statistical concepts used has also been provided.

The preferred specification for the baseline hazard rate is a piecewise linear exponential. I have specified a competing risks model which allows for two destination states out of unemployment: full-time work and other states. The likelihood functions for the LSUS data are given by Equations 3.34 and 3.36 respectively for the single risk and the competing risks models.

[5]The piecewise exponential hazard rate in the competing risks is given by Equation 3.23.

Chapter 4

An assessment of the use of competing risks models with a flexible baseline hazard rate.

4.1 Introduction

In this chapter, two methodological issues are approached using the LSUS data. The first concerns the use of competing risks models of unemployment duration. The second relates to the specification of a flexible baseline hazard. Both comparisons hinge on intuitive arguments more than on formal testing and relate on using the LSUS data.

The use of competing risks model of the duration of unemployment is quite recent. Katz and Meyer (1988, p. 1) distinguishing the finding of a new job from recall to the previous job, conclude that *"the recall and new job exit probabilities have quite different time patterns and are affected in opposite ways by the explanatory variables"*. Narendranathan and Stewart (1993, p. 79) allowing for exit into full-time job and other states, find, instead, that *"the general pattern of the estimated effects of most of the variables"* in the competing risks hazards *"is similar to the single-risk model, although some of the more important effects are more pronounced in the full-time job hazard"*. The two studies might have led to different conclusions simply because of the different cause-specific hazards considered or of the different countries (the first, the US, the second, the UK) they looked at.

Often the data available to do not allow one to distinguish the states entered upon leaving unemployment. For this reason and also given the large number of studies that have adopted a single risk framework it might be useful to gather further evidence on the possible implications of not distinguishing the destination states out of unemployment by comparing the estimates of single and competing risks models for the LSUS data. Of course, the conclusions drawn are strictly dependent on the data used and on the destinations states that have been considered.

The majority of previous applied studies on unemployment duration have estimated single risk models with a Weibull baseline hazard, which does allow the sign of time dependency to vary over time. Recent studies have pointed to the importance of adopting a more flexible specification of the baseline hazard rate. For instance, Meyer (1990), Han and Hausman (1990), Narendranathan and Stewart (1993) compared the results obtained modelling a Weibull baseline hazard rate with those obtained modelling a flexible baseline hazard rate. The restrictions imposed by the Weibull model were always rejected. Narendranathan and Stewart (1993, p. 81) do however conclude that *"the estimated effects of the economic and socio-demographic factors considered are very similar in the two specifications when there are no time varying coefficients"*. It seems therefore interesting to compare the estimation results obtained with the LSUS data using a monotonic or a flexible specification of the baseline hazard.

The focus of interest of this Chapter is the assessment of the implications of using competing risks models with a flexible baseline, for the case of the LSUS data. The reader is referred to Chapter 5, for a detailed discussion of the explanatory variables used in the analysis and of their impact. The coefficients on the explanatory variables are commented upon here only in so far as they shed light on the differences between estimating single or competing risks models and flexible or monotonic baseline hazards, using the LSUS data.

The structure of the chapter is as follows. The data used are described briefly in Section 4.2. Next, in Section 4.3, the single and competing risks models are estimated first by non-parametric Kaplan-Meier method and the results of estimation of the two models are compared. Then, in Section 4.4, the parametric models are estimated and the results of estimation compared. In Section 4.5, I test the independence of the single risk model and the competing risks cause-specific hazards, adopting the method suggested by Narendranathan and Stewart (1991). Two sets of assumptions are tested for: the equality of the coefficients on the explanatory variables and the equality of the baseline hazards. In Section 4.6, I compare the estimated baseline hazards for the single risk and the competing risks models under the assumption of a piecewise linear exponential or a Weibull baseline hazard rate. The estimated coefficients on the explanatory variables under the two specifications are also compared. Finally, I test for the sensitivity of the competing risks model with a piecewise linear baseline to different specifications of the expected earnings variable. Conclusions to the Chapter follow.

4.2 The data

The main features of the LSUS data were described in Chapter 2 together with the construction of some of the explanatory variables. A subsample of the male unemployed that participated in both survey interviews has been

selected for the econometric analysis (see Section 2.4.1).

The overall number of observations is 2035, of which 933 exited into full-time work and 267 into other states. The remaining observations are right-censored (835). The duration of the unemployment spells by the exit states is given in Table 4.1. Two destination states out of unemployment are considered: full-time work and other states[1]. Given the small number of the unemployed exiting to economic states other than full-time work, as shown in Section 2.4.3, it is not sensible to disaggregate further in the econometric analysis the other states exit. This is one of the reasons for grouping the unemployed exiting to states other than full-time work in the same category of "other states". Another reason is that it is easier to compare my findings to those of Narendranathan and Stewart (1993) who allowed also for exit into a job or to other states. As already pointed out, the destination "other" states is not at all homogeneous, since it includes very different labour force and out of the labour force states (see Sections 2.4.1 and 2.4.3).

Table 4.1 shows that exit from unemployment is equally spread over time. No particular increase in the frequency of exits can be detected at any time, at least on the basis of simple visual inspection. Perhaps, between week 48 and week 52 the frequency of the unemployed exiting to other states is higher than in other time intervals. The number of the unemployed exiting to other states is generally smaller in any time interval than that of the unemployed exiting into full-time work. Given the small number of the unemployed exiting to other states in certain weeks, it might not be very reasonable to estimate a weekly baseline hazard rate. However, I am interested in estimating as precisely as possible the baseline hazard rate for the conditional probability of exiting from unemployment into full-time work. The estimation of a weekly baseline for the full-time work exit should not present major problems.

Descriptive statistics of the explanatory variables are given in Table 4.2. The reader is referred to Chapter 5 for a detailed description of the variables considered. It emerges that the unemployed exiting into other states tend to be older on average than those exiting into full-time work. The unemployed exiting into full-time work are on average more likely to have been in full-time work for the largest part of the year before entering unemployment, to be married and to have children aged less than five years.

[1]In previous work (Stancanelli, 1993a), I have allowed for three destinations: full-time work, sickness and other states.

Table 4.1: *Unemployment duration by the exit states*

Unemployment duration in Weeks	*Frequency of Exits*	
	Full-Time Work Exit	*Other States Exit*
12	*2*	*2*
13	*16*	*9*
14	*27*	*16*
15	*35*	*7*
16	*33*	*9*
17	*34*	*5*
18	*27*	*6*
19	*27*	*3*
20	*22*	*4*
21	*23*	*4*
22	*16*	*2*
23	*18*	*3*
24	*23*	*4*
25	*21*	*6*
26	*23*	*8*
27	*22*	*5*
28	*25*	*5*
29	*21*	*4*
30	*21*	*5*
31	*24*	*12*
32	*20*	*1*
33	*33*	*5*
34	*25*	*3*
35	*17*	*4*
36	*11*	*1*
37	*21*	*6*
38	*16*	*8*
39	*17*	*6*
40	*15*	*3*
41	*13*	*5*
42	*12*	*3*
43	*15*	*4*
44	*24*	*5*
45	*16*	*3*
46	*15*	*5*
47	*17*	*2*
48	*13*	*10*
49	*12*	*8*
50	*15*	*3*
51	*7*	*8*
52	*11*	*9*
53	*16*	*5*
54	*14*	*4*
55	*11*	*5*
56	*9*	*3*
57	*8*	*6*
58	*10*	*5*
59	*10*	*2*
60	*12*	*6*
61	*7*	*6*
62	*6*	*2*
63	*9*	*2*
64	*8*	
65	*6*	
66	*2*	
Sum	*933*	*267*

Table 4.2: *Descriptive statistics of the economic variables*

	Full-time work exit		*Other states exit*		*Full sample*	
Variable	*Mean*	*SD*	*Mean*	*SD*	*Mean*	*SD*
Left truncation period	*13.3923*	*1.0545*	*13.4082*	*1.1214*	*13.4069*	*1.0571*
Unemployment duration (weeks)	*33.6506*	*14.3393*	*36.3258*	*15.7641*	*46.9327*	*19.1035*
In f/t work most of the year before U.	*0.6988*	*0.4590*	*0.5768*	*0.4950*	*0.6531*	*0.4761*
Unemployed most of the year before U.	*0.2186*	*0.4136*	*0.2097*	*0.4079*	*0.2216*	*0.4154*
Sick out of work most of the time in the year before U.	*0.0161*	*0.1258*	*0.0599*	*0.2378*	*0.0364*	*0.1872*
Professional/Intermediate Occupation	*0.2004*	*0.4005*	*0.2172*	*0.4131*	*0.1730*	*0.3783*
Unskilled Occupation	*0.0386*	*0.1927*	*0.0487*	*0.2156*	*0.0580*	*0.2338*
Occupation not available	*0.0740*	*0.2618*	*0.0599*	*0.2378*	*0.0708*	*0.2565*
Age 20-24	*0.1426*	*0.3498*	*0.3974*	*0.2970*	*0.1238*	*0.3295*
Age 25-34	*0.3548*	*0.4787*	*0.2846*	*0.4521*	*0.3229*	*0.4677*
Age 35-44	*0.2765*	*0.4475*	*0.2397*	*0.4277*	*0.2482*	*0.4320*
Age 45-54	*0.1768*	*0.3817*	*0.2210*	*0.4157*	*0.1975*	*0.3982*
Age 55-58	*0.0493*	*0.2166*	*0.1573*	*0.3648*	*0.1076*	*0.3100*
Has any child old less than 5	*0.3516*	*0.4777*	*0.2472*	*0.4322*	*0.3410*	*0.4742*
Married	*0.8950*	*0.3068*	*0.8240*	*0.3816*	*0.8673*	*0.3393*
experiences some shortage of money	*0.7856*	*0.4106*	*0.6891*	*0.4637*	*0.7327*	*0.4427*
House owner outright/with mortgage	*0.4234*	*0.4944*	*0.4270*	*0.4956*	*0.3808*	*0.4857*
County unemployment rate	*13.4799*	*3.2047*	*13.3816*	*3.1789*	*13.5856*	*3.2049*
Receives only UB at t1	*0.3719*	*0.4836*	*0.4195*	*0.4944*	*0.3617*	*0.4806*
Receives no UB nor SB	*0.0418*	*0.2002*	*0.1049*	*0.3070*	*0.0462*	*0.2100*
Amount UB and SB, in pounds, logs.	*3.4952*	*0.8431*	*3.1671*	*1.1507*	*3.4778*	*0.8748*
Predicted earnings, in £, logs.	*4.4592*	*0.5527*	*4.4261*	*0.5965*	*4.4485*	*0.5194*
pred. earn. not available	*0.0129*	*0.1127*	*0.0150*	*0.1217*	*0.0113*	*0.1057*
Earnings, in pounds, logs.	*2.5667*	*2.2699*	*2.1090*	*2.2639*	*2.3771*	*2.2561*
Earnings not available	*0.4390*	*0.4958*	*0.5281*	*0.5001*	*0.4683*	*0.4991*

The number of units exiting to full-time is 933, to other exits 267 and the total number of units, including the right-censored is 2035. The dichotomous variables take unitary value when the condition stated for each of them is satisfied. The mean unemployment duration is computed over the completed unemployment spells, excluding the right-censored and the observations exiting to states other the one considered in turn. The logarithms are taken over the non-zero observations. The state occupied for the largest part of the year before is the state occupied for the largest number of weeks as relative to the other possible states. The LSUS survey allows for the following states: full-time work, part-time work, sickness, registered unemployment, government training scheme, full-time education, housework, prison and other states. The state occupied is recorded week-by-week for each week in the year before the commencement of the observed unemployment spell. The base for the three dummies above is given by the unemployed that resulted to be in one of the other possible states considered for the largest proportion of weeks.

4.3 Non-parametric estimates of single and competing risks models

The survivor functions and the hazard rates for the single and the competing risks models have been estimated by non-parametric Kaplan-Meier[2] method. The advantage of non-parametric models is that no restrictive functional form is imposed on the data. However, individual heterogeneity is not allowed for. One must therefore be careful in interpreting the results.

[2]The Kaplan-Meier method is described in Section 3.7 of Chapter 3.

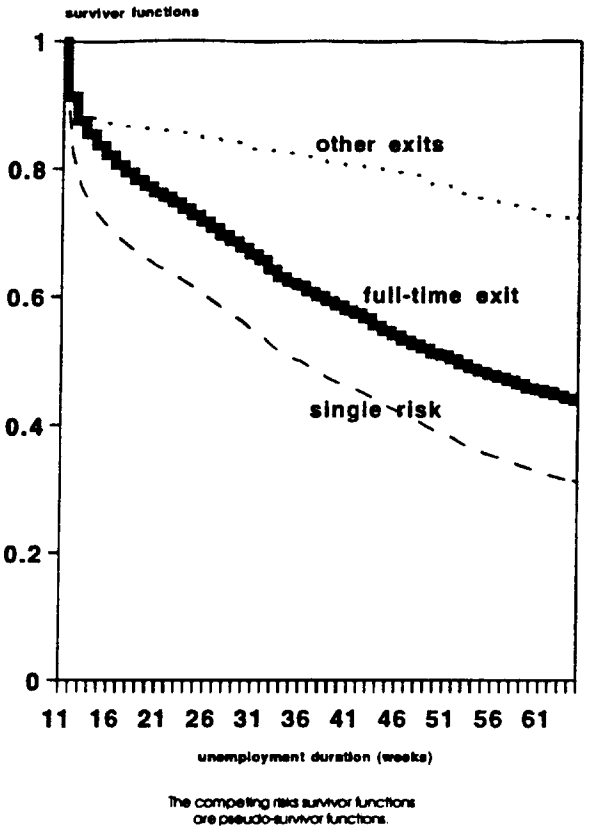

Figure 4.1: *Kaplan-Meier estimates of the survivor functions*

The Kaplan-Meier estimates of the survivor function and the integrated hazards are given in the Tables 8.3, 8.4 and 8.5, in the Appendix, together with the standard errors. The two competing risks pseudo-survivor functions of exit into full-time work and into other destinations are plotted in Figure 4.1 together with the single risk survivor function. The figure starts at the 11th week of unemployment since the behaviour of the survivor function in the weeks before is not observed because of the left truncation of the sample.

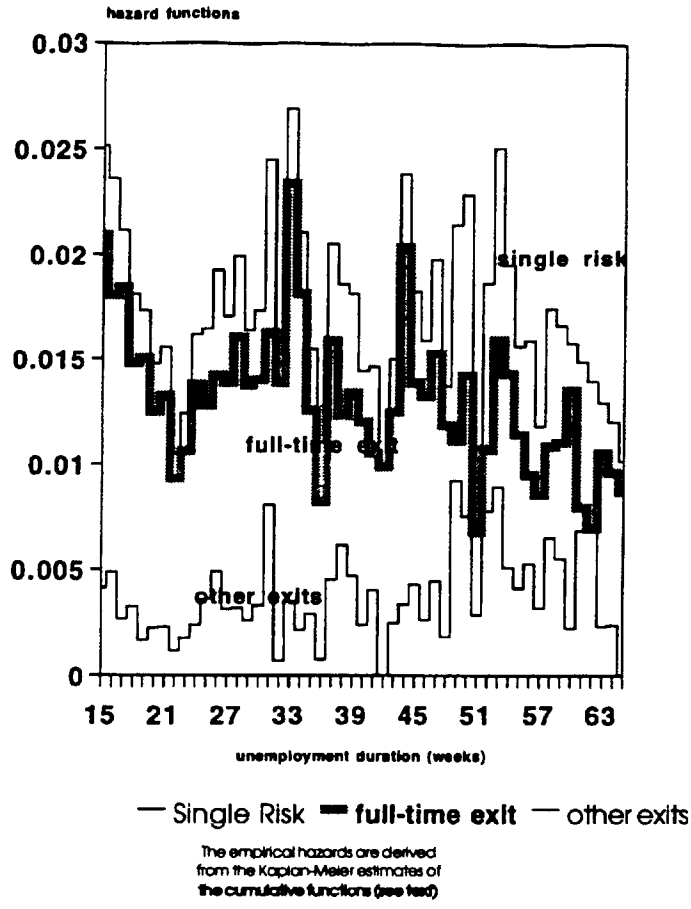

Figure 4.2: *Kaplan-Meier estimates of the hazard rates*

The steeper is the slope of the estimated (pseudo) survivor function, the higher is the hazard of leaving unemployment to enter that specific destination state. According to these estimates, the probability of leaving unemployment to enter full-time work is at all times higher than the probability of exiting to other states. The behaviour of the single risk survivor function and of the pseudo-survivor for exit into full-time work is very similar. This result is probably due to the fact that most of the completed spells of unemployment end into full-time work.

The empirical hazard rates for the single and the competing risks models are plotted in the Figure 4.2. These estimates are derived drom the Kaplan-Meier estimates of the integrated hazard as discussed in Section 3.7. The first few weeks of unemployment duration are not shown given the small number of observations exiting.

According to the estimated empirical hazards, the probability of leaving unemployment to enter states other than full-time work is, at any time, lower than the probability of exiting into full-time work. Some spikes are detected. The spikes are much larger for the hazard of exiting into full-time work than for the hazard of exiting into other states. The single risk hazard follows more or less the same path than the full-time work hazard but shows smaller spikes. The reason for this slight difference in behaviour is that the single risk hazard is equal to the sum of the competing risks hazard rates and only a small number of the unemployed exits into other states.

Individual (observed) heterogeneity is allowed for in the next section.

4.4 Results of estimation of the parametric models

The likelihood functions for the single and the competing risks models are described by Equations 3.34 and 3.36. A weekly baseline hazard rate is specified. Unobserved heterogeneity is not allowed for (see Sections 1.5 and 3.6).

As already discussed in Chapter 1, unobserved heterogeneity is likely to bias the estimated sign of time dependency. Lancaster (1979) pointed out that not allowing for unobserved individual heterogeneity might result in spurious findings of negative time dependency. Katz and Meyer (1990) argued that uncontrolled unobserved heterogeneity might lead to spurious findings of positive time dependency. However, it seems that unobserved heterogeneity is more of a problem when a restrictive baseline hazard rate rather is specified. Previous studies that adopted a flexible specification for the baseline hazard rate concluded that allowing for unobserved heterogeneity did not improve substantially upon the estimated coefficients. Introducing individual unobserved heterogeneity would require making assumptions on the form of the distribution of unobserved heterogeneity and on its correlation across the cause specific hazards of the competing risks model. This would impose additional restrictions on the model which will not necessarily result in more realistic estimates of the parameters of the model. This problem was also highlighted by Narendranathan and Stewart (1993). I therefore refrain from allowing for individual unobserved heterogeneity. I do however test for the robustness of the estimates to alternative specifications of the model. I test also for the sensitivity of the estimates to the use of actual earnings instead of imputed earnings as a measure of the distribution of expected earnings. In the next Chapter, I test the sensitivity of the estimated model to a different choice of covariates and to different specifications of the benefit

variable. I also test there for the robustness of the estimated coefficient on the unemployment benefit level.

Results of estimation of the single risk and the competing risks models are shown in Table 4.3. The estimates of the weekly baseline are given in the Appendix. The two exits of the competing risks model are estimated simultaneously[3].

4.4.1 The impact of the explanatory variables

The reader is referred to Chapter 5 —where also a wider number of covariates will be considered— for a discussion of the impact[4] of the explanatory variables. I restrict attention here to comparing the estimates of the single risk model with those of the competing risks model.

A first inspection of the results indicates that the impact of the explanatory variables differs considerably between the single risk model and the competing risks model. For instance, the coefficient on "full-time work for most of the time in the year before entering unemployment" is not statistically significant in the single risk model; it is instead significant in the two competing risks exits but showing opposite signs. Having been in full-time work the largest part of the year before entering unemployment raises by 50% ($exp0.3922 = 1.48$) the chances of finding a full-time job and halves ($exp - 0.73 = 0.48$) the probability of exiting to states other than full-time work. The single risk model by blending these two opposite effects together leads to the conclusion that this variable is not significant.

Having been unemployed for most of the year before the observed unemployment spell has a similar impact. This variable has an impact not statistically different from zero in the single risk model; it raises by 40% the hazard of exit into full-time work; but decreases by 40% the hazard of exit into other states. A possible explanation for this result is the so called "repeated unemployment"; i. e. the fact that having been in unemployment raises the chances of exiting into a full-time job, to go back into unemployment after a while. An alternative explanation for this result is that having occupied the state of unemployment in the year before the commencement of

[3]This does not affect the estimates of the coefficients, since no restrictions across the two exits were imposed, but only the value of the likelihood function, which is equal to the product of the two corresponding "single risk" likelihoods, which would have resulted if the two exits were to be estimated separately as single risk models with everybody not exiting to the given exit being treated as if right-censored.

[4]The impact of a dichotomous explanatory variable, "z", on the hazard rate can be interpreted as follows. Let us think of two unemployed persons with the same values of the explanatory variables except for the dichotomous variable "z" and let us assume that "z" takes value one for one of them (person A) and equals zero for the other (person B). The hazard rate of A is equal to "$\exp(\beta_z)$" times the hazard rate of B; i. e. A stands "$\exp(\beta_z)$" higher (lower) chances of leaving unemployment than B. The coefficient on a continuous explanatory variable specified in logarithms gives the elasticity of the hazard rate with respect to that variable.

Table 4.3: *Results of estimation: single and competing risks models*

Variable label	Single risk model		Competing risks model			
			Full-time exit		Other states exit	
	Coeff	SE	Coeff	SE	Coeff	SE
F/t work for most of the year before	0.0866	0.1080	0.3922*	0.1363	-0.7290*	0.1849
Unemployed for most of the time in year before	0.0750	0.1181	0.3336*	0.1468	-0.5393*	0.2096
Sick for most of the time in year before	-0.2612	0.2069	-0.4539	0.2895	-0.1585	0.3025
Profess. /Interm. Occup.	0.2377*	0.0864	0.1933*	0.0982	0.4127*	0.1814
Unskilled Occupation	-0.4219*	0.1476	-0.4828*	0.1719	-0.2194	0.2891
Age 20–24	0.1351	0.0973	0.2126*	0.1071	-0.1688	0.2345
Age 35-44	-0.1155	0.0809	-0.1559*	0.0904	0.0372	0.1823
Age 45-54	-0.5180*	0.0961	-0.6113*	0.1101	-0.2059	0.2006
Age 55-58	-1.0490*	0.1323	-1.4814*	0.1722	-0.1849	0.2286
Has any child aged < 5	-0.2783*	0.0760	-0.2887*	0.0843	-0.2775	0.1758
Married	0.2445*	0.0973	0.3777*	0.1163	-0.0891	0.1803
Experiences money shortage	0.2569*	0.0739	0.3050*	0.0861	0.0934	0.1458
House owner	0.3309*	0.0646	0.3310*	0.0731	0.3089*	0.1378
County unemployment rate	-0.0196*	0.0092	-0.0168	0.01046	-0.0252	0.0192
Receives only UB at t1	0.2581*	0.0740	0.2331*	0.0839	0.3920*	0.1572
Receives no UB nor SB	-0.3211	0.3036	-0.4413	0.3578	-0.2662	0.5878
Amount UB and SB	-0.2119*	0.0723	-0.1735*	0.0828	-0.3606*	0.1473
Predicted earnings	0.2473	0.1764	0.5463*	0.1992	-0.7692*	0.3767
No pred. earn.	1.1666	0.8362	2.4498*	0.9463	-3.1645	1.7775

Single risk max. loglikelihood:-5907; competing risks max. loglikelihood: -6451. Weekly baseline estimates are shown in the appendix. Descriptive statistics of the explanatory variables are provided in the preceding table, in the data section, where also some variables are defined. A * indicates that the coefficient is significantly different from zero at the 5% two-sided level. The number of units exiting to full-time is 933, to other exits 267 and the total number of units, including the right-censored is 2035. The logarithms are taken over the non-zero observations. The state occupied for the largest part of the year before is the state occupied for the largest number of weeks as relative to the other possible states. The LSUS survey allows for the following states: full-time work, part-time work, sickness, registered unemployment, government training scheme, full-time education, housework, prison and other states. The state occupied is recorded week-by-week for each week in the year before the commencement of the observed unemployment spell. The base for the three dummies above is given by the unemployed that resulted to be in one of the other possible states considered for the largest proportion of weeks.

the observed spell of registered unemployment raises the individual chances of leaving unemployment more than having been in any of the other base states; i. e. government training scheme, full-time education, part-time work, other states.

Also the estimated coefficient on the individual predicted earnings is not significantly different from zero in the single risk model; it is statistically significant in the competing risks model and shows opposite signs for the two exits.

These findings are rather worrying given the considerable bulk of previous studies that used single risk models and drew policy conclusions on the basis of the estimates obtained. However, if one compares the results of estimation of the single risk model with those of the full-time exit hazard many variables are found to have the same sign and are not too different in absolute value. In

particular, the estimated elasticity of the hazard rate with respect to changes in the level of the unemployment benefit does not vary much across the two models, being equal to 0.21 in the single risk model, to 0.17 for the exit into full-time work and to 0.36 for the other states exit. These estimates are close to those of Narendranathan and Stewart (1993), for the second quarter of the year (0.19).

4.4.2 The piecewise exponential baseline hazard rates

The estimated baseline hazards of the single risk model is plotted in Figure 4.3. The baseline hazard of exit into full-time work is also plotted for comparison purposes. The coefficients on the weekly constants (shown in Table 8.6 in the Appendix) are all statistically significant for both baseline hazards[5]. The baseline of exit into other exits is not plotted since almost none of the estimated coefficients differed significantly from zero. The estimated coefficients are shown in Table 8.6 in the Appendix. These coefficients are larger than those in the full-time hazard and show more variation in size.

The behaviour of the baseline of "other states" is probably due to severe problems of unobserved heterogeneity arising from having grouped together many destination states; such as part-time work, government training scheme, sickness and other non-labour force states. This might also explain why many of the coefficients of the explanatory variables of Table 4.3 are not significantly different from zero in the other states exit. It was found above that variables exercising influence of opposite sign on the cause-specific hazards of the competing risks model, have insignificant effect in the single risk model. Of course, an alternative explanation is that statistical non significance arises from the small number of observations exiting to other states (267 or 13% of the sample considered).

From Figure 4.3, it emerges that both the single risk hazard and the full-time work hazard show large spikes and humps over time. The baseline hazard rate for the full-time job exit shows spikes at week 33, 37, 43–44, 47, 50, 52–53 and 60, from the commencement of the registered unemployment spell. The significance of the differences between consecutive pairs of the baseline coefficients is tested using the information on their variances and covariances. The null hypothesis that each consecutive pair of coefficients, α_l, α_{l+1} is not different from each others is tested as follows:

$$Ho : \alpha_l = \alpha_{l+1}, \qquad \alpha_l - \alpha_{l+1} = 0;$$

$$SE(\alpha_l - \alpha_{l+1}) = \sqrt{var(\alpha_l) + var(\alpha_{l+1}) - 2cov(\alpha_l, \alpha_{l+1})}.$$

[5]An exception is the first week 11, given that there were no completed spells at that time, so that the graph starts at the 12th week of unemployment. No coefficients are estimated for the first 10 weeks because of the left truncation of the sample during the first about three months of unemployment.

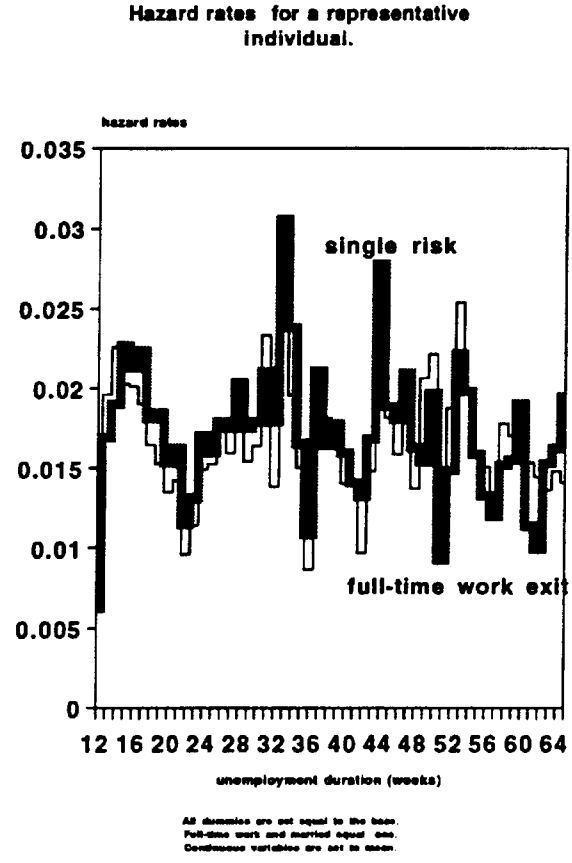

Figure 4.3: *Piecewise linear exponential models: weekly baselines*

The results are given in Table 8.7 in the Appendix. I conclude that while the estimated coefficients are all significantly different from zero (except for the coefficient on week 11) only few of the spikes plotted in Figure 4.3 are statistically significant. However, only successive pairs of coefficients have been considered. On the basis of this evidence no clear pattern of time dependency emerges. The baseline hazard rate appears quite flat, in the sense that the estimated coefficients on the weekly constants do not differ significantly from each other.

Next, I have tested the validity of the restriction imposed by allowing the baseline to vary each month instead than each week. The estimated monthly coefficients of the baseline hazard of exit into full-time work were all significantly different from zero. None of the estimated monthly coefficients for the other exits were statistically significant. This result is in line with the previous findings for the weekly baseline. The monthly baseline hazard rate for the exit into full-time work is plotted in Figure 4.4.

The validity of the imposed by specifying a monthly rather than a weekly piecewise linear baseline hazard rate is tested using a likelihood ratio-test. The test statistic gave the following result:

$$LR = 2(-6451.0 - (-6491.9)) = 81.8 \sim \chi^2_{82},$$

which implies that the restrictions imposed by the monthly baseline cannot be strongly rejected on statistical grounds. This result is probably due to

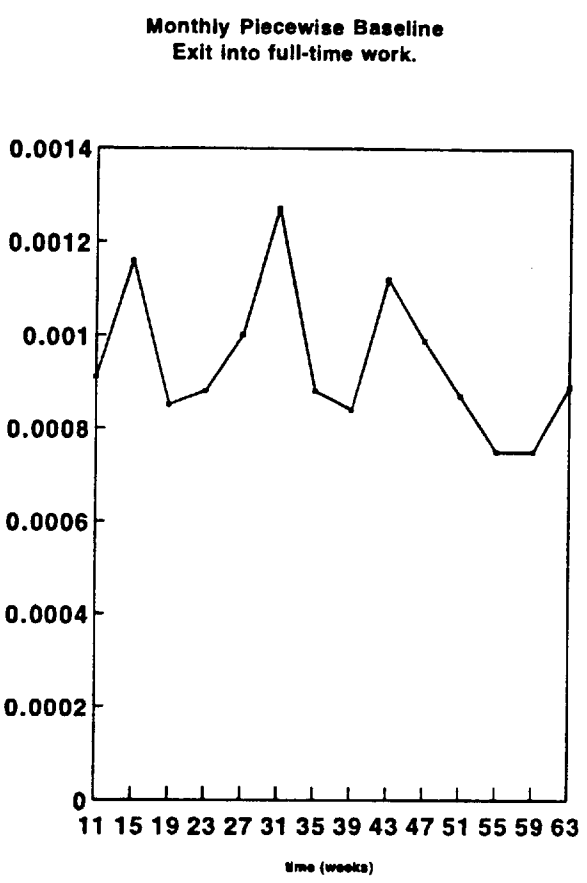

Figure 4.4: *Monthly piecewise linear exponential baseline*

the non-significance of the estimated coefficients on the segments of the baseline hazard for exit into other states (since the values of the two maximum likelihood considered relate to the competing risks of exit into full-time work and other states). It is however, I believe, reasonable to allow for a weekly baseline as long as there are enough degrees of freedom since all the estimated coefficients on the full-time work hazard are significant. Moreover, a monthly baseline would impose the restriction that the baseline hazard rate follows the same pattern each month. This restriction cannot be justified on economic grounds.

The significance of the differences between consecutive coefficients of the monthly baseline hazard of exit into full-time work was tested as above. The results are shown in Table 8.8 in the Appendix. Not many of the coefficients are significantly different from each other. There appears to be a significant rise in the hazard rate from week 11 to 15 and then a fall from week 15 to 19.

Constancy of the dependency on time of the (conditional) probability of leaving unemployment is next tested by estimating a simple exponential model of the hazard rate. The estimated parameter for the baseline hazard of exit into full-time work is strongly significant and equal to 0.00145 (exp(-6.54), SE 0.93) while the estimated baseline parameter of exit into other states did not differ significantly from zero. The restriction imposed by assuming a constant baseline is rejected against a weekly varying baseline on

the basis of a likelihood ratio test. The likelihood ratio test gave the following result:

$$LR = 2(-6451.0 - (-6521.6)) = 141.2 \sim \chi^2_{106}.$$

I have also tested the restriction of imposing a constant baseline hazard on the data against a monthly baseline. This restriction is also rejected on the basis of a likelihood ratio test ($LR = 59.4 \sim \chi^2_{26}$).

According to these findings no clear pattern of time dependency emerges although constancy of the baseline hazard is rejected. These findings are in line with previous UK studies which generally did not find important time dependency effects. For instance, Lancaster (1979) finds (in a single risk model with a Weibull baseline hazard) significant negative time dependency which does however "disappear" if unobserved heterogeneity is allowed for (by means of a gamma distribution). Nickell (1979) finds negative duration dependency which becomes negligible once unobserved heterogeneity is allowed for. Atkinson et al. (1984), who estimate a Weibull baseline hazard without controlling for unobserved heterogeneity, find evidence of significant negative time dependency. It seems reasonable that the results obtained here using a flexible specification of the baseline hazard rate are close to those obtained (in previous studies) specifying a Weibull baseline hazard rate and allowing for unobserved heterogeneity. In Section 4.6, a Weibull baseline is estimated for the LSUS data and the results compared. The reader is referred to that section for further conclusions.

4.5 Testing for the independence of duration and the exit states

I follow here the approach suggested by Narendranathan and Stewart (1991) of conducting simple tests of independence of unemployment duration and the destination states, given the destination states. I test the hypotheses of equality of the coefficients on the explanatory variables and on the baseline constants across the single and competing risks models estimated for the LSUS data.

Narendranathan and Stewart (1993) develop some test statistics for the following general model of the proportional hazard rate family[6]:

$$\theta_i(t) = \psi_1(t) exp[\alpha + x_i(t)'\beta],$$

where they assume in order to identify the model that $\psi_1(0) = 1$ as initial condition. The alternative would have been to assume $\alpha = 0$, as they themselves point out. They do not assume a priori any functional form for the baseline hazard. Their conclusions are therefore independent of the form specified for the baseline hazard. The model I have used is equivalent to their formulation if $\alpha = 0$ is assumed instead than $\psi_1(0) = 1$.

[6]The proportional hazard rate was defined in the introduction and in Section 3.4.

4.5.1 Equality of the covariates impact

The null hypothesis of proportionality of the conditional probabilities of exit into a given destination for the competing risks and the single risk models, at a particular elapsed duration, t, is given by equality of the β vector of coefficients on the explanatory variables. The assumption tested is that of equality of the vector of coefficients on the explanatory variables at any point in time. Any assumption on the specification of the baseline hazard does not affect the testing procedure. The null hypothesis is the following:

$$\beta_j = \beta_h = \beta,$$

which can be written in terms of the hazard rate, specified as a piecewise linear exponential, as:

$$\theta i(t) = exp[x_i(t)'\beta] \sum_k exp\alpha_{lk}, \qquad l = 11, 12, \cdots, 65,$$

where l is a function of time.

The conditional probability of exiting into state k at a certain time t, given that at least one spell is completed at that time is:

$$p_{ki}(t) = \theta_{ik}(t)/\theta_i(t), \qquad k = 1, 2;$$

which is, under the null hypothesis, simply equal to:

$$p_{ki}(t) = [exp\alpha_{lk}]/[\sum_j exp\alpha_{lj}], \qquad l = 11, 12, \cdots, 65.$$

Under the null hypothesis:

$$\theta_{ki}(t) = p_{ki}(t)\theta_i(t),$$

so that the log-likelihood can be written as:

$$LogL = \sum_i \{\sum_k c_{ki}[logp_k(t_i)\theta_i(ti)] - \int_0^{t_i} \theta_i(u)du\}, \tag{4.1}$$

$$= \sum_i \{c_i log\theta_i(t_i) - \int_0^{t_i} \theta_i(u)du + \sum_k c_{ki} logp_k(t)\},$$

where the first two terms correspond to the single risk likelihood and the last term is an adjustment factor. The indicator "c_{ki}" takes value one when the individual "i" exits to state "k". Maximization of the last term with respect to $p_k(t)$ and subject to $\sum_k p_k(t) = 1$ gives the following adjustment factor:

$$\sum_t \sum_k E_{kt} ln(E_{kt}/(\sum_j E_{jt}),$$

where E is the number of completed spells.

The maximized log-likelihood under the null hypothesis (restricted maximum log-likelihood) is then equal to the maximized log-likelihood of the single risk model plus the above adjustment factor. The unrestricted likelihood is the competing risks likelihood. A likelihood ratio-test can be used to test the validity of the restrictions:

$$LR = 2\{lnL^c_{max} - (lnL^s_{max} + Q)\},$$

where Q stands for the adjustment factor, c denotes the competing risks model and s the single risk model.

The test statistics gives the following result:

$$LR = 2\{-6451 - [-5907 - 587.314]\} = 86.6 \sim \chi^2_{19},$$

which leads to rejection of the null hypothesis.

4.5.2 Equality of the baseline hazards

Another interesting hypothesis to test is the equality of the baseline hazards for the given exit states. The piecewise exponential baseline hazards for the two competing risks exits and for the single risk model are shown in the Appendix. The hypothesis of equality of the baselines was rejected on the basis of visual inspection and intuitive arguments. A formal test of this hypothesis is carried out here. Under the null hypothesis, assuming that the covariates do not vary over time, the baseline hazards are equal:

$$\psi_{1j}(t) = \psi_{1h}(t) = \psi_1(t), \tag{4.2}$$

where ψ_1 is the baseline hazard rate. Under the assumption of a piecewise exponential baseline hazard rate, the null hypothesis can be rewritten as:

$$exp\alpha_{I_{1j}} = exp\alpha_{I_{1h}} = exp\alpha_{I_1},$$

$$I_l = \{t|\tau_l \leq t < \tau_{l+1}\}, \qquad l = 1, 2, \cdots, 65,$$

where the unit of time is the week and the first 11 weeks are ignored because of the left truncation of the sample. Under the null, the conditional probability of exiting from unemployment into state k at time t, given that some spells are completed at that time, is the following:

$$p_{ki}(t) = exp(x_i'\beta_k)/\sum_j exp(x_i'\beta_j), \qquad k = 1, 2.$$

The conditional probability above is independent of time. The restricted log-likelihood is the following:

$$LogL = \sum_i c_i log\psi_1(t) + c_{1i}(x_i'\beta_1) + c_{2i}(x_i'\beta_2) - (exp(x_i'\beta_1) + exp(x_i'\beta_2)) \tag{4.3}$$

$$\int_0^{t_i} \psi_1(u)du\}.$$

The restricted maximum likelihood can then be calculated, following Narendranathan and Stewart (1991), by duplication of the sample and estimation of a single risk model where the conditional probability of leaving unemployment, θ_i, is expressed as below:

$$\theta_i = \psi_1(t)exp[x_i'\beta + z_i x_i \delta]; \tag{4.4}$$

$$\theta_i = exp(\alpha_1)exp[x_i'\beta + z_i x_i \delta], \qquad l = 11, 12, \cdots, 65,$$

where $z = c_2$ in the original sample; i. e. it takes value one for the observations exiting to the state other destinations and zero value otherwise. In the duplicate sample, all the observations are, instead, right-censored at the time of completion/right-censoring and z is defined as $1 - c_2$; i. e. it takes value one for the observations that either exit into full-time work or are "truly" right-censored.

The idea behind this procedure is to estimate the same baseline for the two exits but different coefficients on the explanatory variables for the two exits. The duplicated sample where all the observations are right-censored is only going to affect that part of the likelihood relating to the estimated survivor functions, since the contribution to the likelihood of completed spells is the density function and of right-censored spells is, precisely, the survivor function. As a result, the estimated coefficients on the explanatory variables give the β_1 coefficients and the estimated coefficients, δ, on the explanatory variables multiplied by the new variable z provide estimates of $\beta_2 - \beta_1$.

The model above was actually estimated adopting a monthly specification of the piecewise linear exponential baseline, to simplify the burden of estimation given that the covariates are here more than twice as many as before. Results of estimation are shown in Table 8.9, in the Appendix. Equality of the monthly baselines seems an interesting hypothesis as much as equality of the weekly baselines. The likelihood for the unrestricted model was estimated as before but specifying monthly baseline constants. A likelihood-ratio test gave the following result:

$$LR = 2\{-6491.9 - (-5731.9)\} = 1520 \sim \chi^2_{14}.$$

The null hypothesis of equality of the single and competing risks baselines is strongly rejected on the basis of the above evidence.

To sum up, on the basis of the formal tests carried out here the proportionality of the cause-specific hazards has been rejected. The results of the analysis conducted here support the use of competing risks models.

4.6 A comparison with the Weibull model

The piecewise linear exponential model is compared here with a Weibull model. The main difference between the piecewise linear and the Weibull baseline is that the Weibull baseline implies monotonic time dependency, while the piecewise baseline allows the sign of time dependency to vary over time[7]. The same explanatory variables as before are considered. The results of estimation for the Weibull model are shown in Table 4.4 below.

Table 4.4: *Weibull specification: single and competing risks models*

Variable label	Single risk model		Competing risks model			
			Full-time exit		Other states exit	
	Coeff	SE	Coeff	SE	Coeff	SE
Constant	-4.4998*	0.9565	-7.0673*	1.1715	-0.3798	1.7342
F/t work year before	0.0991	0.1241	0.4757*	0.1713	-0.6772*	0.1949
Unemployed year before	0.0857	0.1356	0.4058*	0.1824	-0.5016*	0.2080
Sick out of work year before	-0.2968	0.2381	-0.5530	0.3562	-0.1395	0.2849
Profess. /Interm. Occup.	0.2689*	0.1004	0.2331	0.1208	0.3806*	0.1767
Unskilled Occupation	-0.4810*	0.1726	-0.5863*	0.2154	-0.1992	0.2731
Age 20–24	0.1491	0.1120	0.2539	0.1322	-0.1633	0.2221
Age 35-44	-0.1309	0.0932	-0.1890	0.1112	0.0393	0.1721
Age 45-54	-0.5842*	0.1165	-0.7372*	0.1465	-0.1742	0.1893
Age 55-58	-1.1875*	0.1708	-1.7915*	0.2572	-0.1514	0.2146
Has any child < 5	-0.3138*	0.0896	-0.3499*	0.1066	-0.2472	0.1677
Married	0.2789*	0.1130	0.4582*	0.1467	-0.0863	0.1708
experiences money shortage	0.2883*	0.0868	0.3680*	0.1090	0.0764	0.1373
House owner	0.3748*	0.0781	0.3989*	0.0947	0.2876*	0.1337
County unemployment rate	-0.0224*	0.0106	-0.0204	0.0129	-0.0239	0.0183
Receives only UB at t1	0.2885*	0.0867	0.2774*	0.1042	0.3589*	0.1533
Receives no UB nor SB	-0.3793	0.3500	-0.5377	0.4396	-0.2915	0.5568
Amount UB and SB	-0.2442*	0.0848	-0.2100*	0.1027	-0.3465*	0.1460
Predicted earnings	0.2786	0.2028	0.6579*	0.2487	-0.7145	0.3666
No pred. earn.	1.3171	0.9615	2.9498*	1.1784	-2.9232	1.7142
Log Weibull constant	-0.1372	0.0744	-0.1983*	0.0896	0.0601	0.1307

Single risk max. loglikelihood:-5938.5; competing risks max. loglikelihood: -6519.7. Descriptive statistics of explanatory variables are provided in the table 4.2. A * indicates statistical significance at the two-sided 5% level.

The time dependency parameter is statistically significantly different from zero only for the exit into full-time work[8]. In this case, it is equal to "0.82" and indicates negative time dependency. In the single risk Weibull, the "log(c)" parameter is significantly different from zero only at the 10% two-sided level and it signals negative time dependency. These results are in line with the findings of negative time dependency of previous British authors that used a restrictive specification for the baseline hazard rate (when they did not allow for unobserved heterogeneity), as discussed at the bottom of

[7]See Section 3.3.

[8]The Weibull parameter "c", which gives the sign of the dependency on time of the hazard rate (see Section 3.3), is equal to the exponential of the coefficient shown in Table 4.4.

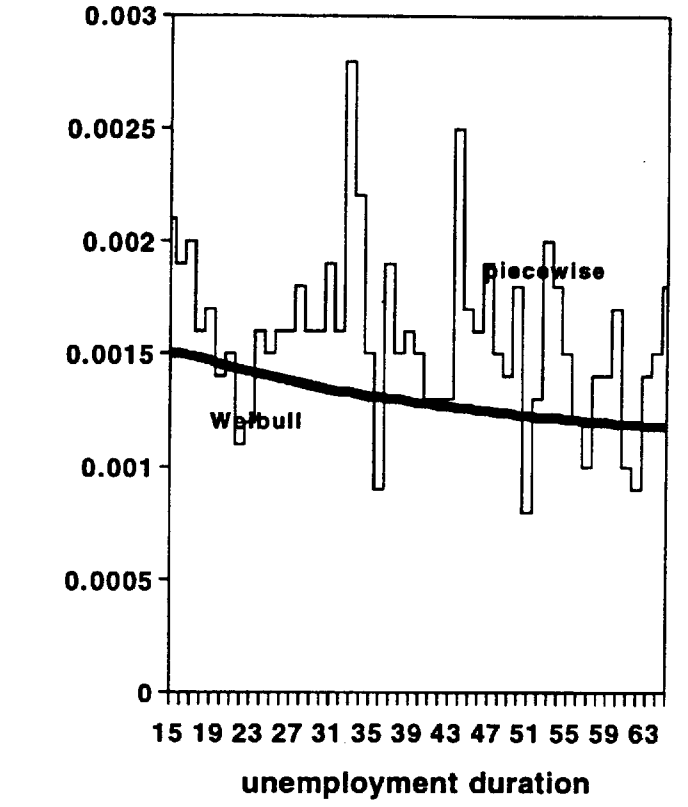

Figure 4.5: *Weibull and piecewise linear exponential baselines*

Section 4.4.2.

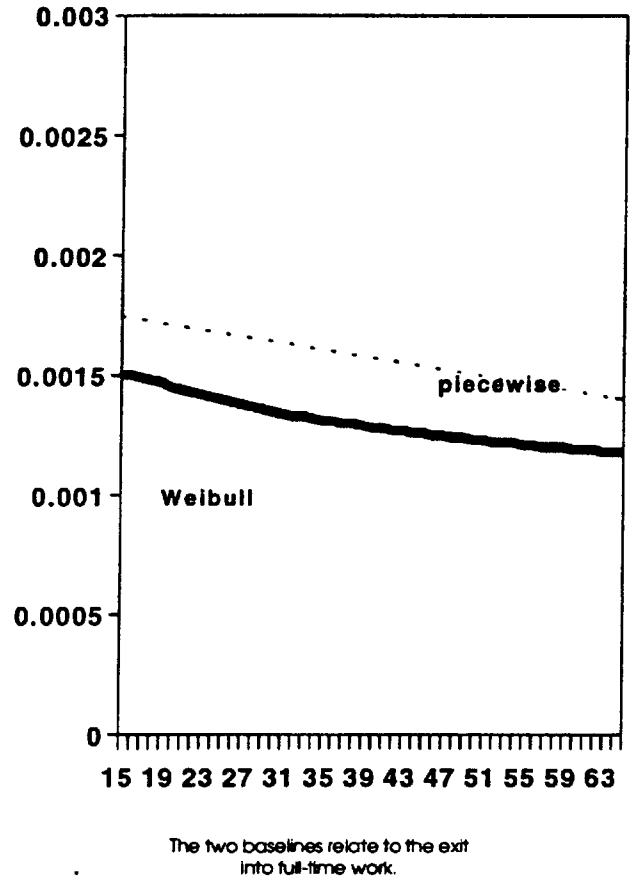

Figure 4.6: *Is there negative time dependency in the long run?*

The other exits "log(c)" parameter is not significantly different from zero . The statistical non-significance of the other exits "log(c)" parameter is probably due to the considerable heterogeneity of the states grouped under the category "other states". One could also explain by the same argument the fact that the single risk time dependency parameter is not statistically significant. The estimated coefficients on the baseline constants of the exit into other states were neither significantly different from zero.

The Weibull baseline for exit into full-time work is plotted in Figure 4.5 together with the piecewise linear exponential baseline (which is allowed to vary each week).

For the purpose of comparison, I have simply drawn a line through the steps of the piecewise exponential baseline. This line is plotted in Figure 4.6 together with the Weibull baseline hazard rate. Both lines are decreasing over time. Negative time dependency would seem to emerge in both cases. However, in Section 4.4.2 no significant pattern of time dependency was detected. It is likely that the significant Weibull estimate of negative time dependency would disappear if unobserved heterogeneity were allowed for. This is what I would expect following previous UK findings, which were summarized at the bottom of Section 4.4.2. It seems therefore preferable to estimate a flexible baseline hazard rate.

The coefficients on the explanatory variables turn out to be quite similar in sign, absolute value and statistical significance to the piecewise linear co-

efficients. This result is in line with the previous findings of Narendranathan and Stewart (1993) who concluded that the impact of the explanatory variables was very similar if a Weibull model or a model with a more flexible baseline were estimated (at least in the absence of time varying covariates).

4.7 Sensitivity to the specification of the earnings variable

The earnings variable should capture the expected earnings of the unemployed. A "smoothed" earnings variable —computed as described in Section 2.4.4— was used instead of the reported earnings for the reasons given in Section 2.4.4. However, the use of imputed earnings instead of observed earnings may introduce addtional problems into the model; such as in particular unobserved heterogeneity, at least if the variance of the additional error is large. I present below the estimates of the model were reported earnings in the last job are entered among the regressors.

The results of estimation are presented in Table 4.5. I find that the estimated coefficients are very close to those of the model estimated with the smoothed earnings variable. The value of the maximised log-likelihood is also quite close to that of the other model.

Table 4.5: *Results of estimation: using reported usual earnings*

Variable label	Competing risks model			
	Full-time exit		Other states exit	
	Coeff	SE	Coeff	SE
F/t work for most of the year before	0.3093*	0.1418	-0.7118*	0.1987
Unemployed for most of the time in year before	0.2717	0.1472	-0.5186*	0.2107
Sick for most of the time in year before	-0.4686	0.2897	-0.1742	0.3035
Profess. /Interm. Occup.	0.2570*	0.0893	0.2170	0.1638
Unskilled Occupation	-0.4783*	0.1718	-0.1823	0.2891
Age 20–24	0.1762	0.1051	-0.0774	0.2312
Age 35–44	-0.1212	0.0885	-0.0529	0.1782
Age 45-54	-0.5887*	0.1088	-0.2806	0.1979
Age 55-58	-1.4872*	0.1719	-0.2273	0.2287
Has any child aged < 5	-0.2955*	0.0842	-0.2754	0.1757
Married	0.4105*	0.1154	-0.1443	0.1779
Experiences money shortage	0.3198*	0.0860	0.0984	0.1463
House owner	0.3163*	0.0732	0.2695 *	0.1380
County unemployment rate	-0.0158	0.0104	-0.0259	0.0192
Receives only UB at t1	0.2094*	0.0844	0.3881 *	0.1592
Receives no UB nor SB	-0.4839	0.3536	-0.2430	0.5813
Amount UB and SB, £, log.	-0.1991*	0.0819	-0.3596 *	0.1457
Reported earnings, £, log.	0.4371*	0.1033	0.1381	0.1968
No reported earnings, £, log.	1.8801*	0.4711	0.6467	0.8799

Competing risks max. loglikelihood: -6447.331. Weekly baseline estimates are shown in the appendix. Descriptive statistics of the explanatory variables are provided in the preceding table, in the data section, where also some variables are defined. A * indicates that the coefficient is significantly different from zero at the 5% two-sided level. The number of units exiting to full-time is 933, to other exits 267 and the total number of units, including the right-censored is 2035. The logarithms are taken over the non-zero observations. The state occupied for the largest part of the year before is the state occupied for the largest number of weeks as relative to the other possible states. The LSUS survey allows for the following states: full-time work, part-time work, sickness, registered unemployment, government training scheme, full-time education, housework, prison and other states. The state occupied is recorded week-by-week for each week in the year before the commencement of the observed unemployment spell. The base for the three dummies above is given by the unemployed that resulted to be in one of the other possible states considered for the largest proportion of weeks.

4.8 Summary of the results and conclusions

The focus of interest of this chapter has been on the importance of allowing for the destination states entered upon leaving unemployment and on the specification of the baseline hazard rate.

I have investigated the difference between the estimation results of the single and competing risks models, using the LSUS data and distinguishing exit into full-time work from exit into other states. My conclusion is that, on the basis of both intuitive arguments and formal statistical testing, the use of competing risks models is to be strongly recommended. The estimation results indicate that the coefficients on some of the explanatory variables differ substantially in sign, statistical significance and absolute value across the single risk model and the two cause-specific hazards of the competing risks model. Also the coefficients of the baseline hazards differ considerably across the single risk model and the two cause-specific hazards of the competing risks model. Likelihood ratio tests of the null hypotheses of the equality of the coefficients on the explanatory variables and the baseline hazard rates across the single and the competing risks models reject these hypotheses strongly.

Therefore, my conclusion is that one should be careful in interpreting the results of estimation of single risk models.

I have compared the estimates obtained allowing the baseline hazard to vary weekly with those of a Weibull model, which impose monotonic time dependency. I have found that the estimated coefficients on the explanatory variables do not differ substantially if a Weibull or a piecewise linear exponential model are specified. This result is in line with the previous findings of Narendranathan and Stewart (1993) for the UK. It suggests that the specification of the baseline hazard rate does not affect the impact of the explanatory variables, at least not to a large extent. In the case of the LSUS data, the Weibull estimates indicate negative time dependency in the full-time job cause-specific hazard. The piecewise linear exponential estimates of the baseline hazard for the exit into full-time job do not instead show a significant time dependency pattern. I conclude that it is preferable to estimate a flexible baseline hazard rate.

Alternative specification of a piecewise exponential baseline hazard rate were tried out: weekly segments, monthly segments and constant exponential. The estimated coefficients on the segments of the baseline hazard of the "other exit states" did not turn out statistically significant. No clear pattern of time dependency could be detected for the cause-specific hazard of exit into full-time work. This result is in line with previous UK studies, such as Lancaster (1979) and Nickell (1979). Interestingly, these authors using quite restrictive specifications of the baseline hazard rate found evidence of negative time dependency which did however become not significant upon controlling for unobserved heterogeneity.

Chapter 5

A competing risks model of the re-employment probability

5.1 Introduction

Previous UK studies on unemployment duration have focused on the impact of the level of unemployment benefit on the hazard rate, given the obvious importance of this issue for economic policy. However, there was no agreement on the significance of the benefit impact (see Section 1.2) until the study of Narendranathan et al. (1985) who found robust evidence in favour of a significantly negative benefit impact. The authors found that the benefit effect declined during the first six months of unemployment and they estimated a benefit elasticity that varied (with unemployment duration) between 0.08 and 0.65. I anticipate that using the LSUS data I conclude that the level of unemployment benefit does not affect significantly the re-employment probability or at least that the data do not one allow to detect a significant impact.

In this chapter, I estimate the impact of many socio-economic and institutional factors on the re-employment probability. The individual re-employment probability is modelled as a conditional probability in a competing risks framework. The only previous study that estimated a competing risks model of unemployment duration distinguishing exit into a job from other exits for the UK is due to Narendranathan and Stewart (1993). The authors choice of covariates is more limited than in the model of this chapter.

The structure of the chapter is the following. In the next Section 5.2, the explanatory variables of the model are defined. These are individual demographic characteristics, family composition variables, unemployment benefit variables, expected income from work, some proxies deemed to capture financial hardship, search activity, leisure valuation and demand side conditions. In Section 5.3, the results of estimation of the econometric model are discussed. A competing risks hazard with a piecewise baseline is specified and the baseline is allowed to vary each week. Two destination states are modelled: full-time work and other states. Part-time work is considered first

with the "other destinations" and then together with full-time work, for sensitivity analysis purposes. The limited number of the unemployed that end their unemployment spell into part-time work does not allow me to consider part-time work by itself. The robustness of the coefficient on the level of unemployment benefit to the choice of covariates is tested in Section 5.4. Then, in Section 5.5 I compute the predicted mean unemployment duration under alternative sets of assumptions. Conclusions follow.

5.2 A description of the data

Descriptive statistics of the variables employed are presented in Table 5.1 for the full sample selected for the econometric analysis (see Section 2.4.1) and for the subset of the unemployed that exited into full-time work or into any job; i. e. full-time or part-time work. The explanatory variables are defined below.

5.2.1 Demographic and family composition variables

The only demographic variable considered here is the age of the unemployed person. Older age is expected to affect negatively the individual (conditional) probability of leaving unemployment to enter full-time work. The survey did not collect any information on the race of the unemployed. Narendranathan and Stewart (1993) using the DSS Cohort Study 1978/79 allowed race to affect unemployment duration. Its impact was found to be statistically insignificant. Gender is not considered because I have restricted attention to the male unemployed. The impact of gender on unemployment duration is treated in a separate piece of work (Stancanelli, 1994).

I model the impact of age with a series of dummies that take value one in given age intervals. The base for these dummies are the unemployed aged between twenty-five and thirty-four years. About 10% of the unemployed are aged between fifty-five and fifty-eight. The same figure for the unemployed that found a job by the time of the second survey interview is about 5%. The proportion of younger unemployed aged between twenty and forty-four is slightly larger for the subset that exited into employment than for the full sample.

Table 5.1: *Descriptive statistics of the economic variables*

	F-t work exit		*F-t/p-t work exit*		*Full sample*	
Variable	*Mean*	*SD*	*Mean*	*SD*	*Mean*	*SD*
Left truncation period	13.3923	1.0545	13.4056	1.0655	13.4069	1.0571
Unemployment duration (weeks)	33.6506	14.3393	33.8392	14.5053	46.9327	19.1035
F/t work for most of the year before U.	0.6988	0.4590	0.6933	0.4614	0.6531	0.4761
Unemployed for most of the year before U.	0.2186	0.4136	0.2138	0.4102	0.2216	0.4154
Sick, no work for most of the year before U.	0.0161	0.1258	0.0150	0.1216	0.0364	0.1872
Professional/Intermediate Occupation	0.2004	0.4005	0.2068	0.4052	0.1790	0.3783
Unskilled Occupation	0.0386	0.1927	0.0390	0.1936	0.0580	0.2338
Occupation not available	0.0740	0.2618	0.0719	0.2585	0.0708	0.2565
Age 20-24	0.1426	0.3498	0.1439	0.3511	0.1238	0.3295
Age 25-34 (base group)	0.3548	0.4787	0.3506	0.4774	0.3229	0.4677
Age 35-44	0.2765	0.4475	0.2707	0.4446	0.2482	0.4320
Age 45-54	0.1768	0.3817	0.1828	0.3867	0.1975	0.3982
Age 55-58	0.0493	0.2166	0.0519	0.2220	0.1076	0.3100
Has any child old less than 5	0.3516	0.4777	0.3467	0.4761	0.3410	0.4742
Married	0.8950	0.3068	0.8911	0.3117	0.8673	0.3393
Spouse working 1 month before U.	0.3301	0.4705	0.3357	0.4725	0.2688	0.4434
Searches less than before	0.0407	0.1978	0.0400	0.1960	0.0958	0.2944
Values Leisure more than Labour	0.0965	0.2954	0.1019	0.3027	0.1371	0.3440
experiences some shortage of money	0.7856	0.4106	0.7802	0.4143	0.7327	0.4427
House owner outright/with mortgage	0.4234	0.4944	0.4266	0.4948	0.3808	0.4857
County unemployment rate	13.4799	3.2047	13.4416	3.2161	13.5856	3.2049
Receives only UB at t1	0.3719	0.4836	0.3746	0.4843	0.3617	0.4806
Receives no UB nor SB	0.0418	0.2002	0.0490	0.2159	0.0462	0.2100
logs. benefit (£)	3.4952	0.8431	3.4534	0.8903	3.4778	0.8748
logs. benefit time varying (£)	3.2779	1.1543	3.3900	1.0067	2.9885	1.3465
Predicted earnings, in £, logs.	4.4592	0.5527	4.4560	0.5561	4.4485	0.5194
predicted earnings not available	0.0129	0.1127	0.0130	0.1133	0.0113	0.1057
other unemployment income, £, logs	2.4021	1.5203	2.4102	1.5217	2.4817	1.6513
other employment income, £, logs	2.1877	1.5687	2.6347	1.4672	2.3036	1.6978
reported earnings, £, logs	2.5667	2.2699	2.5452	2.2675	2.3771	2.2561
no reported earnings, £, logs	0.4330	0.4958	0.4366	0.4962	0.4683	0.4998

The number of units exiting to full-time is 933, to full-time and part-time is 1001 and the total number of units, including the right-censored is 2035. The dichotomous variables take value one when the condition stated for each of them is satisfied. The mean duration for exit into full-time work or full-time and part-time work is computed excluding the right-censored observations and the observations exiting to states other than the one under consideration. The logarithms are taken over the non-zero observations. The mean unemployment duration is shorter for the unemployed that exited into any job than for the full sample because the full sample includes also individuals with right-censored spells.

The family composition variables considered are marital status and the presence of any child aged less than five years in the nuclear family. It it is quite common in applied analyses of unemployment duration to include marital status among the explanatory variables. The for this choice is that greater family "needs" should drive people faster back to work. Instead, the presence of young children in the household might increase the opportunity cost of labour and therefore raise the reservation wage.

5.2.2 Previous work history variables

The individual past work history is expected to influence the job offer probability. Two groups of variables have been considered here. The first relates to the activity undertaken for most of the year before the commencement of the unemployment spell. The second concerns the professional qualifications of the unemployed.

The activity undertaken for most of the year before the commencement of the unemployment spell is defined as follows. The survey collects retrospective information on the economic activity of the sample participants in each week during the year before the commencement of the unemployment spell. Using this information, I could compute the activity undertaken for the largest number of weeks in the year before the commencement of the unemployment spell. If the activity undertaken for the largest number of weeks was "full-time work", then the dummy "in full-time work for most of the year before U. " takes value one. The other two dummies for "unemployed" or "sick and out of work" have been computed similarly. The base for these dummies are the unemployed that spent most of the year before the commencement of the unemployment spell in other states; i. e. full-time education, government training schemes, part-time work, in care of family and any other state.

About 65% of the unemployed were in full-time work for most of the year before becoming unemployed. The corresponding figure for the unemployed that took up a job before the end of the observation period is five percentage points larger. About 22% of the sample were unemployed most of the year before the commencement of the unemployment spell. Almost 4% were sick and out of work for the largest part of the year before the commencement of the new unemployment spell. The corresponding figure for the unemployed that left unemployment to take up employment is less than two percentage points.

The LSUS survey collects information about the so called "socio-economic" class which generally reflects individual educational attainments and last job position (occupation) held. I have computed two that take value one if the unemployed person was classified respectively as a professional or intermediate worker and as an unskilled worker. About 17% of the unemployed had professional or intermediate qualifications and almost 6% had no work skills. The base for the dummy is formed by the unemployed that had skilled

and semi-skilled (manual or non-manual) qualifications. The proportion of professional or intermediate workers is higher among the unemployed that went back into employment before the end of the observation period. The proportion of unskilled workers is instead lower.

I also use a dummy to allow for cases when the "social class" nformation was not available. These were less than 1% of the unemployed, as shown in Table 5.1.

5.2.3 Income variables

The income variables relate to the income while unemployed; the expected income from work; and financial constraints.

Unemployment income variables

The unemployment benefit variables considered first are the amoun and the type of unemployment benefit received. As discussed in Section 1.3 to which the reader is referred, in the UK there are two types of unemployment benefit: the national insurance benefit (UB) and the social assistance benefit (SB).

I allow for the type of benefit with a dummy that takes value one if the unemployed reported only UB at the first interview. Recipients of (only) UB are likely to behave differently from recipients of SB (with or without UB) because they fear that their benefit entitlement will expire[1]. I have also constructed a dummy taking value one for the unemployed that report not benefit receipts at the first interview. They are probably awaiting to receive unemployment benefit or they are temporarily disqualified from benefit receipts (since they are registered at benefit offices). However, it is not known when and if they will start to receive any unemployment benefit. The dummy should hopefully control for the unknown facts. In Table 5.1, it is shown that about 4% of the unemployed in the sample are in this situation.

The benefit level is a time-varying variable. It is constructed as described in Section 2.5.4, to which the reader is referred. Also the benefit reported at the first interview is considered for sensitivity analysis purposes. The mean and the standard deviation of the level of unemployment benefit (in logarithms), allowing and not allowing for time variation, are reported in Table 5.1. When the benefit level is allowed to vary over time, its mean appears to decrease slightly. This result is quite reasonable and it is explained by the fact that some proportion of the unemployed receiving (only) UB at the first interview cease to receive any benefit upon UB entitlement exhaustion.

The amount of benefit is entered in logarithms. The corresponding coefficient provides then an estimate of the elasticity of unemployment duration to changes in the level of unemployment benefit. This is equivalent to assuming

[1]In Chapter 6, the impact of benefit entitlement duration will be explored more in depth.

a constant elasticity and is quite common in most previous studies. Other authors enter the benefit amount in levels; for instance Katz and Meyer (1988, 1990), Atkinson et al. (1984), (in Table 3). One could also enter the benefit amount as the numerator of the "replacement ratio" variable, with expected income from work in the denominator. However, it is preferable to the two variables separately and to test for the "replacement ratio" restriction; i. e. for the equality of the absolute values of their coefficients[2].

Some authors consider the total unemployment income as an explanatory variable and take into account all income sources that accrue to the unemployed person; for instance, Narendranathan and Stewart (1993). For comparison purposes I employ the "other unemployment incomes" variable, which is added to the unemployment benefit. This variable includes any other social security benefit (Section 2.5.5) that accrue to either partners and the earnings of the spouse if any (at the first interview). These variables are measured at the first interview. I assume that they do not vary over the course of the unemployment spell since there is not enough information on any changes in the survey. Moreover, any changes in the spouse's earnings during the partner's unemployment spells should be treated carefully (if available) given their potential endogeneity[3].

Financial situation proxies

The expected effect of financial constraints is to raise the (conditional) probability of leaving unemployment as discussed in Section 1.2 of Chapter one[4]. I model here the tightness of the unemployment income budget or the financial resources of the unemployed using replies to the following question have been used for this purpose:

"are you experiencing any shortage of money?".

This question was asked to both partners at the first interview. I have computed a dummy called "money bad" which takes value one if the unemployed person or his spouse replied positively to the question. Of course, this variable indicates only subjective views of income shortage. About 70% of the unemployed (or their spouses) replied positively to this question (see Table 5.1). This seems plausible because unemployment benefit receipts are much lower on average than expected earnings from work (see Table 5.1). The proportion of the unemployed that consider that they suffer from some shortage of money is higher among those that exit into full-time (and part-time) work than it is for the full sample.

[2]In what follows this restriction is however not tested given the insignificance of the benefit coefficient.

[3]The literature has pointed out that the unemployment benefit addition for dependent spouse might have a disincentive impact on the spouse participation in the labour force. See, for instance, Garcia (1989) or Micklewright and Giannelli (1990).

[4]The relationship between the financial situation of the unemployed and the duration of unemployment is investigated in Chapter 7

Another variable which might capture, at least to a certain extent, the financial conditions of the unemployed is "house ownership". The unemployed that own a house might have easier access to institutional credit. They might also be wealthier than the other unemployed. Moreover, house ownership might proxy unobserved individual characteristics such as the capacity of planning ahead or a good sense of organization or reliability or stability or simply "social status". In any case the expected impact of house ownership on the (conditional) probability of leaving unemployment is positive given that this variable proxies either access to credit and financial wealth or "good" (with respect to one's chances of leaving unemployment) unobserved individual characteristics.

I have constructed a dummy that takes value one if the unemployed own the house where they live (either outright or with a mortgage)[5]. The base for the dummy are the unemployed that live in any other type of accommodation; i. e. they live in council flats/houses or rent accommodation privately or live in caravans/boats (this last category is very small as plausible). The proportion of house owners is slightly higher (by about 4% percentage points) among the unemployed that exit into a job than in the (full) reference sample (Table 5.1).

Expected income from work

The expected income variable has been computed as shown in Section 2.4.4. The motivation for constructing an expected income variable is also given in Section 2.4.4, to which the reader is referred.

Average reported earnings appear considerably lower (see Table 5.1) than average predicted earnings simply because a large number of the unemployed (47%) do not report earnings —and this is one of the reason for estimating expected earnings for the unemployed. Most of the unemployed do however provide information on their last occupation so that it was possible to impute expected earnings for all but 1% of the sample (see Section 2.4.4). I have constructed a dichotomous variable taking value one for the unemployed for whom predicted earnings could not be computed.

The "other expected income from work" variable is summed up to the expected income in order to obtain estimates of the total expected income from work. "Other expected income form work" includes spouse's earnings and other income such as social security receipts by either partners. These variables relate to the amounts reported at one month before the commencement of the unemployment spell. These sources of income may not vary over time and in any case their variation may not depend on whether the individual is experiencing unemployment or it may be difficult to forecast. In Table 5.1 it is indeed shown that the variables other unemployment income

[5]The impact of outright ownership might differ from that of ownership with a mortgage. This possibility was tested and rejected.

and other employment income do not differ much at the individual level in mean or standard deviation.

5.2.4 The individual intensity of search

The expected impact of higher search intensity on the (conditional) probability of leaving unemployment is positive. I have constructed a proxy for search intensity using the respondents replies to the following question:

> *"Here is a list of things people do. We would like to know whether you do each thing more or less than you did four or five months ago, before you started/restarted signing on at an unemployment benefit office ··· Visiting an unemployment benefit office or a job centre".*

This question was part of a self-completion booklet and was to be replied for a long list of activities such as watching television, gardening, visiting friends, going out for a drink, taking part in a sport, and many others, for each of which the unemployed had to tick either of four boxes: *"no change, more than before, less than before"*. It is unlikely that the unemployed regarded the question as a control question on their search activity because the question was posed in a casual and indirect way.

The question was asked at the time of the first interview. It compares the frequency with which some activities were undertaken at the time of the first interview and one month before the commencement of the observed unemployment spell. This question captures only search activity that involves visiting a job centre or an unemployment benefit office, while job search might be undertaken also by reading job advertisements in the papers, asking friends and relatives or contacting directly the potential employers. However, Layard et al. (1991, Chapter V, pp. 238) report that one of the most widely used method of job search by the unemployed in the UK is search through job centres. About 80% of the unemployed used this method of search according to figures extracted from the Employment Gazete (Layard et al. 1991, Table 7, Chapter 5). In the DSS Cohort Study of the Unemployed 1987/88 several questions on the unemployed search activity were asked. It turned out that 90% of the male unemployed in the sample reported to search for job vacancies through employment agencies[6]. In this respect, the measure of search available is likely to cover to a reasonable extent the actual search activity by the unemployed. A drawback is that it covers also "going to an unemployment benefit office" which is just associated with the receipt of unemployment benefit and does not say anything about search activity.

I have constructed a dummy that takes value one if the unemployed replied that they were visiting job centres (and unemployment benefit offices) less than before. The dummy takes value zero if the answer to the

[6]The information on search activity by the unemployed contained in this survey is currently been analysed by Narendranathan and Stewart.

question was "no change" or "more than before". This dummy captures the change in the intensity of search activity —narrowly defined as visiting a job centre, if one disregards "visiting the benefit offices"— sometime before the commencement of the sampled spell of unemployment, rather than the current intensity of search. It does therefore cover also the search activity that the unemployed might have undertaken before actually becoming unemployed. The fact that somebody reports to be "visiting benefit offices less than before" might imply that they are not receiving any unemployment benefit. This effect should be captured by the dummy for the receipt of no benefit which was described above and should not affect the estimated coefficient on the search intensity dummy.

About 10% of the sample reports a decreasing search activity. The same figure is even smaller (about 4%) if the unemployed that exit into a job are considered, as shown in Table 5.1. This implicitely shows that the figure is higher for the right-censored (and perhaps for those unemployed that exit into states other than employment).

5.2.5 The individual valuation of leisure

The expected impact of higher individual leisure valuation on the (conditional) probability of exiting from unemployment is negative. This prediction was justified in Section 1.2. One has to be careful that the unemployed's valuation of leisure might change during the course of the unemployment spell as a consequence of the prolonged experience of unemployment. This would imply that leisure valuation is endogneous to the model. Therefore, I use information that relates to the observed commencement of the unemployment spell.

I proxy the valuation of leisure as relative to labour, using replies to the following question:

> "*If you were to get enough money to live as comfortably as you would like for the rest of your life, would you want to have a job or would you prefer not to work?*".

Here leisure is defined as the alternative use of time. There is of course no perfect or best way to find out how much the individuals value leisure, but I feel that questions of the type *"how much do you value leisure when unemployed as opposed to leisure when working"* would have performed much worse.

The variable *"leisure"* has been constructed, taking value one if the unemployed replied they would not work to the above question, zero otherwise. There might be different degrees of valuing leisure as relative to labour which are not allowed for by this measure. A small number of the unemployed, about 14% (see Table 5.1), replied positively to the question above. The proportion of the unemployed that "value leisure more than labour" is slightly lower, about 10%, among the unemployed that exited from unemployment

into a job. This proportion is therefore higher for the right-censored (and perhaps for the unemployed that exit into states other than employment).

5.2.6 The labour force participation of the spouse

The impact of the labour force participation of the spouse of the unemployed on the probability of leaving unemployment has been mostly overlooked, in both the theoretical and the applied literature on unemployment duration. One reason is the potential endogeneity of this variable. Another explanation is that "causality" has typically been assumed to run from husband to wife, and not viceversa.

According to the "additional worker effect", the impact of the wife's labour force participation on the husband's (conditional) probability of leaving unemployment should be negative. If the wife works, then some minimum subsistence level of income should be granted and there would be less pressure on the husband find himself a job. Previous studies for the US (Murphy and Topel, 1989) have found no evidence in favour of this hypothesis. For the UK, Wadsworth (1991) finds that the working decisions of the two partners are complementary rather than substitute.

The spouses' labour force participation might raise the husband's chances of leaving unemployment by means of creating more contacts with the labour market and generating additional information about job opportunities. In a traditional society, it is also possible that unemployed husbands of working wives might regard themselves as "diminished" in their patriarchal role and have a higher incentive to find a job than unemployed husbands of non-working wives. Another explanation is that the two partners are likely to have a similar background. Husbands of "working women" are more likely to be working as well or else to have shorter unemployment spells.

One problem with this variable is its potential endogeneity to the model since the two partners' work participation decision may not be independent from each other. In order to avoid this, I have considered the spouse's participation decision sometime before the partner's unemployment spell began. I have constructed a dummy that takes value one if the spouse reported to be in a full-time or part-time job one month before her partner started his unemployment spell[7].

Almost 90% of the unemployed in the sample were married (or cohabiting); about 40% of their spouses were found to work. The proportion of spouses that work is on average six percentage points higher among the unemployed that found a new job by the end of the observation period (see Table 5.1).

[7]Since the unemployment spell of the partner (or husband) is only observed from three months after it started onwards, the indicator chosen of the labour force participation of the spouse relates actually to four months before.

5.2.7 Demand side conditions

The conditions on the demand side of the labour market influence the job offer probability.

I employ as an indicator of demand side conditions the rate of unemployment in the local area. The most disaggregated geographical unit available for the LSUS data was the county(some 30 counties are distinguished). This measure is not the ideal but it might still be useful to capture demand side conditions. The best option would have been to use the unemployment rate in the "travel to work area", which was for instance used by Narendranathan et al. (1985). However, this information was not available. Other authors, such as for example Atkinson et al. (1984) used the regional rate if unemployment as an indicator of demand side conditions.

The county unemployment rate considered relate to October 1983, which corresponds more or less to the time of the first survey interview[8]. It is not allowed to vary over time since the relative position of the British counties did not change considerably over the course of the year falling between the two survey interviews.

Alternatively or in addition to this measure, one could have considered the job vacancy rate. However, it has been shown for instance by Layard et al. (1991) that there is no clear cut relationship between the duration of unemployment and the rate at which vacancies are formed. The authors find that even when the overall level of unemployment increases the rate at which vacancies are filled does not appear to change substantially. In my view, one possible explanation for this finding is that the statistics available on the vacancy rate are not an appropriate measure of the jobs available on the (local) labour market. Indeed, there is no compulsory registration of vacancies at state job centres in Britain while the only measured vacancies (at least on a national basis) are those at job centres.

5.3 The results of estimation of the model

The estimated model is given by Equation 3.36 of Chapter 3. A weekly baseline hazard rate is specified. Two destination states out of unemployment are considered: full-time work and other states. Alternatively the first destination includes in addition to observations that exited into full-time work also spells that ended into part-time work (of either greater or less than ten hours per week). Results of estimation are given in Table 5.2 below.

Model (1) of Table 5.2 is the same as the competing risks model of Chapter 4 (see Table 4.3) but it additionally includes those variables that proxy search activity, value of leisure and spouse participation in the labour force. A likelihood ratio test ($\chi^2_6 = 60.2$) indicates that the null hypothesis that

[8]The first interview took place sometime in the Autumn of 1983, as discussed in Chapter 1.

the coefficients on the additional variables of model (1) are not significantly different from zero can be rejected. A perhaps surprising result is that the estimated coefficient on benefit receipts is now not significantly different from zero. The robustness of this result will be investigated in the next Section 5.4 to which the reader is referred.

Specifications (1) and (2) differ in the definition of the unemployment benefit variable, which is allowed to vary over time in specification (2). The values of the maximised log-likelihoods for the two models, (1) and (2), are almost identical and the estimated parameters are very much similar in statistical significance, sign and absolute value. The coefficient on the time varying benefit variable (specification (2)) is however larger in absolute value and it shows a smaller standard error. Model (3) considers exit into either a full-time or a part-time job together. The estimated coefficients of model (3) are very close to those of model (1) and model (2). An explanation for this result is that not many people exited into part-time work. This is quite plausible since the sample covers only male unemployed. I refer in the discussion of the impact of the covariates to the results of estimation of model (2).

Age and family composition

The base for the age dummies are the unemployed aged between 25 and 34. The unemployed aged between 20 and 25 do not stand significantly different re-employment probabilities than those aged between 25 and 34, *ceteris paribus*. Older age has, instead, a significantly negative impact on the re-employment probability. The probability of leaving unemployment to take up a full-time job is, respectively, 17%, 45% and 72% lower for the unemployed whose age falls between 35 and 44, 45 and 54, 55 and 58. The chances of finding a full-time job are considerably lower for the older unemployed, all things equal. These findings are in line with those of previous studies. The corresponding estimated coefficients (and standard errors) in Narendranathan and Stewart (1993) for the age ranges 35-44, 45-54, 55-59 and for exit into full-time work are respectively $-0.17(0.15), -0.61(0.15), -0.92(0.19)$.

The estimated coefficient on marital status is not significantly different from zero[9]. The presence of children aged less than 5 years reduces by 18% the probability of re-employment in a full-time job. This result is perhaps surprising given that the sample includes only male unemployed. An explanation is that the presence of young children raises the value of the time spent at home and with it the opportunity cost of labour.

[9]This variable was instead estimated to affect significantly the re-employment probability in the simpler model of Table 4.3. This difference in results is probably due to having controlled for the labour force participation of the spouse in model (2).

Table 5.2: *Results of estimation of the re-employment probability*

Covariate label	*Full-time exit (1)*		*Full-time exit (2)*		*Any job exit (3)*	
	Coeff	*SE*	*Coeff*	*SE*	*Coeff*	*SE*
F/t work year before	*0.3963**	*0.1366*	*0.3961**	*0.1365*	*0.1732*	*0.1345*
Unemployed year before	*0.3443**	*0.1472*	*0.3439**	*0.1472*	*0.2353*	*0.1232*
Sick out of work year before	*-0.3459*	*0.2898*	*-0.3459*	*0.2898*	*-0.5622*	*0.2839*
Profess. /Interm. Occup.	*0.1976**	*0.0976*	*0.1970**	*0.0976*	*0.2501**	*0.0934*
Unskilled Occupation	*-0.4385**	*0.1720*	*-0.4395**	*0.1720*	*-0.4205**	*0.1653*
Age 20-24	*0.2101*	*0.1074*	*0.2098*	*0.1073*	*0.2159**	*0.1035*
Age 35-44	*-0.1885**	*0.0901*	*-0.1879**	*0.0900*	*-0.1843**	*0.0875*
Age 45-54	*-0.5979**	*0.1095*	*-0.5998**	*0.1096*	*-0.5577**	*0.1052*
Age 55-58	*-1.2661**	*0.1721*	*-1.2735**	*0.1728*	*-1.2212**	*0.1638*
Any child aged < 5	*-0.1954**	*0.0872*	*-0.1950**	*0.0869*	*-0.1853**	*0.0843*
Married	*0.1356*	*0.1266*	*0.1370*	*0.1231*	*0.1132*	*0.1174*
Spouse working month before	*0.3759**	*0.0894*	*0.3742**	*0.0866*	*0.3735**	*0.0834*
Searches less than before	*-0.7793**	*0.1724*	*-0.7824**	*0.1723*	*-0.8507**	*0.1681*
Values Leisure more	*-0.2747**	*0.1144*	*-0.2769**	*0.1145*	*-0.2311**	*0.1081*
Experiences money shortage	*0.1983**	*0.0858*	*0.2006**	*0.0859*	*0.1894**	*0.0823*
House owner	*0.2991**	*0.0739*	*0.2986**	*0.0730*	*0.3133**	*0.0706*
County unemployment rate	*-0.0185*	*0.0106*	*-0.0184*	*0.0106*	*-0.0219**	*0.0101*
Receives only UB at t1	*0.2046**	*0.0861*	*0.2008**	*0.0844*	*0.1967**	*0.0815*
Receives no UB nor SB	*0.0469*	*0.3810*	*0.0367*	*0.2772*	*0.0389*	*0.2529*
Benefit amount (£) logs.	*-0.0295*	*0.0922*				
Benefit amount time varying (£) logs.			*-0.0328*	*0.0596*	*-0.0713*	*0.0549*
Predicted earnings (£) logs.	*0.6075 **	*0.2004*	*0.6049**	*0.2005*	*0.4477**	*0.1933*
No predicted earnings	*2.7294**	*0.9524*	*2.7183**	*0.9527*	*2.0170**	*0.9186*
	Max. lik. :-6420.9		*Max. lik. : -6420.8*		*Max. log-lik. :-6330.5.*	

*The values of the maximum log-likelihoods are those of the competing risks models, estimated simultaneously. The other exits estimates are not shown. The any job exit includes full-time and part-time work. A * indicates statistical significance at the two sided 5% level. Descriptive statistics of the explanatory variables are given in the preceding data section.*

Previous work experience

Having been in full-time work for most of the year before the commencement of the unemployment spell raises the chances of leaving unemployment to enter full-time work by about 50%. This result is in line with theoretical expectations. It is perhaps surprising that having been unemployed for most of the year before the commencement of the new unemployment spell raises also the hazard rate by about 50%. An explanation is provided by the phenomenon of repeated or recurrent unemployment; i. e. the fact that some individuals tend to go in and out of unemployment (employment). This phenomenon is likely to interest the "marginal" or less qualified workers for whom it is more difficult to find a permanent job. It is unlikely that some unemployed are abusing the unemployment benefit system by working just long enough to gain new entitlement to unemployment benefit since voluntary job quits lead to 6 months suspension of benefit. It is instead probable that low qualified workers tend to find temporary jobs; therefore, experience unemployment repeatedly.

The estimated effect of having been sick and out of work for most of the previous year is not significantly different from zero. However, it shows a negative sign, as one would expect.

Those unemployed qualified as professional or intermediate workers have significantly higher chances (by 22%) than the others to be re-employed in a full-time job. The unskilled are instead 35% less likely to be re-employed in a full-time job; i. e. the duration of their unemployment spell is on average 35% longer, *ceteris paribus*.

Income variables

The impact of the level of unemployment benefit is not significantly different from zero. However, it shows negative sign, as expected. The robustness of this result is tested for in the next Section 5.4. The coefficient on the dummy for the receipt of "only UB" at the first interview is significantly different from zero. It signals that the re-employment probability increases by 22% for recipients of "only UB" once other things have been controlled for. The coefficient on the dummy for receipt of no unemployment benefit is not statistically significant. An explanation for this result is that the situation of those unemployed that do not report any benefit is probably very heterogeneous.

The estimated elasticity of the hazard rate to changes in expected future earnings is equal to 0.60. Average earnings per week are about £85. Therefore, a 10% increase in mean expected earnings would lead to a reduction in mean expected unemployment duration of 15.5 weeks. Given the insignificance of the benefit variable, I do not test for the replacement ratio restriction; i. e. for the equality in absolute value of the coefficients on benefit receipts and expected earnings.

Those unemployed for whom expected earnings were not available do not show significantly higher chances to leave unemployment to take up full-time employment[10].

The impact of experiencing a shortage of money is significantly different from zero and positive, as one might expect. Being financially constrained raises the re-employment probability by about 22%. It is plausible that the unemployed that feel financially constrained will tend to be less choosy about jobs offers and consider as "acceptable" or "desirable" jobs that they would not normally consider as such. However, one might question further who are the unemployed that do not feel financially constrained. More evidence on this issue is provided in Chapter 7.

The unemployed that own the house were they live, either outright or with a mortgage, appear to stand 34% higher chances than the others to live unemployment to take up a full-time job. The corresponding estimate in Narendranathan and Stewart (1993) is very close and equal to $0.20(SE0.09)$. House ownership might capture individual characteristics ranging from financial wealth and access to credit, to capacity of organization and of planning forward (see Section 5.2).

The intensity of search and the value of leisure

The coefficient on the proxy for diminished search activity indicates that the unemployed that visit job centres (and unemployment benefit offices) less than they used to before the commencement of their unemployment spell, see their chances of leaving unemployment to take up a full-time job halved, *ceteris paribus*. Valuing leisure more than labour, were lifetime budget constraints not binding, reduces the probebility of re-employment by some 24%.

The estimated coefficient on an additional interaction term of diminished search activity and the level of unemployment benefit is statistically not significantly different from zero and positive, but its inclusion renders the estimated coefficient on diminished search larger $(-0.9, SE0.44)$. A likelihood ratio test cannot reject the null hypothesis that the coefficient on this interaction term is not significantly different from zero.

The spouse's labour force participation

The fact that the spouse was working full-time or part-time one month before the commencement of the partner's unemployment spell raises by about 50% the partner's chances of leaving unemployment to enter a full-time job. This

[10]The base for the dummy "predicted earnings not available" are the unemployed for whom "predicted" earnings were available. The level of expected earnings is zero for the unemployed for whom expected earnings were not available. The mean value of (log) expected earnings is 4.5. So these people because they have no expected earnings "lack" $4.5 * 0.6(\hat{b}) = 2.7$, which is the coefficient on "no predicted earnings". This implies that instead of including a dummy for these people I could have given them the mean expected earnings. The overall message is that this group is no different from the other.

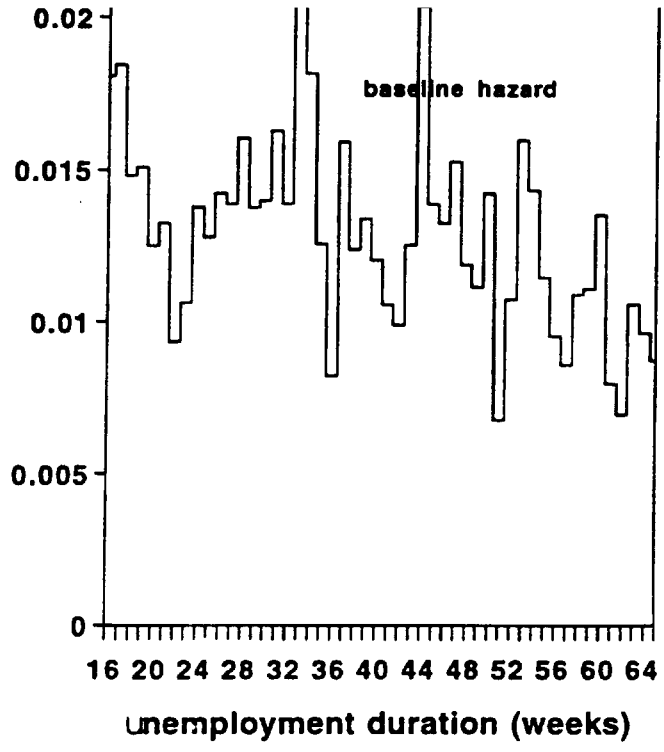

Figure 5.1: *Piecewise exponential baseline of model (2)*

result confirms perhaps the view that a "working spouse" represents more contacts with the labour market. The reader is referred to Section 5.4 for further discussion of this result.

Demand side conditions

The estimated coefficient of living in a county with a higher unemployment rate is negative and significantly different from zero, as predicted by economic theory. A one percentage point increase in the average county rate of unemployment reduces the hazard of leaving unemployment by 3%, all things equal. The size of this impact might be considered quite significant, at least relative to previous UK studies. For instance, Narendranathan and Stewart (1993) found a non-significant impact of the unemployment rate in the travel to work area (which is the geographical area they consider). However, the data employed by these authors relate to times (1978/79) when the unemployment rate was generally much lower than in the period of time covered by the LSUS survey (1983/84).

The estimated baseline hazard rate

The estimated baseline hazard rate of model (2) is plotted in Figure 5.1. The estimated coefficients on the weekly baseline constants (for the exit into full-time work) are all strongly significantly different from zero except for the first

week 11 when nobody exited from unemployment. The behaviour over time of the baseline hazard rate is quite smooth. Some spikes are detected at week 33, 37, 43 and 51–53, with a hump at weeks 50–54. The significance of the spikes found in the baseline hazard rate and the sign of the dependency on time were discussed in Sections 4.4.2 and 4.6, to which the reader is referred.

5.4 More on the impact of the unemployment benefit

The finding of a non-significant benefit level effect contrasts with previous UK studies; such as Narendranathan et al. (1985) and Narendranathan and Stewart (1993, 1993a)[11]. It seems therefore interesting to test for the robustness of this result.

As briefly reviewed in Section 1.2, there was a considerable debate on the statistical significance of the influence of the unemployment benefit level on the rate of escape from unemployment in the late seventies in Britain. Nickell (1979) and Lancaster and Nickell (1980) found a significant benefit effect. Atkinson et al. argued that previous findings of a significant unemployment benefit effect were not robust. Narendranathan et al. (1985) (using the DSS Cohort Study of the Unemployed of 1979/80) proved instead the robustness of the benefit effect. Narendranathan and Stewart (1993, 1993a) —who also employed the 1978/79 DSS Cohort Study— found also a significant effect of unemployment income on the hazard rate. The authors looked at the effect of unemployment income — defined as the sum of unemployment benefit, spouse earnings and any other income accruing to either partner— rather than at the impact of the unemployment benefit level by itself. The unemployment income elasticity was found to be -0.68 in the first three months of unemployment and -0.19 in the successive three months, in a single risk framework. The corresponding competing risks estimates, for exit into full-time work were respectively -0.78 and -0.23. However, the benefit effect was found to became not significant after about six months of unemployment (Narendranathan et al. ,1985, Narendranathan and Stewart, 1993, 1993a).

For the US, Katz and Meyer (1988) found a significant large and positive impact of the benefit level on the probability of recall to the previous job (1.64); and a significant large and negative impact on the new job finding probability (-1.11). These estimates are larger if unobserved heterogeneity is allowed for. The impact of the benefit level in the new job finding probability became not significantly different from zero after benefit exhaustion. They did instead find a not significant (small and positive) benefit impact, in a single risk model. The benefit level is generally found not to have a significant impact in the Netherlands; for instance, Lindeboom and Theeuwes (1993).

The finding of a non significant benefit impact, obtained with the LSUS

[11] However, the benefit impact was found not significant also in Atkinson et al. (1984).

data, can be explained if one considers the following points:

- the LSUS sample consists of persons that were unemployed for longer than three months;
- in the time period covered by the LSUS survey the unemployment rate was much higher than in 1978/79;
- when the LSUS survey was carried out the Earnings Related Supplement which used to link the level of the national insurance Unemployment Benefit (UB) to previous earnings had just been abolished[12].

For these reasons, one would expect to find a smaller benefit effect using the LSUS data rather than the 1978/79 Cohort Study[13].

The robustness of the estimated coefficient on the benefit level to the inclusion/exclusion of groups of variables is tested for in Table 5.3. Also the following types of sensitivity tests are carried out: the total income variables are specified rather than the benefit level and the predicted earnings; the model is estimated only for the recipients of unemployment benefit (at the first interview); an interaction variable for the combined effect of the level of benefit receipt and the type of benefit received is added to the model.

The estimated coefficient on the unemployment benefit level[14] becomes significant only if the dummy for the spouse's labour force participation is dropped from the model. It becomes slightly more significant if also the dummy for the presence of any child aged less than five in the nuclear family is dropped from the model. The estimated coefficient is now half the size of that estimated by Narendranathan and Stewart (1993) for the second quarter of the unemployment spell, which was equal to "0.23".

[12]This implies that the level of benefit receipts does not vary considerable across the unemployed and therefore it is more difficult to get a precise estimate of the benefit level effect.

[13]However, the unemployment benefit level may not be measured with accuracy in the LSUS survey since it is self-reported and limited information is available on its variation over time. The DSS Cohort Studies do, instead, contain administrative information on unemployment benefit receipts and on their pattern of variation over time. No particularly firm conclusions about the impact of the level of unemployment benefit on the hazard rate should therefore be drawn on the basis of the LSUS data.

[14]The reader should perhaps be reminded that the level of unemployment benefit is measured in £and that the variable is entered in logarithms. The estimated coefficient represents an elasticity.

Table 5.3: *The robustness of the benefit coefficient in the full-time exit*

Sensitivity analysis	*Benefit Coeff.*	*SE*	*Max log-lik.*	*lik. ratio test*
Preferred model of table 5.2	*-0.0328*	*0.0596*	*-6420.8*	
Variables dropped from the model:				
Spouse's work activity one month before husbands'unemployment	*-0.0918**	*0.0558*	*-6431.8*	$\chi^2_2 = 22$
Spouse works and cny child less than 5 years old	*-0.1202**	*0.0545*	*-6439.7*	$\chi^2_4 = 37.8$
Diminished Search and Leisure	*-0.0365*	*0.0926*	*-6441.4*	$\chi^2_4 = 41.2$
Diminished Search, Leisure, Money shortage, HouseOwn	*0.0112*	*0.0604*	*-6453.3*	$\chi^2_8 = 65$
All previous work history variables: Activity most of the year before U. and qualifications	*-0.0232*	*0.0601*	*-6447.3*	$\chi^2_{10} = 53$
only UB recipients dummy	*-0.0730*	*0.0554*	*-6426.0*	$\chi^2_2 = 10.4$
Variables added:				
Interaction of benefit level and dummy for receipt of only UB	*0.0805*	*0.1052*	*-6419.2*	$\chi^2_2 = 3.2$
Other Unemployment Income (-0.0386, SE 0.0835), entered in logs (£), in addition to the benefit amount and the expected income variables	*-0.0286*	*0.0605*	*6420.6*	
Other Unemployment Income (-0.0433, SE 0.0903) and Other Expected Income from work (0.0282, SE 0.2065), entered in logs (£) in addition to the benefit amount and the expected income variables	*-0.0277*	*0.0609*	*6419.5*	
Benefit amounts and expected earnings (0.0061, SE 0.0019) entered in levels (£)	*-0.0038*	*0.0025*	*-6417.6*	
Total Unemployment Income (-0.0105, SE 0.0631) and Total Expected Income form Work (0.2008), entered (in logs, £) instead of the benefit amount and the expected earnings variables.			*6427.9*	
Model estimated only for benefit recipients	*-0.0389*	*0.0929*	*-6089.0*	

*The unemployment benefit variable considered is the time varying unemployment benefit. The level of unemployment benefit is measured in £and it is entered in logarithms. The estimated coefficient represents an elasticity. The estimated model is a competing risks model and the results shown relate to the full-time work escape rate. A likelihood ratio test of the statistical significance of imposing the restrictions is provided in the last column. The test statistics show degrees of freedom double than the number of variables dropped, since the two exits are estimated simultaneously. All variables except for the ones dropped in turns are the same as in the model of table 5.2. Descriptive statistics of the explanatory variables are given in table 5.1. A * indicates statistical significance at the 5% one sided level. The figures in brackets in the first column give the value of the etimated coefficients.*

It is difficult to think of a sensible explanation for this result. It is perhaps the case that the flat rate nature of the unemployment benefit —both UB and SB are flat rate— prevents one from the detection of the pure effect of the benefit level. When the benefit is found to be significant one is actually simply estimating the work disincentive effects of the additions for dependent spouse and children[15]. Once these factors have been controlled for the estimated benefit parameter is not significant any longer. However, the spouse's participation in the labour force relates to four months before the first survey interview (see Section 5.2) and the dependent children additions are payable for dependent children of any age up to sixteen, so that only part of these additions (if any) is captured by the dummy for the presence of any child aged less than five in the household.

However, the fact that the estimated standard errors on the benefit level coefficient do not vary much if the variable "spouse work" is included in the model supports the view that this variable proxies contacts with the labour market. If the variable "spouse work" removed all identifying variance in the benefit level, the standard error on the benefit coefficient should be much larger when this variable is included than when it is excluded.

None of the other specifications affect the significance —or better, the insignificance— of the impact of the benefit level. In particular, the coefficients on other unemployment income is found not significant. However, it shows the expected negative sign.

5.5 The predicted mean unemployment duration

I have computed the predicted mean duration[16] of unemployment for given values of the explanatory variables. Following Katz and Meyer (1990), the predicted survivor function at a given week t is the predicted probability of a spell lasting until t, which can be written as:

$$\hat{G}_i(t) = G_i(t|\hat{\psi}(\tau); x_i(\tau)'\hat{\beta}; \tau = 0, \cdots, t-1) = exp\{-\int_0^t \hat{\theta}_i(u)du\}, \quad (5.1)$$

where the hat indicates estimated quantities. The aggregate survivor function for the sample is given by:

$$\bar{G}(t) = \frac{1}{N}\sum_{i=1}^{N} \hat{G}_i(t), \quad (5.2)$$

[15]Given the flat rate nature of both UB and SB most of the variation in the amounts of unemployment benefit is due to the additions for dependent spouse and children. The dummy for the presence of young children in the household may capture the disincentive effect of the unemployment benefit additions for dependent children. The dummy for the spouse's labour force participation might capture the incentive effect of not receiving any benefit addition for dependent spouse. The incentive and disincentive effects are defined with respect to the unemployed's decision to go back to work.

[16]The statistical formula for the expected mean duration was defined in Section 3:2.

where N equals the number of observations. The predicted mean weeks of unemployment are set equal to the predicted weeks of unemployment accumulated by week t; i. e. :

$$M(t) = \sum_{\tau=1}^{t} \bar{G}(\tau). \tag{5.3}$$

Table 5.4: *Predicted mean unemployment duration, before exiting into full-time work*

Set of values of the explanatory variables	*mean dur* $t = 71$	*mean dur* $t = 62$
(a) All explanatory variables are set equal to zero (except for the weekly baseline constants)	*57.8*	*50.09*
(b) All continuous variables take their mean value (unemployment rate, log benefit level and log predicted earnings). The dichotomous variables full-time work in the year before and married take unitary value	*41.5*	*37.2*
(c) All continuous variables take their mean value. The dichotomous variables full-time work year before, married, spouse working, searching less than before, valuing leisure more than labour and receiving only UB take value one	*47.5*	*41.7*
(d) All continuous variable take their mean value, marital status equals one	*46.5*	*41*
(e) All continuous variable take their mean value. Marital status, full-time work year before, spouse working one month before take value one	*35.7*	*32.6*
(f) All continuous variable take their mean value. Marital status, full-time work year before, last occupation unskilled take value one	*47.0*	*41.4*
The observed mean duration for exit into full-time work, including the right-censored, is of 47.3 weeks. The estimated coefficients on the explanatory variables used for the simulations are those of model (2) of table 5.2. The baseline estimates of model (2) are plotted in Figure 5.1.		

The results are shown in the Table 5.4 and relate to exit into full-time work. The final time t was set equal to 71 weeks, which is the maximum duration observed, and, alternatively, to 62 weeks, given that all right-censoring takes place from week 63 onwards and that very few observations exit from unemployment after week 63 (see Table 2.2). Unless otherwise indicated, the dichotomous variables take value zero, which corresponds to the base group.

The observed mean unemployment duration for the unemployed that exited into full-time work is equal to 33.6 weeks; observed mean unemployment duration for the sample amounts to 46.9 weeks (see Table 5.1). These figures exclude the right-censored. The appropriate figure for comparison purposes is here the mean unemployment duration for exit into full-time work taking

into account also right-censoring; i. e. 47.3 weeks. The observations exiting to states other than full-time work are treated as if right-censored.

I find that the predicted mean unemployment duration under the assumptions made does not differ much from the observed sample mean. This result is not surprising given that in all specifications but (a) the continuous variables have been set equal to their average value and most of the dummy variables have been set equal to zero, which corresponds to the base group[17].

An unemployed person that receives unemployment benefit equal to the sample average, has expected earnings equal to the sample average, was in full-time work most of the the year before the commencement of the unemployment spell and is married (case b), has a predicted mean unemployment duration equal to about 41 weeks. For an unskilled worker in the same situation (case f) the mean unemployment duration is longer and equal to 47 weeks. For a skilled or semi-skilled worker — which is the base for the occupation dummies— in the same situation but whose spouse was working sometime before the commencement of the sampled unemployment spell (case e), the predicted mean unemployment duration is lower and equal to about 38 weeks.

5.6 Summary and conclusions

In this chapter, I have estimated a competing risks model of the re-employment probability. It is possible to conclude that the variables considered explain reasonably the determinants of the individual probability of leaving unemployment to take up a full-time job. Most variables were found out to affect the hazard rate significantly and in the expected direction. I summarize below the results that I consider most interesting.

A perhaps surprising result is that the level of unemployment benefit does not affect significantly the full-time work hazard rate. This result was shown to be reasonably robust. Only if the dummies for the spouse's participation in the labour force and the presence in the household of any child aged less than five years were dropped from the model would the impact of the unemployment benefit level become statistically significant —and even then it is still small. There is no obvious explanation for this result. The insignificance of the benefit impact might be due to the limited information on the pattern of benefit receipts available in the LSUS data. However, previous UK studies that found a significant benefit level effect (with administrative data on benefit receipts) relate to the late seventies when the unemployment rate was much lower and there was more variation in the level of benefit receipts.

I find that diminished search intensity reduces the re-employment prob-

[17]The base group for any of the dummies considered has been defined in Section 5.2. It normally corresponds to the most numerous group. One exception is the dummy for the economic activity undertaken for most of the year before the commencement of the sampled unemployment spell.

ability by 50%. Valuing leisure more than labour reduces the (conditional) probability of leaving unemployment to enter full-time work by about 24%. If the spouse worked one month before the partner started his unemployment spell, the re-employment probability increases by about 50%. My preferred interpretation for this result is that a "working spouse" represents tighter contacts with the labour market, which result in a higher probability of finding a job.

The presence of a child aged less than five years in the household reduces by 1/5 the probability of finding a full-time job. This result is rather surprising since the sample consists only of male unemployed. Interestingly, this impact is as large as that of leisure preferences. Perhaps, valuing staying at home with young children instead of working is one way of expressing preferences for leisure over labour.

I conclude that considering part-time work together with full-time work does not affect substantially the results of estimation of the re-employment probability. This is probably due to the small number of observations exiting into part-time work.

Chapter 6

Unemployment duration and the duration of entitlement to unemployment benefit

6.1 Introduction

In this chapter, I investigate the relationship between the duration of entitlement to unemployment benefit and the individual probability of leaving unemployment for Britain. The 1993 UK Budget announced that the duration of entitlement to the national insurance unemployment benefit (UB) will be reduced to six months from 1995. No attempt has been made until the present study to estimate the impact of the potential duration of entitlement to unemployment benefit on the hazard rate for the UK, at least to my knowledge. Some evidence on whether the impact of the benefit level varies as the unemployment spell lengthens is gathered by Narendranathan and Stewart (1993a).

It has been pointed out that the rules governing the duration of entitlement to unemployment benefit might contribute to explain differences in unemployment rates across western economies more than differences in the level of unemployment benefit (Katz and Meyer, 1990). Cross-countries comparisons based on aggregate time-series data (on the unemployment rate and on the level and the duration of the unemployment benefit) have confirmed that the duration of entitlement to unemployment benefit might influence considerably the rate of unemployment and the expected duration of unemployment in a number of countries among which also the U. K. (Jackman et al., 1991, Burda, 1988). The main drawback of such studies is that it is hard to construct an average aggregate measure of the level and the duration of the unemployment benefit since normally benefit payments vary considerably across individuals.

The institutional features of the unemployment benefit scheme in the UK were described in Section 1.3 of Chapter 1. In the UK (as in the US,

the Netherlands and many other countries), the national insurance benefit has a limited duration while the social assistance benefit (means-tested) has potentially infinite duration (as long as the unemployed show that they are actively searching for work). Since the UK unemployed may receive both benefits at the same time, it is not straightforward to think of the potential or expected duration of entitlement to unemployment benefit. In what follows, I distinguish two groups of the unemployed with respect to the expected duration of entitlement to unemployment benefit: the recipients of "only UB" and the recipients of "SB with or without UB". I make an attempt to obtain estimates of the impact of the expected exhaustion of the national insurance benefit (UB) on the hazard rate. Two approaches to detecting the benefit entitlement effect with the LSUS data are adopted. First, the preferred model is estimated for the two groups of the unemployed pooled together. Some timevarying dummies are used to model the residual entitlement period to unemployment benefit for "only UB" recipients. Second, the model is estimated separately for the two groups of unemployment benefit recipients. The two estimated hazard rates are compared.

I investigate also whether the expected duration of entitlement to unemployment benefit has a different impact on the re-employment hazard than on the hazard of exit to states other than full-time work. The implications for economic policy are indeed quite different. Findings of a negative influence of longer durations of entitlement to unemployment benefit on the re-employment probability would favour the introduction of economic policies aimed at reducing the duration of entitlement to unemployment benefit. If, instead, the exhaustion of entitlement to the benefit leads to withdrawal from the labour force, the same policy would be ineffective in increasing the rate of exit from unemployment into employment, which I assume should be the aim of economic policy (together with granting a minimum level of subsistence income to the unemployed).

The structure of this chapter is the following. In section 2, the background theory and the existing evidence are reviewed. The impact of the expected exhaustion of entitlement to unemployment benefit on the hazard rate is discussed for the case of the UK. In section 3, the problems of detecting an entitlement effect with microdata are illustrated. First, typical problems that have arisen in the literature are reviewed and related to the LSUS data. Next, the drawbacks that are peculiar to the LSUS data are discussed. In section 4, the main features of the data for the purpose of the analysis of the duration of benefit entitlement are described. The results of estimation are discussed in Section 5. Section 6 concludes.

6.2 Theory and existing evidence

Theoretical models predict[1] that as the time of exhaustion of entitlement to unemployment benefit nears the probability of leaving unemployment increases, all things equal. Moffit and Nicholson (1982), who adopt a conventional labor-leisure framework of analysis, argued that the unemployed's budget constraint has a kink at the time of exhaustion of unemployment benefit and that most unemployed maximize their utility at this kink. Within a job search framework of analysis, Mortensen (1977) proved that allowing for the limited duration of unemployment benefit introduces a non-stationarity into the model. The author argued that as the time of exhaustion of entitlement to unemployment benefit nears, the unemployed increase their search intensity and lower their reservation wage. This results in a rise in the hazard rate. Van den Berg (1990) developed a formal model of non-stationarities in job search models, proving that an expected decline in the level of benefit paid raises the unemployment benefit elasticity of the hazard rate. The expected exhaustion of unemployment benefit is comparable to an expected decline in the level of unemployment benefit.

Some empirical studies have found evidence in favour of the hypothesis that the hazard rate rises near the time of unemployment benefit exhaustion; for example, for the US, Katz (1986), Katz and Meyer (1988, 1990), Meyer (1990), Han and Hausman (1990); for Canada, Ham and Rea (1987); for the Netherlands, van den Berg (1990) and Lindeboom and Theeuwes (1993).

Previous UK studies have allowed the impact of the level of unemployment benefit on the hazard rate to vary over time with the lengthened duration of the unemployment spell (Narendranathan et al. 1985, Narendranathan and Stewart, 1993 ad 1993a). These studies have detected a declining impact of the level of unemployment benefit on the hazard rate as the spell of unemployment progresses. In particular, the effect of the level of unemployment benefit is found to become statistically insignificant after the first five or six months of unemployment. However, this evidence is inconclusive with respect to the impact of the potential duration of entitlement to unemployment benefit since the UK unemployment benefit schemes are such that the level of unemployment benefit paid does not necessarily decline during the course of the unemployment spell (see for instance Table 2.12). Some limited evidence on the effect of benefit entitlement on unemployment duration was gathered by Wadsworth (1991a), who used data matched from two years of the British Labour Force Survey (1983/84) and found that non-claimants of unemployment benefit had a lower unemployment duration; due however to a higher withdrawal rate from the labour force. Also, Schimdt and Wadsworth (1993), who employed matched data from the Labour Force Survey for the period 1983–89 to investigate the impact of entitlement to un-

[1]Some insights into the issue of the duration of entitlement to unemployment benefit and a brief review of the literature were provided in Section 1.3 of Chapter 1.

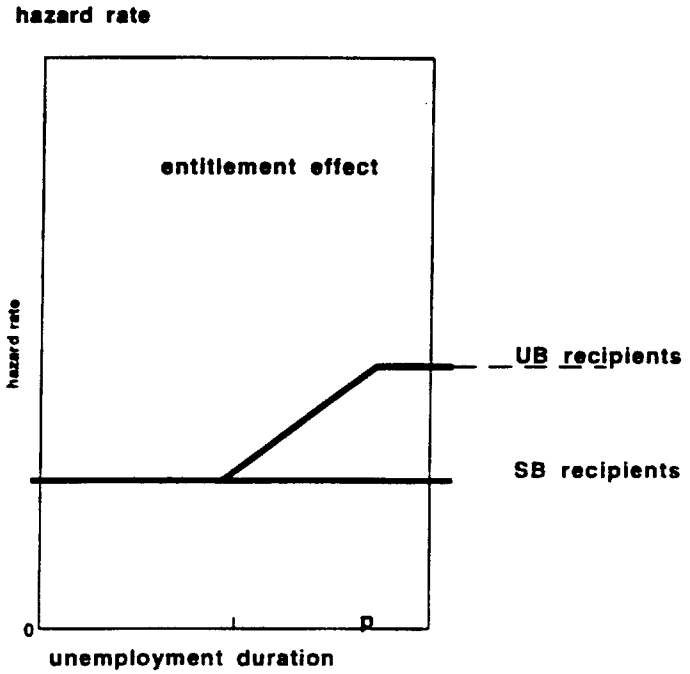

Figure 6.1: *The duration of the entitlement effect*

employment benefit on individual search intensity, concluded that excluding workers from the benefit system leads them to search less extensively.

An important feature of the UK benefit scheme is that the two benefits, UB and SB, can be received simultaneously, if the unemployed's resources including UB fall below their needs (as mentioned in Section1.3). In this case, the unemployed will receive both benefits at the same time. Having successfully passed the means-tested requirements for the award of the social assistance benefit (SB) when they were already receiving the national insurance benefit (UB) (for instance, at the commencement of their unemployment spell), should make the unemployed quite confident that upon exhaustion of entitlement to UB the foregone UB payment will be replaced by a corresponding SB payment. Therefore, I assume that the unemployed receiving both types of benefits at the commencement of their unemployment spell behave as SB recipients rather than as UB recipients with respect to their expectations about the potential duration of the unemployment benefit.

The predicted impact of the expected exhaustion of entitlement to benefit on the hazard rate in the case of Britain is summarized in Figure 6.1. Two groups of the unemployed with respect to unemployment benefit receipts at

the commencement of their unemployment spell are distinguished: recipients of (only) UB and recipients of SB (either by itself or in addition to UB). The type of unemployment benefit received is assumed not to vary over time, at least until time "p" when the national insurance unemployment benefit UB expires. The theory is inconclusive about the time at which the hazard rate starts to rise due to the expectation that the benefit will expire in the near future. I assume that for recipients of "only UB" this time is equal to about ten weeks before the exhaustion of entitlement to UB. A similar assumption was made by Katz and Meyer (1988) for a sample of US unemployed. The hazard rate for the recipients of SB (with or without UB) does not vary as a function of the duration of entitlement to benefit since SB is unlimited in time.

After the exhaustion of UB (time p) the hazard rate might remain constant or decrease. However, if the (once) "only UB" unemployed begin to receive SB upon UB exhaustion, from time p onwards their behaviour is likely to be similar to that of the SB unemployed. The hazard rate might sharply decrease and then remain constant.

6.3 The problems with the detection of the entitlement effect

Nothwithstanding the considerable evidence in favour of the hypothesis that the hazard rate rises significantly as the time of benefit exhaustion nears, some authors are sceptical about the possibility of detecting a true impact of unemployment benefit exhaustion on the individual re-employment probability (for instance; Fallick, 1991). It is fair to say that there are problems which may impair the detection of the "true" or "pure" impact of the expected exhaustion of benefit entitlement on the hazard rate.

The first and most serious problem concerns the identification of the effect of the duration of entitlement to unemployment benefit from that of time dependency in the hazard. The duration of entitlement to unemployment benefit may be fixed at the same period of time for everybody in the sample. This is normally the case in the US and in the UK. However, in the UK the duration of entitlement to benefit is different for recipients of "only UB[2]" and recipients of "SB" (either on its own or together with UB). This difference should hopefully allow to identify the effect of the (expected) limited duration of UB. However, even if a flexible baseline hazard rate is modelled it might be difficult to identify separately the effect of the benefit entitlement duration

[2]The duration of entitlement may vary for the recipients of UB because of the so called "linked spell rule", which links together for entitlement duration purposes spells of unemployment separated by less than a fixed number of weeks of work. This possible source of variation was allowed for in the construction of the entitlement duration variable (see Section 2.5.1 and Table 2.10). It was however found that the maximum potential duration of UB is 52 weeks for more than 90% of UB recipients.

from any time dependency effect.

A second problem arises from the fact that normally the duration of entitlement coincides with periods such as six months (for instance, in the U. S.) or the year (in the U. K.). There is some evidence that the unemployed tend to misreport their unemployment durations by rounding up their responses to periods of time such as six months or a year. Evidence in favour of such rounding errors in survey responses is gathered by Poterba and Summers (1984) and by Sider (1985). Sider (1985) finds for the US that unemployment durations are typically rounded by the survey participants to durations of a month, a quarter, half a year, a year. Therefore, in the case of self-reported unemployment durations, the detection of large increases in the re-employment probability near the time of benefit exhaustion may perhaps be explained by rounding errors made by the unemployed in reporting their unemployment durations rather than by an entitlement exhaustion effect. This type of argument is used, for instance, by Fallick (1991), who analyses a sample of US displaced workers and concludes that there is no evidence that any spike in the hazard rate can be explained by the exhaustion of entitlement to unemployment benefit. Administrative data on the duration of unemployment spells, such as those used for instance be Katz and Meyer (1988) —who found a significant entitlement duration effect— are however free from such rounding error problems.

In the case of the LSUS data rounding errors of this type are unlikely to be a problem. at the second interview, the unemployed are asked retrospective questions about what they were doing in each week during the period of time falling between the first and the second interview (I have constructed the duration of the unemployment spells using such information, as described in Section 2.4.2). In this case, there should be less scope for rounding error than when questions such as *"for how long were you unemployed?"* are asked. It is instead not possible to exclude that replies to such questions on the economic activity week-by-week might be affected by recall error. However, recall errors should not constitute a major problem for the identification of the benefit entitlement duration effect since they are probably random and they are unlikely to be clustered at a year (which is the time of exhaustion of UB). On the other hand, given the small size of the sample considered, the fact that there is not much variation in the length of the entitlement period might raise the chances of detecting a spike in the hazard rate near the time of benefit exhaustion by simply plotting the empirical hazard rate.

One further problem that might create a "spurious" entitlement duration effect is that some unemployed may stop claiming unemployment benefit a few weeks before their entitlement actually expires. For instance, in some US states there is a limit on the total amount of unemployment benefit payable in a year (which depends on the individual's last earnings). If this limit is binding the unemployed might receive a very small payment in the last week of entitlement to benefit and not bother to collect it. Meyer (1990, footnote 22) finds evidence in favour of this hypothesis. He concludes, however, that

only a small part of the increase in the hazard rate just before benefit exhaustion can be explained by this phenomenon. In the UK, UB is flat rate and there is no yearly ceiling on the amounts paid, so that this problem does not arise.

6.4 The data

The reader is referred to Chapter 2 for a description of the main features of the data and to Chapter 5 (Section 5.2) for a definition of the explanatory variables of the model. In this Section, I review some of the results of the (descriptive) analysis of the type and amount of benefit that were presented in Chapter 2. Then, I provide descriptive statistics of the explanatory variables for the two groups of benefit recipients (see Table 6.1) and comment upon the main differences. A description of the time varying dummies employed to capture the impact of the expected exhaustion of entitlement to benefit concludes this section.

6.4.1 Descriptive statistics of benefit receipts

Out of the 2035 observations selected for the econometric analysis, 736 (36.2%) unemployed reported to receive (only) UB at the commencement of their registered unemployment spell —approximated by the time of the first interview— and 1205 (59.2%) reported to receive SB, either by itself or together with UB (see Table 2.8). Those unemployed that reported that they did not receive any unemployment benefit at the first interview (about 5%)[3] are dropped from the econometric analysis since it is not known what type of benefit they might have received thereafter.

About 95% of the unemployed that were receiving SB, either by itself or in addition to UB, at the first interview, were still receiving SB at the second interview (see Table 2.9)[4]. This implies that the unemployed that receive SB at the commencement of their unemployment spell are likely to continue to receive SB throughout their unemployment spell. In particular, the unemployed that receive SB together with UB at the commencement of their unemployment spell are likely to continue to receive SB after exhausting their entitlement to UB. The rationale for these results is that the

[3]These unemployed are perhaps awaiting unemployment benefit receipts or temporarily suspended from benefit receipt or misreporting their benefit receipts. They might also be unemployed that have already exhausted their entitlement to UB because of the link spell rule.

[4]The unemployment benefit received at the second interview is recorded only for the unemployed that have not yet left unemployment (right-censored spells). One should keep this in mind since the type of unemployment benefit received over time might affect the probability of leaving unemployment, i. e. the probability of being still unemployed (right-censored) at the second interview.

unemployed that pass the means-test at the commencement of their unemployment spell are likely to continue to pass it thereafter. This implies that upon UB exhaustion a similar amount of SB is probably to be received. The type of benefit received varies instead considerably over time for the unemployed that receive only UB at the commencement of their unemployment spell. These findings support at least in part the assumptions I have made concerning the impact of the potential duration of unemployment benefit. In particular, about 40% of the "only UB" unemployed (with right-censored spells) report to receive no benefit at all at the second interview (see Table 2.9).

However, in Table 2.12 it was shown that quite a considerable number (more than 30%) of the "only UB" unemployed with right-censored spells report to receive (at the second interview) amounts of unemployment benefit much larger than those reported at the first interview. This is probably due to that fact that their financial situation (or their family situation) is changed in a substantial manner. Of course, one should always remember that information on unemployment benefit receipts at the second interview is only available for the right-censored unemployed and that the type and amount of benefit received might contribute to determine the rate of exit from unemployment. Therefore, it is possible and presumably likely that those unemployed that already exited from unemployment before the time of the second interview experienced on average a larger drop in the amount of benefit received (especially at the time of UB exhaustion) than those right-censored unemployed. In particular, about 77% of the "only UB" unemployed are observed to exit from unemployment before the second interview. The same figure for the recipients of SB (with or without UB) is about 56%.

Moreover, from the evidence presented in Table 2.12, it emerges that about 60% of the "only UB" unemployed experiences a complete (for about 44%) or partial (for about 15%) drop in the amount of benefit received at the second interview relative to the amount reported at the first interview. The same figure for recipients of SB (with or without UB) is about 32% with only 3% of them reporting to receive no benefit at all (at the second interview).

The following problems that may impair the detection of the effect of the duration of entitlement to UB are due to some particular features of the LSUS survey. The first is that the number of the unemployed observed is quite small: 736 unemployed report receiving "only UB" (at the first interview) out of about 2000 unemployed. The second is that, as already discussed in several occasions (see Sections 2.3 and 2.5.3), the survey contains little information on any changes in the pattern of benefit receipts over time. It would, instead, be important in order to estimate the effect of the benefit entitlement duration to know how benefit receipts vary over time; whether the "only UB" unemployed gain entitlement to SB upon UB exhaustion (or maybe even sometime before); whether, when and by how much the amount of benefit received falls during the course of the unemployment spell. Finally, it is possible that some unemployed misreported the type of benefit

received, which would obviously impair the analysis I carry out. I find for the LSUS sample that almost 90% of the "only UB" recipients (at the first interview) are likely to have correctly reported the type of benefit received (see Section 2.5.3).

6.4.2 Descriptive statistics of the explanatory variables

Descriptive statistics of the explanatory variables are given in Table 6.1. It emerges that those who receive "only UB" (at the first interview) are more likely on average to have held a full-time job for most of the year before the commencement of their registered unemployment spell. This finding is plausible given the rules that regulate entitlement to UB (see section 1.3).

Recipients of "only UB" are also more likely to be older on average than other unemployment benefit recipients. This might be perhaps explained by the fact that the category "recipients of only UB" exclude the unemployed receiving SB together with UB; i. e. joint payments of UB and SB.

Table 6.1: *Descriptive statistics of the economic variables*

	Receive only UB		*Receive SB*		*Benefit receipients*	
Variable	*Mean*	*SD*	*Mean*	*SD*	*Mean*	*SD*
Left truncation period	*13.3736*	*1.0473*	*13.4241*	*1.0527*	*13.4049*	*1.0507*
Unemployment duration (weeks)	*45.5340*	*19.3854*	*48.2415*	*18.7074*	*47.2148*	*19.0078*
F/t work most of the year before U.	*0.8111*	*0.3917*	*0.5693*	*0.4954*	*0.6610*	*0.4735*
Unemployed most of the year before U.	*0.1141*	*0.3182*	*0.2896*	*0.4538*	*0.2231*	*0.4164*
Sick no work most of the year before U.	*0.0272*	*0.1627*	*0.0398*	*0.1957*	*0.0350*	*0.1839*
Professional/Intermediate Occupation	*0.2269*	*0.4191*	*0.1295*	*0.3358*	*0.1664*	*0.3725*
Unskilled Occupation	*0.0584*	*0.2347*	*0.0614*	*0.2402*	*0.0603*	*0.2381*
Occupation not available	*0.0421*	*0.2010*	*0.0846*	*0.2785*	*0.0685*	*0.2527*
Age 20-24	*0.0924*	*0.2898*	*0.1477*	*0.3550*	*0.1267*	*0.3328*
Age 25-34	*0.2065*	*0.4051*	*0.4033*	*0.4908*	*0.3287*	*0.4699*
Age 35-44	*0.1929*	*0.3949*	*0.2805*	*0.4494*	*0.2473*	*0.4316*
Age 45-54	*0.2948*	*0.4563*	*0.1286*	*0.3349*	*0.1917*	*0.3937*
Age 55-58	*0.2133*	*0.4099*	*0.0398*	*0.1957*	*0.1056*	*0.3074*
Has any child old less than 5	*0.1630*	*0.3697*	*0.4639*	*0.4989*	*0.3496*	*0.4770*
Married	*0.8859*	*0.3182*	*0.8506*	*0.3566*	*0.8640*	*0.3429*
Spouse working 1 month before U.	*0.4565*	*0.4984*	*0.1261*	*0.3321*	*0.2514*	*0.4339*
Searches less than before	*0.1399*	*0.3472*	*0.0622*	*0.2417*	*0.0917*	*0.2887*
Values Leisure more than Labour	*0.1875*	*0.3906*	*0.0966*	*0.2965*	*0.1324*	*0.3390*
Experiences some shortage of money	*0.5897*	*0.4922*	*0.8274*	*0.3781*	*0.7372*	*0.4402*
House owner outright/with mortgage	*0.4715*	*0.4995*	*0.3129*	*0.4639*	*0.3730*	*0.4837*
County unemployment rate	*13.4753*	*3.2963*	*13.6656*	*3.1372*	*13.5935*	*3.1990*
UB/SB amount in £, logs	*3.3945*	*0.2979*	*3.7999*	*0.4316*	*3.6462*	*0.4335*
Predicted earnings, in £, logs.	*4.5001*	*0.4325*	*4.4200*	*0.5330*	*4.4504*	*0.4967*
pred. earn. not available	*0.0068*	*0.0822*	*0.0124*	*0.1109*	*0.0103*	*0.1010*
total savings at tk. £	*2990.7*	*8694.6*	*367.5*	*1488.0*	*1362*	*5624.7*
total debt at tk. £	*466.5*	*1661.0*	*673.6*	*3146.7*	*633.7*	*2753.9*
Weeks of UB left, 10-6					*0.1059*	*0.3070*
Weeks of UB left, 5-1					*0.0965*	*0.2960*
Weeks of UB left, 0					*0.0910*	*0.2877*
1-3 weeks past UB exhaustion					*0.0894*	*0.2853*
4 or more weeks past UB exhaustion					*0.0820*	*0.2743*

The number of the unemployed receiving only UB is 736; 1205 get SB or jointly SB and UB. The total number of benefit recipients is then 1941. The dichotomous variables take value one when the condition stated for each of them is satisfied. The mean duration is taken over all observations (including the right-censored observations). The logarithms are taken over the non-zero observations. Time tk relates to one month before the commencement of the unemployment spell. "U." stands for the observed unemployment spell.

The recipients of UB that are aged over forty-five might be more likely to have higher levels of savings and therefore they may not be entitled to SB. Younger recipients of UB are instead more likely to have lower levels of savings and may gain entitlement to SB (together with UB). Indeed, it appears that receipients of "only UB" at the first interview are on average much wealthier than recipients of SB (with or without UB)[5]. This might also explain why a lower proportion of the "only UB" unemployed report to suffer from a shortage of money and a larger proportion report to own the house where they live.

From inspection of Table 6.1, it emerges that the proportion of the unemployed with diminished search intensity is higher among recipients of only UB. Similarly the proportion of the unemployed that value leisure more than labour is higher among the "only UB" group. Perhaps, also these findings might be due to the fact that the only UB unemployed are on average wealthier than the unemployed that receive SB (either by itself or together with UB). It seems plausible that higher levels of financial wealth may result in a lower intensity of search and a higher valuation of leisure as relative to labour (see Chapter 7).

The distribution of the total savings of the unemployed receiving only UB or SB (with or without UB) at the first interview (which is the time to which most of the variables discussed above relate) is shown in Table 6.2 below. It appears that "only UB" recipients are considerably wealthier than "SB recipients". The percentage of "only UB" unemployed that report zero amounts of savings (about 28%) is considerably lower than the corresponding figure (about 51%) for "SB" unemployed. Almost 100% of the unemployed receiving SB (with or without UB) at the first interview report savings below £3000. The same figure for recipients of "only UB" is about 73%. It is interesting to compare the number of the unemployed in the different benefit groups with savings below (above) £3000 since this was the threshold level of savings above which the unemployed was not entitled to the means-tested benefit (SB) in 1982/83. This is done in Table 6.3 below. The number of "only UB" unemployed with savings above the threshold is considerable higher than that of SB recipients.

Although the relationship between the unemployed's financial resources and unemployment duration is the subject of Chapter 7 (to which the reader is referred for a more complete treatment of the issue), I shall test for the sensitivity of the results obtained to the inclusion of savings and debt amounts among the regressors in Section 6.5.3.

[5]The levels of savings and debt of the unemployed given in Table 6.1 relate to one month before the commencement of the observed unemployment spell. This reference time was chosen since the level of savings and debt at the time of the first interview might be endogeneous to the model; i. e. they might be simultaneously determined with the duration of the unemployment spell.

Table 6.2: *The amounts of savings at the first interview*

amounts, £	*Receive only UB*		*Receive SB with or without UB*	
	%	*cum. %*	*%*	*cum. %*
0	*28.3*	*28.3*	*51.3*	*51.3*
≤ 100	*17.1*	*45.4*	*29.4*	*80.7*
≤ 200	*5.4*	*50.8*	*4.5*	*85.2*
≤ 500	*5.0*	*55.8*	*4.4*	*89.6*
≤ 1000	*6.0*	*61.8*	*5.0*	*94.6*
≤ 2000	*6.3*	*68.1*	*4.5*	*99*
≤ 3000	*4.7*	*72.8*	*0.7*	*99.8*
≤ 5000	*7.8*	*80.6*	*0.1*	*99.9*
≤ 10000	*7.2*	*87.8*	*0.1*	*100*
≤ 15000	*3.9*	*91.7*		
≤ 20000	*2.1*	*93.8*		
≤ 30000	*3.5*	*97.3*		
≤ 50000	*1.7*	*99.0*		
≤ 100000	*1.0*	*100*		
mean value, £	*4248.1 (SD 11047.0)*		*153.9 (SD 427.6)*	
The total savings variable is defined in Chapter 7 or 1. The total amounts of savings relate to the principal respondent; i. e. the spouse's savings (if any) are not taken into account.				

Table 6.3: Percentage of the unemployed with savings above the threshold level

Time	*Receive only UB*	*Receive SB (with or without UB)*
tk	*22%*	*3%*
t1	*27%*	*1%*
The amounts of savings considered are total savings reported and relate to the principal respondent; i. e. the spouse's savings (if any) are not taken into account. The total savings variable is defined in Chapter 7. The times tk and t1 relate respectively to one month before the commencement of the unemployment spell and to the first interview.		

6.4.3 The "time left to benefit exhaustion " dummies

The impact of the expected exhaustion of UB is modelled with a set of time varying dummies which take value one in some chosen intervals of time[6] for the unemployed that reported to receive only UB at the first interview. The base for these dummies are the unemployed reporting to receive SB (either by itself or together with UB) and the "only UB" unemployed in week 11–41 (except when this additional dummy is included).

Some "only UB" unemployed (about 6%; i. e. 49 out of 736) that reported at the first interview benefit amounts obviously different from the flat rates UB amounts (which are fixed by law) were recoded for the purpose of the construction of these dummies as receiving SB[7]. The following intervals of time

[6]This approach is basically the same than that adopted by Katz and Meyer (1988).

[7]These unemployed reported benefit amounts of less than £12.50, which corresponds to

before exhaustion of entitlement to unemployment benefit were considered:

- ten to six weeks of entitlement to UB left before exhaustion of entitlement;
- five to one weeks of entitlement to UB left;
- zero weeks of entitlement to UB left; i. e. last week of entitlement to UB, which corresponds to the 52nd week of unemployment for more than 90% of the recipients of UB at the first interview;
- from one to three weeks past the exhaustion of entitlement to UB;
- four weeks or more past the exhaustion of entitlement to UB.

These time varying dummies were constructed using the method suggested by Rohwer (1992)[8]. Introducing these additional time varying dummies for recipients of "only UB" in the time intervals going from about week 42 onwards is equivalent to shifting the baseline hazard rate for the recipients of "only UB" from week 42 onwards. I test for the sensitivity of the results to the introduction of a further time varying dummy taking value one from week 11 (the first observed week of unemployment) to week 41 for the "only UB" group. The sensitivity of the estimates to alternative assumptions about the form of the baseline hazard rate is also tested.

6.5 Results of estimation

The conditional probability of leaving unemployment is first estimated by non-parametric methods. Next, the model given by Equation 3.36 of Chapter 3 is estimated for the full sample of benefit recipients, i. e. for the two groups of benefit recipients pooled together. Then, the model is estimated separately for two groups of benefit recipients and the two estimated hazard rates are compared.

6.5.1 Non-parametric Kaplan-Meier estimates

Non-parametric estimates of the survivor functions were obtained using the Kaplan-Meier method. I have estimated the survivor functions for the full sample of benefit recipients and for the two groups of "UB only" and "SB" recipients[9]. The survivor functions for the single risk model and for the

the November 1982 minimum reduced rate without any addition for dependants, or larger than £43, which corresponds more or less to the full rate with additions for dependent spouse and children at November 1982.

[8]This method has also been adopted to construct the time varying unemployment benefit level variable.

[9]These two groups are defined with respect to reported benefit receipts at the time of the first interview.

Figure 6.2: *Single risk survivor functions*

competing risks of exit into full-time work or into "other states"[10] were also estimated. The duration of the completed spells of unemployment by the exit states for recipients of only UB and for recipients of SB (with or without UB) is presented in Table 8.10, in the Appendix. The estimated survivor functions for the two groups and for the single risk case are plotted and compared in Figure 6.2.

The survivor function for recipients of "only UB" (see Figure 6.2) lies above that for the other benefit recipients until about week 50, thereafter the two curves tend to coincide. This implies that the (conditional) probability of leaving unemployment is higher at any time until about week 50 for the "SB, with or without UB" group rather than for the "only UB" group. After week 50, the two groups do not differ substantially with respect to their probability of leaving unemployment. One possible explanation for this result lies in the fact that the "only UB" unemployed are older on average than the "SB" unemployed. The expected impact of older age on the hazard rate is negative[11].

[10]The reader is referred to the preceding Chapters for a definition of the "other states" exit.

[11]Another explanation is that the "only UB" unemployed are richer on average. However, this possibility is excluded on the basis of some tests carried out in the last section.

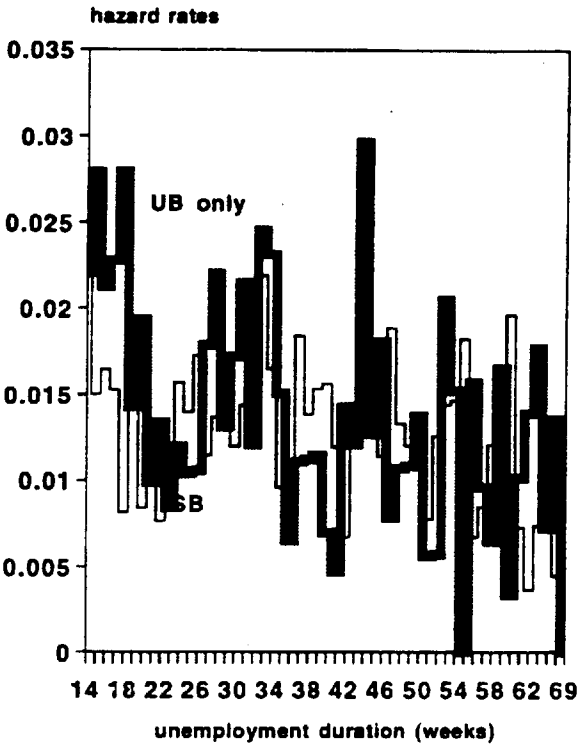

Figure 6.3: *Empirical hazard rates. Full-time work exit*

However, according to the Log Rank test statistics[12], based on the Kaplan-Meier standard errors, the two groups of benefit recipients are statistically different ($\chi^2_2 = 9.0$). If the 95% confidence intervals are taken into account, the two curves are significantly different only during the first few weeks. Indeed, after the first few weeks of unemployment, the survivor curve for the "SB" group is "contained" between the two 95% confidence bands for the survivor curve of the "UB only" group (see Figure 6.2). The survivor curve for the "UB only" group is contained between the two 95% confidence bands of the survivor function for the "SB" group[13].

[12]See Kalbfleish and Prentice, 1980, pp. 16–18.

[13]The lower bound of the survivor curve for the "SB" group is not plotted since the survivor function for this group lies behind or coincides with the UB survivor function at any time, which implies that its confidence band lies also behind the "UB" survivor function at any time. This confidence band is simply not plotted for clarity purposes in order not to burden the figure with several curves.

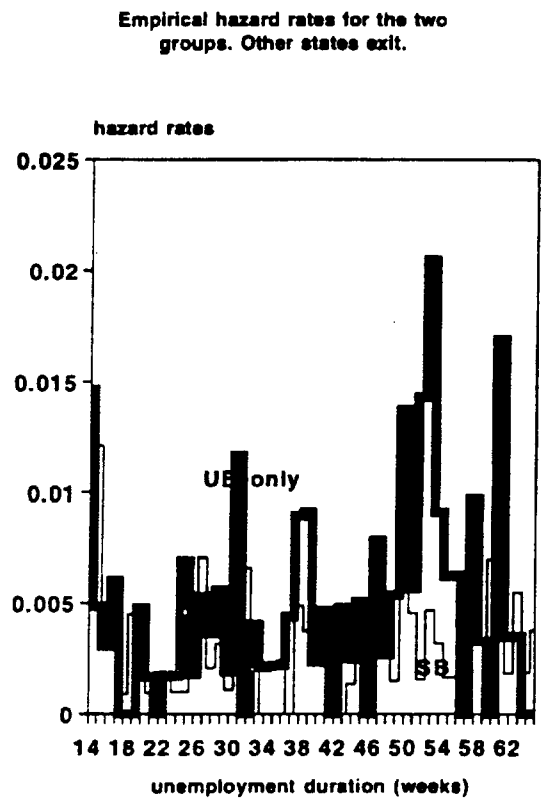

Figure 6.4: *Empirical hazard rates. Other states exit*

The two hazards for the competing risks of exit into full-time work and into other states are plotted respectively in Figures 6.4 and 6.5.1. The empirical hazard rate of the "only UB" unemployed shows larger spikes than that of the "SB with or without UB" unemployed for either exit considered. The behaviour of the two hazards over time is quite different for either exit. However, the Kaplan-Meier estimates do not allow for individual heterogeneity. In the next sections individual (observed) heterogeneity is allowed for.

6.5.2 Results of estimation of the pooled groups model

The model of Equation 3.36 of Chapter 3 is estimated for the two groups of unemployment benefit recipients pooled together. The baseline hazard rate is allowed to vary each week. The estimated model is very similar to that estimated in Chapter 5 except for the inclusion of the UB entitlement duration dummies among the explanatory variables. However, the model of Chapter 5 was estimated also for the unemployed that reported not to receive any unemployment benefit at the first interview.

The estimated impacts of the explanatory variables for the exit into full-time work (see Table 6.4) are very similar to those of the model estimated in Chapter 5 (model 2 of Table 5.2) except for the estimated coefficient on the dichotomous indicator of whether the unemployed was in receipt of "only UB" at the first interview, which is now found not significantly different from zero. This result confirms perhaps my view that the dummy for the receipt of only UB at the commencement of the unemployment spell (the time of the first interview, in practise) captures at least in part the impact of differences in entitlement duration between the two benefit recipients groups. When the impact of the duration of entitlement to UB is modelled separately —via the five time varying dummies— the estimated coefficient on the "only UB" dummy becomes not significantly different from zero. The reader is referred to Chapter 5 for a discussion of the impact of the explanatory variables. The discussion below focuses on the impact of the duration of entitlement to unemployment benefit.

The first, perhaps surprising, result is that the expected exhaustion of entitlement to UB affects the probability of leaving unemployment to exit to states other than full-time job more than the probability of re-employment into a full-time job.

In the exit to full-time work only the coefficients on the first and the last dummy are significantly different from zero. However, the estimated coefficients on the UB entitlement duration dummies have all the expected positive sign. In the other states exit, all the UB entitlement duration dummies except the first are found to have a statistically significant impact. The estimated coefficients are positive as predicted by economic theory. The robustness of the estimated coefficients on the two sets (for the two exits considered) of the benefit entitlement duration dummies is tested below (see Table 6.6).

It is perhaps difficult to interpret the estimates of the impact of UB approaching exhaustion for the exit into full-time work. According to the estimates of Table 6.4, at the time when there are ten to six weeks of entitlement to UB left, the chances of exiting from unemployment into full-time work are, for recipients of "only UB", two and half times higher in each week, all things equal. Then, from four weeks after benefit exhaustion onwards the chances of leaving unemployment increase by about 70%, in relation to the base, in each of the following weeks. Instead, from five weeks before exhaus-

tion of entitlement to UB to three weeks past exhaustion, the chances of leaving unemployment to enter full-time work are not significantly different in relation to the base.

Table 6.4: *Results of estimation*

Variable	*Full-time exit*		*Other states exit*	
label	*Coeff*	*SE*	*Coeff*	*SE*
F/t work year before	*0.3658**	*0.1457*	*-0.6759**	*0.2132*
Unemployed year before	*0.3389**	*0.1559*	*-0.4452*	*0.2349*
Sick year before	*-0.4124*	*0.3114*	*0.1916*	*0.1629*
Profes. /Interm. Occ.	*0.2003**	*0.1008*	*0.4823**	*0.1992*
Unskilled Occupation	*-0.4585**	*0.1745*	*-0.1580*	*0.2903*
Age 20-24	*0.2097*	*0.1090*	*-0.0901*	*0.2417*
Age 35-44	*-0.1877**	*0.0918*	*0.1172*	*0.1910*
Age 45-54	*-0.6489**	*0.1133*	*-0.2286*	*0.2194*
Age 55-58	*-1.2946**	*0.1759*	*-0.0489*	*0.2496*
Has any child aged < 5	*-0.2225**	*0.0886*	*-0.2079*	*0.1891*
Married	*0.1779*	*0.1251*	*-0.2789*	*0.2007*
Spouse working month before	*0.3389**	*0.0905*	*0.3193*	*0.1743*
experiences money shortage	*0.2490**	*0.0889*	*-0.2170*	*0.1918*
Searches less than before	*-0.8005**	*0.1794*	*-0.0829*	*0.3430*
Values Leisure more	*-0.2672**	*0.1185*	*-0.1121*	*0.2170*
House owner	*0.3000**	*0.0752*	*0.2145*	*0.1823*
County unemployment rate	*-0.0212*	*0.0109*	*-0.0764*	*0.0891*
Receives only UB	*-0.0905*	*0.0943*	*0.1911*	*0.1476*
UB/SB time varying	*-0.0375*	*0.0643*	*-0.0269*	*0.0204*
Predicted earnings	*0.6631**	*0.2079*	*-0.8274**	*0.4152*
No pred. earn.	*3.1247**	*0.9846*	*-3.1826*	*1.9351*
- 6-10 weeks	*0.9313**	*0.2452*	*0.5486*	*0.6013*
- 5-1 weeks	*0.1513*	*0.2768*	*1.2464**	*0.3880*
0 weeks	*0.6993*	*0.6799*	*3.6317**	*0.6735*
+ 1-3 weeks	*0.3486*	*0.3074*	*1.7034**	*0.4178*
+ 4 weeks-∞	*0.5463**	*0.2051*	*0.9519**	*0.3490*

Max. likelihood: 6142.7. A weekly baseline is estimated. The estimated coefficients on the weekly segments of the baseline hazard rate are given in Table 8.11 in the Appendix. A likelihood ratio test of the restrictions imposed by estimating the model (2) of Chapter 5 only for the recipients of benefit, instead of the present model with the benefit duration dummies, rejected the restrictions ($\chi^2_{10} = 108.4$). *Descriptive statistics of explanatory variables are provided in the preceding table, in the data section. A * indicate statistical significance at the two-sided 5% level.*

I have made an attempt to find a plausible interpretation of these results. It is possible that the "only UB" unemployed being aware that UB will expire after a year of entitlement start to increase their search effort in the last ten weeks of entitlement (if they have not yet succeeded in securing themselves a job). However if they have not yet found a job in the last month before benefit exhaustion they might decrease their search effort back to normal levels because of discouragement or perhaps hope that they will be able to claim SB upon UB exhaustion. About one month after exhaustion, the "once only UB" unemployed that were neither able to find a job nor perhaps to claim SB, will again increase their search intensity but not as much as before (when there were from ten to six weeks of entitlement left) since they are now more discouraged.

Let us look next at the probability of leaving unemployment to states other than full-time work. In this case, all the UB entitlement duration dummies except the first, relating to ten to six weeks left before unemployment benefit exhaustion, are significant. This result might perhaps be seen as confirming the previous "story", if one agrees (or assumes) that exit to full-time work is the unemployed's "first best". When there are five to one weeks of benefit entitlement left, the chances of leaving unemployment to other states (for the "only UB unemployed" that have not yet exited from unemployment) increase by three and half times as much in each of these weeks and in relation to the base. In the last week of entitlement to UB, the probability of exiting to states other than full-time work for "only UB" recipients increases enormously, by about 38 times (in relation to the base). Then, from one to three weeks after benefit exhaustion, the probability of exiting to other states is five and a half times higher in each of these weeks and in relation to the base. From four weeks after exhaustion onwards the chances of exiting to other states are in each week two and half times larger (in relation to the base).

It is possible that part of the enormous increase in the probability of exiting to other states in the last week of entitlement is due to some unemployed classifying themselves as non-registered unemployed under the category "other economic activities". Rounding error in the replies seems here not a good explanation, given that the year would seem the most likely figure to round responses to but the dummy for 0 weeks of entitlement left is not even significantly different from zero for the exit into full-time work.

Finally, it is interesting to notice that all but one of the benefit duration dummies that are statistically significant are also significantly different from the preceding and following dummy, as shown in Table 6.5. This implies that the impact of the expected exhaustion of entitlement to unemployment benefit is different in at least some of the time intervals considered. The robustness of the estimated entitlement duration effect is tested below.

The baseline hazard for the exit into full-time work is plotted in Figure 6.5. The baseline of the estimated model presented in Chapter 5 (see Figure 5.1), where the entitlement effect was not controlled for, is also plot-

Table 6.5: *The significance of the differences between the estimated coefficients of the benefit entitlement duration dummy*

Benefit duration dummy	$\hat{\beta}_n - \hat{\beta}_m$	$\hat{SE}(\beta_n - \beta_m)$
Full-time exit		
-6–10 weeks, -5–1 weeks	0.78*	0.36
-5–1 weeks, 0 weeks	0.55	0.729
0 weeks, +1–3 weeks	0.37	0.74
+1–3weeks, +4 weeks–∞	0.10	0.35
Other states exit		
-6–10 weeks, -5–1 weeks	0.69	0.59
-5–1 weeks, 0 weeks	2.39*	0.76
0 weeks, +1–3 weeks	1.93*	0.75
+1–3weeks, +4 weeks–∞	0.75*	0.48
A * indicates significance at the two sided 5% level.		

ted for comparison purposes. The estimated coefficients on the weekly steps of the baseline hazard are given in Table 8.11 in the Appendix.

The two estimated baselines follow a similar pattern over time. Spikes in the hazard are found at the same points in time as before[14]. However, if the effect of the limited duration of UB is controlled for (with the UB entitlement duration dummies), the spikes after week forty are smaller in size. Part of the variation over time is captured by the UB entitlement duration dummies. The estimated coefficients of the weekly baseline for the exit to other states are not significantly different from zero. The baseline for the exit to other states is, therefore, not plotted.

Some sensitivity analysis

It seems interesting to compare the results obtained here with those of the corresponding single risk model[15] since many of the previous studies were carried out in a single risk framework of analysis. This is done in Table 6.6 together with other types of sensitivity tests. All the estimated coefficients on the UB entitlement duration dummies in the single risk model are significantly different from zero and positive. The dummy for the last week of entitlement to UB is more than twice the size of the other dummies. However, we know that most of this impact is due to exit into states other than

[14]The significance of any spike in the baseline was discussed in Section 4.4.2 of Chapter 4, where a simpler model was estimated.

[15]The likelihood function for this model is given by Equation 3.34 of Chapter 3.

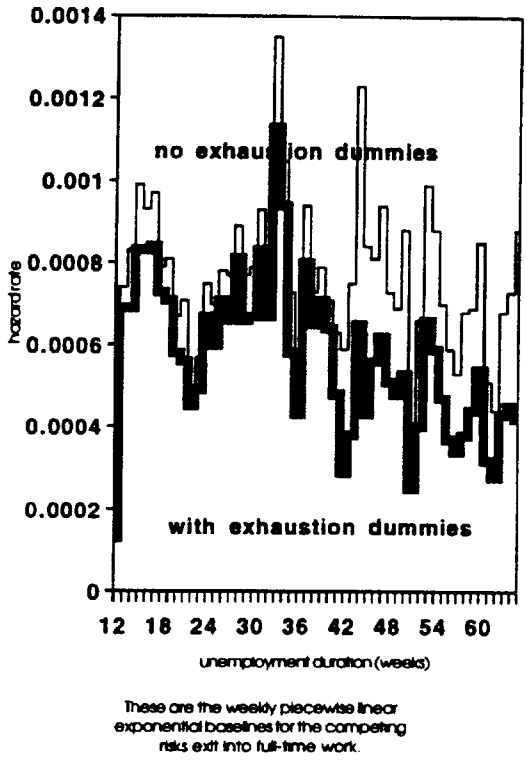

Figure 6.5: *Baseline with/without benefit entitlement duration dummies*

full-time work. This confirms the importance of distinguishing the exit states out of unemployment whenever the data available allow one to do so.

The main model of Table 6.4 was re-estimated without the dummy for recipients of "only UB" (Model (1) of Table 6.6). A likelihood ratio test, in the last column of Table 6.6, indicates that the null hypothesis that the estimated coefficient on this dummy is not significantly different from zero cannot be rejected. The significance and the sign of the estimated coefficients on the set of time varying dummies for the duration of entitlement to UB are not affected by the exclusion of this dummy from the model. The magnitude of the estimated coefficients on the entitlement duration dummies does not change substantially.

The model was then estimated with an additional time varying dummy taking value one from week 11 (corresponding to the observed commencement of the unemployment spell because of the left truncation problem) to week 41 for recipients of only UB (Model (2)). The addition of this variable allows the baseline hazard rate to shift for recipients of "only UB" from the commencement of the unemployment spell rather than from week 42. The estimated coefficient on this additional variable is statistically not significant for any of the two competing risks exits. A likelihood ratio test of Model (2) against Model (1) indicates that the null hypothesis that the coefficient on the additional variable is not significantly different from zero cannot be

rejected[16].

Model (3) has only the entitlement duration dummies (and the weekly "pieces" of the baseline hazard rate) as explanatory variables of the model. The interest of estimating such a model is to check whether there is any significant change in the estimated coefficients on the entitlement duration dummies. The significance and the sign of the estimated coefficients is not affected except for the coefficient of the dummy "week 0 to week 41", which becomes significant for the hazard of exit into full-time work. Also, the sign of the estimated coefficient on the dummy "week 47–51" (not significant) becomes now negative for the full-time work hazard. The magnitude of the estimated coefficients does not change substantially, at least for those coefficients that are statistically significant (except for the coefficient on the dummy "week 0–41" in the full-time hazard).

In model (4), the baseline hazard rate is allowed to vary every two weeks rather than each week. I am interested in whether changes in the specification of the baseline hazard rate affect the estimated coefficients on the benefit duration dummies.

[16]Model (2) does not contain the dummy for receipt of "UB only" among the regressors since, when the time varying dummy "week 0 to week 41" is added to the model, the set of time varying dummies for the UB entitlement duration left and the dummy for receipt of "only UB" become highly collinear. This is the reason why model (2) is compared with model (1) rather than with the "Base" model.

Table 6.6: *Sensitivity analysis*

Estimated Model (Same covariates as main model)	*Unemployment Benefit entitlement duration Dummies*						*Max.*
	- 9-52 weeks	*- 6-10 weeks*	*- 5-1 weeks*	*0 weeks*	*+ 1-3 weeks*	*+ 4-∞*	*Loglik.*
Base Model, f-t		*0.9313**	*0.1513*	*0.6993*	*0.3488*	*0.5463*	*-6142.7*
		(0.2452)	*(0.2768)*	*(0.6799)*	*(0.3074)*	*(0.2051)*	
Base Model, oth		*0.5486*	*1.2464**	*3.6317**	*1.7034**	*0.9519**	
		(0.6013)	*(0.3880)*	*(0.6735)*	*(0.4178)*	*(0.3490)*	
Single risk model		*0.8695**	*0.4845**	*1.7570**	*0.8741**	*0.7070**	*-5656.3*
		(0.2268)	*(0.2183)*	*(0.4744)*	*(0.2258)*	*(0.1689)*	
Model (1), f-t,		*0.8862**	*0.1049*	*0.6652*	*0.3153*	*0.5073**	*-6143.7*
		(0.2404)	*(0.2724)*	*(0.6783)*	*(0.3047)*	*(0.2002)*	*M(1), Base M*
Model (1), oth.		*0.4349*	*1.1368**	*3.5145**	*1.6006**	*0.8418**	$\chi^2_2 = 2$
		(0.5924)	*(0.3740)*	*(0.6637)*	*(0.4050)*	*(0.3320)*	
Model (2), f-t	*-0.0905*	*0.8408**	*0.0608*	*0.6088*	*0.2583*	*0.4558**	*-6142.6*
	(0.0943)	*(0.2450)*	*(0.2762)*	*(0.6814)*	*(0.3109)*	*(0.2078)*	*M(2), M(1)*
Model (2), oth	*-0.2170*	*0.3316*	*1.0294**	*3.4147**	*1.4864**	*0.7349**	$\chi^2_2 = 2.2$
	(0.1918)	*(0.6003)*	*(0.3862)*	*(0.6704)*	*(0.4187)*	*(0.3467)*	
Model (3), ft	*-0.1929**	*0.6895**	*-0.1152*	*0.4978*	*0.1271*	*0.4068**	*-6308.1*
	(0.0765)	*(0.2417)*	*(0.2732)*	*(0.6836)*	*(0.2976)*	*(0.1945)*	*M(3), M(2)*
Model (3), oth	*-0.1833*	*0.3362*	*1.0533**	*3.5277**	*1.6134**	*0.8832**	$\chi^2_{40} = 331.0$
	(0.1628)	*(0.5924)*	*(0.3754)*	*(0.6477)*	*(0.3840)*	*(0.3089)*	
Model (4), f-t	*-0.0901*	*0.7524**	*0.0880*	*0.3795*	*0.3229*	*0.4452**	*-6170.0*
	(0.0943)	*(0.2324)*	*(0.2735)*	*(0.6252)*	*(0.3046)*	*(0.2065)*	*M(4), M(2)*
Model (4), oth	*-0.2126*	*0.2369*	*0.9454**	*1.9547**	*1.6666**	*0.7912**	$\chi^2_{54} = 54.8$
	(0.1917)	*(0.5723)*	*(0.3726)*	*(0.5998)*	*(0.3971)*	*(0.3434)*	
Weibull, f-t	*-0.1637*	*1.1873**	*0.2318*	*0.0760*	*0.6174*	*0.6565*	*-6210.4*
Weibull parameter exp(-0.55) (SE 0.13)*	*(0.1669)*	*(0.4114)*	*(0.4470)*	*(1.0253)*	*(0.5081)*	*(0.3732)*	
Weibull, oth	*-0.4439*	*0.1136*	*2.2906**	*3.5466 **	*3.2953**	*1.7266*	
Weibull parameter exp(-0.71) (SE 0.32)*	*(0.4217)*	*(1.0646)*	*(1.0697)*	*(1.6218)*	*(1.3758)*	*(0.9652)*	

*The base model is the model of Table 6.4. The only UB dummy taking value one for the recipients of only UB (which was however not significantly different from zero) is dropped from model (1), (2), (3), (4), (5). Model (1) is the same as the base model except for the exclusion of this "UB only" dummy from the regressors. Model (2) is the same as model (1) except for the inclusion of the dummy "Week 0-41", which takes value one from week 1 to week 41 for the recipients of only UB. Model (3) has only the time varying dummies "time left to exhaustion of UB" as regressors and a weekly baseline. Model (4) is the same as model (2) but the baseline hazard rate is allowed to vary each two weeks rather than each week as in the base model, in model (1), (2) and (3). Standard errors in brackets. A * indicate statistical significance at the two-sided 5% level. "f-t" stands for full-time work exit, "oth" stands for other states exit. Likelihood ratio tests between the two models indicated are given in the last column.*

From Table 6.6, it emerges that the statistical significance, the sign and the magnitude of these coefficients do not change substantially except for the coefficients on the last week of entitlement (week 52). The coefficients on the last week of entitlement, for both hazards of exit into full-time work and into other states, are in model (4) almost half the size than the corresponding coefficients in model (2). In particular, the estimated coefficient on the last week of entitlement for the other states hazard indicates that the other exits hazard increases during the last week of UB entitlement for "only UB" recipients by seven times according to model (4) and by about 34 times according to model (2).

I have then estimated a Weibull model (see Table 6.6). The sign of the estimated coefficients on the benefit duration dummies is the same as in model (2). The significance and the magnitude of the estimated coefficients are quite different from those of model (2). In particular, the estimated coefficients of the other states hazard rate are larger in magnitude than in model (2). The estimated coefficients on the last dummy ("+ 4 weeks onwards) is not significant in any of the two exits, while it was significant for both exits in model (2). Significant negative time dependency is detected in both hazards. One would expect the Weibull model to shed a more considerable amount of unobserved heterogeneity than the piecewise linear model (see Section 4.6).

I conclude that the estimated coefficients on the benefit entitlement duration dummies are quite robust, though the estimates are sensitive to different specification of the baseline hazard rate[17]. There is robust evidence that the expected exhaustion of the insurance benefit (UB) raises the hazard rate of the "only UB" unemployed to a higher level near the time of benefit exhaustion. In particular, the hazard of exiting into states other than full-time work increases more than the full-time job hazard, as the expected exhaustion of UB nears.

6.5.3 Are the two benefit recipients groups significantly different?

I have estimated the model separately for the two groups of benefit receipients. I am interested in the detection of differences in the hazard rates of the two groups. Following the theoretical predictions, the hazard rate for the recipients of "only UB" should show larger spikes near the time of benefit exhaustion, while the baseline hazard rate for recipients of SB (with or without UB) should be smoother.

The results of estimation for the explanatory variables[18] and for the exit into full-time work are given in Table 8.12 in the Appendix. The estimated hazard rates for a representative person[19], for both groups of benefit recip-

[17]This might indicate the presence of unobserved heterogeneity which should be taken care of, in future work.

[18]The benefit duration dummies are then dropped from the model.

[19]The representative individual is constructed assuming that all continuous variables

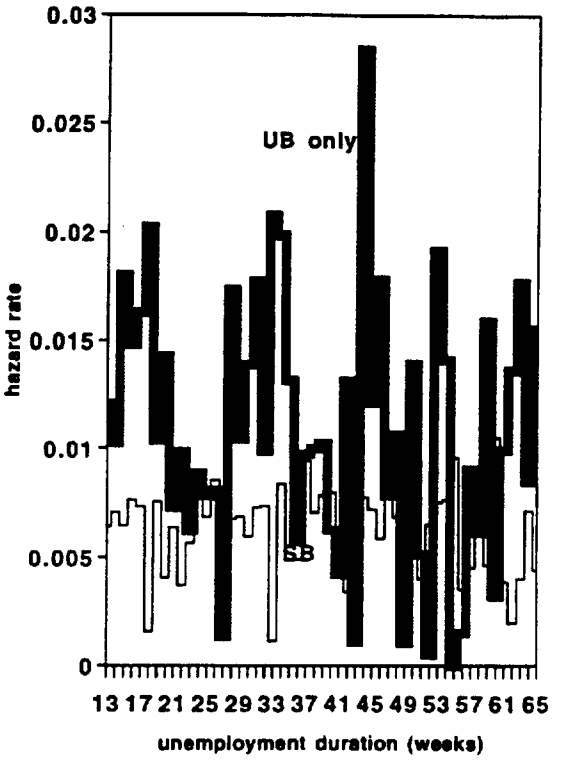

Figure 6.6: *Hazard rates. Full-time work exit*

ients and for exit into full-time work are plotted in Figure 6.6. The corresponding hazards for exit into other states are plotted in Figure 6.5.3.

The estimated baseline hazard for recipients of "only UB" shows larger spikes and more variability than the estimated baseline hazard for the other benefit recipients. The behaviour of the unemployed that receive only UB at the first interview —which is the best approximation of the starting time of the unemployment spell, allowed for by the LSUS data— is more sensitive to the elapsing of time than the behaviour of the unemployed receiving SB. However, part of this difference is due to the smaller number of the "only UB" unemployed.

take their mean value; the dummies for whether the unemployed is "married" and he has "any child aged less than five" take value one; all the other dummies are set to zero; i. e. the unemployed person is in the base group for these dummies.

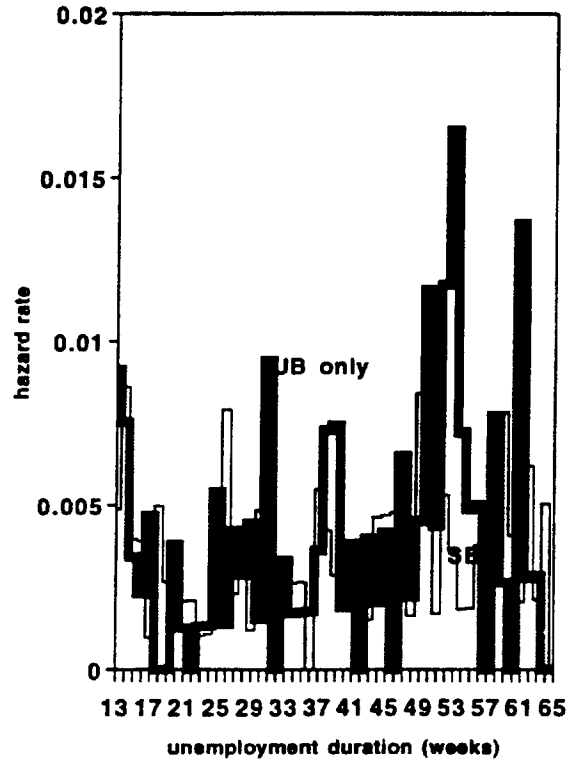

Figure 6.7: *Hazard rates. Other states exit*

The null hypothesis that the two groups of benefit recipients are not different in their (conditional) probabilities of leaving unemployment is rejected on the basis of a likelihood ratio test ($\chi^2_{150} = 186.8$). This result confirms that the type of benefit received at about the commencement of the unemployment spell might affect substantially the individual probability of leaving unemployment.

I have also tested how different levels of savings (and debt) affect the behaviour of the two groups of the unemployed. Indeed, the two groups have very different distributions of savings, as shown in Section 6.4.2. I am interested in the unemployed's savings more than in the unemployed's debt since the award of SB is determined on the basis of the unemployed's level of savings.

I test the null hypothesis that the coefficients on the additional variables for the levels of savings and debt are not significantly different from zero by means of a likelihood ratio test.

Table 6.7: *The impact of savings on the hazard rates of two groups*

Variable label	*Recipients of only UB*		*SB/ joint SB and UB*	
	Full-time work	*Other exits*	*Full-time work*	*Other exits*
Total savings,in 100 £	*-0.0012 (0.0010)*	*-0.0012 (0.0013)*	*0.0004 (0.0032)*	*0.0026 (0.0041)*
Total debt, in 100 £	*0.0009 (0.0032)*	*0.0054 (0.0053)*	*-0.0049* (0.0025)*	*-0.0027 (0.0043)*
Max. log-lik.	*-2326.9*		*-3636.1*	
Lik. ratio test	$\chi^2_4 = 3.6$		$\chi^2_4 = 37.8$	

*The estimated models are the same as those of Table 8.12, except for the inclusion of the savings and debt variables. The baseline hazard rates are allowed to vary each week. Descriptive statistics of explanatory variables are provided in the Table 6.1. A * indicates statistical significance at the two-sided 5% level. Standard errors are given in brackets. The likelihood ratio tests are carried out against the models of Table 8.12.*

The estimated coefficients on levels of savings and debt are reported in Table 6.7[20]. None of them is significantly different from zero except for the coefficient on the debt variable for the full-time work hazard of the "SB with or without UB" group. The negative sign on this coefficient is perhaps due to the fact that debt proxies access to credit and access to credit has a positive impact on the reservation wage. Also, debt may proxy low skill or low motivation (for this group).

According to the likelihood ratio tests, the null hypothesis that the additional variables (savings and debt levels) have a non-zero impact on the hazard rate cannot be rejected for the "only UB group" but it can be rejected for the "SB with or without UB" group. On the basis of these results one would conclude that the hazard rate of the "only UB" unemployed is not affected by their levels of savings (or debt). Similarly, the hazard of the "SB with or without UB" group is not affected by the unemployed's savings. It is instead affected to a minor extent by the unemployed's debt. These results would suggest that allowing for the impact of savings (and debt) on the hazard rate does not affect the estimates of the benefit duration effect. However, the impact of savings on the hazard rate is analysed more in depth in Chapter 7.

[20]The savings and debt variables relate to one month before the commencement of the unemployment spell. This reference time is chosen in order to avoid potential endogeneity problems. The analysis of the relationship between the unemployed's financial resources and the (conditional) probability of leaving unemployment is the subject of Chapter 7, to which the reader is referred.

6.6 Conclusion

In this chapter, I have estimated the relationship between the potential duration of entitlement to the national insurance benefit (UB) and the probability of leaving unemployment.

My conclusion is that there is some evidence that the hazard rate of "only UB" recipients rises near the time of exhaustion of entitlement to UB. However, the estimated size of the impact of the expected exhaustion of benefit entitlement is sensitive to the specification of the baseline hazard adopted. There is a larger effect of the expected benefit entitlement duration on the other states hazard than on the full-time job hazard. This finding highlights further the importance of using competing risks specifications of the hazard rate.

Future studies should aim at gathering more evidence on the impact of the expected exhaustion of the benefit on the re-employment probability. The issue is especially important in light of the 1993 Budget announcement that the duration of UB will be cutted down to six months since 1995.

In order to draw firm economic policy conclusions a larger sample size is probably needed.

Chapter 7

The unemployed's financial resources and the probability of re-employment

7.1 Introduction

The impact of different levels of savings and debt on the individual re-employment probability is a neglected area of research. Most developed economies provide some social assistance in terms of financial support to the unemployed, presumably on the assumption that unemployed persons may experience financial hardship and that they may face borrowing constraints. In spite of this, there is very limited evidence on the level of financial resources of the unemployed and on whether the levels of financial resources influence the duration of the unemployment spell.

In the simplest model of job search, the assumption of risk neutrality leads to the specification of the objective function of the unemployed in terms of income maximization rather than in terms of the maximization of the utility derived from income. Under this set up, only the difference in expected future income streams is expected to affect the duration of unemployment and the unemployed's level of financial resources plays no role. However, if the assumption of risk neutrality is relaxed, the level of financial resources might exercise an influence on the probability of leaving unemployment. It has been argued that higher degrees of risk aversion result in a lower reservation wage and a shorter unemployment spell (Kohn and Shavell (1974) and Pissarides (1974)). One would expect that higher levels of savings (given a certain degree of risk aversion) raise the reservation wage. Savings can be used to support living standards during unemployment and higher savings allow the unemployed to be more "choosy" about accepting job offers.

I study the impact of the unemployed's financial resources on the individual re-employment probability using the the LSUS data. These data contain rich information on the unemployed's levels of savings and debt. I also look

at the impact on the re-employment probability of redundancy payments and other once-off payments associated with the commencement of the unemployment spell. These payment may be seen as an (unexpected) increase in the level of savings.

A drawback of the analysis is that the unemployed may misreport the levels of their savings and debt. In particular, the level of savings might be affected by misreporting because the unemployed might fear that they may be used by the interviewers to check their entitlement to the means-tested social assistance benefit, SB. There is, however, no evidence in this sense.

The structure of the Chapter is the following. In Section 7.2, I review the existing theory (and evidence) and discuss the expected impact of financial resources on the re-employment probability. Next, in Section 7.3 the data are described. I present some descriptive analysis of the levels of savings and debt reported by the unemployed at different points in time. The results of estimation are discussed in Section 7.4. Alternative specifications of the savings and debt variables are tried out. The last Section concludes.

7.2 The theoretical framework

The literature on the relationship between the financial resources of the unemployed and the probability of leaving unemployment is very limited. In the simplest job search model, the assumption of risk neutrality of the job seeker prevents one from allowing non-labour income and financial wealth to affect the individual re-employment probability. If the assumption of risk neutrality is relaxed, one would expect that higher levels of financial wealth result in higher reservation wages and longer unemployment durations, for a given degree of risk aversion.

Kohn and Shavell (1974) and Pissarides (1974) argued (within the framework of job search theory) that the more the unemployed are risk averse the lower is their reservation wage. This implies a negative relationship between higher degrees of risk aversion and the duration of unemployment. Feinberg (1976) tested empirically this hypothesis by means of multiple regression analysis, using the US Panel Study of Income Dynamics. The author proxied risk aversion using replies to questions on conditions of car, having car insurance, use of seat belts, cigarette smoking, savings available. He concluded that risk aversion has a significantly negative impact on the expected duration of unemployment. The work of Feinberg is about the only empirical work on savings and unemployment duration of which I am aware. Other studies are for example MacKay and Reid (1972), who allowed for the impact of redundancy payments on the probability of leaving unemployment, although this was not their focus of interest. Their work relates to the UK.

The issue of savings and unemployment duration was instead explicitly tackled by Ioannides (1981), although from a different point of view than that of the present study. The author modelled the relation between sav-

ings and unemployment duration in a job search framework and focusing on the dissavings (savings) induced by unemployment (employment). Ioannides (1981) concluded that, under the assumption of a perfect capital market, the steady state rates of savings during periods of employment and of dissavings during periods of unemployment are independent of wealth and constant. In this model savings are an endogeneous variable and the capital market is assumed to be perfect.

More interesting for the purpose of the applied analysis carried out in this Chapter is the work of Danforth (1979), who relates the unemployed's decision to accept a given job offer to their financial endowments. Danforth (1979) develops a job search model in which the unemployed are assumed to maximize the utility they derive from consumption rather than from income. Within this framework, assuming additively separable utility function and decreasing absolute risk aversion, the author proves the following three propositions:

- higher levels of wealth result in lower acceptance probabilities, i. e. *"the rich are more selective"* (Danforth, 1979, p. 111);
- an increase in the level of wealth raises the expected duration of unemployment; i. e. *"the rich search longer"* (Danforth, 1979, p. 111);
- expected returns from search increase with increased search time, i. e. *"the rich get richer"* (Danforth, 1979, p. 111).

Danforth (1979) does not provide any empirical test of these propositions.

To sum up, according to the theoretical predictions the impact of financial wealth on the re-employment probability is negative, for a given degree of risk aversion. However, higher degrees of risk aversion result in a lower reservation wage and a shorter unemployment duration (Kohn and Shavell, 1974 and Pissarides, 1974).

I estimate here the impact of savings and debt on the re-employment probability. The level of savings represents a measure of the unemployed's financial wealth. However, it may also proxy risk aversion since more risk averse individuals are likely to save more. I allow for the impact of "once-off" payments such as redundancy payments on the individual-remplyment probability. These payments represent an (unexpected) increase in the unemployed's level of wealth. Following the second of Danforth's propositions, increases in the unemployed's level of wealth affect negatively the individual re-employment probability and result in longer expected unemployment durations. Instead, higher levels of debt may raise the unemployed's search intensity and lead to shorter unemployment durations (all things equal). However, debt may also proxy access to credit. The unemployed that have access to credit can afford to be more choosy about accepting job offers and may therefore have higher reservation wages (and longer expected unemployment durations).

7.3 A description of the data

7.3.1 Some descriptive analysis of the savings and debt of the unemployed

The LSUS survey is very rich in information on the financial situation of the unemployed. The principal objective of this survey was, as stated by the survey planners:

> *"to flesh out discussions of the financial situation of the unemployed people by looking at the implications of levels of income, savings and debts for the material living standards of the families concerned. A particular focus of interest was the extent to which living standards change during a spell of unemployment"* (Heady and Smith, 1989, p. 1).

The unemployed and their spouses were asked many questions on the types and amounts of savings accumulated or debt run up. The questions covered the situation one month before the commencement of the unemployment spell and after the commencement of the unemployment spell. I have constructed total savings and debt variables using the information contained in the survey as follows.

The *savings* of the unemployed at different points in time have been defined as the total amount of money held under any of the following forms:

- *a bank current account,*
- *a bank deposit account or bank savings account,*
- *a Post Office Giro account,*
- *a National Savings Bank account at the Post Office,*
- *a Trustee Savings Bank account,*
- *a building society account,*
- *stock shares or other securities,*
- *Premium bonds,*
- *a Christmas Club,*
- *any other form of savings.*

I have computed a separate variable for the amounts of any "once-off payments" such as redundancy payments or "pay in lieu of notice" received just before the commencement of the unemployment spell.

The total level of *debt* of the unemployed at different points in time was computed summing up debt run up under the following forms:

- *informal debt*, money owed to friends or relatives,
- *institutional debt*, money owed to
  - a *money lender, pawnbroker*,
  - a *bank, under a personal loan agreement and/or as an overdraft*,
  - a *finance house*,
  - a *credit card company*,
  - *any other person or organization.*
- *arrears debt*, defined as any arrears with
  - *mortgage payments, including any endowment policy on the mortgage*
  - *rent payments*
  - *rate, water rate or sewerage payments*
  - *gas and electricity bills*
  - *HP payments*
  - *insurance premiums*, excluding any endowment policy on mortgage,
  - *any other household bills, such as telephone bills*,
  - *any other regular payments.*

I have constructed a separate variable for the amount of mortgage capital outstanding, if any, at the first interview.

Summary descriptive statistics of the savings and debt reported by the unemployed are presented below. I consider two times for the purpose of comparison: one month before the commencement of the unemployment spell and at the first interview. I look at the net change in the level of individual resources passing from one point in time to the next. Next, the resources reported by the spouse (if any) of the unemployed are analysed.

Some unemployed refused to reply to questions concerning their savings and/or debt. These were very few unemployed (less than 1% of the sample) and they are coded as if they had reported zero amounts. Some unemployed that reported extremely large amounts of savings or debt (greater than 6 figures in £) were coded by the survey planners as "-2". They are also very few (2 cases) and I have recoded them as if reporting zero amounts[1]. Overall the unemployed in these two categories represent much less than 1% of the sample, at any time. There should be no large (additional) error introduced since it is not possible to exclude that some of the unemployed that reported zero amounts of savings were actually misreporting larger amounts.

[1]They are however excluded from the econometric analysis which is carried out in Chapter 7.

Table 7.1: *The amounts of savings before and after the commencement of the unemployment spell*

upper bounds amounts reported in £	*Percentage of the unemployed that reported such amounts*					
	One month before U			*At the first interview*		
	%	*% (*)*	*cum. % (*)*	*%*	*% (*)*	*cum. % (*)*
0	*38.2*			*42*		
≤ 10	*8.5*	*13.8*	*13.8*	*13.2*	*22.8*	*22.8*
≤ 100	*11.3*	*18.3*	*32.1*	*11.1*	*19.2*	*42*
≤ 300	*10.6*	*17.2*	*49.3*	*7*	*12*	*54*
≤ 500	*5.9*	*9.6*	*58.9*	*2.7*	*4.7*	*58.7*
≤ 1000	*6*	*9.7*	*68.6*	*5.5*	*9.5*	*68.2*
≤ 1500	*3.6*	*5.8*	*74.4*	*3.1*	*5.3*	*73.5*
≤ 2000	*3.4*	*5.5*	*79.9*	*2.3*	*4*	*77.5*
≤ 3000	*2.8*	*4.5*	*84.4*	*2*	*3.5*	*81*
≤ 4000	*2.3*	*3.7*	*88.1*	*2.2*	*3.8*	*84.8*
≤ 5000	*1.6*	*2.6*	*90.7*	*1.2*	*2*	*86.8*
≤ 10000	*2.3*	*3.7*	*94.4*	*2.8*	*4.8*	*91.6*
≤ 15000	*1.1*	*1.9*	*96.3*	*1.6*	*2.8*	*94.4*
≤ 20000	*0.7*	*1.2*	*97.5*	*0.9*	*1.6*	*96*
≤ 25000	*0.6*	*1*	*98.5*	*1.1*	*1.8*	*97.8*
≤ 50000	*0.6*	*1*	*99.5*	*0.9*	*1.6*	*99.4*
≥ 50000	*0.3*	*0.5*	*100*	*0.4*	*0.6*	*100*

The table relates to the subsample of male participants in both sample interviews (2035 units, see Section 2.4.1). The (*) indicates that the percentage is taken over the observations that report positive savings.

The amounts of total savings reported by the unemployed (not considering the amounts reported by their spouses) one month before the commencement of the unemployment spell and about three months into the spell are described in table 7.1. About 38% of the unemployed report having no savings one month before the commencement of the unemployment spell. About 8.5% report small positive amounts of savings of less than £10, at the same date. These small amounts of savings reflect probably transaction balances held in accounts rather than in the pocket. About 11% of the unemployed report positive amounts of savings larger than £10 and smaller than £100, one month before the commencement of the unemployment spell. Almost 11% report positive amounts larger than £100 and smaller than £300. Almost 6% report positive amounts larger than £300 and less than £500. Overall, almost 35% of the unemployed report positive amounts of savings of less than £500 one month before the commencement of the unemployment spell.

The unemployed with savings larger than £500 and less than £1000 are 6%. Almost 10% of the unemployed report savings larger than £1000 and less than £3000. The reader should perhaps be reminded that £3000 correspond to the threshold level of the savings of the nuclear family below which the unemployed would gain entitlement to the means-tested unemployment benefit in 1982 (if they had passed the income test)[2]. Overall, about 50%

[2]The award of SB is conditional on passing both an income test and an assets test. The reference period is taken to be 1982 since all the information on benefit receipt at the

of the unemployed report positive amounts of savings of less than £3000, one month before the commencement of the unemployment spell. Including also the unemployed reporting no savings at the same date, the corresponding figure becomes about 90%. The proportion of the unemployed reporting amounts of savings larger than £3000 is about 10% one month before the commencement of the unemployment spell.

Almost half (46.9%) of the unemployed with positive amounts of savings at the first interview report savings of less than £3000. Overall (including the zero amounts) 89% of the unemployed report amounts of savings lower than £3000 at the first interview.

About four percent less of the unemployed report having any savings after the commencement of their unemployment spell: the percentage of the unemployed that report no savings (of any type) is 38.2% one month before the commencement of the unemployment spell and 42% three months into the spell. Instead, the number of the unemployed with savings of less than £10 increases by about 5% at the first interview (three months into the unemployment spell). The number of the unemployed with savings greater than £100 but less £5000 decreases slightly at the first interview relative to one month before the commencement of the observed unemployment spell. Instead, the number of the unemployed with savings larger than £5000 goes up by about 2%. This is probably explained by the receipt of redundancy or severance payments.

Table 7.2: *The amounts of once-off payments*

Upper bounds	*Percentage of the unemployed that reported such amounts*		
in £	*%*	*% (*)*	*cum. % (*)*
0	*56.4*		
≤ 10	*0.1*	*0.2*	*0.2*
≤ 100	*8.8*	*20.1*	*20.3*
≤ 300	*11.1*	*25.5*	*45.8*
≤ 500	*3.7*	*8.5*	*54.3*
≤ 1000	*4.1*	*9.4*	*63.7*
≤ 1500	*2.3*	*5.3*	*69.0*
≤ 2000	*1.6*	*3.7*	*72.7*
≤ 3000	*2.4*	*5.4*	*78.1*
≤ 4000	*1.8*	*4.0*	*82.1*
≤ 5000	*1.7*	*3.8*	*85.9*
≤ 10000	*3.0*	*6.7*	*92.6*
≤ 15000	*1.1*	*2.5*	*95.1*
≤ 20000	*1.1*	*2.5*	*97.6*
≥ 20000	*1.1*	*2.5*	*100*

The table relates to the subsample of male participants in both sample interviews (2035 units, see Section 2.4.1). The (*) indicates that the percentage is taken over the non-zero observations.

first interview is collected in 1982 amounts, which were in force until November 1983.

Descriptive statistics of the amounts of redundancy payments and other "once-off" payments are given in Table 7.2. These payments are due to the ending of a work contract: they go from redundancy and severance payments to pay in lieu of notice and "week in hand". About 43% of the sample report to have received some "once-off" payment just before the commencement of their unemployment spell. However, the majority (54%) of the unemployed with positive "once-off' payments reports payments of less than £500. These small amounts are "week in hand" or "pay in lieu of notice" payments. Some of the unempoyed report instead much larger amounts. For instance, about 6% (of those that report positive "once-off" payments) report amounts larger than £10000. These large amounts are redundancy or severance payments.

The total amounts of debt run up with friends, relatives, financial institutions and being in arrears with one's payments are shown in Table 7.3. Almost 50% of the unemployed report some debt one month before the beginning of their unemployment spell and almost 60% report some debt three months into the unemployment spell (at the time of the first interview). The majority of the unemployed that are in debt owes amounts of money not larger than £500, at the two times considered. About 90% of them reports amounts of debt not larger than £3000 and about 95% not larger than £5000, at any time considered.

Table 7.3: *The amounts of total debt run up*

Upper bounds amounts reported in £	*Percentage of the unemployed that reported such amounts*					
	One month before U			*At the first interview*		
	%	*% (*)*	*cum. % (*)*	*%*	*% (*)*	*cum. % (*)*
0	*53.7*			*41.6*		
≤ 10	*0.3*	*0.7*	*0.7*	*1.3*	*2.3*	*2.3*
≤ 100	*6.2*	*17.7*	*18.4*	*13.7*	*23.5*	*25.8*
≤ 300	*11.4*	*24.6*	*43.0*	*13.8*	*23.6*	*49.4*
≤ 500	*6.3*	*13.6*	*56.6*	*6.9*	*11.8*	*61.2*
≤ 1000	*8*	*17.3*	*73.9*	*10.5*	*18.0*	*79.2*
≤ 1500	*3.6*	*8.2*	*82.1*	*3.5*	*6.0*	*85.2*
≤ 2000	*2*	*4.3*	*86.4*	*2.4*	*4.1*	*89.3*
≤ 3000	*2.5*	*5.4*	*91.8*	*2.5*	*4.2*	*93.5*
≤ 4000	*1.2*	*2.6*	*94.4*	*1.3*	*2.2*	*95.7*
≤ 5000	*0.7*	*1.5*	*95.9*	*0.7*	*1.2*	*96.9*
≤ 10000	*0.9*	*1.9*	*97.8*	*0.9*	*1.5*	*98.4*
≤ 15000	*0.4*	*0.9*	*98.7*	*0.4*	*0.7*	*99.1*
≤ 20000	*0.2*	*0.4*	*99.1*	*0.1*	*0.2*	*99.3*
≤ 25000	*0.1*	*0.2*	*99.3*	*0.1*	*0.2*	*99.5*
≤ 50000	*0.3*	*0.7*	*100*	*0.3*	*0.5*	*100*
≥ 50000	*0*	*0*	*100*	*0*	*0*	*100*

The table relates to the male participants in both sample interviews. The (*) indicates that the percentage is taken over the non-zero's observations. Total debt is defined as the sum of informal, institutional and arrears debt. The debt run by the spouses is not taken into account in this table.

Next, I look at the amounts of debt of different types reported. I distinguish informal debt (with friens and relatives) from institutional debt (with banks or other financial institutions) and arrears with payments. In table 7.4, I show the amounts of informal and institutional debt reported by the unemployed. About 11% of the unemployed owe money to friends and/or relatives one month before the commencement of their unemployment spell. The corresponding figure three months into the spell is almost 20%. The majority of the unemployed that report owing some money to friends and/or relatives, have borrowed amounts of less than £300 (respectively, 56.4% one month before unemployment and 68.2% about three months into the spell). About 90% of the unemployed with "informal debt" have borrowed from friends and relatives sums of not more than £1000. Overall, the amounts of debt run up with friends and relatives are quite small. However, the number of the unemployed in debt with their friends and relatives increases (almost doubles) three months into the unemployment spell relative to one month before the commencement of the unemployment spell.

The number of the unemployed that run "institutional debt" is larger than the number of the unemployed in "informal debt". The number of the unemployed in debt with some financial institutions is about 31% one month before the commencement of the unemployment spell. The same figure is about 35%, after three months of unemployment, at the first survey interview. However, the majority (about 53% one month before the commencement of the unemployment spell and about 58% three months into the spell) of those reporting some debt with financial institutions owes sums not larger than £500. About 70% of them report owing sums not larger than £1000 and about 90%, not larger than £3000.

The amounts of arrears with payments reported are illustrated in table 7.5. The proportion of the unemployed that reports to be in arrears with some payments is about 19% one month before the beginning of the unemployment spell. The corresponding figure three months into the spell is about 34% . About 40% of the unemployed are in arrears with their payments for not more than £100 and about 80% for not more than £300, at the two points in time. Very few unemployed are in arrears with their payments for more than £1000, at the two times considered. From the table, it emerges that the number of people that are in arrears with some payments (see list of arrears payments above) is 15% higher, passing from one month before the commencement of the unemployment spell to three months into the spell. The number of the unemployed that are in arrears with their payments is almost doubled three months after the commencement of their unemployment spells.

Table 7.4: *Institutional and informal debt before and after the commencement of the unemployment spell*

INFORMAL DEBT	*Percentage of the unemployed that reported such amounts*					
Upper bounds,	*One month before U*			*At the first interview*		
in £	*%*	*% (*)*	*cum. % (*)*	*%*	*% (*)*	*cum. % (*)*
0	*89*			*79.9*		
≤ 10	*0.2*	*1.8*	*1.8*	*1.5*	*7.5*	*7.5*
≤ 100	*2.6*	*23.6*	*25.4*	*7.2*	*35.8*	*43.3*
≤ 300	*3.4*	*31*	*56.4*	*5*	*24.9*	*68.2*
≤ 500	*1.7*	*15.5*	*71.9*	*2.6*	*12.9*	*81.1*
≤ 1000	*1.7*	*15.5*	*87.4*	*2.1*	*10.4*	*91.5*
≤ 1500	*0.4*	*3.6*	*91*	*0.5*	*2.5*	*94.0*
≤ 2000	*0.3*	*2.7*	*93.7*	*0.3*	*1.5*	*95.5*
≤ 3000	*0.3*	*2.7*	*96.4*	*0.4*	*2*	*97.5*
≤ 4000	*0*	*0*	*96.4*	*0.2*	*1*	*98.5*
≤ 5000	*0.3*	*2.7*	*99.1*	*0.2*	*1*	*99.5*
≤ 10000	*0.1*	*0.9*	*100*	*0.1*	*0.5*	*100*
INSTITUTIONAL DEBT	*Percentage of the unemployed that reported such amounts*					
Amounts reported	*One month before U*			*At the first interview*		
in £	*%*	*% (*)*	*cum. % (*)*	*%*	*% (*)*	*cum. % (*)*
0	*68.9*			*65.3*		
≤ 10	*0.5*	*1.6*	*1.6*	*1.3*	*3.7*	*3.7*
≤ 100	*4.5*	*14.5*	*16.1*	*6.4*	*18.4*	*22.1*
≤ 300	*6.6*	*22.0*	*38.1*	*8*	*23*	*45.1*
≤ 500	*4.6*	*14.8*	*52.9*	*4.7*	*13.6*	*58.7*
≤ 1000	*5.6*	*18.0*	*70.9*	*5.8*	*16.7*	*75.4*
≤ 1500	*2.4*	*7.3*	*78.2*	*2.6*	*7.5*	*82.9*
≤ 2000	*2*	*6.4*	*84.6*	*1.8*	*5.2*	*88.1*
≤ 3000	*1.6*	*5.8*	*90.4*	*1.5*	*4.3*	*92.4*
≤ 4000	*0.7*	*2.3*	*92.7*	*0.7*	*2.0*	*94.4*
≤ 5000	*0.4*	*1.3*	*94.0*	*0.4*	*1.2*	*95.6*
≤ 10000	*0.7*	*2.3*	*96.3*	*0.7*	*2*	*97.6*
≤ 15000	*0.4*	*1.3*	*97.7*	*0.3*	*0.9*	*98.5*
≤ 20000	*0.2*	*0.7*	*98.4*	*0.1*	*0.3*	*98.8*
≤ 25000	*0.1*	*0.3*	*98.7*	*0.1*	*0.3*	*99.1*
≤ 50000	*0.3*	*1*	*99.7*	*0.3*	*0.9*	*100*
≥ 50000	*0.1*	*0.3*	*100*	*0*	*0*	*100*

The table relates to the male participants in both sample interviews. The (*) indicates that the percentage is taken over the non-zero's observations. Informal debt is defined as the debt run up with friends and/or relatives. Institutional debt is defined as above as debt run up with some financial institutions such as banks, building societies, etc. etc. .

Table 7.5: *The amounts of arrears debt before and after the commencement of the unemployment spell*

Upper bounds amounts reported in £	*Percentage of the unemployed that reported such amounts*					
	One month before U			*At the first interview*		
	%	*% (*)*	*cum. % (*)*	*%*	*% (*)*	*cum. % (*)*
0	*81.2*			*66.6*		
≤ 10	*0.3*	*1.6*	*1.6*	*1.1*	*3.3*	*3.3*
≤ 100	*7.4*	*39.4*	*41.0*	*14.8*	*44.3*	*47.6*
≤ 300	*6.8*	*36.2*	*77.2*	*11.2*	*33.5*	*81.1*
≤ 500	*1.9*	*10.1*	*87.3*	*2.6*	*7.8*	*88.8*
≤ 1000	*1.7*	*9*	*96.3*	*2.8*	*8.4*	*97.2*
≤ 1500	*0.5*	*2.7*	*99.0*	*0.5*	*1.6*	*98.8*
≤ 2000	*0.1*	*0.5*	*99.5*	*0.2*	*0.6*	*99.4*
≤ 3000	*0.1*	*0.5*	*100*	*0.2*	*0.6*	*100*

The table relates to the male participants in both sample interviews. The (*) indicates that the percentage is taken over the non-zero's observations. Arrears debt is defined as above as arrears with any types of payments. Debt run by the spouses is not taken into account here.

In Table 7.6, I compare the unemployed's "net savings" at the time of the first survey interview and one month before the commencement of the unemployment spell. I have computed the "net savings" of the unemployed at a given point in time by subtracting the total debt from the total savings, for the unemployed that reported positive amounts of savings or debt. The proportion of the unemployed that reported either non-zero amounts of savings or non-zero amounts of debt one month before the commencement of the unemployment spell is 80%. The corresponding figure at the first survey interview is 86.3% and at the second survey interview 78.9 %. The unemployed that reported no savings nor debt at a given point in time have zero net balances. They are not included in the table below.

One month before the commencement of the unemployment spell, about 35% of the unemployed have negative net balances. About 45% have positive balances at the same date. About 20% of them have zero net balances, as already mentioned —this figure is not reported in the table. About 6% of the unemployed is in (net) debt for less than £100 one month before the commencement of the sampled unemployment spell. About 13% is in (net) debt for amounts larger than £100 and smaller than £500. About 6% is in debt for more than £500 and less than £1000. Overall, about 25% of the unemployed has negative (net) balances of less than £1000, one month before the commencement of their unemployment spell. Almost 10% of the unemployed has negative (net) balances of more than £1000, at the same date.

About 10% of the unemployed has positive net balances of less than £100, one month before the commencement of the their unemployment spell. About 11% has positive net balances larger than £100 and smaller than £500. About 5% has positive net balances larger than £500 and less than £1000. Overall, about 26% of the unemployed has positive net balances smaller than

£1000, one month before the commencement of the unemployment spell. Almost 9% has positive net balances larger than £1000 and smaller than £3000. Overall, about 17% of the sample has positive net balances larger than £1000.

The number of the unemployed in (net) debt is about 11% higher at the time of the first interview relative to one month before the commencement of the their unemployment spell. The proportion of the unemployed with (net) debt of more than £5000 is not much different at the two points in time. Instead, the number of the unemployed with debt of less than £1000 has grown considerably (by about 11%) after the commencement of the unemployment spell. In particular, the proportion of the unemployed that are in debt for small amounts of money, of less than £100, has almost doubled.

Table 7.6: *The amounts of net savings before and after the commencement of the unemployment spell*

Upper bounds amounts reported in £	*Percentage of the unemployed that reported such amounts*					
	One month before U			*At the first interview*		
	%	*% (*)*	*cum. % (*)*	*%*	*% (*)*	*cum. % (*)*
≤ -10000	*0.6*	*1*	*1*	*0.8*	*0.9*	*0.9*
≤ -5000	*0.9*	*1.25*	*2.2*	*0.9*	*1.0*	*1.9*
≤ -1000	*8.2*	*10.2*	*12.4*	*9.6*	*11.1*	*13.0*
≤ -500	*6.4*	*8.0*	*20.4*	*8.7*	*10.1*	*23.1*
≤ -100	*13.6*	*17.0*	*37.4*	*17.2*	*19.9*	*43.0*
< 0	*6*	*7.5*	*44.9*	*11.6*	*13.4*	*56.5*
≤ 100	*10.4*	*13.0*	*57.9*	*9.0*	*10.4*	*66.9*
≤ 500	*11.3*	*14.1*	*72.0*	*6.5*	*7.5*	*74.4*
≤ 1000	*4.6*	*6.1*	*78.1*	*4.4*	*5.1*	*79.5*
≤ 3000	*6.7*	*10.6*	*88.9*	*7.1*	*8.2*	*87.7*
≤ 5000	*3.4*	*4.2*	*92.1*	*2.9*	*3.4*	*91.1*
≤ 10000	*2.1*	*2.6*	*94.7*	*2.6*	*3.0*	*94.1*
≥ 10000	*3.4*	*4.2*	*100*	*4.9*	*5.7*	*100*

The table relates to the male participants in both sample interviews. The (*) indicates that the percentage is taken over the non-zero's observations. Net savings are equal to total savings minus total debt, at a given point in time. The unemployed that reported both zero amounts of savings and zero amounts of debt (at a certain time) are not included in the Table. These were 406 one month before the commencement of the unemployment spell and 261 at the time of the first survey interview.

The proportion of the unemployed with positive net savings has gone down except for the unemployed that have (net) savings larger than £10000, whose number has slightly increased. This slight increase in the number of the unemployed with net savings of more than £10000 is likely to be due to the receipt of redundancy or severance payments. In Table 7.2, I show that about 3% of the unemployed received some "once-off" payments larger than £3000. This evidence seems to indicate that overall the financial situation of the unemployed has deteriorated since the commencement of their unemployment spell except for the unemployed that received some "once-off" payments larger than £10000. Some evidence on the change in the individual level of net financial resources of the unemployed at the two points in time is presented in Table 7.8 below.

In Table 7.7, I distinguish two groups of the unemployed: the unemployed that exited from unemployment before the time of the second interview (with completed unemployment spells) and the unemployed that were still unemployed at the second interview (with right-censored unemployment spells). I compare their "net savings" at the first survey interview.

Table 7.7: *The net savings of the unemployed with completed or right-censored unemployment spells*

Upper bounds amounts reported in £	*Percentage of the unemployed that reported such amounts*					
	Completed spells			*Right-censored spells*		
	%	*% (*)*	*cum. % (*)*	*%*	*% (*)*	*cum. % (*)*
≤ -10000	*0.7*	*0.8*	*0.8*	*1.0*	*1.2*	*1.2*
≤ -5000	*1.0*	*1.1*	*1.9*	*0.7*	*0.8*	*2.0*
≤ -1000	*10.4*	*11.9*	*13.8*	*8.4*	*10.0*	*12.0*
≤ -500	*8.7*	*9.9*	*23.7*	*8.7*	*10.4*	*22.4*
≤ -100	*17.0*	*19.4*	*43.1*	*17.7*	*21.1*	*43.5*
< 0	*9.7*	*11.0*	*54.1*	*14.2*	*16.9*	*60.4*
≤ 100	*10.0*	*11.4*	*65.5*	*7.5*	*8.9*	*69.3*
≤ 500	*8.1*	*9.2*	*74.7*	*4.1*	*4.9*	*74.2*
≤ 1000	*4.7*	*5.4*	*80.1*	*4.0*	*4.8*	*79.0*
≤ 3000	*7.8*	*8.9*	*89.0*	*6.2*	*7.4*	*86.4*
≤ 5000	*3.2*	*3.6*	*92.6*	*2.5*	*3.0*	*89.4*
≤ 10000	*2.4*	*2.7*	*95.3*	*2.9*	*3.4*	*92.8*
≥ 10000	*4.0*	*4.6*	*100*	*6.1*	*7.3*	*100*

The table relates to the male participants in both sample interviews. The unemployed with completed spells of unemployment are 1210. The unemployed with right-censored unemployment spells are 825. The (*) indicates that the percentage is taken over the non-zero's observations. Net savings are equal to total savings minus total debt, at a given point in time. The unemployed that reported both zero amounts of savings and zero amounts of debt (at a certain time) are not included in the Table. These were 149 of the unemployed with completed spells and 132 of the unemployed with right-censored spells. The amounts reported by the spouses are not considered in this table.

The unemployed with completed unemployment spells are 1210 and about 88% of them reported non-zero amounts of savings or debt at the time of the first interview. The unemployed with right-censored unemployment spells are 825 and about 84% of them reported non-zero amounts of savings or debt. The unemployed with zero net balances do not figure in the table below. They are 12% of the unemployed with completed spells and 16% of the unemployed with right-censored spell.

From Table 7.7, it emerges that the distribution of the "net savings" of the unemployed in the two groups does not differ substantially. The proportion of the unemployed with (net) debt is slightly higher among the right-censored group: about 60% of the right-censored group and about 54% of the completed spells group have negative assets balance at the time of the first survey interview. However, the proportion of the right-censored unemployed with net savings larger than £5000 is larger than that of the unemployed with completed unemployment spells.

Table 7.8: *The change in net savings of the unemployed over time*

Upper bounds	*Percentage of the unemployed that reported such amounts*			
Absolute change	*net change t1*		*net change t2*	
in £	*%*	*cum. %*	*%*	*cum. %*
≥ - 5000	1.0	1.0	4.1	4.1
≥ - 3000	1.3	2.3	2.9	7.0
≥ - 1000	8.6	10.9	9.3	16.3
≥ - 500	7.6	18.5	6.7	23.0
≥ - 100	20.8	39.3	14.3	37.3
≥ - 10	12.7	52.0	8.5	45.8
< 0	1.5	53.5	7.6	53.4
0	17.7	71.2	0.6	54.0
≤ 10	1.5	72.7	2.0	56.0
≤ 100	8.3	81.0	10.2	66.2
≤ 500	7.7	88.7	15.4	81.6
≤ 1000	2.8	91.5	7.4	89.0
≤ 3000	3.2	94.7	6.3	95.3
≤ 5000	1.5	96.2	1.5	96.8
≥ 5000	3.8	100.0	3.2	100.0

The table relates to the male participants in both sample interviews. The times tk, t1, t2 relate respectively to one month before the commencement of the unemployment spell, to the time of the first interview and to the time of the second interview. Net savings are equal to total savings minus total debt, at a given point in time. The absolute change in net savings at the first interview (t1) is computed subtracting the net savings at time tk from the net savings at time t1. Similarly, the absolute change in net savings at the second interview (t2) is computed subtracting the net savings at time t1 from the net savings at time tk.

Next, I look at the individual change in the level of net savings passing from one month before the commencement of the unemployment spell to the time of the first interview (three months into the spell) and from the time of the first interview to the time of the second interview (fifteen months into the spell). The results are summarized in Table 7.8.

About 53% of the unemployed experience a reduction in the level of their net financial resources passing from one month before the commencement of the unemployment spell to three months into the spell. About 1% of the unemployed sees their net financial balances decrease by less than £10. About 13% loses between 10 and 100 £. About 21% loses between 100 and 500 £. About 7% loses between 500 and 1000 £and about 8% loses between 1000 and 3000 £. About 2% loses more than £3000. Almost 18% experiences no change in the level of their net financial balances, passing from one month before the commencement of their unemployment spell to three months into the spell.

Almost 29% sees their financial resources increase after the commencement of their unemployment spell. However, almost 10% gains less than £100. Almost 8% gains between 100 and 500 £. Almost 3% gains between 500 and 1000 £and about 3% gains between 1000 and 3000 £. About 5% experience an increase of more than £3000 in the level of net financial balances. The increases in the level of net financial balances are explained in large part by the receipt of "once-off" payments associated with the ending of a previous work contract and the commencement of the observed spell of unemployment, as discussed above.

Table 7.9: *The total family savings*

Upper bounds amounts reported in £	*Percentage of the unemployed that reported such amounts*					
	Married people			*Single people*		
	%	*% (*)*	*cum. % (*)*	*%*	*% (*)*	*cum. % (*)*
0	*34.1*			*39.6*		
≤ 10	*1.4*	*2.1*	*2.1*	*12.3*	*20.3*	*20.3*
≤ 100	*28.3*	*41.9*	*44.0*	*9.6*	*15.9*	*36.1*
≤ 500	*11.4*	*17.3*	*61.2*	*10.0*	*16.5*	*52.6*
≤ 1000	*5.5*	*8.3*	*69.5*	*10.0*	*16.5*	*69.1*
≤ 2000	*6.1*	*9.2*	*78.7*	*6.6*	*10.9*	*80.0*
≤ 3000	*3.2*	*4.8*	*83.5*	*2.3*	*3.8*	*83.8*
≤ 5000	*2.8*	*4.2*	*87.7*	*2.2*	*3.7*	*87.5*
≤ 10000	*3.2*	*4.8*	*92.5*	*2.2*	*3.7*	*91.2*
≤ 20000	*2.7*	*4.0*	*96.5*	*3.3*	*5.5*	*96.7*
≤ 50000	*2.2*	*3.3*	*99.3*	*1.2*	*2.0*	*98.7*
≥ 50000	*0.5*	*0.7 0*	*100*	*0.7*	*1.2*	*100*

The table relates to the time of the first interview. The family savings are equal to the sum of the savings of the unemployed and their spouses, for the unemployed that were married at the time of the first interview. The family savings are equal to the total savings of the unemployed for the unemployed that were single persons at the first interview. The married unemployed at the first interview were 1765. The single unemployed were 270. The (*) indicates that the percentage is taken over the non-zero's observations.

The proportion of the unemployed that see their net financial balances decrease passing from the first to the second survey interview is about 53%. This figure is almost identical to the corresponding figure for the change in net financial resources passing from one month before to three months into the unemployment spell. The proportion of the unemployed that experience no change in the level of their financial resources is now much smaller and equal to less than 1%. About 34% of the sample sees their financial resources increase passing from the first survey interview to the second. About 12% of the unemployed experience an increase of less than £100 in the level of their net financial balances. About 15% gains between 100 and 500 £and about 7% gains between 500 and 1000 £. About 6% sees their net financial resources go up by more than £1000 and less than £3000. About 5% gains more than £3000.

Overall, the number of the unemployed that sees their net financial balances increase passing from the first to the second survey interview is higher than the corresponding number from one month to three months into the spell. This result is simply due to the fact that a large number of the unemployed have gone back to work between the first and the second survey interview. In general, it is possible to conclude that the financial resources of the unemployed change considerably during the course of the unemployment spell. The largest number of the unemployed sees their net financial balances go down during the course of the unemployment spell.

In Table 7.9, I compare the savings and debt of the (married) unemployed and their spouses with those of the single persons in the sample, at the first interview. It is to be remembered that in the LSUS survey the unemployed are defined as "married" if they are "either married or cohabiting" with their partners. I compare the total family savings of the unemployed, which were computed adding the spouse's savings to the unemployed savings.

The number of families that report zero savings at the first interview is slighlty higher among the unemployed that are not married. About 39% of the married unemployed report amounts of family savings different from zero. The corresponding figure for the single people is about 34%. This seems plausible since the spouse's savings are taken into account in the figures for the married people. About 20%. of the single people that report positive amounts of savings have savings of less than £10. The proportion of married people with savings of less than £10 is, instead, much smaller and equal to about 2%. However, about the same proportion (about 83% of the positive replies) in both groups of married and single people report savings of less than £3000. Overall, the distribution of savings of the two groups of the unemployed does not appear particularly different.

The total family debt for the two groups is compared in Table 7.10. The proportion of married unemployed that reports some debt at the first interview is slightly larger than that of single people. About 37% of the married people report zero amounts of debt. The same figure for the single people is about 42%. The distribution of the amounts of debt owed is very similar for

Table 7.10: *The total family debt*

Upper bounds	*Percentage of the unemployed that reported such amounts*					
amounts reported	*Married people*			*Single people*		
in £	*%*	*% (*)*	*cum. % (*)*	*%*	*% (*)*	*cum. % (*)*
0	*37.5*			*42.6*		
≤ 10	*2.5*	*4.0*	*4.0*	*1.8*	*3.1*	*3.1*
≤ 100	*13.9*	*22.2*	*26.2*	*14.5*	*25.2*	*28.4*
≤ 500	*21.4*	*34.2*	*60.4*	*20.4*	*35.5*	*63.9*
≤ 1000	*12.1*	*19.3*	*79.7*	*9.6*	*16.7*	*80.6*
≤ 2000	*6.5*	*10.3*	*89.9*	*4.8*	*8.3*	*88.9*
≤ 3000	*2.8*	*4.4*	*94.3*	*3.0*	*5.3*	*94.2*
≤ 5000	*2.1*	*3.4*	*97.7*	*1.1*	*1.9*	*96.1*
≤ 10000	*1.0*	*1.5*	*99.2*	*1.1*	*1.9*	*98.0*
≥ 10000	*0.7*	*0.9 0*	*100*	*1.1*	*1.9*	*100*

The table relates to the time of the first interview. The family debt is equal to the sum of the debt of the unemployed and their spouses, for the unemployed that were married at the time of the first interview. The family debt is equal to the total debt of the unemployed for the unemployed that were not married at the first interview. The unemployed married at the first interview were 1765. The unemployed not married were 270. The (*) indicates that the percentage is taken over the non-zero's observations.

the two groups of the unemployed. For instance, the proportion of married people with debt less than £500 is about 26%. The same figure for single people is about 28%. The proportion of the unemployed with debt of less than £3000 is the same for both groups (about 94%). Marital status does not seem to affect substantially the amount of debt run by the unemployed.

7.3.2 Descriptive statistics of the explanatory variables

Descriptive statistics of the explanatory variables are given in Table 7.11 below for the full sample considered (2030 observations) and for the unemployed with positive amounts of savings or debt (1629 observations). The reader is referred to Section 5.2 for a definition of the explanatory variables. I discuss below the specification of the savings and debt variables. Some unemployed and/or their spouses reported very large amounts of savings/debt, of more than six figures in pounds. In the survey, the amounts reported by these persons were coded as "-2" rather than the actual reported amount. Since it turns out that only five unemployed and/or their spouses reported such large amounts of savings and/or debt, these cases are dropped from the econometric analysis carried out in this Chapter. Indeed, they might have been misreporting their savings/debt, but even if they reported the true amounts, they would anyway be too few to be representative of the incredibly rich or incredibly poor unemployed.

The levels of savings and debt are measured one month before the com-

mencement of the unemployment spell to avoid potential endogeneity problems. Indeed, the level of the financial resources of the unemployed may vary during the course of the unemployment spell as a function of the duration of the unemployment spell. As a consequence, the savings and debt of the unemployed at the first (or the second) survey interview might be endogeneous to the model.

About 80% of the sample reported positive amounts of debt and/or savings one month before the commencement of their unemployment spell (see Table 7.6). I find that the distribution of savings and debt of the unemployed is very skewed, with some unemployed reporting, for example, amounts of (total) savings or (total) debt of less than £10 and some unemployed reporting amounts larger than £10000. Some unemployed did not report any amounts of savings and/or debt or refused to reply to these questions. It is of course possible, as already discussed above that these people did not reply sincerely to the questions. Sensitivity of the results of estimation of the econometric model to the exclusion/inclusion of these observations is checked.

From Table 7.11, it emerges that the unemployed that report positive amounts of savings or debt have higher mean expected earnings (called "predicted" earnings in the Table) than the full sample, which includes the unemployed that report zero amounts of savings or debt. No other substantial differences emerge between the two groups (except for differences in the mean levels of savings and debt).

The savings and debt variables are entered separately into the model. The expected impact of higher levels of savings is to raise the unemployed's reservation wage by making the unemployed more "choosy" about accepting job offers (for a given degree of risk aversion). The expected impact of savings on the re-employment hazard rate is, therefore, negative. However, savings may also proxy risk aversion since the more risk averse individuals will tend to save more. Higher degrees of risk aversion are expected to result in lower reservation wages and shorter unemployment durations. If savings proxy risk aversion, then higher level of savings will be associated with shorter unemployment duration.

The expected impact of debt is not clearcut either. Higher levels of debt may lower the unemployed's reservation wage and result in shorter unemployment durations. However, debt may also proxy access to credit. In this case, the expected impact of higher levels of debt on the hazard rate is similar to the expected impact of higher levels of savings (for a given degree of risk aversion). The unemployed that can borrow more can also afford to be more choosy about accepting job offers. If debt proxies access to credit, the expected impact of higher levels of debt on the hazard rate is negative.

The savings and debt variables are specified in levels. A logarithmic specification does not seem appropriate since it would imply that proportional increases in these variables have equi-proportionate effects on the hazard. It seems plausible that a 100% increase in debt (or savings) has a different

impact if debt (or savings) increases, for instance, from £500 to £1000 than from £10 to £20. This choice is supported by the fact that the distributions of savings and debt are very skewed. However, savings and debt will be entered in logs for the purpose of sensitivity analysis.

The "once-off" payments associated with the ending of the previous work contract and the commencement of the unemployment spell may be seen as representing an (unexpected) increase in the level of savings. The expected impact of this variable on the hazard rate is negative. This variable is entered in levels for the same reasons given above. A logarithmic specification is also tried out.

The savings and debt variables considered relate to the unemployed person. In the econometric analysis, I test also for the significance of the family's savings and debt. These are equal to the sum of the unemployed's savings (or debt) and the spouse's savings (or debt).

Table 7.11: *Descriptive statistics of the economic variables*

	Full sample		*Non zero wealth*	
Variable	*Mean*	*SD*	*Mean*	*SD*
Left truncation period	*13.407*	*1.057*	*13.395*	*1.040*
Unemployment duration (weeks)	*44.588*	*17.755*	*43.600*	*17.718*
F/t work most part year before U.	*.653*	*.476*	*.692*	*.462*
Unemployed most part year before U.	*.222*	*.415*	*.193*	*.395*
Sick, no work most part year before U.	*.036*	*.187*	*.029*	*.169*
Professional Occupation	*.019*	*.137*	*.021*	*.145*
Intermediate Occupation	*.154*	*.361*	*.172*	*.378*
Unskilled Occupation	*.058*	*.234*	*.048*	*.215*
Occupation not available	*.071*	*.256*	*.056*	*.230*
Age 20-24	*.124*	*.329*	*.112*	*.316*
Age 25-34	*.323*	*.468*	*.319*	*.466*
Age 35-44	*.248*	*.432*	*.249*	*.432*
Age 45-54	*.198*	*.398*	*.202*	*.402*
Age 55-58	*.108*	*.310*	*.118*	*.323*
Has any child old less than 5	*.341*	*.474*	*.328*	*.470*
Married	*.867*	*.339*	*.875*	*.330*
Spouse working 1 month before U.	*.269*	*.443*	*.295*	*.456*
Searches less than before	*.096*	*.294*	*.096*	*.295*
Values Leisure more than Labour	*.137*	*.344*	*.142*	*.350*
experiences some shortage of money	*.733*	*.443*	*.726*	*.446*
House owner outright/with mortgage	*.381*	*.486*	*.432*	*.495*
County unemployment rate	*13.586*	*3.205*	*13.500*	*3.224*
Receives only UB at t1	*.362*	*.481*	*.381*	*.486*
Receives no UB nor SB	*.046*	*.210*	*.050*	*.217*
benefit time varying (£)	*3976.682*	*1855.357*	*3945.793*	*1877.564*
Predicted earnings, in £	*9097.565*	*2257.838*	*9201.237*	*2335.497*
predicted earnings not available	*.011*	*.106*	*.012*	*.110*
total savings one month before U. £	*1440.582*	*5736.440*	*1799.622*	*6361.343*
total debt one month before U. £	*618.116*	*2750.075*	*772.171*	*3054.499*
total family savings one month before U. £	*1668.01*	*7559.100*	*1682.200*	*8133.040*
total family debt one month before U. £	*648.887*	*2765.740*	*689.290*	*2995.420*
mortgage capital outstanding, £	*2383.12*	*17423.920*	*2675.89*	*19123.990*
"once-off payments", £	*1211.604*	*4423.350*	*1376.549*	*4584.826*

The number of units that report non zero savings or non zero debt is 1629. The total sample is made of 2030 unemployed. The dichotomous variables take value one when the condition stated for each of them is satisfied. The mean unemployment duration is computed including the right-censored observations. The total family savings are equal to the sum of the savings of the unemployed person and their spouses for the married people and to the unemployed's savings for the single people. The total family debt is equal to the sum of the debt of the unemployed person and their spouses for the married people and to the unemployed's debt for the single people. "U. " stands for "the unemployment spell".

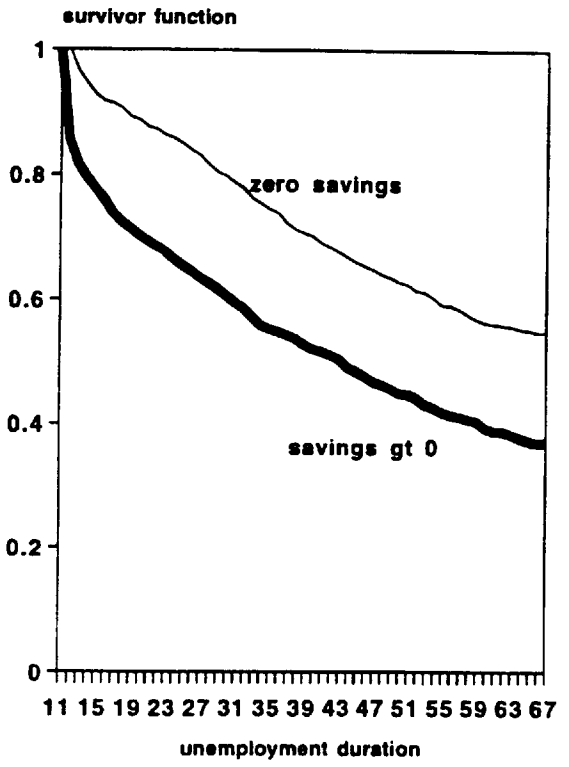

Figure 7.1: *Kaplan-Meier Survivor Functions for the Re-employment Probability*

7.4 Results of estimation

Non parametric Kaplan-Meier estimates are provided first. Next, the results of estimation of the more complex econometric model are discussed. This is a competing risks model of the re-employment probability. Two destination states out of unemployment have been allowed for: full-time work and other states. I am interested in the results for the exit into full-time work. The likelihood function for the model is given by Equation 3.36 of Chapter 3. The baseline hazard rate, a piecewise linear, is allowed to vary each month.

7.4.1 Non parametric estimates

I have carried out some non-parametric analysis of the re-employment probability for the unemployed with different levels of savings and debt. The survivor functions of different (mutually exclusive) groups of the unemployed have been estimated by Kaplan-Meier method. I compare the estimated survivor functions by means of visual inspection and also using the Log-Rank test[3], which is based on the estimated standard errors of the survivor functions.

The survivor function of the unemployed that reported zero levels of savings one month before the commencement of the their unemployment spell (776 observations) is compared in Figure 7.1 with the survivor function of

[3]A good reference for a description of this test is Kalbfleish and Prentice (1980).

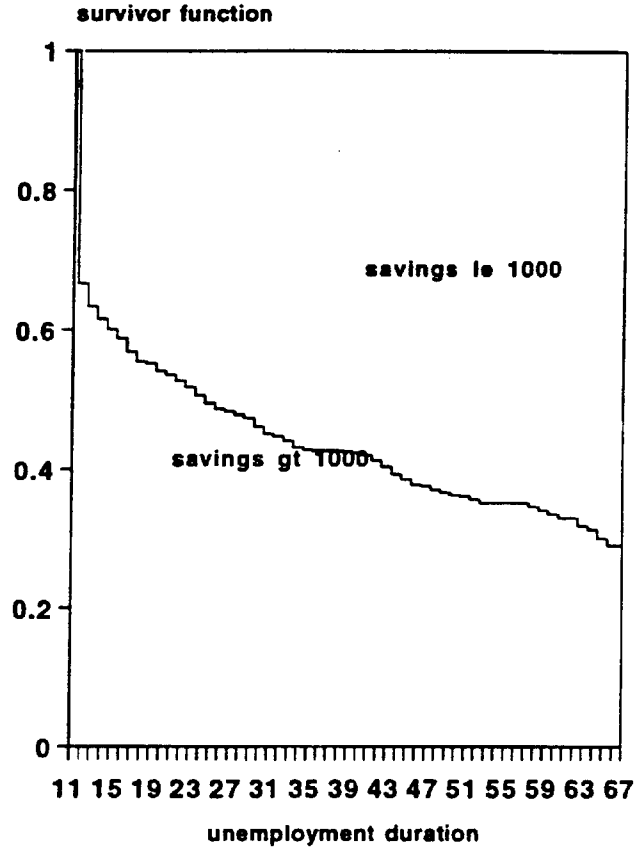

Figure 7.2: *Kaplan-Meier Survivor Functions for the Re-employment Probability*

the unemployed that reported positive amounts of savings at the same date (1254 observations). The survivor function for the unemployed with positive amounts of savings lies below that for the unemployed that reported zero amounts of savings. The Log-Rank test rejects strongly the null hypothesis that the survivor functions of the two groups of the unemployed are not significantly different ($\chi^2_2 = 24.1$). According to these non-parametric estimates, the unemployed with positive levels of savings are more likely to exit from unemployment to take up a full-time job (at any time) than the unemployed with no savings. However, the non-parametric estimates do not allow for heterogeneity of the two groups of the unemployed. It is possible that the unemployed with positive levels of savings have other "good" characteristics (unaccounted for here) which might contribute to explain the results illustrated in Figure 7.1.

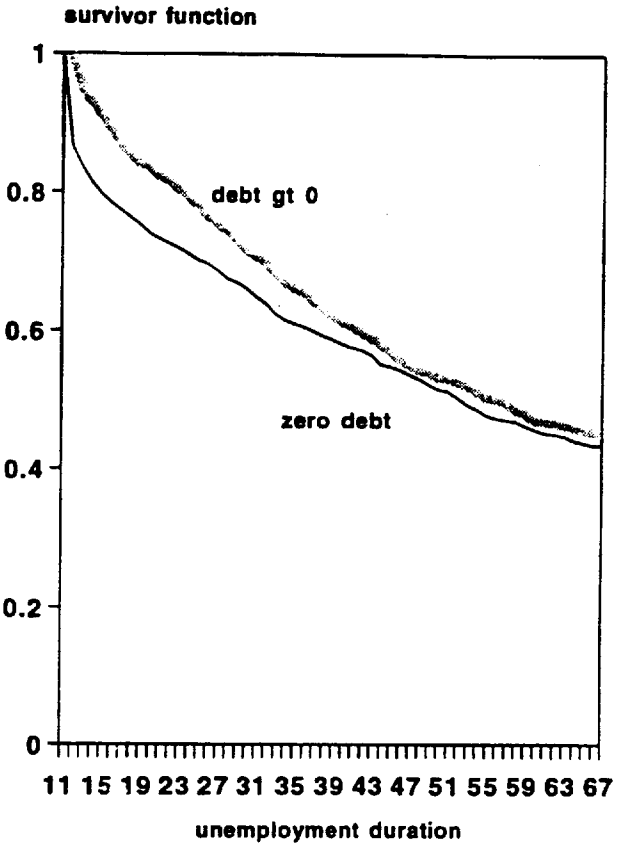

Figure 7.3: *Kaplan-Meier Survivor Functions for the Re-employment Probability*

In Figure 7.2, I compare the survivor function of the unemployed with savings larger than (or equal to) £1000 one month before the commencement of the their unemployment spell (415 observations) with the survivor function of the unemployed with savings of less than £1000 at the same date (1615 observations). The estimated survivor functions of the two groups follow a similar pattern than that of the survivor functions of Figure 7.1. The survivor function of the unemployed with savings larger than (or equal to) £1000 lies below the survivor functions of the unemployed with savings of less than £1000. According to these results, higher levels of savings raise the individual re-employment probability.

However, the null hypothesis that the survivor functions of the two groups are not significantly different cannot be rejected on the basis of the Log-Rank test ($\chi^2_2 = 1.74$). I have obtained similar results by distinguishing the unemployed with level of savings higher than £3000 (203 observations) one month before the commencement of the unemployment spell and the unemployed with savings of less than £3000 (1827 observations). The survivor function of the unemployed with savings larger than £3000 lies below that of the unemployed with savings of less than £3000, at any point in time. The Log-Rank test can not reject the null hypothesis that the survivor functions of the two groups are not significantly different ($\chi^2_2 = 0.96$).

In Figure 7.3, I compare the survivor function of the unemployed that

reported zero amounts of debt (942 observations) one month before the commencement of the their unemployment spell with the survivor function of the unemployed that reported positive amounts of debt (1088 observations) at the same date. The survivor function of the unemployed with no debt lies below that of the unemployed with positive debt, at any time. This implies that the unemployed in debt are less likely to exit unemployment to take up a full-time job than the unemployed that are not in debt. However, the Log-Rank test cannot reject the null hypothesis that the survivor functions of the two groups are not significantly different ($\chi^2_2 = 5.1$).

7.4.2 Parametric estimates

Alternative specifications of the savings and debt variables were tried out. The results of estimation are given in Table 7.12, Table 7.13 and Table 7.14 below. The reader is referred to Chapter 5 for a discussion of the estimated impact of the explanatory variables of the model. The discussion below focus on the impact of the financial resources variables. I present first the results of estimation of a model where savings and debt are entered in levels. Next, I show my favourite model, where some spline functions are specified to capture the impact of savings and debt on the re-employment probability. I conclude this section with a discussion of some sensitivity analysis.

In specification (1) and (2) of Table 7.12, the model is estimated separately for the full sample (specification 1) and for unemployed that reported positive amounts of savings or debt (specification 2). The impact of the explanatory variables does not differ much across the two models. In particular, higher level of savings are found not to affect significantly the individual re-employment probability. The sign of the coefficient on the level of savings is negative. This might confirm the view that higher levels of savings raise the reservation wage and lower the re-employment probability. The coefficient on debt is slightly significant and shows that debt affects negatively the individual re-employment probability. This finding supports the view that debt proxies access to credit. Access to credit may allow the unemployed to be more choosy about accepting job offers and therefore lower the re-employment probability. The impact of debt is, however, very small. A 10% increase in the level of debt evaluated at the mean (equal to about 14 hundred £) lowers the re-employment hazard by less than 1%.

Table 7.12: *Results of estimation*

	All (1) unemployed		*Reporting non-zero(2) Savings or Debt*	
Variable	*Coeff.*	*SE*	*Coeff.*	*SE*
F/t work year before	*0.3713**	*0.1368*	*0.4194**	*0.1520*
Unemployed year before	*0.3027**	*0.1477*	*0.3729**	*0.1672*
Sick year before	*-0.3808*	*0.2900*	*-0.2692*	*0.3488*
Profess. /Interm. Occ.	*0.2110**	*0.0985*	*0.2129**	*0.1050*
Unskilled Occupation	*-0.4393**	*0.1720*	*-0.5629**	*0.2100*
Age 20-24	*0.2230**	*0.1075*	*0.1855*	*0.1228*
Age 35-44	*-0.1786**	*0.0903*	*-0.1247*	*0.0979*
Age 45-54	*-0.5951**	*0.1107*	*-0.5506**	*0.1213*
Age 55-58	*-1.2643**	*0.1734*	*-1.2035**	*0.1825*
Has any child aged < 5	*-0.1989**	*0.0871*	*-0.1886**	*0.0962*
Married	*0.1404*	*0.1237*	*0.0935*	*0.1360*
Spouse working	*0.3656**	*0.0873*	*0.3060**	*0.0935*
Searches less	*-0.7549**	*0.1728*	*-0.7659**	*0.1919*
Values Leisure more	*-0.2781**	*0.1146*	*-0.3269**	*0.1250*
experienceas money shortage	*0.1890**	*0.0862*	*0.2224**	*0.0963*
House owner	*0.3373**	*0.0739*	*0.3022**	*0.0800*
County U rate	*-0.0202**	*0.0106*	*-0.0194*	*0.0117*
Receives only UB	*0.2006**	*0.0849*	*0.1913**	*0.0930*
Receives no UB, SB	*0.0200*	*0.2799*	*-0.1269*	*0.2991*
UB/SB time varying, logs	*-0.0325*	*0.0603*	*-0.0571*	*0.0630*
Predicted earnings. logs.	*0.6488**	*0.2021*	*0.6330**	*0.2175*
No pred. earn.	*2.9561**	*0.9591*	*2.6823**	*1.0409*
Savings. in 100 £	*-0.0009*	*0.0006*	*-0.0010*	*0.0006*
Debt. in 100 £	*-0.0037*	*0.0019*	*-0.0042**	*0.0020*
Maximum log-likelihood	*-6436.9*		*-5317.2*	

The unemployed that reported amounts of savings or debt greater than 6 figures in pounds were excluded from the analysis. Savings and debt relate to the amounts reported as to one month before the commencement of the unemployment spell. The results relate to exit into full-time work. The maximum likelihood is computed by joint estimation of the two competing risks. Descriptive statistics of the explanatory variables are given in Table 7.11.

In Table 7.13, the results of estimation of alternative specifications of the financial resources variables are presented. In specification (3), the level of "once off" payments is entered among the regressors. The estimated coefficient on the level of "once-off" payments is statistically significant and shows negative sign as expected. The "once-off" payments are assumed to represent (unexpected) increases in the level of financial resources associated with the commencement of the unemployment spell (and the ending of a previous work contract). According to the theoretical predictions, an increase in the level of financial wealth allows the unemployed to be more "choosy" about accepting job offers and therefore lowers the re-employment probability. The impact of "once-off" payments on the re-employment hazard is quite small. A 10% increase in the amount of "once-off" payments (evaluated at the mean of 12 hundred £) lowers the re-employment probability by less than 1%. The impact of debt (from specification 1) is unaffected by the additional regressor. The estimated coefficient on savings (not significant) becomes slightly smaller in absolute value.

In specification (4) of Table 7.13, the savings and debt variables are specified using two spline functions. The rationale for this specification is the large skewness of the distributions of savings and debt of the unemployed. The impact of the continuous savings (or debt) variable is allowed to differ for different values of savings (or debt). The following three intervals of savings (or debt) values are considered:

- savings (or debt) less than £500;
- savings (or debt) greater or equal than £500 and less than £3000;
- savings (or debt) greater or equal than £3000.

The coefficients of the debt spline are not significantly different from zero. However, they show negative sign except for the first one, which relates to small amounts of debt of less than £500. It is possible that amounts of debt of less than £500 signal "no access to credit". There is, however, no firm explanation for this result. The impact of "once-off" payments does not vary much from specification (3). The coefficients of the savings spline are now statistically significant. The first two —on amounts of savings up to £500 and amounts of savings from £500 up to £3000— show positive sign while the last one —on amounts of savings larger than £3000— has negative sign. One possible explanation for this result is that smaller amounts of savings proxy, at least to a certain extent, individual risk aversion. In this case, then, higher levels of (small) savings represent higher degrees of risk aversion and higher degrees of risk avrsion result in lower reservation wages and shorter unemployment duration. Levels of savings larger than £3000 might, instead, capture only to a limited extent individual risk aversion and to a larger extent the impact of higher levels of financial wealth on the re-employment hazard, which is expected to be positive for a given degree of

risk aversion. An increase of 10% in the level of savings (evaluated at the level of 2.5 hundred pounds of savings) raises the re-employment hazard by about 1%, for the persons with savings below £500. An increase of 10% in the level of savings (evaluated at the level of 12.5 hundred pounds of savings) raises the hazard rate by about 1%, for the unemployed with savings between £500 and £3000.

Table 7.13: *Results of estimation*

	Specification (3)		*Specification (4)*		*Specification (5)*	
Variable	*Coeff.*	*SE*	*Coeff.*	*SE*		
F/t work year before	*0.3982**	*0.1371*	*0.3816**	*0.1377*	*0.3662**	*0.1372*
Unemployed year before	*0.3090**	*0.1477*	*0.3246**	*0.1483*	*0.3094**	*0.1478*
Sick year before	*-0.3931*	*0.2901*	*-0.3188*	*0.2915*	*-0.3486*	*0.2905*
Profess. /Interm. Occ.	*0.2151**	*0.0980*	*0.1855*	*0.0983*	*0.1854*	*0.0981*
Unskilled Occupation	*-0.4407 **	*0.1720*	*-0.3874**	*0.1726*	*-0.3893**	*0.1725*
Age 20-24	*0.2325**	*0.1076*	*0.2482**	*0.1076*	*0.2446**	*0.1075*
Age 35-44	*-0.1808**	*0.0903*	*-0.1848**	*0.0910*	*-0.1944**	*0.0908*
Age 45-54	*-0.5703**	*0.1104*	*-0.5990**	*0.1116*	*-0.6068**	*0.1114*
Age 55-58	*-1.2178**	*0.1744*	*-1.3085**	*0.1768*	*-1.3207**	*0.1762*
Has any child aged < 5	*-0.1983**	*0.0871*	*-0.1960**	*0.0874*	*-0.1897**	*0.0873*
Married	*0.1391*	*0.1237*	*0.1667*	*0.1236*	*0.1651*	*0.1236*
Spouse working	*0.3564**	*0.0875*	*0.3487**	*0.0879*	*0.3573**	*0.0876*
Searches less	*-0.7036**	*0.1729*	*-0.6789**	*0.1729*	*-0.6796**	*0.1728*
Values Leisure more	*-0.2540**	*0.1147*	*-0.2660**	*0.1151*	*-0.2728**	*0.1149*
Experiences money shortage	*0.1509*	*0.0865*	*0.1804**	*0.0873*	*0.1874**	*0.0871*
House owner	*0.3465**	*0.0739*	*0.2757**	*0.0765*	*0.2764**	*0.0758*
County U rate	*-0.0326**	*0.0106*	*-0.0225**	*0.0106*	*-0.0220**	*0.0106*
Receives only UB	*0.2212**	*0.0849*	*0.1824**	*0.0857*	*0.1862**	*0.0856*
Receives no UB, SB	*-0.0228*	*0.2802*	*-0.0692*	*0.2807*	*-0.0575*	*0.2805*
UB/SB time varying, logs	*-0.0412*	*0.0604*	*-0.0397*	*0.0606*	*-0.0352*	*0.0605*
Predicted earnings, logs.	*0.7123**	*0.2026*	*0.6545**	*0.2035*	*0.6575**	*0.2035*
No pred. earn.	*3.2306**	*0.9610*	*3.0001**	*0.9647*	*3.0194**	*0.9646*
Savings, in 100 £	*-0.0006*	*0.0008*				
Debt, in 100 £	*-0.0037**	*0.0019*			*-0.0033*	*0.0018*
"Once-off payments, in 100 £	*-0.0030**	*0.0012*	*-0.0031**	*0.0013*	*-0.0031**	*0.0013*
Spline, 0≥ savings < £500, in 100 £			*0.0489**	*0.0214*	*0.0482**	*0.0213*
Spline, £500 ≥ savings < £3000, in 100 £			*0.0123**	*0.0062*	*0.0123**	*0.0062*
Spline, £3000≥ savings, in 100 £			*-0.0028**	*0.0013*	*-0.0028**	*0.0013*
Spline, 0≥ debt < £500, in 100 £			*0.0223*	*0.0196*		
Spline, £500 ≥ debt < £3000, in 100 £			*-0.0131*	*0.0077*		
Spline, £3000≥ debt, in 100 £			*-0.0022*	*0.0022*		
Maximum log-likelihood	*-6433.3*		*-6416.9*		*-6421.0*	

The estimation is carried out for the full sample except for the unemployed that reported amounts of savings or debt greater than 6 figures in pounds, who were excluded from the analysis. The level of savings and debt relate to the amounts reported as to one month before the commencement of the unemployment spell. The results relate to exit into full-time work. The maximum likelihood is computed by joint estimation of the two competing risks, full-time work and other exits. Descriptive statistics of the explanatory variables are given in Table 7.11.

An increase of 10% in the level of savings (evaluated at 40 hundred pounds of savings) lowers the re-employment hazard by about 1%. The hypothesis that a linear relationship between savings and the re-employment probability (as in specification 3) is to be preferred to this piecewise linear specification is tested with a likelihood ratio test ($\chi^2_4 = 32.8$). The null hypothesis that the additional spline coefficients are not significantly different from zero is rejected.

Specification (5) of Table 7.13, is the same as specification (4) except for the specification of the level of debt that is now entered linearly as before. The coefficient on the level of debt is not significant but shows negative sign, as expected. The coefficients on the savings spline and on the level of "once-off" payments do not change relative to specification (4).

To conclude, a spline specification of the savings of the unemployed performs best. The robustness of the spline estimates is tested to alternative specifications of the savings and debt variables, as illustrated in Table 7.14. In specification (a), the savings and debt variables are entered in logarithms instead than in levels. The implication of the logarithmic specification is that of a constant elasticity, as already discussed in the data section above. The coefficient on (logs) savings is significant and positive. It is close in absolute value to the coefficient on the first spline segment in specification (4). The coefficient on debt is not significant and shows negative sign, as before. The impact of the "once-off" payments is close to that found in previous specifications. The impact of "once-off" payments becomes not significant if this variable is entered in logs (specification b).

In specification (c), the family's levels of savings and debt are entered among the regressors (instead of the unemployed's level of savings and debt). None of the two variables is found to affect significantly the re-employment probability. The estimated impact of "once-off" payments is larger in absolute value with respect to previous specification.

In specification (d), some dummies that take value one for given levels of savings and debt are entered among the regressors instead of the actual levels of savings or debt. The intervals of savings and debt levels considered are the same used for the splines of specification (4) and (5) above. All the savings dummies show positive sign (and are statistically significant). The estimated coefficient on the last savings dummy is however smaller than the coefficients on the previous two savings dummies.

Table 7.14: *Some more results*

Specification	*Coeff.*	*SE*	*Max. log-lik.*
(a) Same specification as (3) but savings and debt variables are entered in logs			*-6433.9*
(a) Savings, in logs of 100 £	*0.0412**	*0.0198*	
(a) Debt, in logs of in 100 £	*-0.0223*	*0.0268*	
(a) Once off payments, in 100 £	*-0.0031**	*0.0012*	
(b) Same as (a) but "once-off" payments are also in logs			*-6437.6*
(b) Savings, in logs of 100 £	*0.0387**	*0.0198*	
(b) Debt, in logs of in 100 £	*-0.0231*	*0.0269*	
(b) Once off payments, in logs of 100 £	*-0.0283*	*0.0269*	
(c) Same specification as (3) but I consider the family savings and debt			*-6310.3*
(c) Total family savings, in £100	*-0.0009*	*0.0014*	
(c) Total family debt, in £100	*0.0001*	*0.0004*	
(c) Once off payments, in 100 £	*0.0050**	*0.0017*	
(d) Same as specification (1) but some dummies are used to capture the impact of savings and debt levels			*-6423.7*
(d) D1=1 if 0 > savings < £500	*0.2155**	*0.0810*	
(d) D2=1 if £500 ≥ savings < £3000	*0.4629**	*0.1016*	
(d) D3=1 if £500 ≥ savings < £3000	*0.4143**	*0.1427*	
(d) D4=1 if 0 > debt < £500	*-0.0359*	*0.0818*	
(d) D5=1 if £500 ≥ debt < £3000	*0.0396*	*0.0885*	
(d) D6=1 if £500 ≥ debt < £3000	*-0.4623**	*0.2001*	
(e) Same as specification (4) except for the exclusion of "once-off" payments			*-5301.7*
(e) Spline, 0≥ savings < £500, in 100 £	*0.0476**	*0.0234*	
(e) Spline, £500 ≥ savings < £3000, in 100 £	*0.0120*	*0.0062*	
(e) Spline, £3000≥ savings, in 100 £	*-0.0029**	*0.0012*	
(e) Spline, 0≥ debt < £500, in 100 £	*0.0194*	*0.0214*	
(e) Spline, £500 ≥ debt < £3000, in 100 £	*-0.0126*	*0.0076*	
(e) Spline, £3000≥ debt, in 100 £	*-0.0022*	*0.0022*	
(f) No regressors are entered except for the variables below and the monthly constants of the baseline hazard rate			*-6584.6*
(f) "Once-off payments, in 100 £	*0.0009*	*0.0012*	
(f) Spline, 0≥ savings < £500	*0.0938**	*0.0376*	
(f) Spline, £500 ≥ savings < £3000	*-0.0021*	*0.0106*	
(f) Spline, £3000≥ savings	*-0.0007*	*0.0013*	
(f) Spline, 0≥ debt < £500	*-0.0734*	*0.0415*	
(f) Spline, £500 ≥ debt < £3000	*0.0340**	*0.0135*	
(f) Spline, £3000≥ debt	*-0.0036*	*0.0038*	

The model is estimated for all the unemployed but those that reported amounts of savings or debt greater than 5 figure in pounds, as in model (1) above. The level of savings and debt relate to the amounts reported as to one month before the commencement of the unemployment spell. The results relate to exit into full-time work. The maximum likelihood is computed by joint estimation of the two competing risks. Descriptive statistics of the explanatory variables are given in the Appendix.

This confirms the results obtained with the spline specification of the impact of savings. The coefficient on the last debt dummy —which takes value one for the unemployed with debt of more than £3000— is significantly different from zero and negative. The differences with the results of estimation of specification (4) are not so large since none of the estimated coefficients on the splines nor on the dummies are strongly significant. For the purpose of comparison, specification (e) is equivalent to specification (4) except for the exclusion of the "once-off " payments (which were also excluded from specification d).

In specification (f), only the savings and debt variables (and the piecewise constants of the baseline hazard rate) are entered among the regressors. The level of "once-off" payments is now not significant. Only the first of the estimated coefficients on the savings spline is significant and shows positive sign. The coefficient on the second segment of the debt spline is significant and negative.

None of these alternative specifications of the savings and debt variables is found to perform better than specification (4). The detection of a significant but small impact of the level of financial resources of the unemployed on the individual re-employment probability is confirmed.

7.5 Summary and conclusions

In this chapter, I have investigated the impact of the level of financial resources of the unemployed on the re-employment probability, using the LSUS data.

From the descriptive analysis of the savings and the debt of the unemployed, the following facts emerged. The savings and debt of the unemployed vary in some cases considerably during the course of the unemployment spell. In particular, while the savings of some unemployed increase because of the receipt of "once-off" payments (for example redundancy payments) associated with the commencement of the unemployment spell, the number of the unemployed in debt increases as well. The net financial resources deteriorate for about 53% of the unemployed, passing from one month before the commencement of the unemployment spell to three months into the spell (when the first survey interview took place). The corresponding figure, passing from the first to the second survey interview, is also 53%. About 43% of the unemployed reported "once-off" payments due to the ending of the previous work contract. The amounts of such payments vary considerably across the unemployed, going from less than £100 to over £10000.

On the basis of this descriptive analysis, it is not possible to conclude on any association between the level of net financial resources of the unemployed and their exiting from unemployment before the time of the second survey interview. The distribution of the net resources of the unemployed with right-censored or completed unemployment spell does not differ substantially.

Marital status is not found to affect to a large extent the financial wealth of the unemployed.

The conclusion of the econometric analysis are the following. I find some evidence that the level of financial resources affects the individual re-employment probability. In particular, I find that the receipt of redundancy payments or other "once-off" payments associated with the commencement of the unemployment spell and the ending of a previous work contract has a negative impact on the re-employment hazard rate. This type of payments represent an (unexpected) increase in the level of individual savings and their expected impact on the hazard rate is negative (Danforth, 1979). The magnitude of the impact of these "once-off" payments on the re-employment hazard is, however, very small. A 10% increase in the level of "once-off" payments (measured in hundred pounds) is found to raise the re-employment hazard rate by about 1%.

The savings of the unemployed have a significant non-linear impact on the re-employment probability. The impact of savings is significantly different from zero if a non-linear specification of the savings variable is adopted, such as for instance a linear spline or a logarithmic specification or a series of dummies taking value one for given intervals of savings. Savings are found to affect positively the re-employment probability. An explanation is that higher levels of savings proxy higher degrees of risk aversion. According to the theoretical predictions (Kohn and Shavell, 1974 and Pissarides, 1974), more risk averse individuals have lower reservation wages and shorter unemployment durations. The evidence on the sign and the significance of the impact of savings is, however, not very robust. The impact of savings on the re-employment hazard (when significant) is quite small.

Chapter 8

Conclusions

In this thesis, my aim was to gain new insights into the determinants of unemployment duration in the UK. For the analysis, I have used the "Survey of Living Standards during Unemployment (LSUS)", which was collected by the Office of Population Censuses and Surveys on behalf of the Department of Social Security. These data had not yet been analysed except for the reports made by the survey planners (Heady and Smith, 1989). The objectives of the analysis carried out in the thesis were laid out in detail in Chapter 1. I resume below the main results obtained and the conclusions that I have been able to draw.

In Chapter 2, I provided a discussion of the LSUS data. From a descriptive points of view, I analysed the information on the exit states out of unemployment and the receipt of unemployment benefit. The major findings of Chapter 2 are summarized below.

- It emerged that the LSUS data are well worth using for the analysis of individual unemployment duration in the UK. The longitudinal structure of the survey allows one to observe the duration of the individual unemployment spells and the economic states entered upon leaving unemployment. This is especially interesting since only one previous study had access to and made use of similar information on the exit states out of unemployment for Britain (Narendranathan and Stewart, 1993).

- The LSUS survey covers a period of time (1983/84) when the rate of unemployment in the UK was quite high (averaging about 12–13%) and close to the current levels. Previous published UK studies relate instead to the late seventies, which were characterized by much lower unemployment rates.

- An advantage of the LSUS data is that they contain rich information on many socio-economic characteristics and psychological attitudes of the unemployed.

- The main drawback of the survey is that it contains limited information on the pattern of unemployment benefit receipts over time.

- The descriptive analysis of the data highlighted the importance of allowing for the variety of economic states that exist in reality.

- The analysis of unemployment benefit receipts revealed how the type and the amount of unemployment benefit received may vary considerably across the unemployed persons and over time. However, the limited information on any changes in the pattern of unemployment benefit receipts available made necessary some imputation of these changes.

In Chapter 3, I have laid out the econometric model that I use in the thesis. This is a reduced form model of the individual probability of leaving unemployment. The dependency on time of the (conditional) probability of leaving unemployment is modelled using a flexible specification for the baseline hazard rate, a piecewise linear. Two exit states out of unemployment are distinguished, full-time work and other states, and modelled by means of a competing risks specification.

A comparison of the results of estimation of single and competing risks models of the (conditional) probability of leaving unemployment, using the LSUS data, has been carried out in Chapter 4. In the same Chapter, alternative specifications of the baseline hazard rate (using the LSUS data) were tested. The following conclusions were drawn.

- I found that some of the estimated coefficients on the explanatory variables differed considerably across the single and competing risks models. In particular, some variables relating to individual previous work history turned out to affect significantly but in opposite directions the two cause-specific hazard rates of the competing risks model while they were not significant in the single risk model.

- These results contrast with the findings of Narendranathan and Stewart (1993) who used a similar competing risks specification of the individual probability of leaving unemployment for the UK and concluded that most variables affected the single hazard and the re-employment hazard in a similar manner. However, I do actually find that the impact of the level of unemployment benefit —which is the focus of interest of most previous U. K. studies— does not differ considerably across the single and competing risks hazards.

- The equality of the single and competing risks hazards was rejected on the basis of the tests suggested in Narendranathan and Stewart (1991).

- No significant pattern of time dependency could be detected. This finding is in line with previous UK studies that used more restrictive specifications of the baseline hazard rate but allowed for unobserved heterogeneity.

- Slightly significant negative time dependency emerged in the full-time work hazard rate if a Weibull model were imposed on the data (without allowing for unobserved heterogeneity). This finding is also line with those of previous UK studies.

- The impact of the explanatory variables did not differ considerably if a Weibull baseline hazard rate were specified instead of a piecewise linear. This conclusion confirms previous findings of Narendranathan and Stewart (1993) who investigated the same issue.

Estimating the impact of socio-economic and institutional factors on the individual re-employment probability is the object of Chapter 5. Most of the explanatory variables considered are found to affect significantly the re-employment probability and to have the expected sign. In particular, the following results were obtained.

- The effect of the level of unemployment benefit was found to be not significantly different from zero. This result was robust to alternative specifications of the benefit variable and to the inclusion (exclusion) of groups of variables.

- The estimated coefficient on the level of benefit became slightly significant only when controls for the spouse's labour force participation (measured some time before the commencement of unemployment spell) and for the presence (in the family) of any child aged less than five are dropped from the model. It is difficult to find an explanation for this result.

- The estimated coefficient on a proxy for search activity indicated that the individuals that search less show significantly lower chances (by 50%) to find a full-time job. Valuing leisure more than labour was also found to lower the re-employment probability by about a quarter.

- Age was found to affect significantly the unemployed's chances of finding a full-time job. In particular, the unemployed aged between 45 and 54 have about 50% lower chances of finding a full-time job and the unemployed aged between 55 and 58 have about 70% lower re-employment probability.

- The unemployed with higher expected earnings were found to have significantly shorter unemployment duration. A 10% increase in mean expected earnings results in a reduction of about 15 weeks in expected mean unemployment duration.

- The unemployed that feel financially restrained have instead about 22% higher chances of finding a full-time job, all things equal.

- The unemployment rate in the geographical area of residence considered (the county) was found to affect significantly but to a limited extent the unemployed's chances of finding a full-time job. This result is in line with previous UK findings.

In Chapter 6, I investigated the relationship between the duration of entitlement to unemployment benefit and the individual probability of leaving unemployment. There are no previous UK studies that looked at this issue using micro data. I have distinguished two groups of the unemployed with respect to unemployment benefit receipts at the commencement of their unemployment spell: the first group is made up of recipients of "only UB" —UB has a limited duration of 52 weeks; the second groups includes the recipients of SB (now called Income Support), either by itself or in addition to UB —SB has unlimited duration. On the basis of the analysis carried out, the following conclusions can be drawn.

- There is quite firm evidence (using the LSUS data) that the expected exhaustion of UB affects the individual probability of leaving unemployment.

- The estimated size of the impact of the duration of unemployment benefit is sensitive to the specification of the baseline hazard rate.

- I find that the hazard rate of the "only UB" group increases as the time of expected exhaustion of entitlement to unemployment benefit approches. However, the exhaustion of the benefit is found to "push" the unemployed into states other than full-time work. The estimation of a single risk model would have prevented one from detecting such behaviour.

- The two groups of benefit recipients were found to differ significantly with respect to their job finding experience.

In Chapter 7, I looked at the relationship between the level of financial resources of the unemployed and the re-employment probability. The results obtained can be summarized as follows.

- Higher levels of savings are expected to raise the re-employment hazard rate for a given degree of risk aversion. Savings may proxy individual risk aversion. Higher degrees of risk aversion are expected to lower the reservation wage and therefore to raise the re-employment hazard. Therefore, it is not possible to conclude a priori on the expected impact of savings on the re-employment hazard.

- The receipt of rendundancy payments or other "once-off" payments associated with the ending of a previous work contract and the commencement of the unemployment spell can be seen as an (unexpected)

increase in the unemployed's financial wealth. The expected impact of an increase in the level of financial wealth on the re-employment hazard is negative. The unemployed that become wealthier can afford to be more "choosy" about accepting job offers.

- Debt is expected to affect the re-employment hazard by lowering the unemployed's reservation wage. However, debt may proxy the unemployed's access to credit and therefore exercise a negative impact on the re-employment hazard. The unemployed that have access to credit can afford to be more "choosy" about accepting job offers.

- Some descriptive analysis of the financial resources of the unemployed led to the conclusion that the distributions of savings and debt of the unemployed are very skewed with some unemployed reporting very small amounts of savings and debt (smaller than £10) and some unemployed reporting considerably large amounts (larger than £10000).

- It emerged that the net financial resources of the unemployed tend to vary considerably following the commencement of the unemployment spell. Although a large proportion of the unemployed experience a reduction in the level of their financial assets during the course of the unemployment spell, some of them see their net balances improve. This is possibly explained by the receipt of "once-off" payments associated with the ending of a work contract and the commencement of the unemployment spell.

- The level of savings was found to affect significantly the re-employment probability, if a non-linear specification of the savings variable is adopted. The coefficients in alternative non-linear specification of savings were (generally) significant and positive. The estimated magnitude of the impact was however quite small. A 10% increase in the level of savings would raise the re-employment hazard by about 1%. in my preferred specification (4).

- The eimpact of the level of debt on the re-employment hazard is not particularly significant. It shows negative sign.

- The receipt of "once-off" payments associated with the commencement of the unemployment spell and the ending of a previous work contract was found to affect significantly the hazard rate. The impact of "once-off" payments is quite small. A 10% increase in the level of such "once-off" payments raises the re-employment hazard by less than 1%.

To sum up, I think that the analysis carried out in the thesis led to some interesting results. Future studies should aim at gathering more evidence on the impact of the duration of unemployment benefit and of the unemployed's financial resources on the re-employment probability.

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Appendix

Table 8.1: *Variables of the earnings equation*

Variable	Variable description	Subsample		Sample	
		Mean	SE	Mean	SE
age		38.28	11.45	37.80	11.19
married		.87	.33	.85	.35
manager		.15	.36	.13	.34
foreman		.12	.32	.09	.29
employee		1.00	.03	.90	.30
selfemp	self-employed	.00	.00	.11	.32
Ind1	Energy & Water	.05	.22	.04	.19
Ind2	Minerals & Chemicals	.06	.23	.07	.25
Ind3	Engineering	.17	.37	.17	.38
Ind4	Other Manufacturing	.11	.31	.10	.30
Ind5	Construction	.21	.41	.18	.38
Ind6	Hotels & Catering	.17	.38	.17	.37
Ind7	Transport	.07	.26	.07	.26
Ind8	Finance	.04	.21	.04	.19
Ind9	Other Services	.10	.30	.11	.31
Noind	Information not available	.01	.12	.05	.22
G1	Employers/managers large firm	.03	.16	.02	.15
G2	Employers/managers small firm	.02	.15	.02	.15
G3	Professional/self-employed	.00	.06	.01	.09
G4	Professional/employees	.03	.17	.03	.17
G5	Intermediate non-manual	.09	.29	.10	.29
G6	Junior non-manual	.03	.17	.03	.17
G7	Personal service workers	.04	.20	.04	.21
G8	Foremen/supervisors manual	.02	.15	.02	.15
G9	Skilled manual workers	.06	.23	.05	.21
G10	Semi-skilled manual	.02	.14	.02	.14
G11	Unskilled manual	.09	.28	.08	.28
G12	Own account workers	.22	.42	.21	.41
G13	Farmers employers/managers	.04	.18	.05	.21
G14	Farmers own account	.12	.32	.10	.30
G15	Agricultural workers	.17	.37	.15	.36
G16	Members of armed forces	.02	.15	.02	.15
NOOCGR	Information not available	.00	.03	.04	.19

In this table, the "subsample" is the subgroup for which the earnings equation is estimated, 1407 people; the "sample" are the first interview male participants , 2717.

Occupational groups

1. *Employers and managers in central and local government, industry, commerce, etc. — large establishments.* Employers in industry, commerce, etc. ; Persons who employ others in non-agricultural enterprises employing 25 or more persons. Managers in central and local government, industry, commerce, etc. ; Persons who generally plan and supervise in non-agricultural enterprises employing 25 or more persons.

2. *Employers and managers in industry, commerce, etc. — small establishments.* Employers in industry, commerce, etc. — small establishments; as corresponding point above, but in establishments employing fewer than 25 persons. Managers in industry, commerce, etc. — small establishments. As in corresponding point above, but in establishments employing fewer than 25 persons.

3. *Professional workers — self-employed.* Self-employed persons engaged in work normally requiring qualifications of university degree standard.

4. *Professional workers — employees.* Employees engaged in work normally requiring qualifications of university degree standard.

5. *Intermediate non-manual workers:* (a) Ancillary workers and artists; Employees engaged in non-manual occupations ancillary to the professions, not requiring qualifications of university degree standard; Persons engaged in artistic work and not employing others therein; Self-employed nurses, medical auxiliaries, teachers, work study engineers and technicians. (b) Foremen and supervisors non-manual; Employees (other than managers) engaged in occupations included in group 6 below, who formally and immediately supervise others engaged in such occupations.

6. *Junior non-manual workers.* Employees, not exercising general planning or supervisory powers, engaged in clerical, sales and non-manual communications occupations, excluding those who have additional and formal supervisory functions (these are including in the group just above).

7. *Personal service workers.* Employees engaged in service occupations caring for food, drink, clothing and other personal needs.

8. *Foremen and supervisors — manual.* Employees (other than managers) who formally and immediately supervise others engaged in manual occupations, whether or not themselves engaged in such occupations.

9. *Skilled manual workers.* Employees engaged in manual occupations which require considerable and specific skills.

10. *Semi-skilled manual workers.* Employees engaged in manual occupations which require slight but specific skills.

11. *Unskilled manual workers.* Other employees engaged in manual occupations.

12. *Own account workers (other than professional).* Self-employed persons engaged in any trade, personal service or manual occupation not normally requiring training of university degree standard and having no employees other than family workers.

13. *Farmers — employers and managers.* Persons who own, rent or manage farms, market gardens or forests, employing people other than family workers in the work of the enterprise.

14. *Farmers — own account.* Persons who own or rent farms, market gardens, or forests and having no employees other than family workers.

15. *Agricultural workers.* Persons engaged in tending crops, animals, game or forests, or operating agricultural or forestry machinery.

16. *Members of armed forces.*

17. *Inadequately described and not stated occupations.*

Table 8.2: *Reported amounts of UB at the 1st interview*

Value pence	*Freq.*	*Perc.*	*Cum Perc.*	*Value pence*	*Freq.*	*Perc.*	*Cum Perc.*
684	1	.1	.1	2510	2	.3	46.9
800	1	.1	.3	2515	2	.3	47.1
834	1	.1	.4	2517	1	.1	47.3
1250	5	.7	1.1	2520	4	.5	47.8
1625	1	.1	1.2	2530	49	6.7	54.5
1666	1	.1	1.4	2550	4	.5	55.0
1667	1	.1	1.5	2555	1	.1	55.2
1700	1	.1	1.6	2560	27	3.7	58.8
1730	1	.1	1.8	2570	5	.7	59.5
1775	1	.1	1.9	2588	1	.1	59.6
1800	1	.1	2.0	2590	6	.8	60.5
1825	1	.1	2.2	2600	3	.4	60.9
1850	1	.1	2.3	2601	1	.1	61.0
1875	3	.4	2.7	2615	1	.1	61.1
1900	1	.1	2.9	2620	4	.5	61.7
1982	1	.1	3.0	2630	3	.4	62.1
2023	2	.3	3.3	2650	1	.1	62.2
2050	2	.3	3.5	2674	1	.1	62.4
2083	3	.4	3.9	2750	2	.3	62.6
2100	1	.1	4.1	2752	1	.1	62.8
2108	1	.1	4.2	2760	3	.4	63.2
2110	1	.1	4.3	2770	1	.1	63.3
2133	1	.1	4.5	2856	1	.1	63.5
2300	1	.1	4.6	2879	1	.1	63.6
2310	1	.1	4.8	2880	1	.1	63.7
2318	1	.1	4.9	2917	1	.1	63.9
2450	2	.3	5.2	2931	1	.1	64.0
2500	305	41.4	46.6	2950	1	.1	64.1

Reported amounts of UB at the 1st interview.continued

Value pence	Freq.	Perc.	Cum Perc.	Value pence	Freq.	Perc.	Cum Perc.
3033	1	.1	64.3	4170	6	.8	92.3
3070	1	.1	64.4	4175	1	.1	92.4
3100	1	.1	64.5	4180	1	.1	92.5
3114	1	.1	64.7	4195	1	.1	92.7
3135	1	.1	64.8	4200	2	.3	92.9
3200	2	.3	65.1	4330	1	.1	93.1
3300	1	.1	65.2	4433	1	.1	93.2
3345	1	.1	65.4	4450	3	.4	93.6
3465	1	.1	65.5	4530	1	.1	93.8
3490	1	.1	65.6	4540	1	.1	93.9
3500	1	.1	65.8	4590	1	.1	94.0
3545	1	.1	65.9	4600	1	.1	94.2
3750	1	.1	66.0	4630	1	.1	94.3
3780	1	.1	66.2	4634	1	.1	94.4
3800	1	.1	66.3	4650	3	.4	94.8
3805	1	.1	66.4	4700	1	.1	95.0
3900	1	.1	66.6	4840	1	.1	95.1
3974	1	.1	66.7	4850	1	.1	95.2
4000	9	1.2	67.9	4888	1	.1	95.4
4003	1	.1	68.1	4900	1	.1	95.5
4009	1	.1	68.2	4904	1	.1	95.7
4020	2	.3	68.5	4940	2	.3	95.9
4025	4	.5	69.0	5085	1	.1	96.1
4030	1	.1	69.2	5090	1	.1	96.2

Reported amounts of UB at the 1st interview.continued

Value pence	Freq.	Perc.	Cum Perc.	Value pence	Freq.	Perc.	Cum Perc.
4040	5	.7	69.8	5100	1	.1	96.3
4045	71	9.6	79.5	5110	1	.1	96.5
4050	4	.5	80.0	5130	2	.3	96.7
4055	1	.1	80.2	5200	4	.5	97.3
4060	1	.1	80.3	5235	1	.1	97.4
4065	1	.1	80.4	5270	2	.3	97.7
4067	1	.1	80.6	5300	1	.1	97.8
4070	2	.3	80.8	5337	1	.1	98.0
4073	1	.1	81.0	5405	1	.1	98.1
4075	26	3.5	84.5	5465	1	.1	98.2
4080	1	.1	84.6	5473	1	.1	98.4
4090	4	.5	85.2	5770	1	.1	98.5
4095	1	.1	85.3	5800	1	.1	98.6
4100	7	1.0	86.3	5897	1	.1	98.8
4105	17	2.3	88.6	6120	1	.1	98.9
4110	2	.3	88.9	6150	1	.1	99.0
4111	1	.1	89.0	6175	1	.1	99.2
4120	3	.4	89.4	6313	1	.1	99.3
4130	2	.3	89.7	6914	1	.1	99.5
4132	1	.1	89.8	7000	1	.1	99.6
4133	1	.1	89.9	7311	1	.1	99.7
4135	8	1.1	91.0	7756	1	.1	99.9
4150	3	.4	91.4	13020	1	.1	100.0
4170	6	.8	92.3	Total	736	100.0	
4175	1	.1	92.4				

Table 8.3: *Product Limit Estimates: single risk model*

Weeks	Events No.	Cens. No.	Risk Set	Survivor fn.	Error Std.	Cum. Rate
11	0	0	0	1.00000		0.00000
12	4	0	23	0.82609	0.07903	0.19106
13	25	0	382	0.77202	0.07460	0.25874
14	44	0	1127	0.74188	0.07182	0.29856
15	42	0	1692	0.72347	0.07010	0.32370
16	43	0	1845	0.70661	0.06851	0.34728
17	39	0	1864	0.69182	0.06712	0.36843
18	33	0	1838	0.67940	0.06595	0.38654
19	31	0	1805	0.66773	0.06485	0.40387
20	26	0	1774	0.65795	0.06393	0.41863
21	27	0	1748	0.64778	0.06297	0.43420
22	18	0	1721	0.64101	0.06233	0.44471
23	21	0	1703	0.63310	0.06159	0.45712
24	27	0	1682	0.62294	0.06063	0.47330
25	27	0	1655	0.61278	0.05967	0.48975
26	31	0	1628	0.60111	0.05857	0.50898
27	27	0	1597	0.59095	0.05761	0.52603
28	31	0	1570	0.57928	0.05651	0.54597
29	25	0	1539	0.56987	0.05563	0.56235
30	26	0	1514	0.56008	0.05471	0.57967
31	36	0	1488	0.54653	0.05343	0.60416
32	22	0	1452	0.53825	0.05265	0.61943
33	38	0	1430	0.52395	0.05130	0.64636
34	29	0	1392	0.51303	0.05027	0.66742
35	21	0	1363	0.50513	0.04953	0.68294
36	12	0	1342	0.50061	0.04910	0.69193
37	27	0	1330	0.49045	0.04814	0.71244
38	24	0	1303	0.48141	0.04729	0.73103
39	23	0	1279	0.47276	0.04648	0.74917
40	18	0	1256	0.46598	0.04584	0.76361
41	18	0	1238	0.45921	0.04520	0.77825
42	12	0	1220	0.45469	0.04477	0.78814
43	18	0	1208	0.44791	0.04413	0.80315
44	28	0	1190	0.43738	0.04314	0.82696
45	21	0	1162	0.42947	0.04239	0.84520
46	18	0	1141	0.42270	0.04176	0.86110
47	22	0	1123	0.41442	0.04098	0.88089
48	15	0	1101	0.40877	0.04044	0.89460
49	23	0	1086	0.40011	0.03963	0.91601
50	24	0	1063	0.39108	0.03878	0.93885
51	10	0	1039	0.38731	0.03842	0.94852
52	19	0	1029	0.38016	0.03775	0.96715
53	25	0	1010	0.37075	0.03686	0.99222
54	19	0	985	0.36360	0.03618	1.01170
55	15	0	966	0.35796	0.03565	1.02735
56	15	0	951	0.35231	0.03512	1.04324
57	11	0	936	0.34817	0.03473	1.05507
58	16	0	925	0.34215	0.03416	1.07252
59	15	0	909	0.33650	0.03363	1.08915
60	14	0	894	0.33123	0.03313	1.10494
61	13	0	880	0.32634	0.03267	1.11982
62	12	0	867	0.32182	0.03224	1.13376
63	11	0	855	0.31768	0.03185	1.14671
64	10	9	835	0.31388	0.03149	1.15876
65	7	135	690	0.31069	0.03120	1.16895
66	2	300	383	0.30907	0.03105	1.17419
71	0	381	1	0.30907	0.03105	1.17419

Table 8.4: *Product Limit Estimates: Exit to full-time work*

Weeks	Events No.	Cens. No.	Risk Set	Survivor fn.	Error Std.	Cum. Rate
11	0	0	0	1.00000		0.00000
12	2	0	23	0.91304	0.05875	0.09097
13	16	3	382	0.87480	0.05707	0.13376
14	27	8	1127	0.85384	0.05584	0.15801
15	35	17	1692	0.83618	0.05476	0.17891
16	33	7	1845	0.82122	0.05385	0.19696
17	34	10	1864	0.80625	0.05293	0.21537
18	27	5	1838	0.79440	0.05220	0.23017
19	27	6	1805	0.78252	0.05147	0.24524
20	22	4	1774	0.77281	0.05087	0.25772
21	23	4	1748	0.76265	0.05025	0.27096
22	16	4	1721	0.75556	0.04981	0.28030
23	18	4	1703	0.74757	0.04932	0.29093
24	23	1	1682	0.73735	0.04869	0.30470
25	21	4	1655	0.72799	0.04812	0.31747
26	23	6	1628	0.71771	0.04748	0.33170
27	22	8	1597	0.70782	0.04688	0.34557
28	25	5	1570	0.69655	0.04618	0.36162
29	21	6	1539	0.68704	0.04560	0.37536
30	21	4	1514	0.67751	0.04501	0.38933
31	24	5	1488	0.66659	0.04434	0.40559
32	20	12	1452	0.65740	0.04378	0.41946
33	33	2	1430	0.64223	0.04285	0.44280
34	25	5	1392	0.63070	0.04214	0.46093
35	17	4	1363	0.62283	0.04166	0.47348
36	11	4	1342	0.61773	0.04135	0.48171
37	21	3	1330	0.60797	0.04075	0.49762
38	16	4	1303	0.60051	0.04029	0.50998
39	17	8	1279	0.59253	0.03980	0.52336
40	15	6	1256	0.58545	0.03937	0.53537
41	13	3	1238	0.57930	0.03899	0.54593
42	12	5	1220	0.57360	0.03864	0.55581
43	15	0	1208	0.56648	0.03821	0.56831
44	24	3	1190	0.55506	0.03751	0.58868
45	16	5	1162	0.54741	0.03704	0.60255
46	15	4	1141	0.54022	0.03660	0.61578
47	17	3	1123	0.53204	0.03610	0.63104
48	13	5	1101	0.52576	0.03572	0.64291
49	12	3	1086	0.51995	0.03536	0.65403
50	15	11	1063	0.51261	0.03491	0.66824
51	7	9	1039	0.50916	0.03470	0.67500
52	11	4	1029	0.50372	0.03437	0.68574
53	16	6	1010	0.49574	0.03388	0.70171
54	14	9	985	0.48869	0.03345	0.71603
55	11	5	966	0.48312	0.03311	0.72748
56	9	6	951	0.47855	0.03284	0.73699
57	8	4	936	0.47446	0.03259	0.74557
58	10	3	925	0.46933	0.03227	0.75644
59	10	7	909	0.46417	0.03196	0.76750
60	12	4	894	0.45794	0.03158	0.78102
61	7	2	880	0.45430	0.03136	0.78900
62	6	8	867	0.45115	0.03117	0.79595
63	9	5	855	0.44640	0.03088	0.80653
64	8	10	835	0.44213	0.03062	0.81616
65	6	137	690	0.43828	0.03040	0.82489
66	2	301	383	0.43599	0.03028	0.83013
71	0	381	1	0.43599	0.03028	0.83013

Table 8.5: *Product Limit Estimates: Exit to other states*

Weeks	Events No.	Cens. No.	Risk Set	Survivor fn.	Error Std.	Cum. Rate
11	0	0	0	1.00000		0.00000
12	2	1	23	0.91304	0.05875	0.09097
13	9	1	382	0.89153	0.05781	0.11481
14	16	18	1127	0.87887	0.05707	0.12911
15	7	33	1692	0.87524	0.05685	0.13326
16	9	32	1845	0.87097	0.05659	0.13815
17	5	33	1864	0.86863	0.05645	0.14083
18	6	34	1838	0.86580	0.05628	0.14410
19	3	34	1805	0.86436	0.05619	0.14577
20	4	24	1774	0.86241	0.05607	0.14802
21	4	34	1748	0.86044	0.05595	0.15032
22	2	11	1721	0.85944	0.05589	0.15148
23	3	10	1703	0.85792	0.05580	0.15324
24	4	24	1682	0.85588	0.05568	0.15562
25	6	23	1655	0.85278	0.05549	0.15925
26	8	18	1628	0.84859	0.05524	0.16418
27	5	30	1597	0.84593	0.05508	0.16732
28	5	14	1570	0.84324	0.05491	0.17051
29	4	32	1539	0.84105	0.05478	0.17311
30	5	19	1514	0.83827	0.05462	0.17642
31	12	22	1488	0.83151	0.05421	0.18451
32	1	30	1452	0.83094	0.05418	0.18520
33	5	24	1430	0.82803	0.05400	0.18871
34	3	30	1392	0.82625	0.05389	0.19086
35	4	14	1363	0.82382	0.05375	0.19380
36	1	21	1342	0.82321	0.05371	0.19455
37	6	6	1330	0.81949	0.05349	0.19907
38	8	27	1303	0.81446	0.05319	0.20523
39	6	12	1279	0.81064	0.05297	0.20993
40	3	18	1256	0.80870	0.05285	0.21232
41	5	13	1238	0.80544	0.05266	0.21637
43	3	25	1208	0.80344	0.05254	0.21885
44	4	20	1190	0.80074	0.05238	0.22222
45	5	18	1162	0.79729	0.05218	0.22653
46	3	18	1141	0.79520	0.05206	0.22917
47	5	14	1123	0.79166	0.05185	0.23363
48	2	20	1101	0.79022	0.05176	0.23545
49	10	9	1086	0.78294	0.05134	0.24470
50	8	13	1063	0.77705	0.05099	0.25225
51	3	16	1039	0.77481	0.05086	0.25514
52	8	7	1029	0.76878	0.05051	0.26295
53	9	14	1010	0.76193	0.05011	0.27190
54	5	17	985	0.75806	0.04989	0.27699
55	4	11	966	0.75492	0.04971	0.28114
56	5	10	951	0.75095	0.04948	0.28641
57	3	11	936	0.74855	0.04934	0.28962
58	6	9	925	0.74369	0.04906	0.29613
59	5	8	909	0.73960	0.04882	0.30164
60	2	14	894	0.73795	0.04873	0.30388
61	6	10	880	0.73292	0.04844	0.31072
62	6	5	867	0.72784	0.04815	0.31767
63	2	6	855	0.72614	0.04805	0.32001
64	2	20	835	0.72440	0.04795	0.32241
71	0	831	1	0.72440	0.04795	0.32241

Table 8.6: *Baseline hazard: Single and Competing risks models*

Weekly Baseline	Single risk		Competing risks			
			Full-time Work		Other exits	
	Coeff	SE	Coeff	SE	Coeff	SE
Week 11	-15.4745	366.2355	-20.3138	1885.278	-13.9144	2576.1550
Week 12	-5.1155*	0.9744	-7.4675*	1.1684	-0.0234	1.8638
Week 13	-4.3693*	0.8588	-6.4728*	0.9626	0.3871	1.7527
Week 14	-4.2298*	0.8489	-6.3554*	0.9491	0.5713	1.7380
Week 15	-4.3369*	0.8489	-6.1789*	0.9443	-0.3430	1.7599
Week 16	-4.3433*	0.8488	-6.2427*	0.9451	-0.1064	1.7500
Week 17	-4.3979*	0.8498	-6.1933*	0.9446	-0.6752	1.7751
Week 18	-4.5436*	0.8528	-6.4036*	0.9489	-0.4673	1.7664
Week 19	-4.6184*	0.8544	-6.3822*	0.9487	-1.1421	1.8127
Week 20	-4.7439*	0.8575	-6.5700*	0.9532	-0.8341	1.7897
Week 21	-4.6898*	0.8568	-6.5101*	0.9524	-0.8134	1.7900
Week 22	-5.0834*	0.8675	-6.8608*	0.9622	-1.4964	1.8584
Week 23	-4.9138*	0.8626	-6.7267*	0.9583	-1.0790	1.8125
Week 24	-4.6432*	0.8566	-6.4622*	0.9520	-0.7722	1.7897
Week 25	-4.6210*	0.8568	-6.5296*	0.9545	-0.3486	1.7668
Week 26	-4.4598*	0.8539	-6.4143*	0.9522	-0.0410	1.7550
Week 27	-4.5766*	0.8567	-6.4369*	0.9532	-0.4910	1.7762
Week 28	-4.4489*	0.8545	-6.2853*	0.9503	-0.4731	1.7763
Week 29	-4.6105*	0.8583	-6.4376*	0.9542	-0.6786	1.7902
Week 30	-4.5504*	0.8571	-6.4157*	0.9539	-0.4383	1.7758
Week 31	-4.1969*	0.8508	-6.2531*	0.9507	0.4628	1.7428
Week 32	-4.7176*	0.8622	-6.4157*	0.9549	-2.0084	1.9882
Week 33	-4.0902*	0.8499	-5.8798*	0.9446	-0.3667	1.7756
Week 34	-4.3709*	0.8554	-6.1313*	0.9497	-0.8570	1.8129
Week 35	-4.6364*	0.8623	-6.4948*	0.9596	-0.5471	1.7896
Week 36	-5.1863*	0.8829	-6.9202*	0.9763	-1.9243	1.9883
Week 37	-4.3533*	0.8562	-6.2511*	0.9537	-0.1126	1.7662
Week 38	-4.4490*	0.8590	-6.5006*	0.9617	0.1966	1.7545
Week 39	-4.4729*	0.8599	-6.4211*	0.9596	-0.0724	1.7662
Week 40	-4.7031*	0.8670	-6.5306*	0.9637	-0.7531	1.8127
Week 41	-4.6843*	0.8666	-6.6543*	0.9686	-0.2242	1.7754
Week 42	-5.0751*	0.8825	-6.7192*	0.9719	-14.0259	447.0093
Week 43	-4.6506*	0.8666	-6.4771*	0.9632	-0.7028	1.8127
Week 44	-4.1794*	0.8550	-5.9769*	0.9501	-0.3898	1.7893
Week 45	-4.4476*	0.8620	-6.3622*	0.9612	-0.1503	1.7754
Week 46	-4.5821*	0.8666	-6.4058*	0.9633	-0.6451	1.8126
Week 47	-4.3570*	0.8606	-6.2566*	0.9591	-0.1091	1.7749
Week 48	-4.7264*	0.8727	-6.5102*	0.9684	-1.0152	1.8572
Week 49	-4.3179*	0.8604	-6.5658*	0.9716	0.6234	1.7465
Week 50	-4.2496*	0.8594	-6.3204*	0.9631	0.4300	1.7536
Week 51	-5.0727*	0.8916	-7.0728*	1.0018	-0.5413	1.8119
Week 52	-4.4127*	0.8647	-6.6030*	0.9756	0.4589	1.7535
Week 53	-4.1114*	0.8573	-6.2017*	0.9608	0.6033	1.7494
Week 54	-4.3643*	0.8646	-6.3132*	0.9654	0.0357	1.7746
Week 55	-4.5835*	0.8728	-6.5362*	0.9756	-0.1728	1.7888
Week 56	-4.6334*	0.8754	-6.7173*	0.9857	0.0688	1.7747
Week 57	-4.8599*	0.8863	-6.8194*	0.9925	-0.4298	1.8117
Week 58	-4.4668*	0.8700	-6.5781*	0.9797	0.2825	1.7649
Week 59	-4.5113*	0.8724	-6.5572*	0.9797	0.1174	1.7741
Week 60	-4.5594*	0.8753	-6.3522*	0.9713	-0.7833	1.8571
Week 61	-4.6160*	0.8783	-6.8726*	1.0013	0.3305	1.7652
Week 62	-4.6759*	0.8822	-7.0069*	1.0134	0.3517	1.7657
Week 63	-4.7361*	0.8864	-6.5738*	0.9856	-0.7221	1.8577
Week 64	-4.6504*	0.8914	-6.5128*	0.9924	-0.5317	1.8581
Week 65	-4.7023*	0.9043	-6.3312*	0.9910	-14.0406	646.9083

Table 8.7: *The significance of the differences between the estimated weekly baseline coefficients*

Coefficients	$\hat{\alpha}_l - \hat{\alpha}_{l+1}$	$\widehat{SE}(\alpha_l - \alpha_{l+1})$
$\alpha_{13} - \alpha_{12}$	0.9947	0.75004
$\alpha_{14} - \alpha_{13}$	0.1174	0.31549
$\alpha_{15} - \alpha_{14}$	0.1765	0.25613
$\alpha_{16} - \alpha_{15}$	0.0638	0.24632
$\alpha_{17} - \alpha_{16}$	0.0494	0.24438
$\alpha_{18} - \alpha_{17}$	0.2103	0.25778
$\alpha_{19} - \alpha_{18}$	0.0214	0.27214
$\alpha_{20} - \alpha_{19}$	0.1878	0.28721
$\alpha_{21} - \alpha_{20}$	0.0599	0.29822
$\alpha_{22} - \alpha_{21}$	0.3507	0.32554
$\alpha_{23} - \alpha_{22}$	0.1341	0.34359
$\alpha_{24} - \alpha_{23}$	0.2645	0.31471
$\alpha_{25} - \alpha_{24}$	0.0674	0.38034
$\alpha_{26} - \alpha_{25}$	0.1153	0.30184
$\alpha_{27} - \alpha_{26}$	0.0226	0.29823
$\alpha_{28} - \alpha_{27}$	0.1516	0.29232
$\alpha_{29} - \alpha_{28}$	0.1523	0.29599
$\alpha_{30} - \alpha_{29}$	0.0219	0.30861
$\alpha_{31} - \alpha_{30}$	0.1626	0.29879
$\alpha_{32} - \alpha_{31}$	0.1626	0.30275
$\alpha_{33} - \alpha_{32}$	0.5359*	0.28337
$\alpha_{34} - \alpha_{33}$	0.2515	0.26514
$\alpha_{35} - \alpha_{34}$	0.3635	0.31436
$\alpha_{36} - \alpha_{35}$	0.4254	0.38696
$\alpha_{37} - \alpha_{36}$	0.6691*	0.37221
$\alpha_{38} - \alpha_{37}$	0.2495	0.34512
$\alpha_{39} - \alpha_{38}$	0.0795	0.34829
$\alpha_{40} - \alpha_{39}$	0.1095	0.35424
$\alpha_{41} - \alpha_{40}$	0.1237	0.37895
$\alpha_{42} - \alpha_{41}$	0.0649	0.40033
$\alpha_{43} - \alpha_{42}$	0.2421	0.40029
$\alpha_{44} - \alpha_{43}$	0.5002*	0.31915
$\alpha_{45} - \alpha_{44}$	0.3853	0.32276
$\alpha_{46} - \alpha_{45}$	0.0436	0.35435
$\alpha_{47} - \alpha_{46}$	0.1492	0.35423
$\alpha_{48} - \alpha_{47}$	0.2536	0.36843
$\alpha_{49} - \alpha_{48}$	0.0556	0.40031
$\alpha_{50} - \alpha_{49}$	0.2454	0.38731
$\alpha_{51} - \alpha_{50}$	0.7524*	0.45841
$\alpha_{52} - \alpha_{51}$	0.4698	0.48349
$\alpha_{53} - \alpha_{52}$	0.4013	0.39166
$\alpha_{54} - \alpha_{53}$	0.1115	0.36590
$\alpha_{55} - \alpha_{54}$	0.2230	0.40290
$\alpha_{56} - \alpha_{55}$	0.1811	0.44945
$\alpha_{57} - \alpha_{56}$	0.1021	0.48591
$\alpha_{58} - \alpha_{57}$	0.2413	0.47434
$\alpha_{59} - \alpha_{58}$	0.0209	0.44721
$\alpha_{60} - \alpha_{59}$	0.205	0.42818
$\alpha_{61} - \alpha_{60}$	0.5204	0.47558

The first 11 weeks are not considered because of left truncation of the sample at the three months point. The table relates only to the baseline hazard rate for the exit into full-time work. A * indicates statistical significance at the two-sided 5% level. The estimated weekly coefficients and their standard error are shown in the Appendix.

Table 8.8: *The significance of the differences between the estimated monthly baseline coefficients*

Monthly coefficients	$\hat{\alpha}_l - \hat{\alpha}_{l+4}$	$\hat{SE}(\alpha_l - \alpha_{l+4})$
$\alpha_4 - \alpha_3$	0.2378	0.04868
$\alpha_5 - \alpha_4$	0.3146	0.10890
$\alpha_6 - \alpha_5$	0.035	0.15205
$\alpha_7 - \alpha_6$	0.1367	0.15205
$\alpha_8 - \alpha_7$	0.2371	0.1405
$\alpha_9 - \alpha_8$	0.3618	0.15868
$\alpha_{10} - \alpha_9$	0.0569	0.18149
$\alpha_{11} - \alpha_{10}$	0.2872	0.17841
$\alpha_{12} - \alpha_{11}$	0.1186	0.17844
$\alpha_{13} - \alpha_{12}$	0.1277	0.1974
$\alpha_{14} - \alpha_{13}$	0.1531	0.21716
$\alpha_{15} - \alpha_{14}$	0.0041	0.23428
$\alpha_{16} - \alpha_{15}$	0.1773	0.26158

The first three months are not considered because of left truncation of the sample at the three months point. The table relates only to the baseline hazard rate for the exit into full-time work.

Table 8.9: *Restricted model for testing equality of the baselines*

variable	Coeff	SE
FtworkYb	-0.8061*	0.1758
UnemplYb	-0.6251*	0.1998
SickYb	-0.3014	0.2970
PrIntmOc	0.4506*	0.1788
UnskilOc	-0.2916	0.2890
Age20-24	-0.1748	0.2281
Age35-44	-0.0113	0.1802
Age45-54	-0.3978*	0.1990
Age55-58	-0.5235*	0.2267
ChildLt5	-0.4130*	0.1735
Married	-0.0389	0.1739
Moneybad	0.0781	0.1406
HouseOwn	0.3781*	0.1378
CountyUn	-0.0462*	0.0168
OnlyUBT1	0.4649*	0.1527
NoUBSBT1	-0.2265	0.4995
UBenefit	-0.3831*	0.1218
PredEarn	-0.7789*	0.3024
NoPredEa	-3.1767*	1.4433
z*FtworkYb	1.1357*	0.2171
z*UnemplYb	0.8878*	0.2422
z*SickYb	0.4426	0.4119
z*PrIntmOc	-0.4938*	0.2055
z*UnskilOc	0.0655	0.3368
z*Age20-24	0.3065	0.2487
z*Age35-44	-0.0556	0.2012
z*Age45-54	0.1363	0.2266
z*Age55-58	-0.1522	0.2831
z*ChildLt5	0.2962	0.1933
z*Married	0.1839	0.2104
z*Moneybad	0.1434	0.1621
z*CountyUn	-0.2909	0.1575
z*HouseOwn	4.3050*	1.8337
z*OnlyUBT1	-0.3629 *	0.1784
z*NoUBSBT1	-0.8733*	0.3360
z*UBenefit	0.0001*	0.0000
z*PredEarn	0.0001*	0.0000
z*NoPredEa	0.5494	0.6534
Max. likelihood:-5731.171. A * indicates statistical significance at the 5% one-sided level.		

Table 8.10: *Unemployment duration by the exit states*

Unemployment duration in Weeks	*Recipients of only UB* Frequency of Exits		*Recipients of SB* Frequency of Exits	
	Full-Time Work Exit	*Other States Exit*	*Full-Time Work Exit*	*Other States Exit*
12	0	0	1	1
13	7	5	9	3
14	9	6	15	8
15	17	3	15	4
16	14	2	18	4
17	15	4	17	1
18	18	0	9	5
19	9	0	17	2
20	12	3	9	1
21	6	1	14	2
22	8	0	8	2
23	5	1	12	1
24	7	1	16	1
25	6	4	14	2
26	6	1	17	7
27	10	3	11	2
28	12	2	13	3
29	7	3	13	1
30	9	1	11	4
31	11	6	13	6
32	6	0	13	0
33	12	2	19	2
34	11	1	14	2
35	7	1	6	2
36	3	1	8	0
37	5	2	15	4
38	5	4	11	3
39	5	4	12	2
40	3	1	12	2
41	2	2	9	2
42	6	0	5	0
43	5	2	9	1
44	12	1	11	3
45	5	2	10	3
46	7	0	8	3
47	3	3	13	2
48	4	1	9	1
49	4	2	6	5
50	5	5	6	3
51	2	2	5	1
52	2	5	6	3
53	7	7	9	2
54	5	3	9	1
55	0	2	11	1
56	5	2	4	2
57	3	0	5	2
58	2	3	7	2
59	5	1	5	4
60	1	0	11	2
61	3	5	4	1
62	4	1	2	3
63	5	1	4	1
64	2	0	6	2
65	3	0	2	
66			1	
Sum	347	112	546	126

Table 8.11: *Baseline hazards: model with time varying benefit entitlement duration dummies*

Weekly	Full-time exit		Other states exit	
Baseline	Coeff	SE	Coeff	SE
Week 11	-22.3227	3561.8353	-15.6797	3561.8353
Week 12	-8.9468*	1.3949	-1.0158	1.3949
Week 13	-7.2835*	1.0039	-0.0352	1.0039
Week 14	-7.2893*	0.9929	0.0726	0.9929
Week 15	-7.0883*	0.9874	-0.6833	0.9874
Week 16	-7.0966*	0.9874	-0.8675	0.9874
Week 17	-7.0782*	0.9872	-1.0390	0.9872
Week 18	-7.2289*	0.9903	-1.0022	0.9903
Week 19	-7.2477*	0.9907	-1.9192	0.9907
Week 20	-7.4478*	0.9955	-1.2434	0.9955
Week 21	-7.4855*	0.9967	-1.4816	0.9967
Week 22	-7.6970*	1.0030	-1.8848	1.0030
Week 23	-7.6206*	1.0008	-1.8546	1.0008
Week 24	-7.3033*	0.9931	-1.8626	0.9931
Week 25	-7.4183*	0.9967	-0.7222	0.9967
Week 26	-7.2530*	0.9933	-0.4059	0.9933
Week 27	-7.3249*	0.9955	-0.8591	0.9955
Week 28	-7.1240*	0.9916	-0.8841	0.9916
Week 29	-7.3260*	0.9965	-1.0862	0.9965
Week 30	-7.3031*	0.9962	-0.8430	0.9962
Week 31	-7.0911*	0.9918	0.0847	0.9918
Week 32	-7.3085*	0.9969	-15.5589	0.9969
Week 33	-6.7858*	0.9867	-1.0143	0.9867
Week 34	-6.9685*	0.9909	-1.2301	0.9909
Week 35	-7.4561*	1.0044	-1.2150	1.0044
Week 36	-7.7557*	1.0165	-2.3279	1.0165
Week 37	-7.1348*	0.9961	-0.4855	0.9961
Week 38	-7.3331*	1.0025	-0.3234	1.0025
Week 39	-7.2520*	1.0005	-0.4451	1.0005
Week 40	-7.3587*	1.0042	-1.1677	1.0042
Week 41	-7.6509*	1.0158	-0.8189	1.0158
Week 42	-8.1353*	1.0201	-15.8033	1.0201
Week 43	-7.8684*	1.0105	-1.4831	1.0105
Week 44	-7.3347*	0.9967	-1.1379	0.9967
Week 45	-7.7404*	1.0084	-0.8929	1.0084
Week 46	-7.4818*	1.0057	-1.6555	1.0057
Week 47	-7.3926*	1.0037	-1.1205	1.0037
Week 48	-7.5866*	1.0108	-2.0238	1.0108
Week 49	-7.6414*	1.0139	-0.7489	1.0139
Week 50	-7.5400*	1.0107	-0.5855	1.0107
Week 51	-8.3098*	1.0655	-3.4033	1.0655
Week 52	-7.8311*	1.0229	-0.8688	1.0229
Week 53	-7.3296*	1.0042	-0.7110	1.0042
Week 54	-7.4382*	1.0086	-1.5000	1.0086
Week 55	-7.6639*	1.0184	-1.7766	1.0184
Week 56	-7.8969*	1.0258	-1.0794	1.0258
Week 57	-7.9989*	1.0324	-1.7609	1.0324
Week 58	-7.8621*	1.0255	-0.8294	1.0255
Week 59	-7.7348*	1.0200	-0.8108	1.0200
Week 60	-7.5311*	1.0119	-1.7127	1.0119
Week 61	-8.0475*	1.0406	-0.5945	1.0406
Week 62	-8.1849*	1.0520	-0.9857	1.0520
Week 63	-7.7474*	1.0251	-1.6517	1.0251
Week 64	-7.6964*	1.0319	-1.4670	1.0319
Week 65	-7.7799*	1.0492	-15.9100	1.0492

A * indicates statistical significance at the two-sided 5% level.

Table 8.12: *Results of estimation of the re-employment probability*

Variable label	*Recipients of only UB*		*SB/ joint SB and UB*		*All benefits*	
	Coeff	*SE*	*Coeff*	*SE*	*Coeff*	*SE*
F/t work year before	*-0.1398*	*0.2535*	*0.5969**	*0.1778*	*0.3584**	*0.1556*
Unemployed year before	*0.1673*	*0.2859*	*0.4420**	*0.1868*	*0.4197**	*0.1448*
Sick year before	*-0.6378*	*0.5591*	*-0.3729*	*0.3765*	*-0.4257*	*0.3113*
Profes. /Interm. Occ.	*0.3244**	*0.1609*	*0.0739*	*0.1337*	*0.2034**	*0.1003*
Unskilled Occupation	*-0.5214*	*0.2999*	*-0.4298**	*0.2158*	*-0.4485**	*0.1744*
Age 20-24	*0.3357*	*0.1993*	*0.1531*	*0.1318*	*0.2035*	*0.1087*
Age 35-44	*-0.1134*	*0.1617*	*-0.2378**	*0.1133*	*-0.2084**	*0.0919*
Age 45-54	*-0.8165**	*0.1693*	*-0.4552**	*0.1590*	*-0.6067**	*0.1123*
Age 55-58	*-1.4538**	*0.2243*	*-0.7950**	*0.3173*	*-1.2036**	*0.1742*
Has any child aged < 5	*-0.6509**	*0.1767*	*-0.0672*	*0.1070*	*-0.2355**	*0.0883*
Married	*0.5777**	*0.2366*	*-0.0791*	*0.1554*	*0.1682*	*0.1244*
Spouse working month before	*0.2697**	*0.1292*	*0.3375**	*0.1323*	*0.4071**	*0.0874*
Searches less than before	*-1.1211**	*0.2712*	*-0.4995**	*0.2368*	*-0.7646**	*0.1794*
Values Leisure more	*-0.1046*	*0.1730*	*-0.3279**	*0.1659*	*-0.2588**	*0.1182*
Experiences money shortage	*0.1861*	*0.1269*	*0.2213*	*0.1281*	*0.1905**	*0.0875*
House owner	*0.1981*	*0.1253*	*0.2815**	*0.0988*	*0.3000**	*0.0748*
County unemployment rate	*-0.0273*	*0.0172*	*-0.0144*	*0.0141*	*-2.0014*	*1.0881*
UB/SB time varying	*-0.0958*	*0.0791*	*0.0852*	*0.1155*	*-0.0706*	*0.0556*
Predicted earnings	*0.6910**	*0.3551*	*0.7417**	*0.2612*	*0.6522**	*0.2076*
No pred. earn.	*3.8496**	*1.6559*	*3.1346**	*1.2504*	*3.0254**	*0.9831*

only UB max. lik. -2328.7; SB / joint SB, UB max. lik. -3655; all benefit recipients max. lik. -6077.0. Likelihood ratio test: $186.6 \sim \chi^2_{150}$. *Descriptive statistics of explanatory variables are provided in the preceding table, in the data section. A * indicates statistical significance at the two-sided 5% level. The estimated baseline coefficients are given in the Appendix.*

Table 8.13: *Baseline hazard: Benefit recipients types. Exit into full-time work.*

Weekly	Recipients of only UB		Recipients of SB, joint SB and UB		All benefit recipients	
Baseline	Coeff	SE	Coeff	SE	Coeff	SE
Week 11	-23.4951	14613.5149	-24.2043	6777.3044	-21.0798	2052.2847
Week 12	-23.6266	2839.1168	-9.3265*	1.6010	-8.8373*	1.3882
Week 13	-6.7597*	1.6600	-8.3058*	1.2917	-7.1689*	0.9950
Week 14	-6.9255*	1.6497	-8.2095*	1.2738	-7.1730*	0.9838
Week 15	-6.3620*	1.6321	-8.2986*	1.2741	-6.9696*	0.9783
Week 16	-6.5565*	1.6362	-8.1309*	1.2699	-6.9776*	0.9784
Week 17	-6.4617*	1.6349	-8.1742*	1.2707	-6.9587*	0.9781
Week 18	-6.2430*	1.6323	-8.7997*	1.2913	-7.1078*	0.9813
Week 19	-6.9114*	1.6491	-8.1414*	1.2703	-7.1230*	0.9817
Week 20	-6.5914*	1.6407	-8.7670*	1.2908	-7.3189*	0.9864
Week 21	-7.2682*	1.6660	-8.3106*	1.2754	-7.3538*	0.9877
Week 22	-6.9659*	1.6531	-8.8584*	1.2963	-7.5634*	0.9939
Week 23	-7.4240*	1.6753	-8.4371*	1.2798	-7.4886*	0.9918
Week 24	-7.0725*	1.6587	-8.1290*	1.2715	-7.1676*	0.9841
Week 25	-7.1925*	1.6658	-8.2428*	1.2756	-7.2835*	0.9877
Week 26	-7.1624*	1.6645	-8.0237*	1.2708	-7.1186*	0.9843
Week 27	-6.6246*	1.6447	-8.4419*	1.2833	-7.1887*	0.9864
Week 28	-6.3996*	1.6392	-8.2546*	1.2776	-6.9871*	0.9824
Week 29	-6.9071*	1.6571	-8.2370*	1.2775	-7.1879*	0.9873
Week 30	-6.6239*	1.6462	-8.3829*	1.2827	-7.1650*	0.9870
Week 31	-6.3785*	1.6408	-8.1923*	1.2768	-6.9528*	0.9826
Week 32	-6.9613*	1.6639	-8.1711*	1.2761	-7.1646*	0.9879
Week 33	-6.2185*	1.6382	-7.7610*	1.2668	-6.6389*	0.9776
Week 34	-6.2630*	1.6408	-8.0445*	1.2740	-6.8252*	0.9816
Week 35	-6.6758*	1.6570	-8.5874*	1.2949	-7.3141*	0.9952
Week 36	-7.5065*	1.7140	-8.5781*	1.2949	-7.6139*	1.0074
Week 37	-6.9767*	1.6747	-7.9216*	1.2718	-6.9923*	0.9867
Week 38	-6.9492*	1.6745	-8.2119*	1.2818	-7.1920*	0.9933
Week 39	-6.9257*	1.6742	-8.1062*	1.2785	-7.1124*	0.9913
Week 40	-7.4184*	1.7128	-8.0878*	1.2787	-7.2203*	0.9952
Week 41	-7.8158*	1.7607	-8.3574*	1.2890	-7.5152*	1.0070
Week 42	-6.6765*	1.6617	-8.9392*	1.3232	-7.5007*	1.0068
Week 43	-6.8241*	1.6716	-8.3364*	1.2891	-7.2394*	0.9972
Week 44	-5.9046*	1.6374	-8.1152*	1.2809	-6.7143*	0.9831
Week 45	-6.7555*	1.6732	-8.1915*	1.2846	-7.1209*	0.9951
Week 46	-6.3731*	1.6558	-8.3993*	1.2942	-7.0984*	0.9950
Week 47	-7.1891*	1.7138	-7.8888*	1.2750	-7.0093*	0.9929
Week 48	-6.8860*	1.6890	-8.2425*	1.2882	-7.2025*	1.0000
Week 49	-6.8626*	1.6888	-8.3347*	1.2934	-7.2573*	1.0031
Week 50	-6.6192*	1.6737	-8.3157*	1.2937	-7.1564*	1.0000
Week 51	-7.6157*	1.7487	-8.7769*	1.3223	-7.7896*	1.0282
Week 52	-7.5969*	1.7485	-8.2883*	1.2939	-7.4155*	1.0074
Week 53	-6.3004*	1.6430	-8.1480*	1.2884	-6.9170*	0.9885
Week 54	-6.6039*	1.6607	-8.1289*	1.2883	-7.0279*	0.9930
Week 55	-23.4084	1999.8754	-7.9049*	1.2808	-7.2512*	1.0029
Week 56	-6.5535*	1.6589	-8.9047*	1.3414	-7.4315*	1.0128
Week 57	-7.0485*	1.6987	-8.6652*	1.3222	-7.5327*	1.0194
Week 58	-7.4357*	1.7468	-8.3134*	1.3003	-7.3978*	1.0125
Week 59	-6.4868*	1.6580	-8.6316*	1.3222	-7.2705*	1.0069
Week 60	-8.0879*	1.8836	-7.8104*	1.2806	-7.0618*	0.9986
Week 61	-6.9569*	1.6967	-8.8117*	1.3412	-7.5828*	1.0279
Week 62	-6.6389*	1.6718	-9.4933*	1.4313	-7.7212*	1.0394
Week 63	-6.3788*	1.6567	-8.7743*	1.3411	-7.2858*	1.0123
Week 64	-7.1095*	1.7445	-8.1976*	1.3090	-7.2269*	1.0189
Week 65	-6.5076*	1.6894	-8.6855*	1.3708	-7.3085*	1.0371

A * indicates statistical significance at the 5% two-sided level.