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Transitions out of and back to employment among older men and women in the UK

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Abstract

This paper analyses the labour market transitions of older men and women using data from the British Household Panel Survey (BHPS). I find large peaks in exit rates out of employment at ages 60 (women) and 65 (both sexes) which occur in the exact birthday month. This suggests that pension schemes have strong incentive effects. Discrete-time hazard regression analysis shows that benefits and health status are the two most important determinants of retirement, with effects that are larger than found in previous studies for British and US men. When modelling unobserved heterogeneity I find that women are twice as likely as men to be 'movers' between work and non-work.

Résumé

Nous analysons les transitions sur le marché du travail des travailleurs âgés en nous appuyant sur les données de l'enquête longitudinale des ménages britanniques (BHPS). Nous trouvons des pics de probabilité de départ à la retraite chez les travailleurs âgés de 60 à 65 ans manifestes durant le mois correspondant exactement à leur mois de naissance, suggérant l'existence d'incitatifs importants provenant du système de pension. Une analyse de régression d'un modèle de risques en temps discret montre que l'état de santé et les revenus potentiels ne provenant pas du travail jouent un rôle significatif dans la décision de départ à la retraite et que leurs effets sont plus importants que ne le laissaient prévoir les études antérieures portant sur les hommes britanniques et américains. Quand l'hétérogénéité non observée est prise en considération, nous trouvons que la proportion des femmes quittant le marché du travail est deux fois plus importante que celle des hommes.

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1 Introduction

In most industrialised countries, the average life expectancy has risen substantially while the average retirement age has fallen. This has increased the number of pensioners each worker has to finance through public pension systems. The United Kingdom is no exception: as Banks and Smith (2006: 41) show, the median retirement age has fallen from 65 for the 1910–1914 cohort to 61 for the 1935–1939 cohort. While today there are three working people for each person aged 65+, this value is projected to fall to two by 2030 (OECD 2000: 213–214). This has led to gaps in government budgets which, in turn, have led to important changes in the provision of pensions and healthcare: the share of public pension income has fallen in many countries; the financing need for healthcare, in particular for long-term care, will rise sharply.

In the United Kingdom, the level of net social transfers (public pensions and other benefits) as a share of older people's total income has remained relatively constant over the last decades (although on a lower level than in most countries in continental Europe) while the share of work income has fallen and the share of capital income (including occupational pensions) risen (OECD 2000: 44). The GDP share of publicly financed long-term care is projected to rise by 15–20% from 2000 to 2020 (OECD 2000: 64).

The important role which occupational pension (OP) schemes play is a particular feature of the pension provision in the United Kingdom: capital income (which includes income from OP schemes) contributed less than 30% of pension income in the mid-70s, but more than 40% in the mid-90s (OECD 2000: 44).[1] In my estimation sample from the British Household Panel Survey (BHPS), 65% of men and 46% of women have contributed to, or received money from, an OP scheme at least during one wave.

On the micro level, these developments have contributed to higher numbers of people

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[1] For a more detailed discussion of the UK pension system and an overview of its evolution, see Emmerson (2003).

without adequate retirement income and healthcare entitlement, whether public or private, and have led to a great interest in the understanding of older people's labour market participation and, in particular, their retirement behaviour. When does somebody retire and why? Who might return to work and why? How are these transitions affected by how much one could earn in the labour market and by how much income one could have without working? These are the questions I want to address in this paper.

There are many descriptive papers and policy studies of these issues. There are also two main types of more analytical approaches: structural models of the retirement decision and reduced-form hazard regression models which allow for more freedom in the functional form of agents' underlying economic decision-making.

Lazear (1986) is a valuable survey of structural retirement models. There are (a) purely theoretical microeconomic models, (b) econometric implementations of life-cycle models, and (c) dynamic programming models whose parameters can (or could) be fitted to real-world data. Based on assumptions about individuals' utility functions, these models allow analyses of the effects of exogenous factors on retirement behaviour. For example, Kingston (2000) shows in a theoretical model of the optimal choice of the date of retirement that people with higher assets, lower wages, higher disutility of effort, and lower life expectancy will retire earlier than others.

Hazard regression models (also called duration, survival, or time-to-event models) do not impose assumptions about utility functions, etc. They are used to model discrete choices along a time axis. In the case of retirement, this choice is typically between work and non-work, but can also include a bigger variety of labour market states. While structural retirement models are usually limited to 'the exit' from the labour market (considered to be a one-off event without any option to return to the labour market), many hazard regression models also model the return to the labour market. This facilitates a more precise analysis of how different factors affect retirement behaviour, separating exit behaviour from return

behaviour. In the case of the UK, taking the return into account is particularly important since previous research has highlighted how rarely older people return to work (cf. Smeaton and McKay 2003 and Humphrey et al. 2003) which makes it important to understand why it does or does not occur.

Research on retirement timing using time-to-event analysis has not been very common, partly because detailed longitudinal data are not available for many countries. Examples include Blau (1994) about the US (using the Retirement History Survey) and Mastrobuoni (2000) about Germany (using the German Socio-Economic Panel) and Italy (using the Survey on Household Income and Wealth). Meghir and Whitehouse (1997), using the Retirement Survey (RS), wrote the key article about the UK. These studies all emphasise the importance of pension benefits in determining when people retire, especially through their (decreasing) effect on the probability to return to work.

This paper attempts to build upon this previous research, particularly upon the study by Meghir and Whitehouse (1997), to extend our knowledge of older men and women's labour market transitions in the UK. There are four main contributions of my work.

First, while Meghir and Whitehouse (1997), like most previous research, only analyse men, I compare men and women. This will enable first comparisons across sexes of the effects of the explanatory variables. While men start to receive their Basic State Pension starting from their 65th birthday, women do so starting from their 60th birthday. It is interesting to see whether this difference in the state pension age has any effects on older men and women's labour market behaviour, and if so, which. A related point is that I analyse the exact timing of transitions in more detail than previously done.

Second, while Meghir and Whitehouse (1997) do not include members of OP schemes in their regressions (thus excluding 60% of their sample), arguing that OP schemes were too heterogeneous to be modelled without detailed data about their incentive structures, I try to keep my sample restriction criteria as small as possible to be able to include this majority

in my analysis. Since I do not have data on the particular features of individual OP plans, I compare members and non-members, arguing that even such a simple approach produces some interesting results.

Third, I employ a wider set of explanatory variables. Since it is well known that health is an important determinant of retirement, I include more finely graded health variables than previous studies. I examine the effects of work experience gained in younger years, which Meghir and Whitehouse (1997) do not. I also include more variables on marital status and better data on unemployment rates. A discussion of how I improve upon previous specifications is provided in Section 3.

Fourth and related to the previous point, I use a more up-to-date data set (Meghir and Whitehouse analyse the time period from 1968 to 1989) which is valuable in and of itself since the labour market behaviour of older Britons has changed a lot over the last decades (cf. Campbell 1999).

The data which I am using come from the British Household Panel Survey (BHPS), a longitudinal general-purpose household survey. The first wave was carried out in 1991 and there are currently 14 waves of data available. In this paper, I use waves 1–13. The maturity of the panel enables us to follow people over a relatively long period in their life. The BHPS has the advantage of being a rich, current data set with high data quality.[2]

Section 2 of this paper describes the modelling framework used; Section 3 the data. Section 4 presents non-parametric empirical transition hazard rates as well as the results of the hazard regression models, including predicted hazard rates and predicted median transition ages. Section 5 draws some conclusions.

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[2]There is now also the English Longitudinal Study of Ageing (ELSA), the first two waves of which have been carried out in 2002 and 2004. Wave 3, for which fieldwork is currently underway, will include one-off life history interviews. ELSA is a specialist survey geared towards many of the aspects of this paper, but I do not use it here since it is still in an early stage in terms of the maturity of the panel dimension.

2 Modelling framework

2.1 Definition of labour market states

I use a discrete-time survival model with two hazards. I study not only the exit from employment (E) to non-employment (N), but also the return from N to E which is important for reasons which I have just mentioned. Choosing survival analysis, a reduced-form approach, enables me to study a host of potential factors—OP membership, the potential incomes in and out of work, demographic and other personal characteristics, work-related variables, etc.—simultaneously without having to impose strong structural assumptions.

I treat everybody who reports positive working hours as employed and those who report zero hours as non-employed. The corresponding transition types are then called EN (if you change from employment to non-employment from one month to the next) and NE (vice versa). Men and women are analysed separately.

Self-employed people are kept in the sample since self-employment is an important mode of employment among UK males and growing in its importance among older Britons. The self-employed are analysed in the same way as employees. For endogeneity reasons, I will not use a dummy variable to distinguish between these two groups.

My employment state (E) only includes individuals with positive hours. By contrast, Peracchi and Welch (1994), who match a number of year-to-year transitions from the US Current Population Survey (CPS) to analyse the labour market transitions of older men and women in a simpler methodological framework, regard the unemployed, i.e., those actively looking for a job, as full-time or part-time employed depending on their previous job. I justify my approach with the fact that the focus of my paper is really on genuine labour market activity rather than on search or the receipt of unemployment benefits. Furthermore, finding good measures for whether somebody is 'actively searching' is rather difficult.

Treating all those with zero work hours as non-employed follows Blau (1994). Using

a more aggregated non-employment state (N) rather than distinguishing between separate labour market states such as retired, unemployed, and out of the labour force is in line with earlier literature since any definition of 'retirement' or 'unemployed' is ambiguous. As age increases, an increasing proportion of those with zero hours (i.e., of those in my state N) will also be retired according to more subjective measures.

There is a literature on the question whether unemployment and out of the labour force are different from each other. Both Flinn and Heckman (1983) and Jones and Riddell (1999) find that they are, with large differences between the states in the return probabilities to employment. Nonetheless, in my case, the focus is really on employment as opposed to everything else (since employment is associated with labour market earnings, while unemployment and out of the labour force are not), so I decided to merge all non-employment states in accordance with what Blau (1994) and Meghir and Whitehouse (1997) did. A multi-state analysis would be possible, if the sample includes 'enough' events for each transition, but the parsimonious focus on just two labour market states will make it easier to identify the principal mechanisms.

Figure 1 plots the proportions of men and women working during the BHPS panel period (1990–2004) using the definitions just defined. Comparing this figure to Blau's (1994: 122) data reveals striking differences between the UK today and the US in the 1960s and 1970s: Blau (1994) reports that less than 10% of men do not work at age 55, and only approximately 45% at age 65, while in Britain the corresponding figures are 28% and 73%, respectively. In fact, the labour market participation among US men at age 55 is roughly comparable to that among UK men at age 40, since the withdrawal process from the labour market starts much earlier in the UK than in the US. The non-employment share is higher in my UK data than in Blau's (1994) US data for any given age. For women (whom Blau does not analyse), non-employment rates are even higher (44% at age 55 and 88% at age 65). In terms of labour force participation, the UK today is indeed very different from the 1960s and the 1970s in
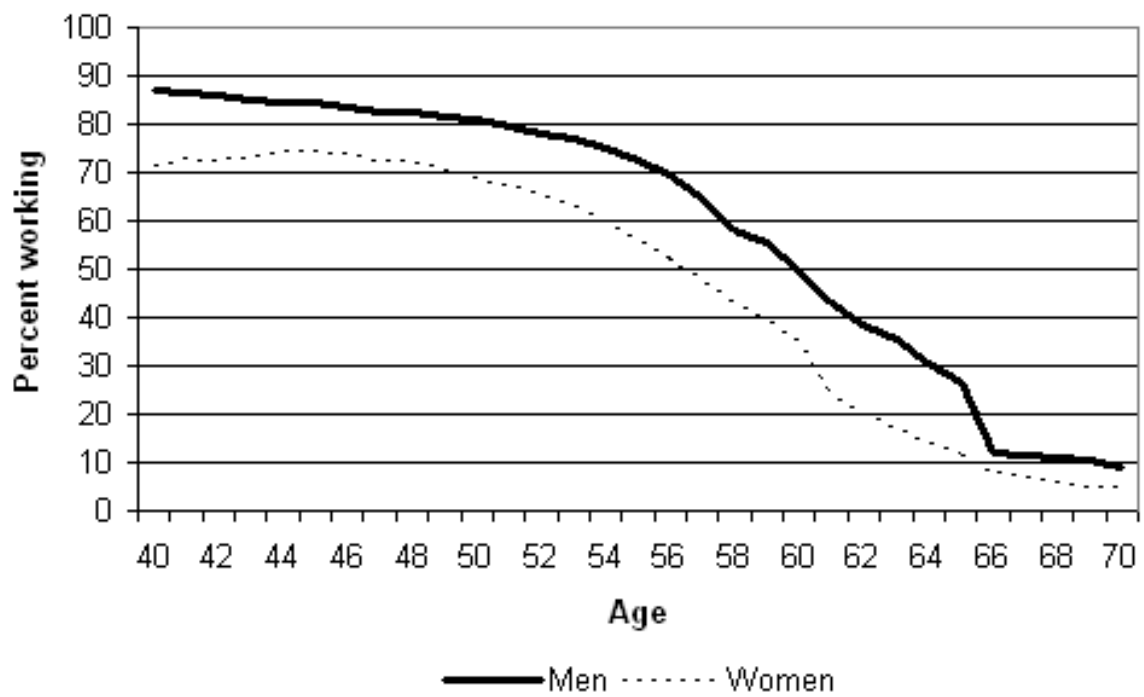
Figure 1: Shares of men and women working

the USA.

2.2   The hazard regression model

I assume that the transitions out of and back to employment follow a proportional hazards model. Since my data are interval-censored, I use a complementary log-log (or cloglog) model for the hazard regressions.

A proportional hazards model implies that the duration profile of the hazard (the so-called 'baseline hazard') is the same for everybody, with the explanatory variables shifting this profile upwards or downwards:

$$\chi(t, \mathbf{x}) = \chi_0(t) \exp(\beta' \mathbf{x}), \tag{1}$$

where χ is the underlying continuous-time hazard rate, χ_0 the baseline hazard, β the vector of coefficients, and $\mathbf{x}$ the vector of explanatory variables. The proportional hazards property is intuitively appealing and also has analytical advantages.

I have monthly data on each person's labour market status even though people may make their transitions on a daily basis. My data are therefore 'interval-censored' with monthly observations which means that I observe data which are grouped into monthly intervals even though the actual transition process is continuous or discrete with time units smaller than a month. Prentice and Gloeckler (1978) show that the cloglog model is the interval-censored discrete-time equivalent of a continuous-time model with the proportional hazards assumption, with the same coefficient vector β as in the continuous-time case.

A result of the above-mentioned analytical advantages of a proportional hazards model is that each coefficient can be transformed to a hazard ratio which is easier to interpret than the underlying coefficient:

$$HR_{x_k} = \frac{\chi(x_k = a + 1)}{\chi(x_k = a)} = \exp(\beta_{x_k}). \tag{2}$$

This is the relative risk associated with a one-unit change in the value of the explanatory variable x_k, all other elements of $\mathbf{x}$ held constant. The usual null hypothesis of $\beta_{x_k} = 0$ is of course equivalent to the null hypothesis of $HR_{x_k} = 1$ since $\exp(0) = 1$. In the discussion of my regression results, I will talk about hazard ratios most of the time.

Finally, the survivor function is given by

$$S(i, j) = \prod_{k=i}^{j} (1 - h_k), \tag{3}$$

i.e., the fraction of people remaining in the origin state at time j out of all those who were in the origin state at time i (i.e., $i < j$). I am using h for the interval-specific (i.e., discrete) hazard rate.

If there was no unobserved heterogeneity, one could simply estimate two separate discrete-choice regressions, one for the exit from employment (each observation then corresponding to a month spent in employment), and one for the return to employment (each observation representing a month spent in non-employment).

However, it is questionable whether all individuals with the same vector of observed explanatory variables face the same expected hazard of making a transition in the labour market. In particular, it seems to be reasonable to assume that there are some individuals who, due to unobservable factors such as 'labour market attachment', are more or less likely than others to experience a given transition. Ignoring unobserved heterogeneity can lead to various biases (cf. Jenkins 2005: 81–89). In my case, unobserved heterogeneity means that I have to take multiple spells into account (one person may make either transition several times) as well as allow for correlated error terms across the two transitions, estimating them jointly.

To model unobserved heterogeneity, I am using the non-parametric approach proposed by Heckman and Singer (1984). One could also assume a bivariate normal heterogeneity distribution; however, Abbring and van den Berg (2007) show that in duration models the heterogeneity distribution usually converges to a Gamma distribution. Also Blau (1994) and Meghir and Whitehouse (1997) use the approach of Heckman and Singer.

Heckman and Singer present a way of fitting a non-parametric distribution of unobserved heterogeneity where the positions and probabilities of the mass points are determined from the data themselves, conditional on the number of mass points chosen by the researcher (typically, one starts off with two mass points and can then try to increase their number, although convergence is usually only achieved with a small number of mass points). Each mass point can be interpreted as an estimated fixed effect for a group of people who share a specific unobserved *ceteris paribus* propensity to make the corresponding transition, and the probability of each mass point as the estimated share of the sample with this specific propensity.

With two mass points, there are $2 \times 2 = 4$ types of people in this model. However, the model never converged when allowing for four groups, or when constraining only one of the four mass point probabilities to zero. I therefore constrained two mass point probabilities to zero, keeping the remaining two.

My model therefore reduces to the following. There are two types of people, group A with a corresponding probability π, and group B with the probability $1 - \pi$. Formally, the transition model for group A can be written as

$$h_{i,EN} = 1 - \exp(-\exp(\beta' \mathbf{x_i})); \tag{4}$$

$$h_{i,NE} = 1 - \exp(-\exp(\gamma' \mathbf{z_i} + \theta_{NE})); \tag{5}$$

and for group B as

$$h_{i,EN} = 1 - \exp(-\exp(\beta' \mathbf{x_i} + \theta_{EN})); \tag{6}$$

$$h_{i,NE} = 1 - \exp(-\exp(\gamma' \mathbf{z_i})), \tag{7}$$

where $\mathbf{x_i}$ and $\mathbf{z_i}$ are the vectors of explanatory variables (i.e., demographic and other personal characteristics as well as variables relating to work, including incentives) and β and γ the corresponding parameter vectors. The θ terms are the mass points of the estimated unobserved heterogeneity distribution; if there were no unobserved heterogeneity, they would disappear.[3]

The signs of the two mass points tell us about the two groups. If θ_{EN} and θ_{NE} are both positive, group A exhibits higher labour market attachment (lower exit risk, higher return probability) and group B lower labour market attachment (higher exit risk, lower return probability). If θ_{EN} and θ_{NE} are both negative, we will have the same two groups, but vice versa. If θ_{EN} is positive and θ_{NE} negative, group A are stayers (lower exit risk, lower return probability) and group B movers (higher exit risk, higher return probability). If θ_{EN} is negative and θ_{NE} positive, we will again have the same two groups, but vice versa. Other pairs of the four groups (higher labour market attachment, lower labour market attachment, stayers, and movers) are not possible due to the choice of the equations in which the two mass points appear.[4]

A further issue arising when estimating a transition model with unobserved heterogeneity is the potential non-randomness of the labour market status of a person's first spell. In my case, whether the first observation of a person in the sample is in employment or in non-employment may be non-random, because belonging to either one of the two groups of people

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[3]An alternative equivalent notation would be $\mathrm{cloglog}(h_{i,EN}) = \beta' \mathbf{x_i}$ and $\mathrm{cloglog}(h_{i,NE}) = \gamma' \mathbf{z_i} + \theta_{NE}$ for the transition model for group A and $\mathrm{cloglog}(h_{i,EN}) = \beta' \mathbf{x_i} + \theta_{EN}$ and $\mathrm{cloglog}(h_{i,NE}) = \gamma' \mathbf{z_i}$ for group B.

[4]The general version of the model with all four groups, i.e., without constraining any mass point probabilities, has four rather than just two θ parameters, one for each equation in the above scheme. This would result in all four groups (higher labour market attachment, lower labour market attachment, stayers, and movers) being estimated.

just mentioned will also influence the probability or being employed or non-employed when first observed. Ignoring this may bias the estimates. Typically, this problem is tackled by including a so-called initial conditions equation, an additional equation in the model which models the probability of being employed when first observed, allowing for correlation in unobservables across equations. The above-mentioned model did however not converge when adding such an initial conditions equation. This is likely to be due to the fact that the variable summarising somebody's work history between ages 15 and 40 is already a powerful predictor of the labour market status during a person's first month in the sample. Experiments which I carried out with a univariate probit model showed that the employment experience variable alone can indeed explain 25% of men's variance of the labour market status when first observed and 29% of women's.

3   Data, sample selection criteria, and variables

3.1   Data

The main data source for this paper is the British Household Panel Survey (BHPS), a longitudinal survey of households containing a wealth of socio-demographic information as well as information on many economic variables. The individuals of a representative sample of 5,500 British households were interviewed in 1991 for the first time and have since been followed, with data from 14 waves (annual interviews) currently available (Taylor 2006). I use waves 1–13 in this paper. Apart from being a rich general-purpose household survey, the key advantages of the BHPS over other surveys are its data quality and its up-to-dateness. While Meghir and Whitehouse (1997), using the Retirement Survey (RS), had 1989 as the last year in their data set, I am able to include data spanning up to 2004 (since 6.4% of wave-13 interviews were carried out in the early months of 2004).

The BHPS provides us with a large number of individuals and spells: the final sample

which I use for my regressions contains 8,361 people (thereof 3,828 men and 4,533 women) and 14,412 spells (thereof 6,543 men's and 7,869 women's), or slightly less than 700,000 person-months. Meghir and Whitehouse (1997: 330) analyse 2,479 spells of 641 men; Blau (1994: 121) 16,385 spells of 7,157 men.[5]

Since information on earnings and benefits (and also on wealth, which I am not using in this paper) is not that detailed in the BHPS, I use the Family Expenditure Survey (FES) as an auxiliary data source to impute this information. The FES is a series of repeat cross-sections, with data being available from 1961. It contains detailed information about income from different sources as well as about the receipt of different types of benefits. Since 1968, the FES has been carried out each year without any gaps. The last FES was carried out in 2000–01 after which it merged with the National Food Survey (NFS) to become the Expenditure and Food Survey (EFS), first carried out in 2001.

I follow Meghir and Whitehouse (1997) who run earnings and benefits regressions using the FES and then use these coefficients to predict lifecycle profiles of earnings and benefits for RS respondents. The methodology they use to combine the two data sets is explained in detail by Arellano and Meghir (1992). As always when using predicted variables as explanatory variables, the hazard regression models using the such 'augmented' RS have to take generated regressor bias into account; otherwise, the standard errors would be underestimated. Pagan (1984) offers a comprehensive treatment of this issue. In my case the prediction of earnings and benefits is carried out for the respondents of the BHPS. I am using the corrected standard error estimator of Murphy and Topel (1985).

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[5]The differences in the number of spells per person arise from differences in the definition of what a 'spell' is. In my data, a spell can be either employment or non-employment, in Blau (1994) it can be full-time employment, part-time employment, or non-employment. Meghir and Whitehouse (1997) do not provide detailed information about how they define a spell but looking at Disney, Meghir, and Whitehouse (1994) suggests that their large number of spells is because they regard job changes as separate spells, even though they do not analyse these job-to-job transitions.

3.2 Sample selection criteria

The number of sample selection criteria is kept as low as possible, and is the same for the presentation of empirical hazard rates as well as for my regressions.

First, I restrict the age range I analyse since I focus on older people. I start the observation window at age 40, disregarding transitions which take place before that age. This is similar to Meghir and Whitehouse (1997) although they also disregarded the job or unemployment spell which was underway at a person's 40th birthday. Blau (1994) considered the spell underway at a person's 55th birthday as well as all subsequent spells. His higher age bound, 55 rather than 40, can be explained by the different labour market behaviour of older men in the USA compared to the UK, as already discussed. Since I observe first changes in the empirical exit and entry hazard rates between ages 40 and 50 (cf. Section 4.1), a lower bound of 40 seems reasonable for my data. I censor spells at age 70 since labour market transitions become rare beyond that age. This allows me to take a look at five years beyond men's state pension age. Meghir and Whitehouse (1997) censor at age 65; Blau (1994) at age 73.

Second, there is a small number of spells (less than 5%) which start before people are born or when people are still very young. Not correcting this could bias the duration variable (some women who never worked are reported to be out of the labour force since birth), and it would bias the variable which I use to summarise a person's work history up to age 40. These erroneously and/or illogically recorded spells are a genuine artefact of the retrospective employment histories in the BHPS and not related to the data organisation procedures I used. I edit all such cases to start with the person's 15th birthday. My employment experience variable therefore summarises a person's work history from age 15 to age 40. All of this also affects a few men who already worked prior to their 15th birthday.

Third, I drop people whose birth year and/or month is missing. Thanks to the high data quality of the BHPS, this criterion drops very few observations (less than 0.1% of spells). The rationale for dropping people whose birth month is missing, rather than generating a (pseudo-

)random number, with which I have also experimented, is that I want to analyse whether transitions out of employment are unusually often observed directly after somebody's 60th or 65th birthday (so-called 'birthday effects'), pointing towards the importance of pension incentives. Generating missing birth months randomly would blur the analysis of birthday effects.

Fourth, I only analyse transitions which occur in or after September 1990 (first wave minus one year). In other words, I do not analyse transitions which are recorded in the retrospective work histories. This needs some explanation. In addition to the question about last year's employment history which is asked in each BHPS wave, BHPS respondents were also asked to provide a full retrospective work history (as well as other life histories) in waves 2 and 3. As already mentioned, I use this information to generate a work-history variable which summarises one's labour market attachment between ages 15 and 40, but I do not use the retrospective months for the analysis of transitions.[6] There is a substantial literature on survey recall bias in economics, sociology, and statistics (cf. for instance Elias 1997 and Paull 2002 using the BHPS; Morgenstern and Barrett 1974 for a more general treatment) which suggests that, especially for the lifespan period under consideration in the present paper, and particularly among older people, there are non-negligible problems with the precision of retrospective data. Furthermore, and even more importantly, retrospective data in the BHPS are limited to only a couple of variables. I would for instance not be able to use health status as an explanatory variable if I used retrospective transitions data.

Fifth and last, I have to drop people for whom I do not observe any work history data before age 40. This is because I cannot generate a meaningful employment experience variable for them. Experiments with a dummy variable for missing work history were unsatisfactory.

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[6]As far as the detailed procedure is concerned, I first use two Stata programmes written by Bardasi (2002), and updated by myself to cover wave 13, to integrate the retrospective information from waves 2 and 3 into the genuine annual panel information from waves 1 to 13. After generating the work history variable from this information, I then delete all months earlier than September 1990.

This criterion drops a bit less than one third of the remaining person-months. Most of the people dropped here are those who were not present in waves 2 and 3 since this is where the retrospective life histories were included in the interview, or who did not answer these questions fully. I do not impute this variable since it is crucial for my analysis. I have carried out some tabulations showing that the average characteristics of people dropped are similar to those of people kept which reassures us that this selection criterion is not overly restrictive.

Summing up, I look at all transitions taking place after August 1990 made by people aged 40 to 70 for whom I observe a retrospective work history and the year and month of birth.

3.3 Variables

3.3.1 Dependent variable

I am interested in the length of employment and non-employment spells. I operationalise the analysis by studying month-to-month changes in the labour market status. I therefore analyse EN and NE transitions as mentioned earlier on.

Similarly to Blau (1994), I fill labour market status gaps of a length of up to two years, assuming midway changes (if the gap length is odd, I create a pseudo-random variable to decide with which information to fill the middle month). Three examples follow. If somebody is observed working in August 1995 and not working in November 1997, the gap between these months is not filled since the gap length exceeds 24 months. If somebody is observed working in August 1995 and not working in November 1995, 'working' is imputed for September and 'not working' for October (even gap length). If somebody is observed working in August 1995 and not working in October 1995, a pseudo-random number is generated to decide whether to impute 'working' or 'not working' for September (middle month of a gap with

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odd length). The first labour market status observed is used to fill all previous months of a person for which the labour market status is missing; the last one for all subsequent missing months, again up to a gap length of 24 months. An example: If somebody enters the sample in August 1994, has twelve months for which the labour market status is missing, and is then observed working in August 1995, the months from August 1994 to July 1995 are imputed with 'working'.

3.3.2 Explanatory variables

Although I am estimating reduced-form models of older people's transitions out of and back to employment, job search models which have attracted a lot of attention in the unemployment literature since the 1970s can be useful in motivating the explanatory variables used. Job search models typically explain unemployment durations and reservation wages with individuals who maximise their utility over an infinite horizon, conditional on the wage distribution. Neumann (1999) gives an excellent survey of empirical implementations of job search models using survival analysis.

Much of the job search literature has focused on duration dependence and unemployment insurance, so it is natural to have elapsed spell duration and the benefit entitlement among my explanatory variables. Also age should be included since we are interested in people who are approaching the end of their working phase. Some measure of the unemployment rate will enable us to evaluate labour market tightness. We need to find something similar to the wage distribution used in job search models to measure employment prospects. Similarly, we should also include some pre-determined measure of individuals' past work experience since this will have an impact on human capital as well as measuring attachment to the labour force. Demographic variables related to household/family circumstances could also be interesting, although we have to be cautious because of potential endogeneity. Lastly, we should include information about each person's health for obvious reasons, and information

about OP membership for reasons which I discussed in the introduction to this paper.

In what follows, I will now go on to present the specific explanatory variables used in my regression analysis.

- Elapsed spell duration (in months)

- Age (in months, minus 479)

- Age 60 dummy $= 1$ if month of 60th birthday or the month thereafter, 0 otherwise

- Age 65 dummy $= 1$ if month of 65th birthday or the month thereafter, 0 otherwise

- Age (in months, minus 479) $\times$ Duration interaction variable

- Marital status

- Health status

- Occupational pension (OP) dummy $= 1$ if ever contributed to or received money from an OP scheme, 0 otherwise

- Child dummy $= 1$ if ever had a child, 0 otherwise

- Employment experience share $=$ months working between ages 15 and 40 divided by total number of months in that person's work history in that age range (using retrospective as well as panel elements)

- Regional unemployment rate

- Predicted (log) income in employment ('earning power')

- Predicted (log) income out of employment ('benefit entitlement')

The specification of **elapsed spell duration duration**, included in levels as well as squared and divided by 1000, is in line with the literature on older people's labour market transitions. I also carried out experiments with dummy variables for certain durations (such as one dummy variable for a one-month duration and one for a duration of more than one year, as in Meghir and Whitehouse, 1997) but they were not statistically significant at any reasonable level.

Similarly, I use **age** as well as its squared value divided by 1000. Again, this is in line with the literature. In addition, to allow for birthday effects as discussed earlier on, I include two age dummy variables for the two exit peaks at ages 60 and 65. Unfortunately, the maximum likelihood model with unobserved heterogeneity does not converge if one includes separate dummy variables for the two birthday months 60 and 65 as well as the $2 \times 2 = 4$ surrounding months. Therefore, and since some regressions which I have run without unobserved heterogeneity have shown that for women, also the coefficients of the dummy variables for the months directly after these two birthday months are statistically significant, I include two variables which equal 1 if the current month is the corresponding birthday month itself or the month directly thereafter.

The coefficients of age and age squared, as well as of all the other explanatory variables, are hardly affected by whether one includes such birthday dummy variables or not, or by which ones one includes (just two dummy variables for the 60th and 65th birthdays, six dummy variables when also covering the surrounding months, etc.). Without birthday dummy variables, the spikes caused by birthday effects are masked completely.

Finally, I also include an **age $\times$ duration interaction variable** (divided by 1000). This variable allows for some interaction between age and duration which may be reasonable in terms of economic theory. The coefficient size is small, but it turns out to be statistically significant. Therefore, the coefficients of the other age and duration variables are not affected very much by including this interaction term.

Age and duration can be identified separately because of the multiple-spell structure of the data. If we analysed a single event which can only occur once, such as death, age and duration would simply be linear transformations of one another. In my analysis however, there are two types of events (exit and return), both of which can occur repeatedly. If somebody stops employment and makes an exit to non-employment, the duration variable will be reset to one while the age variable will of course continue to 'tick'. Therefore, it is the multiple-spell structure of the data which enables separate identification of age and duration effects.

I use four **marital status** dummy variables: divorced; married; separated; widowed. The base category is never married. Gaps in this variable are filled in the same way as gaps in labour market status (cf. Section 3.3.1).

The **health status** variables use information from the following BHPS question: *Please think back over the last 12 months about how your health has been. Compared to people of your own age, would you say that your health has on the whole been. . .* The answer *Excellent* forms my base category, and I have four dummy variables for the remaining answers *Good*, *Fair*, *Poor*, and *Very Poor*. Gaps in this variable are again filled in the same way as gaps in the variable on labour market status.

The **regional unemployment rate** uses information about the claimant count rate in twelve regions of the UK obtained from National Statistics: East, East Midlands, London, North East, Northern Ireland, Northwest and Merseyside, Scotland, Southeast, Southwest, Wales, West Midlands, and Yorkshire and Humberside. I use the not seasonally adjusted across-sex time series.[7]

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[7]The corresponding data series are called DPDD, DPAN, DPDE, DPDA, DPAV, IBWD, DPAU, DPDF, DPAQ, DPAT, DPAR, and DPAM. They are available from `http://www.statistics.gov.uk/statbase/tsdintro.asp` The data for one region (Northwest and Merseyside) are missing between September 1990 (the first 'BHPS month') and December 1993. I apply the time trend of all of England (time series VASS) to fill this gap, scaling the values so that my predicted value for Northwest and Merseyside for the first month in the data (January 1994) is equivalent to the actual one.

I also experimented with the *relative* regional unemployment rate, i.e., dividing the monthly claimant count rates for each region by that region's average over the time period under consideration (September 1990 to May 2004). This would eliminate the region fixed effects which might be useful since the region of residence could be deemed endogenous. The sign and statistical significance of the unemployment variable were not affected by this, and all the other variables had almost the same coefficients. I therefore went back to using the 'normal' (absolute) regional unemployment rate since the hazard ratios for this variable are easier to interpret.

'Earning power' and 'benefit entitlement' are predicted values of each person's individual incomes in and out of the labour market. They include all types of incomes applicable, but for simplicity I shall refer to them as earning power and benefit entitlement. They have a similar purpose as the wage distribution and replacement ratio variables typically used in the job search literature, as already mentioned.

I use predicted rather than actual values for two reasons: first, to overcome potential endogeneity; second, because at one point in time a person can only be either in employment or in non-employment. In line with the literature, I use *individual* rather than household or family income, thereby ignoring household effects. An interesting aspect would be to test in how far predicted household income plays a role for these transitions. Because of endogeneities, this might be particularly important when analysing the joint labour market behaviour of older couples which has not yet been done with UK data.

I use the FES rather than the BHPS to predict these two variables because information on earnings and especially on benefits is much more detailed in the FES than in the BHPS, which is likely to make them more reliable. I drop FES respondents who are younger than 40 or older than 70, i.e., I use the same age group as for my transition regressions. I adjust for inflation using the Retail Price Index of National Statistics.[8] My two income variables

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[8]http://www.statistics.gov.uk/STATBASE/tsdataset.asp?vlnk=229 The exact time series used is CHAW

are measured in January 2004 prices.

Separately for men and for women, I regress the natural logarithm of employees' total income on calendar time (in months since 1900), age, and occupational dummies to estimate the predicted earning power, and the natural logarithm of non-employed people's total income on calendar time and age to estimate the predicted benefit entitlement.[9] As in Meghir and Whitehouse (1997), benefit entitlement is therefore identified through policy changes over time. During the time period my analysis covers, such policy changes included the introduction of the Minimum Income Guarantee in 1997 or its replacement by the Pension Credit in 2003. I run these auxiliary regressions using the natural logarithm of the two income variables as the dependent variables rather than levels to yield a better fit and address potential heteroskedasticity. I present the results of these regressions in Table 4 in the Appendix.

One problem might be a correlation of the variance matrices of the earnings and benefits regressions caused by households in which there are workers as well as non-workers. This could lead to biases in my predictions, and would complicate the Murphy-Topel standard error correction for generated regressor bias (cf. Appendix). To overcome this problem, I generate a pseudo-random variable for each 'mixed' household to decide whether to include its worker(s) or its non-worker(s) in my auxiliary regressions. This should eliminate the correlation of the two variance matrices.

Since this could introduce some non-randomness because of the different treatment of larger households, I also experimented with deciding for *each* household (i.e., also for households where there are only workers or only non-workers) whether to use it in the earnings or the benefits regression. This would of course decrease sample size a lot (because house-

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which is a monthly index number which includes all items, i.e., also housing, indirect taxes, and mortgage interest payments. I change the base month from January 1987 to January 2004.

[9]There are two total income variables in the FES, 'normal total personal gross income' and 'current total personal gross income'; I use the former to smooth transitory income variation.

holds which were assigned to be included in the earnings regression but which only have non-workers and vice versa would have to be dropped). However, the results of the three methods (include everybody; include only either workers or either non-workers in 'mixed' households; assign households randomly to either regression and then drop accordingly) were so similar that I decided to employ the random dropping only for 'mixed' households (the correlations of earnings predicted using the three different methods were always higher than 0.99, similarly for benefits).

Since the predictors, calendar time, age, and occupation, are available in exactly the same way in the BHPS as they are in the FES, I use the coefficients of the auxiliary regressions to predict earning power and benefit entitlement for each BHPS respondent. In contrast to Blau (1994) and Meghir and Whitehouse (1997), I use the natural logarithm of the income variables in the transition regressions (rather than levels). This has two major advantages. First, it simplifies making statements about the elasticities of making a certain transition with respect to the two income variables and second, it enables us to test the replacement rate hypothesis, i.e., whether log earnings and log benefits have equally large coefficients, but with different sign. The replacement rate is defined as $R = \frac{benefits}{earnings}$, therefore $\ln(R) = \ln(benefits) - \ln(earnings)$.

Missings in the occupational dummy variable in the BHPS (which is one of my predictors) are filled as described for the marital status dummy variables. For people for whom I never observe an occupation in the BHPS, i.e., for people who never worked during the panel period, the earning power is set equal to the base category (non-skilled manual).

Predicting the two income variables gives rise to an identification issue similar to that of any instrumental variables approach, i.e., there have to be some predictors in the two income regressions which are assumed not to have direct effects on the transition equations; only indirect effects via the two income variables. That is why I do not use calendar time and occupation in the transition equations; I am assuming that calendar time and occupation do

Variable	Men	Women
Failure	0.0073	0.0075
(Never married)	0.0885	0.0611
Married	0.7812	0.7064
Separated	0.0250	0.0234
Divorced	0.0854	0.1215
Widowed	0.0199	0.0876
Age in months minus 479	137.2959	141.3131
(Age in years)	51.3580	51.6928
Duration	47.4213	46.9202
Employment experience	0.9068	0.6227
Ever had child	0.7654	0.8198
Ever had OP	0.6497	0.4633
(Excellent health)	0.2579	0.2249
Good health	0.4502	0.4501
Fair health	0.1990	0.2216
Poor health	0.0752	0.0813
Very poor health	0.0177	0.0221
Regional unemployment rate	6.4662	6.5982
Weekly earnings	399.9163	202.8809
Weekly benefits	138.3641	58.1184
Person-months	310,287	369,193
Persons	3,828	4,533

Table 1: Sample means (person-averages rather than person-month averages for the three time-constant variables). Earnings and benefits in January 2004 pounds.

not influence the transition hazards directly, only via earnings and benefits (and of course via the regional unemployment rate). I use age in the prediction as well as in the transition equations.

In Table 1, I present the averages of the dependent and explanatory variables for my final estimation sample (variables not used in my regressions are in parentheses). I am reporting person-month averages except for the three time-constant variables employment experience, ever had child, and ever had OP, for which I am reporting person-averages.

How does my specification compare to those of Blau (1994) and Meghir and Whitehouse (1997)? Overall, it is closer to the parsimonious specification of Meghir and Whitehouse

rather than to the huge model which Blau estimates. However, compared to both Meghir and Whitehouse and Blau, I use more detailed information on health (four dummies vs two or one in Meghir and Whitehouse, one in Blau), marital status (four dummies vs none in Meghir and Whitehouse, one in Blau), and unemployment (regional rate vs national). Furthermore, I use three time-constant variables to capture people's OP membership and their parental and work histories. I do not use education since it is not statistically significant when using occupation to predict earning power. Therefore, I decided to use a more parsimonious specification of the transition equations without education variables.

4 Results

4.1 Empirical transition hazard rates by various characteristics

4.1.1 Methodology

This part of the paper reports conditional transition probabilities ('empirical hazard rates') between the two labour market states depending on the age of the person. I calculate them separately for different groups of people depending on their sex and OP status.

Separating these calculations by sex will help to see whether the difference in the state pension age (65 for men, 60 for women) leads to differences in labour market behaviour. Differentiating between OP members and non-members allows to check whether the phenomenon observed by Meghir and Whitehouse (1997), that men who are OP members have a higher return probability than male non-members, can still be observed in more recent data, and whether there is any difference for women in that respect.

These descriptive devices shall be used to point towards interesting features of the data which warrant further investigation in the multivariate parametric analysis which follows.

My definition of the hazard rate is analogous to the Kaplan-Meier hazard, with the

important difference that age rather than duration is the analysis time. The annual EN hazard 'at' age 65 (denoted by $h^{EN}_{65,a}$, with the a subscript for 'annual') is the number of people who made a transition from employment to non-employment between the month directly before their 65th birthday and twelve months thereafter ($EN_{780,792}$, as $65 \times 12 = 780$ and $780+12 = 792$), divided by the risk set.[10] The risk set consists of the number of people in state E directly before their 65th birthday (E_{780}) minus half of those censored between the two relevant points in time ($EC_{780,792}$). This is known as actuarial adjustment.[11] Algebraically,

$$h^{EN}_{65,a} = \frac{EN_{780,792}}{E_{780} - (EC_{780,792})/2}. \tag{8}$$

I also compute 95% confidence intervals for my age-dependent 'hazard' h. These are given by

$$ci_{95\%}(h) = \max[h \pm \Phi^{-1}(0.975)h\sqrt{\frac{1 - (h/2)^2}{f}}, 0], \tag{9}$$

where, in analogy to Greenwood's (1926) formula, $\Phi^{-1}(\cdot)$ is the inverse cumulative standard normal probability distribution function and f the numbers of failures ($EN_{780,792}$ in the previous example).

I talk about differences in the hazard rates being 'statistically significant' if the corresponding 95% confidence intervals do not overlap.

4.1.2   A First Overview: Annual Transition Hazards for Men and Women

The exit hazard, in Figure 2, is very similar for men and for women, with three exceptions: the peak at age 65 is higher for men than for women, the peak at age 60 for women than for men (with these two differences being statistically significant), and in general women's exit

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[10]By analogy, the formula for the *monthly* age-dependent hazard 'at' age 65 ($h^{EN}_{65,m}$) uses $_{780,781}$ subscripts.

[11]Actuarial adjustment therefore deals with the fact that some people who are observed at the starting age are not observed anymore twelve months later. Subtracting 50% of the censored observations from the risk set assumes that the density of censoring over time is symmetric.
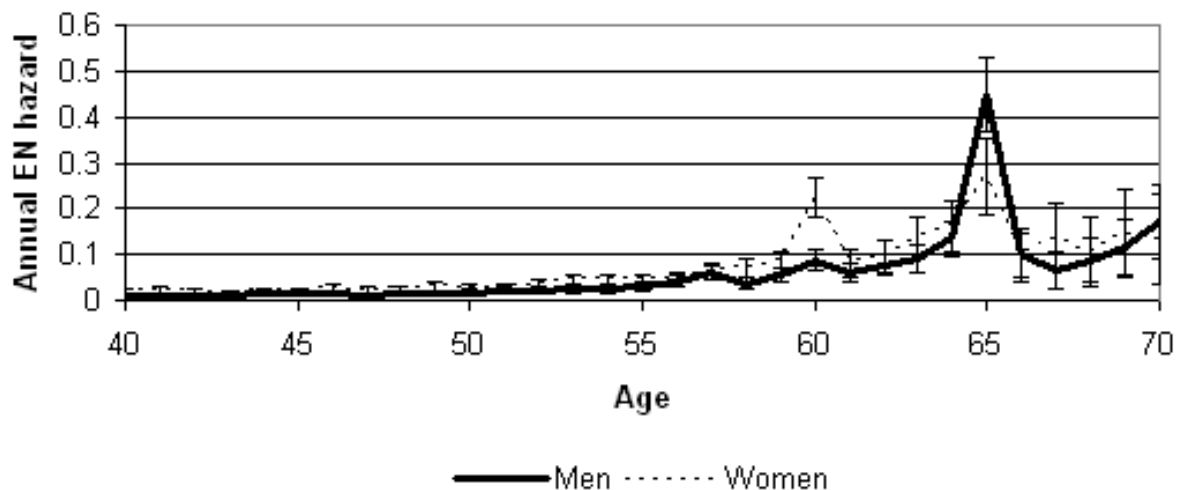
Figure 2: Annual EN hazard by sex

hazard is slightly higher at older ages than men's (although this difference is not statistically significant). Men's age-60 peak is only very minor.

In numbers, the annual hazard of stopping to work 'at' age 65 is 44.97% for men (3.81 times larger than the average of the two surrounding years) and 26.99% (2.08 times larger) for women. At age 60, the corresponding figures are 8.79% (1.52 times larger) for men and 22.30% (2.68 times larger) for women.

The return hazard, in Figure 3, is statistically significantly higher for men than for women up to age 55 and almost completely the same for the two sexes thereafter.

Both for men as well as for women, there are no statistically significant differences in the transition hazards depending on whether or not they ever had children. This is interesting because one might have suspected that women who had children could have different employment patterns later during their life. However, women with children may of course follow a different distribution across labour market states compared to women without children.
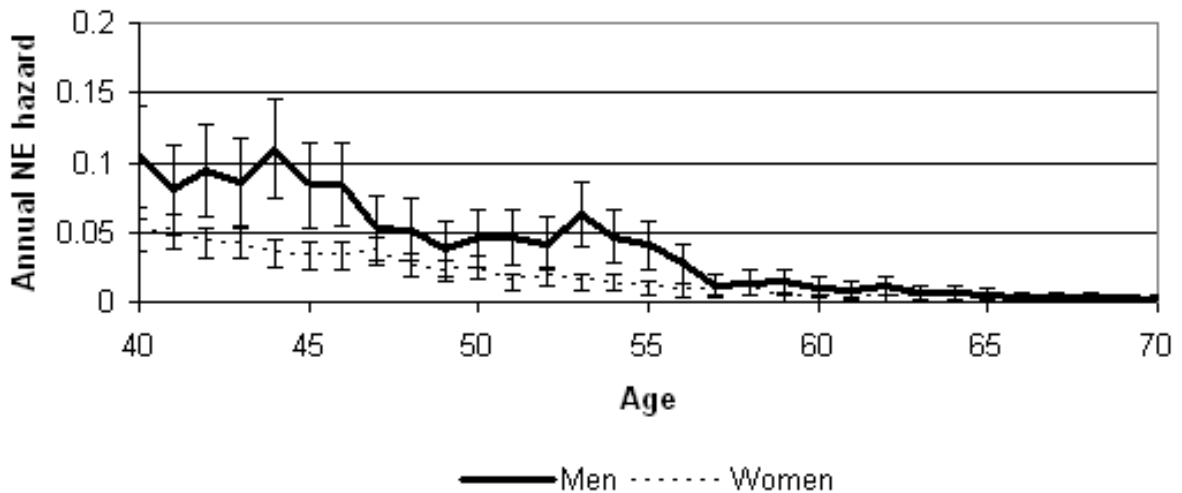
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Figure 3: Annual NE hazard by sex

4.1.3   Assessing birthday effects: monthly transition hazards for men and women

An interesting question is whether there is heaping of transitions at certain ages (so-called 'birthday effects'), or whether transitions are evenly spread over the year. This is because birthday effects at ages which play important roles in a country's pension system could indicate incentive effects of the pension system. Blau (1994) addressed this question using quarterly hazard rates to analyse US data while Meghir and Whitehouse (1997) for the UK only addressed it implicitly when referring to steep parts in the survivor function at age 65 (keep in mind that they analysed men only). In my data, such steep parts can be seen in Figure 1 at age 65 for men and at age 60 for women.

Examining the monthly hazard rates in my own data reveals that birthday effects are important in the UK. A man who is employed directly before his 65th birthday has a 45.38% risk of stopping to work within one month (12.93 times larger than the average of the two surrounding months), a woman 19.49% (4.05 times larger). For the 60th birthday these figures are 3.25% (6.08 times larger) for men and 11.78% (4.53 times larger) for women. When looking at quarterly hazard rates it can be seen that for British men, birthday effects

are approximately twice as large as Blau (1994) found for the USA.

4.1.4   Annual transition hazards by Occupational Pension membership
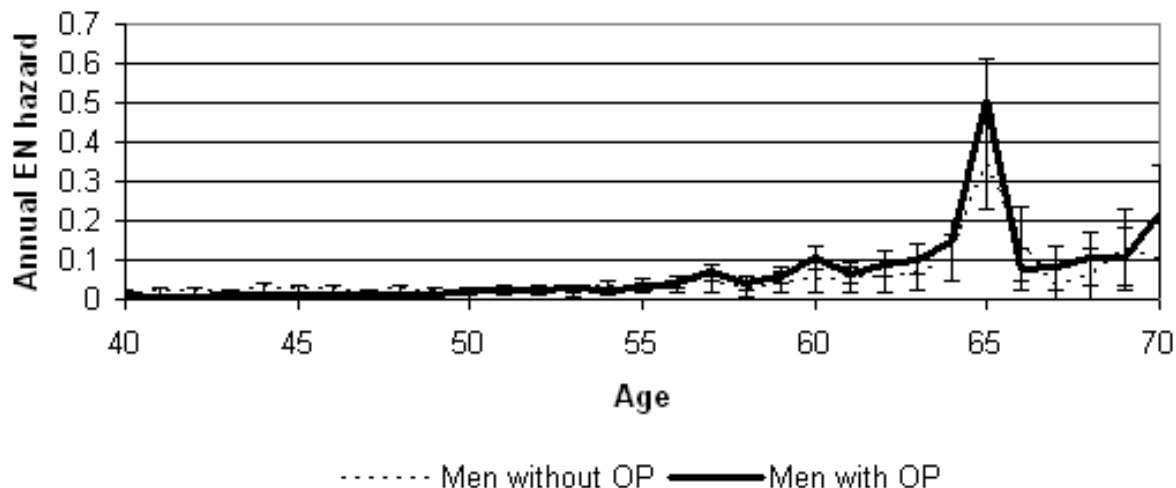

Figure 4: Annual EN hazard by OP status for men

Looking at men in Figures 4 and 5, having ever been member of an OP scheme is not associated with the exit rate from employment, but it does increase the return hazard up to age 55 compared to non-members. This is consistent with Meghir and Whitehouse (1997: 332). OP members also have a higher age-65 exit peak, but the difference is not statistically significant.

For women, in Figures 6 and 7, we observe a similar pattern. The return hazard for OP members is higher than for non-members up to age 55, and the difference is statistically significant (and larger than for men). Furthermore, female OP members have more pronounced exit peaks at ages 60 and 65 than non-members, but these differences are again not statistically significant.
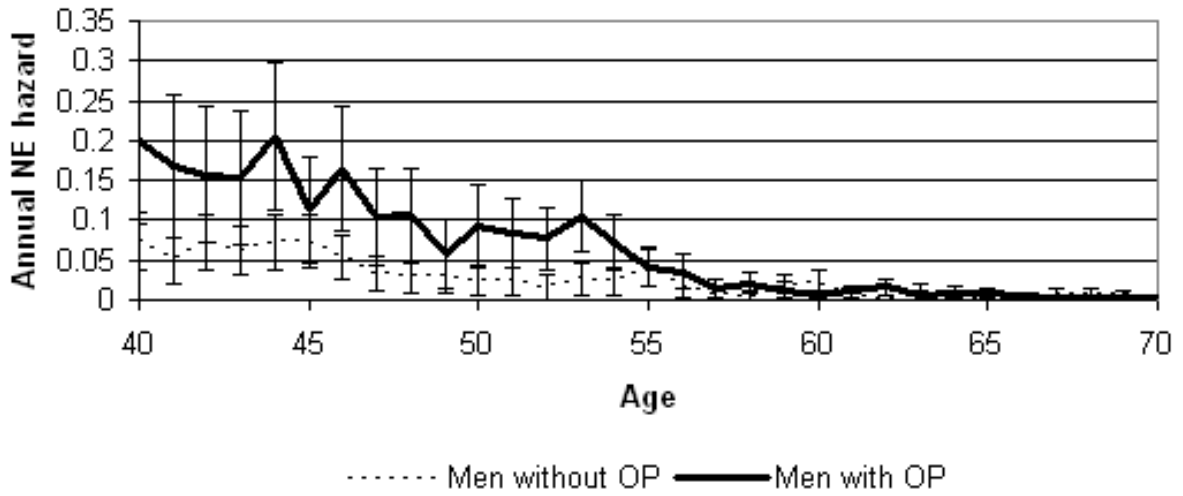
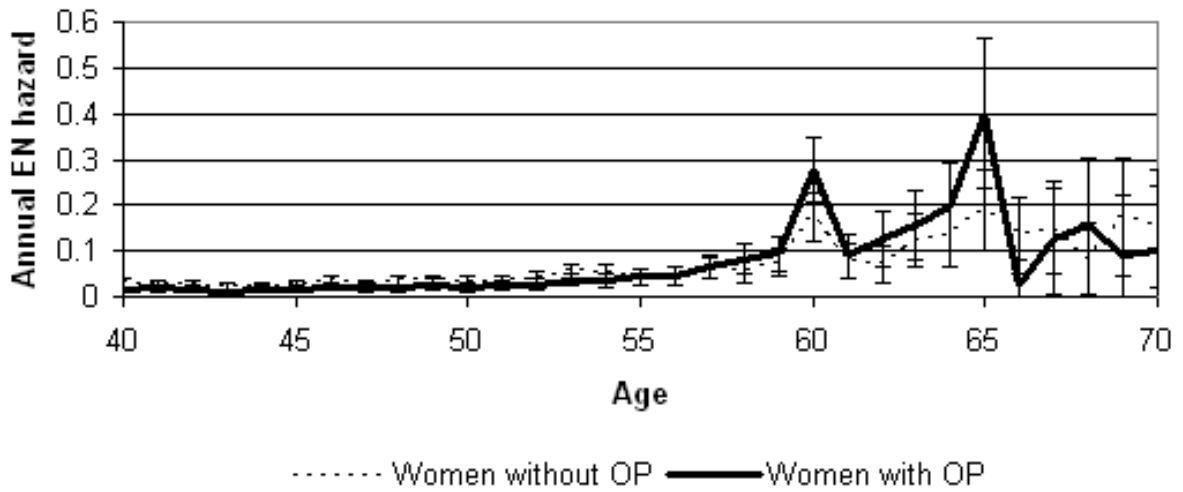Figure 5: Annual NE hazard by OP status for men

Figure 6: Annual EN hazard by OP status for women

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Exit hazard coefficients	Men		Women	
	Without UH	With UH	Without UH	With UH
Married	−0.2501*	−0.1935	−0.3234*	−0.4133***
Separated	−0.2664	−0.2669	−0.0390	−0.2386
Divorced	−0.0719	0.0249	−0.2480	−0.3066*
Widowed	−0.2833	−0.2095	−0.2184	−0.2093
Age	0.0077***	0.0054**	0.0045***	0.0016
Age squared	−0.0136***	−0.0076	−0.0065	0.0033
Age = 60	0.9585***	1.0027***	2.1644***	2.1809***
Age = 65	3.0484***	3.0714***	2.1399***	2.1600***
Duration	−0.0545***	−0.0503***	−0.0623***	−0.0569***
Duration squared	0.0967***	0.0844***	0.1200***	0.1077***
Age × duration	0.0522***	0.0538***	0.0616***	0.0646***
Employment exp.	−1.3094***	−1.1686***	−0.8023***	−0.7567***
Ever had child	−0.0986	−0.1173	−0.3245***	−0.3061***
Ever had OP	0.2104***	0.2265***	−0.1680***	−0.1690**
Good health	0.1434*	0.1464**	0.0265	0.0453
Fair health	0.2560***	0.2510***	0.4133***	0.4330***
Poor health	1.0171***	1.0325***	0.9442***	1.0106***
Very poor health	1.2938***	1.4155***	1.1238***	1.1508***
Unemployment rate	−0.0658***	−0.0346	−0.0961***	−0.0672***
Earnings	−0.5899***	−0.5905***	−0.5633***	−0.5200***
Benefits	−0.0138	0.1495	−0.0128	−0.2615
# of person-months	232,741	see below	212,469	see below
Log likelihood	−7992.5608	see below	−8727.0997	see below

Return hazard coefficients	Men		Women	
	Without UH	With UH	Without UH	With UH
Married	0.1985	0.2394	−0.5307**	−0.4362
Separated	−0.0489	−0.1727	−0.3227	−0.2821
Divorced	0.0161	0.0877	−0.1535	−0.0246
Widowed	−0.1910	−0.0003	−0.4575	−0.3294
Age	0.0071	0.0069	0.0056	0.0035
Age squared	−0.0422**	−0.0392*	−0.0412**	−0.0320**
Duration	−0.0540**	−0.0251	−0.0279*	0.0019
Duration squared	0.3298*	0.2537	0.2740***	0.2375**
Age × duration	−0.0056	−0.0314	−0.0280	−0.0614***
Employment exp.	1.3437***	1.2625***	0.8058***	0.7578***
Ever had child	0.0435	0.1164	0.3252***	0.3506
Ever had OP	0.0762	0.1838	0.4291***	0.4373***
Good health	0.0503	0.1047	0.0139	0.0105
Fair health	−0.3208**	−0.2759*	−0.2872*	−0.2903**
Poor health	−1.3127***	−1.2826***	−1.0996***	−1.1030***
Very poor health	−1.4884***	−1.3899***	−1.8703***	−1.8907***
Unemployment rate	−0.0966	−0.1076	−0.1995***	−0.1925***
Earnings	0.6268***	0.6638***	0.6219***	0.6573*
Benefits	−1.4606***	−1.6306***	−1.4256***	−1.3130***
# of person-months	77,546	310,287	156,724	369,193
Log likelihood	−3738.9565	−11630.479	−5634.803	−14226.431
UH: π (cf. text)		0.0888***		0.8141***
UH: EN mass point		−1.4775***		1.4303***
UH: NE mass point		1.3810***		−1.4009***

Table 2: Regression coefficients without & with unobserved heterogeneity (UH).
***: Statistically significant at the 1% level; **: 5% level; *: 10% level (Murphy-Topel standard errors; cf. Appendix).
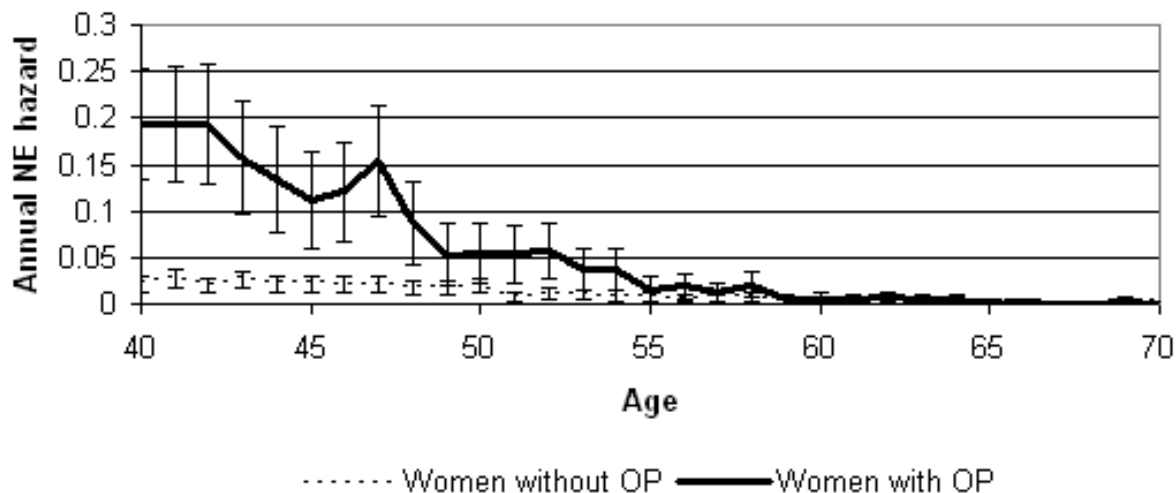
Figure 7: Annual NE hazard by OP status for women

4.2 Hazard regression analysis

Table 2 presents the coefficients of the exit and return regressions. It contains four columns, the first for men without unobserved heterogeneity (a standard cloglog model with the standard errors adjusted for clustering by person, allowing for some adjustment to deal with the fact that we observe most people for more than one month), the second for men with unobserved heterogeneity, the third for women without unobserved heterogeneity, and the fourth for women with unobserved heterogeneity. All the standard errors have been corrected for generated regressor bias using the estimator by Murphy and Topel (1985) which is also discussed in Greene (2003) and Hardin (2002). This estimator is described in the Appendix.

My discussion of the results in this section will focus on the model with unobserved heterogeneity. There are not many differences between the two models but some exist, and since the unobserved heterogeneity parameters all turn out to be highly statistically significant these differences could be crucial.

For reasons which I have already mentioned I will discuss hazard ratios (cf. Section 2.2) rather than coefficients. I will also refer to Figures 10 to 21 which show predicted hazard

rates, and to Table 3 which shows the median transition ages computed from the predicted survivor function in several alternative scenarios. The advantage of these figures and this table is that they make it possible to compare the importance of dummy and continuous variables; for continuous variables, the hazard ratio alone is not very helpful since it does not tell us what the mean and standard deviation of the corresponding variable are. The combination of hazard ratios, predicted hazard rates, and predicted median exit and return ages will enable us to assess which of the explanatory variables have the largest impact.

Let us now examine the regression results in detail.

Most of the marital status coefficients are statistically insignificant. The only two variables which are statistically significant are married and divorced in women's exit regression: married women have a 34% lower exit risk than never married, separated, and widowed women; divorced women a 26% lower exit risk. This may again be a selection effect since married women may be less likely to work in the first place.

The effects of age and duration are not so easy to assess since both variables appear in levels as well as squared, plus there is also the interaction term. One way of analysing the impact of age and duration is to keep one variable constant at a certain level and to vary the other. Doing this reveals an important difference between men and women: the effects of age and duration on exit are almost the same for the two sexes while their effects on return are very different.

Varying age while keeping duration constant at its sex-specific average shows that *ceteris paribus*, increasing age continuously increases the exit risk for both men and women. At age 40, the combined exit hazard ratio of the age and duration variables is 1.0 for both men and women, at age 50 it is 2.3 for men and 1.8 for women, at age 60 (without taking into account the birthday effect dummy variable which I will cover later on) 4.4 for men and 3.7 for women, at age 65 (again without taking into account the birthday effect dummy variable) 5.5 for men and 5.4 for women. Thereafter, the exit hazard ratio of women is higher than

that of men.

Figure 8: Combined *ceteris paribus* effect of age and duration on the predicted monthly NE hazard by age, holding duration constant at its sex-specific average

Doing the same for the return to the labour market as shown in Figure 8 shows that the *ceteris paribus* impact of age on the return hazard (again holding duration constant at its sex-specific average) is inversely U-shaped: the age effect on the return probability reaches its maximum at age 46 for men and at age 41 for women. At age 70 the sizes of the effect for men and for women meet, but up to that age, the age variable has a less negative effect on men's than on women's return hazard. The difference is largest at age 51 where men's combined hazard ratio of the age and duration variables is 1.0 and women's 0.6.

Varying duration while keeping age constant at its sex-specific average shows that *ceteris paribus*, the risk of exiting work falls as duration increases. The effect is virtually identical for men and women. When duration is 1 (i.e., the individual is in the first month of his or her employment spell), men's combined hazard ratio of the age and duration variables is 1.0,

women's 0.9. After one more year in employment (i.e., a value of the duration variable of 13), this hazard ratio has already fallen to 0.5 for both sexes, after another additional year to 0.3 (men) and 0.2 (women), respectively.
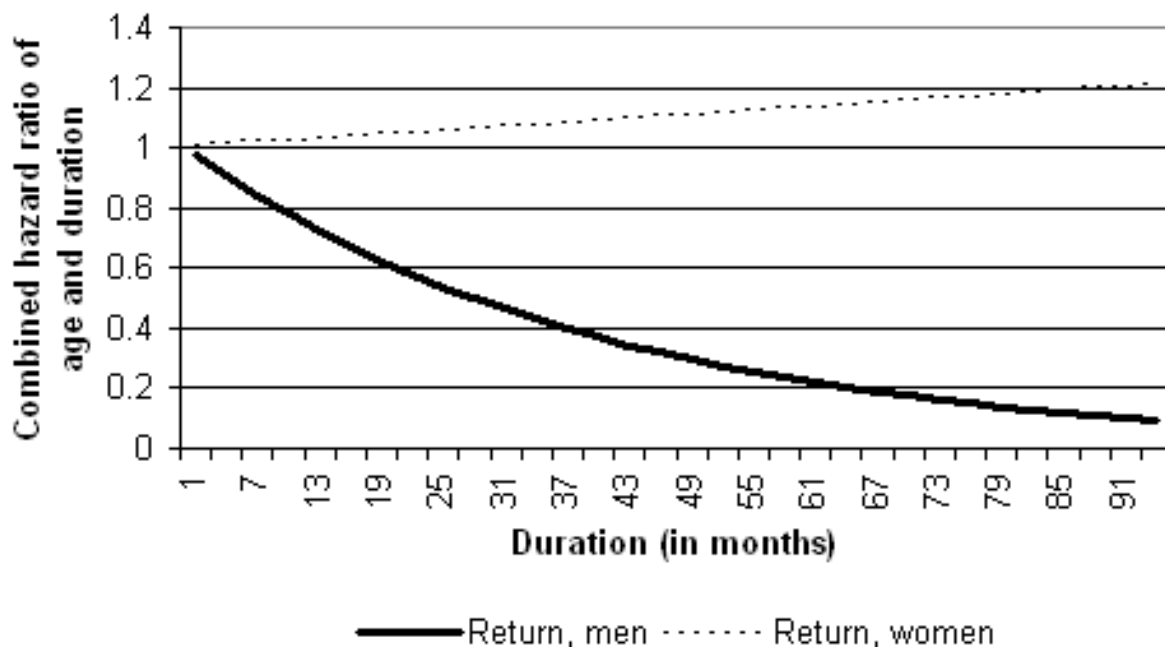

Figure 9: Combined *ceteris paribus* effect of age and duration on the predicted monthly NE hazard by duration, holding age constant at its sex-specific average

Lastly, we can do the same for the effect of duration on return as shown in Figure 9. As for age, I again find that the difference between men and women is mostly due to the return behaviour, not due to the exit behaviour: for men, the *ceteris paribus* effect of duration on return is falling, on women somewhat increasing. This means that the longer a man is out of employment, the lower his *ceteris paribus* probability to come back to work. For women, the opposite is true. When duration is 1, the combined hazard ratio of the age and duration variables is 1 for both men and women, one year later it is 0.7 for men and 1.0 for women, another year later 0.5 for men and 1.1 for women.

So far, I have shown that the effects of age and duration on exit are very similar for

the two sexes, but their effects on return are quite different. Now we also need to take a look at the two birthday effect dummy variables which appear in the exit regression, one for the month of the 60th birthday or the month thereafter, and one for the month of the 65th birthday or the month thereafter. The coefficient sizes are huge and markedly bigger than the size of any other dummy variable. At a man's 60th birthday, the exit risk is 2.7 times higher than can be explained by all the other variables. At a man's 65th birthday, the hazard ratio is 21.6 (sic). For women, as could already be suspected from Section 4.1.3 which discussed evidence for birthday effects in the empirical hazard rates, the effects of the 60th and the 65th birthday are much more similar to each other than for men; the corresponding hazard ratios are 8.9 and 8.7, respectively. This shows that even after controlling for a number of important control variables, birthday effects are an important phenomenon in the labour market behaviour of older Britons.

As expected, employment experience reduces the exit risk and increases the return probability for both men and women. The effects are statistically significant: comparing a man who has always worked when observing his work history between ages 15 and 40 to a man who never has, the former has a 69% lower exit risk than the latter, and a 3.5 times as large return probability. *Prima facie*, this looks huge, but one needs to take into account that the variation in work experience is not that large: as we know from Table 1, the mean value for men is 0.9068 (90.68%); the standard deviation is 0.2306. Table 3 shows that reducing men's employment experience by one third lowers the median exit age of those who were working at age 40 from 53.6 by 3.3 to 50.3, and increases the median return age of those who were not working at age 40 from 42.6 by 1.2 to 43.8.

Comparing a woman who always worked to one who never worked, the former has a 53% lower exit risk than the latter, and a 2.1 times larger return probability. This raw effect is smaller for women than for men. Looking at Table 3 again shows that also the effects of a relative change in the employment experience variable (which takes into account that

women's employment experience has a lower mean than men's) are smaller for women than for men.

Having ever had a child has a statistically significant effect on women's exit risk: women with a child or children have a 26% lower exit risk than women without. This effect is statistically significant at the 1% level. Table 3 shows that the median exit age of women who were working at age 40 is 51.3 if they ever had a child and 2.5 years lower (48.8) if they never had. Having ever had children does not have statistically significant effects on women's return probability or on men's transition probabilities.
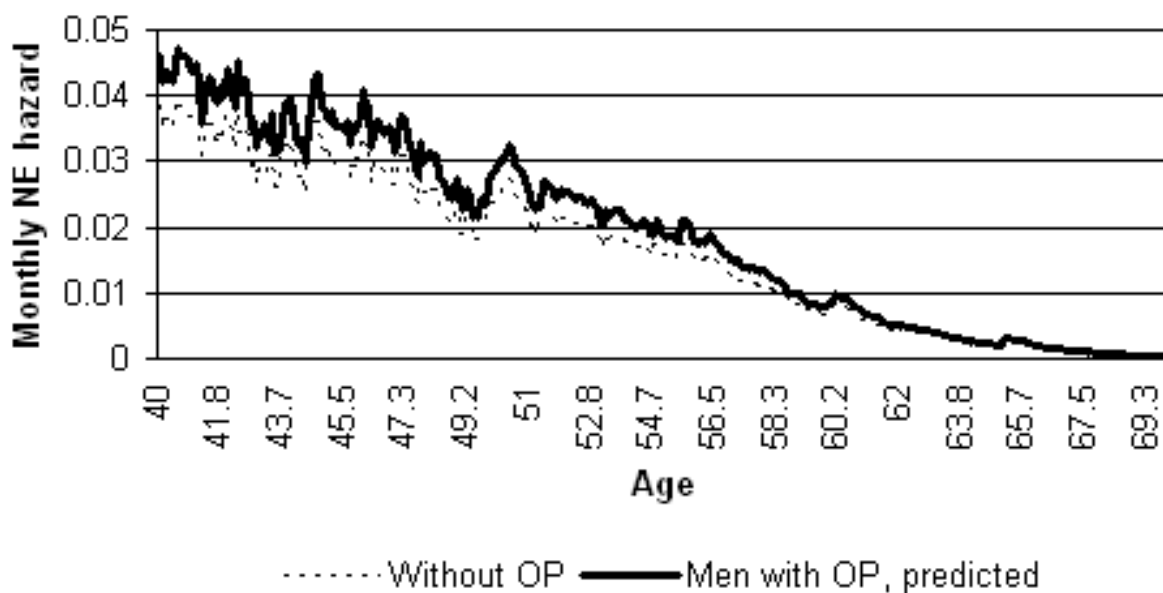

Figure 10: *Ceteris paribus* effect of not being an OP members compared to being one on the predicted monthly NE hazard for men by age

Having ever been member of an OP scheme shows the expected effects for women: it lowers their exit risk by 16% and it increases their return probability by 55% (both statistically significant). Figure 11 shows a hypothetical counterfactual related to women's return probability: the thick line represents the predicted return probability of women who are OP members, with the values of the other explanatory variables equal to those observed among
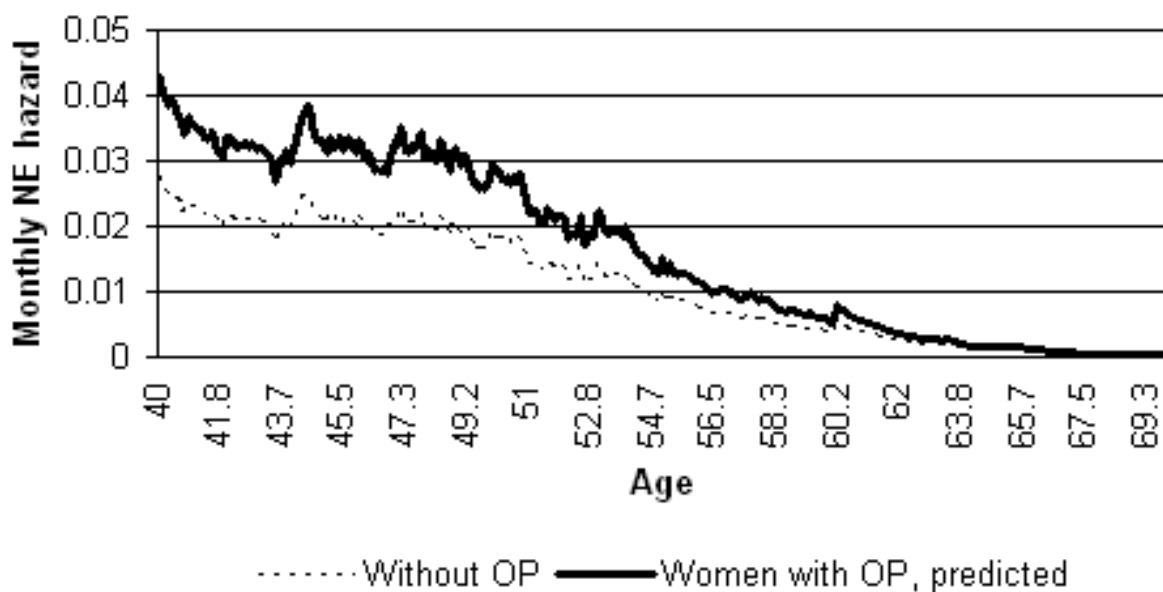
Figure 11: *Ceteris paribus* effect of not being an OP members compared to being one on the predicted monthly NE hazard for women by age

women who have the corresponding age, are out of work, and OP members. The thin dashed line plots the hypothetical counterfactual of making these women non-OP members, keeping the values of all other explanatory variables the same. In terms of the median transition ages shown in Table 3, this means that making women who are OP members non-members decreases their median exit age by 1.5 years and increases their median return age by 0.9 years.

However, the effects of OP membership are less straightforward for men. The effect on the return probability is not statistically significant (cf. Figure 10), and moreover, men who are OP members have an exit risk 25% higher than non-members (statistically significant at the 1% level). This translates into the prediction that men who are not OP members exit the labour market 2.1 years later than members (Table 3). This finding is different to what Meghir and Whitehouse (1997) found when plotting employment survivor functions of men with and without OP schemes and may warrant further examination. The reason could, for
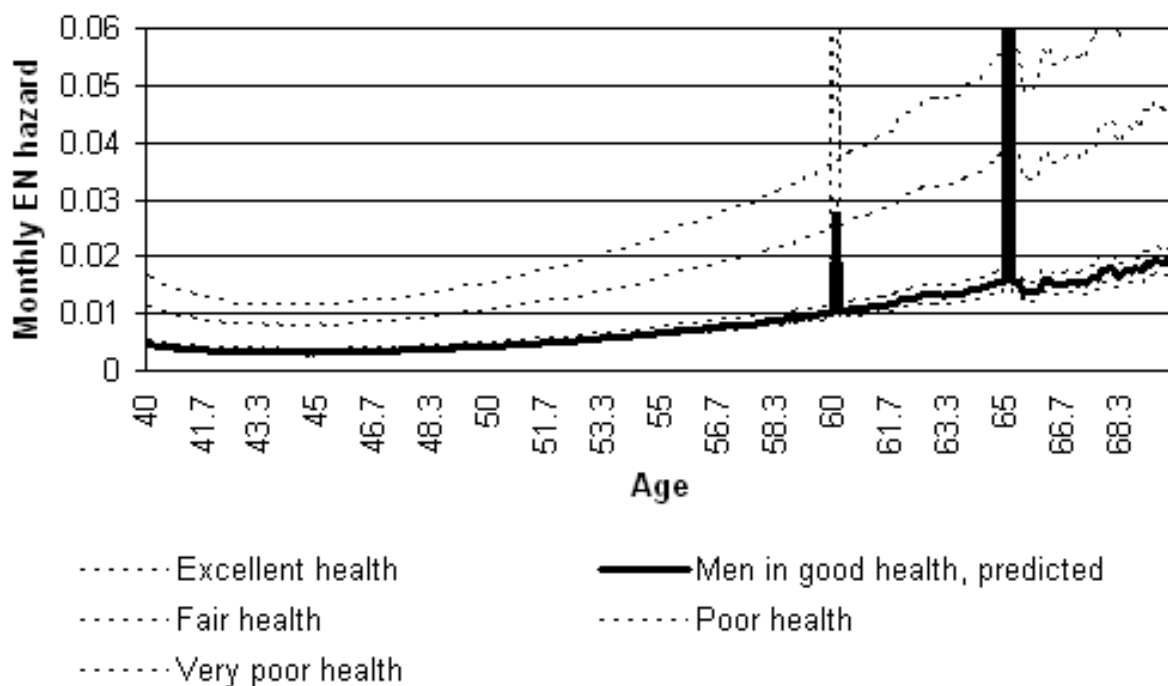
instance, be in wealth versus income effects.

Figure 12: *Ceteris paribus* effects of health compared to being in good health on the predicted monthly EN hazard for men by age

Health status is found to be one of the most important determinants of older people's labour market transitions (Figures 12 to 15). In three out of the four equations, being in good or excellent health does not make any statistically significant difference. However, as we can see from the figures, being in poor or very poor health does make a huge difference. Men in poor health have a 2.8 times larger exit risk than men in excellent health; for women, the corresponding hazard ratio is almost the same (2.7). Being in very poor health further increases this risk: for men, from 2.8 times larger to 4.1 times larger, for women, from 2.7 times larger to 3.2 times larger.

For the return, we see similarly huge effects. Men in poor health have a 72% lower return probability than men in good or excellent health; for women it is 67% lower. Being in very poor health further reduces the chances to return to work: men in very poor health have
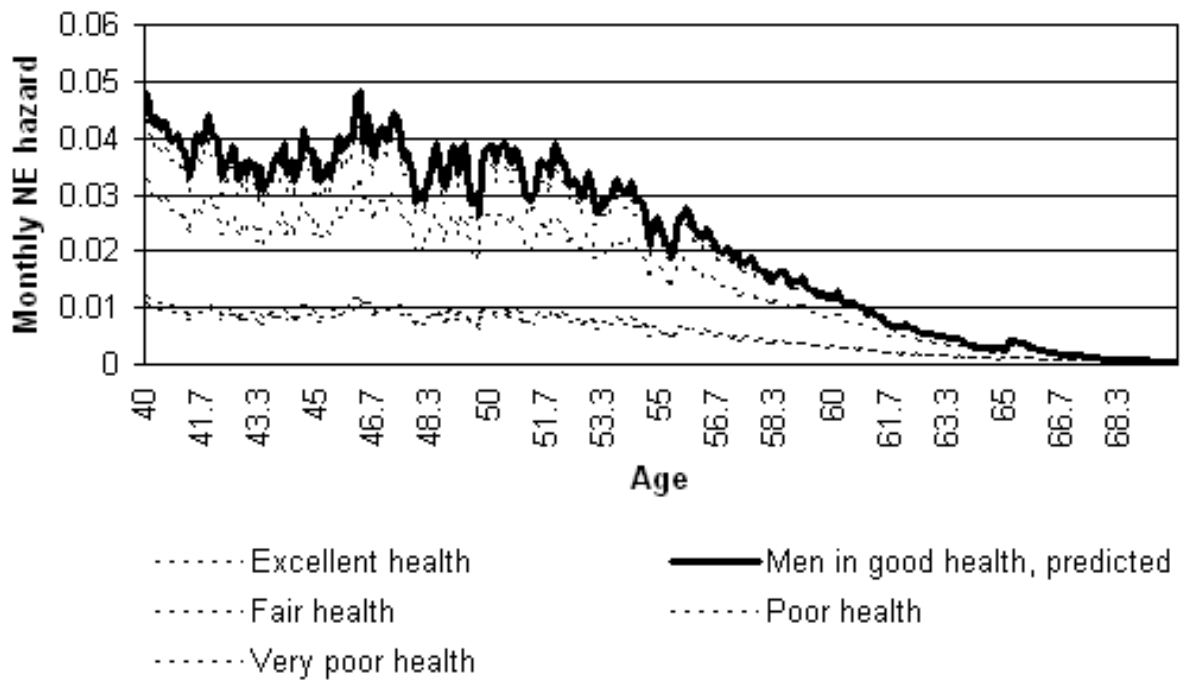
Figure 13: *Ceteris paribus* effects of health compared to being in good health on the predicted monthly NE hazard for men by age
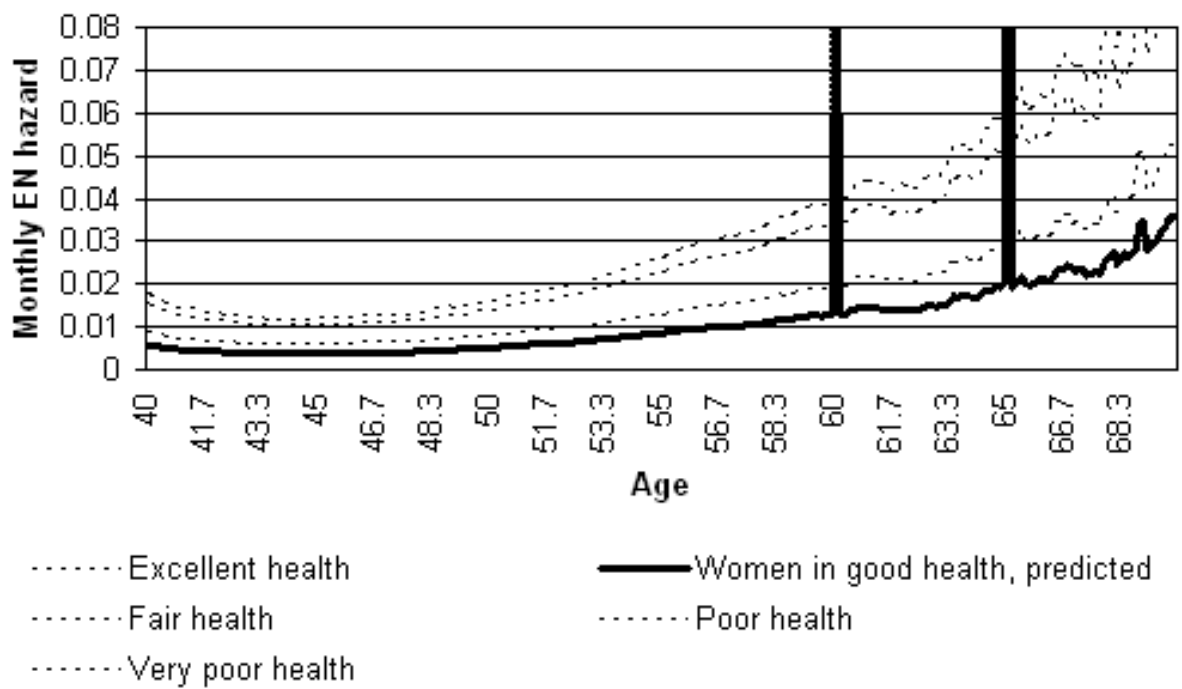
Figure 14: *Ceteris paribus* effects of health compared to being in good health on the predicted monthly EN hazard for women by age
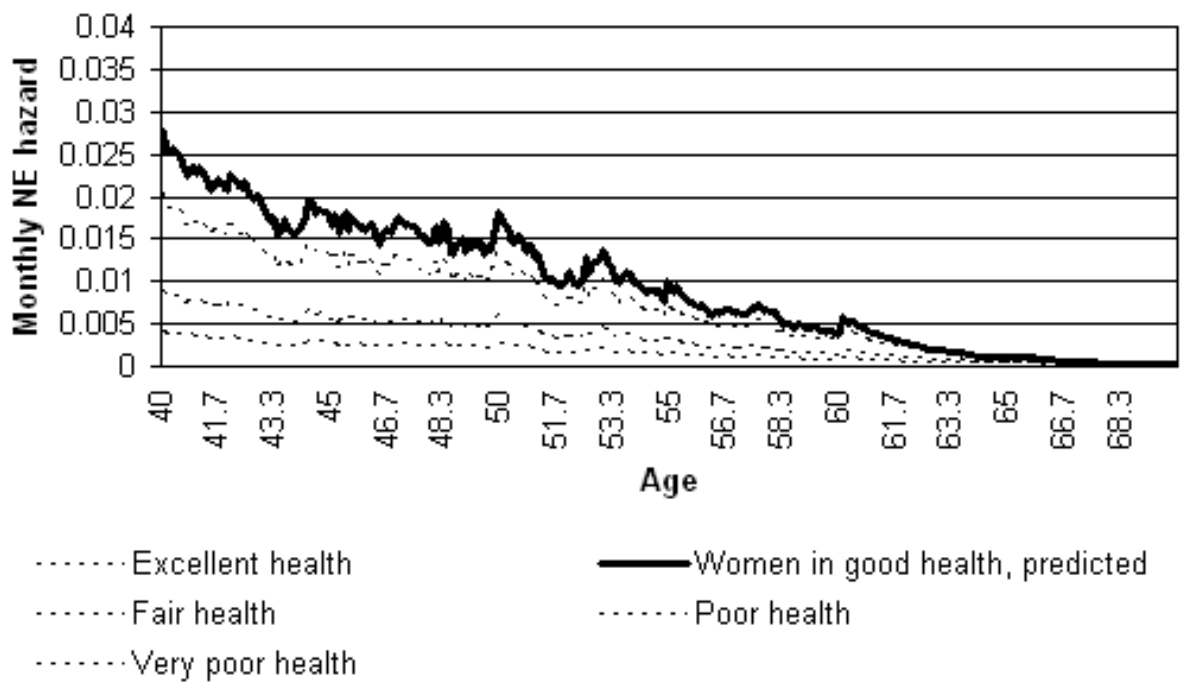
Figure 15: *Ceteris paribus* effects of health compared to being in good health on the predicted monthly NE hazard for women by age

a 75% lower return probability than men in good or excellent health; for women it is even 85% lower.

Of course this also translates into correspondingly huge effects on the median transition ages shown in Table 3. One particularly striking result: for women who were not working at age 40 and continuously in very poor health, I predict only 39.9% to return to work by age 70. Therefore, I cannot even calculate a median return age for this group.

The regional unemployment rate has statistically significant effects on women's labour market transitions. Unfortunately, these are not so easy to interpret: when regional unemployment is high, women make less transitions in either direction. For men, there are no statistically significant effects.

As expected, higher earnings lower one's exit risk and increase one's return probability. This is also in accordance with theoretical results shown in microeconomic optimal retirement decision models such as the one already mentioned earlier on by Kingston (2000). The effects are statistically significant but not that large. As can be seen from Table 3, increasing predicted earnings by one third increases men's median exit age by 1.6 years (from 53.6 to 55.2) and women's by 1.2 years (from 51.2 to 52.4). It also reduces the median return age by 0.5 years for men (from 42.6 to 42.1) and by 0.6 years for women (from 43.3 to 42.7).

The effects of benefits are quite different: they do not have any statistically significant effects on exit, but they do have important effects on return, as shown in Figures 16 and 17 which plot the effects of reducing benefits by one third. The median return age is reduced by 1.3 years for men (from 42.6 to 41.3) and by 1.5 years for women (from 42.6 to 41.1). This means that such a reduction by one third halves the median duration out of work for men; the effect is similarly large for women.

The effects of earnings and benefits which I find are bigger than in Blau (1994) for the US and Meghir and Whitehouse (1997) for the UK. For example, Meghir and Whitehouse (1997: 347), when using the model with unobserved heterogeneity, find that the elasticity
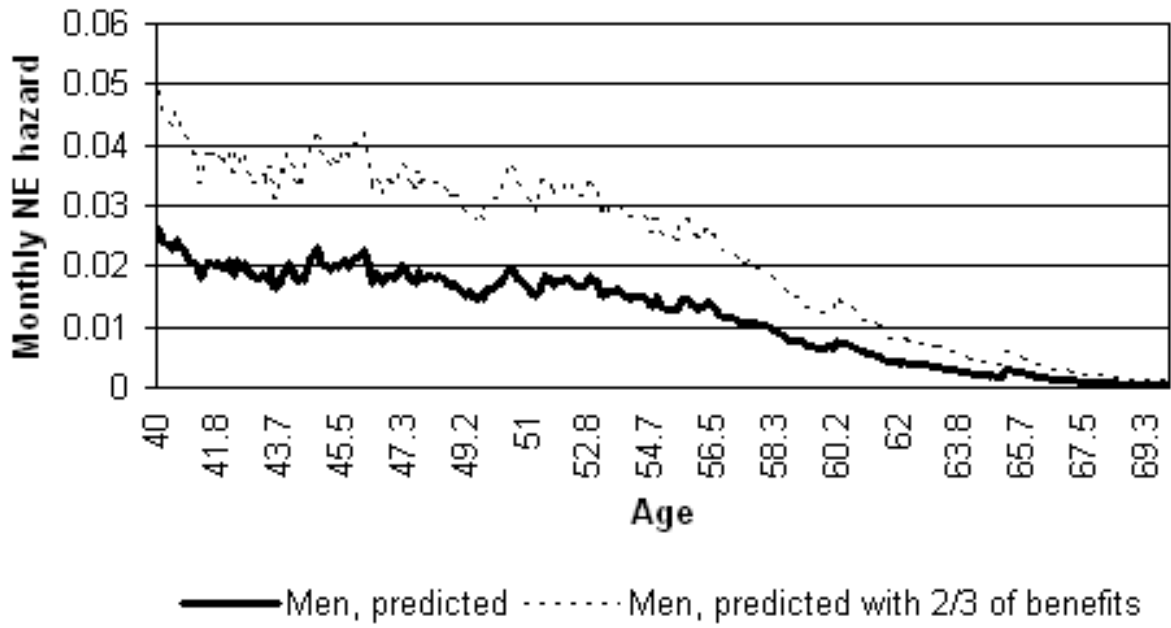
Figure 16: *Ceteris paribus* effect of reducing predicted benefits by one third on the predicted monthly NE hazard for men by age
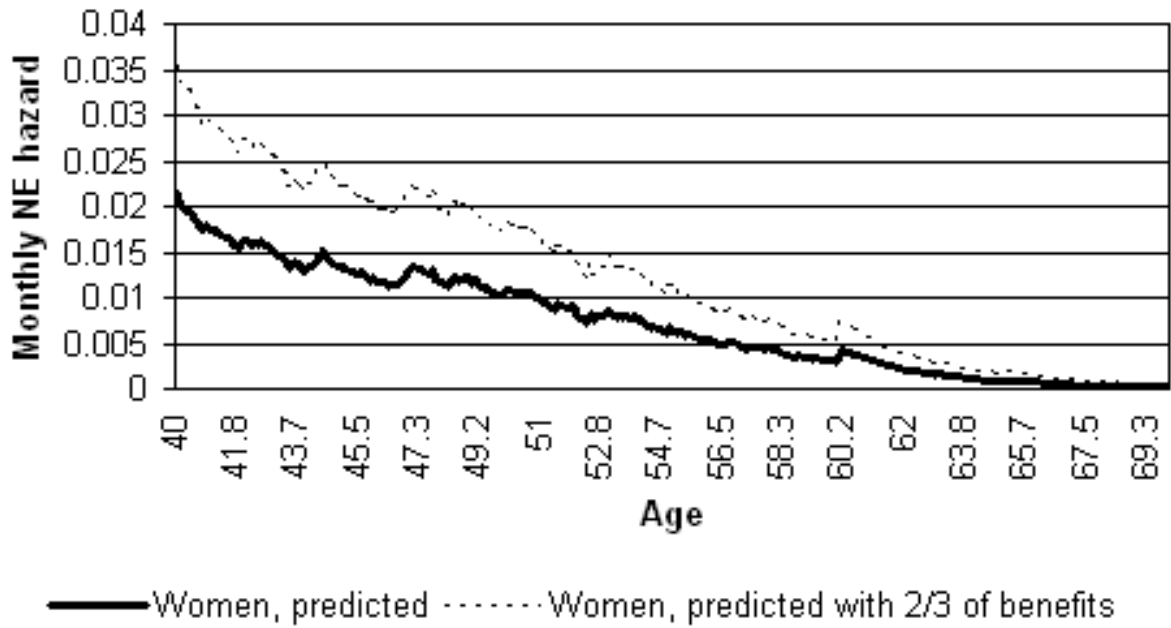

Figure 17: *Ceteris paribus* effect of reducing predicted benefits by one third on the predicted monthly NE hazard for women by age

of exit from the labour market with respect to earnings is -0.54 and the elasticity of entry to the labour market with respect to benefits is -0.36. In my analysis, the corresponding figures are -0.59 and -1.63, respectively. For women, the elasticities which I find are only slightly lower (-0.52 and -1.31, respectively). Blau (1994: 142, 144) finds hardly any effects of the predicted full-time wage rate. He does find large effects of social security benefits; however, they are still smaller than the ones I find.

Putting together what I have said about the effects of earnings and benefits and looking at the confidence intervals of the coefficient estimates, we can already suspect that the replacement ratio hypothesis does not hold. Indeed formal tests of $\beta_{benefits} = -\beta_{earnings}$ were always rejected, for men as well as for women and without UH as well as with UH.

Summing up, we can keep in mind that apart from age, where I find huge peaks at ages 65 (for both sexes) and 60 (for women), health and benefits seem to be the most important determinants of labour market transitions quantitatively. The impact of non-labour income on the return probability could well warrant further examination.

Similar to Meghir and Whitehouse (1997), introducing unobserved heterogeneity does not lead to systematic changes in the coefficients, but there are a couple of changes with respect to statistical significance. I do not formally test for the presence of unobserved heterogeneity (Meghir and Whitehouse develop a score test of unobserved heterogeneity for multiple spell models), but the mass point probabilities and positions are always statistically significant at the 1% level.

The estimated positions of the mass points are virtually the same for the two sexes (they have opposite signs but the corresponding mass point probabilities are also almost the inverse of each other). In terms of hazard ratios, movers among men have a 4.4 times higher exit risk than male stayers, and a 4 times higher return probability.[12] Among women,

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[12] $1/\exp(-1.4775)$ gives the exit hazard ratio of male movers compared to male stayers, $\exp(1.3810)$ the return hazard ratio.

the corresponding hazard ratios are 4.2 and 4.1.[13] 8.88% of men and 18.59% of women are movers, the remainder (91.12% of men and 81.41% of women) stayers. The fact that there are twice as many movers among women as among men, which is statistically significant at any level, may be interesting as a separate result since it shows that women are subject to more labour market transitions; women's employment patterns are less stable than men's even when controlling for a number of explanatory variables.
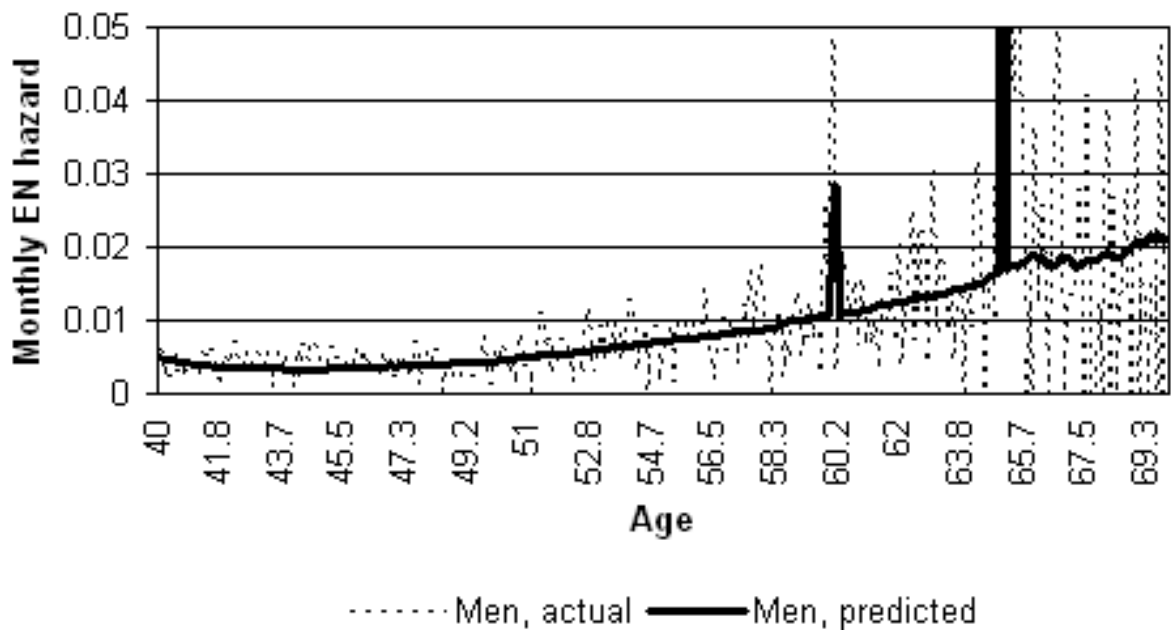
Figure 18: Goodness of fit: actual vs predicted monthly EN hazard for men by age

To conclude this section, let us now take a look at Figures 18 to 21. These four figures compare actual hazard rates to those predicted by my regression model and can be used to assess the goodness of fit or within-sample prediction quality of the model. There is of course a lot of noise caused by small cell sizes (especially at older ages for the exit hazard and at younger ages for the return hazard) which is deliberately not modelled since it does not appear to be systematic, but the general shape of the hazard as well as the two genuine peaks

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[13]exp(1.4303) gives the exit hazard ratio of female movers compared to female stayers, $1/\exp(-1.4009)$ the return hazard ratio.

Predicted median transition ages

Exit	Men	Women
Actual	53.4	50.5
REF: Predicted at sample averages	53.6	51.2
2/3 of experience	−3.3***	−1.6***
4/3 of earnings	+1.6***	+1.2***
2/3 of benefits	+0.6	−0.9
REF: With OP	53.5	52.5
Without OP	+2.1***	−1.5**
REF: Good health	54.2	52.4
Excellent health	+1.4**	+0.4
Fair health	−1.0***	−3.2***
Poor health	−7.4***	−7.4***
Very poor health	−9.8***	−8.1***
REF: With child/children	54.1	51.3
Without child/children	−1.1	−2.5***
Return	**Men**	**Women**
Actual	42.3	43.3
REF: Predicted at sample averages	42.6	43.3
2/3 of experience	+1.2***	+0.6***
4/3 of earnings	−0.5***	−0.6*
2/3 of benefits	−1.3***	−1.5***
REF: With OP	41.2	41.5
Without OP	+0.3	+0.9***
REF: Good health	41.3	42.4
Excellent health	+0.2	±0.0
Fair health	+0.7*	+1.1**
Poor health	+4.8***	+7.2***
Very poor health	+5.4***	>+27.6***
REF: With child/children	42.9	43.3
Without child/children	+0.4	+1.9

Table 3: How do selected explanatory variables change the predicted age by which 50% of those who were employed (upper panel) or non-employed (lower panel) at age 40 make a transition to the other state? Predictions based on the model with unobserved heterogeneity (UH).

According to my predictions, 60.1% of women not working at age 40 and continuously in very poor health have not returned to work by age 70, therefore the '>+27.6***' entry for this group.

***: Statistically significant at the 1% level compared to the reference category ('REF') in the corresponding subpanel; **: 5% level; *: 10% level (Murphy-Topel standard errors; cf. Appendix).
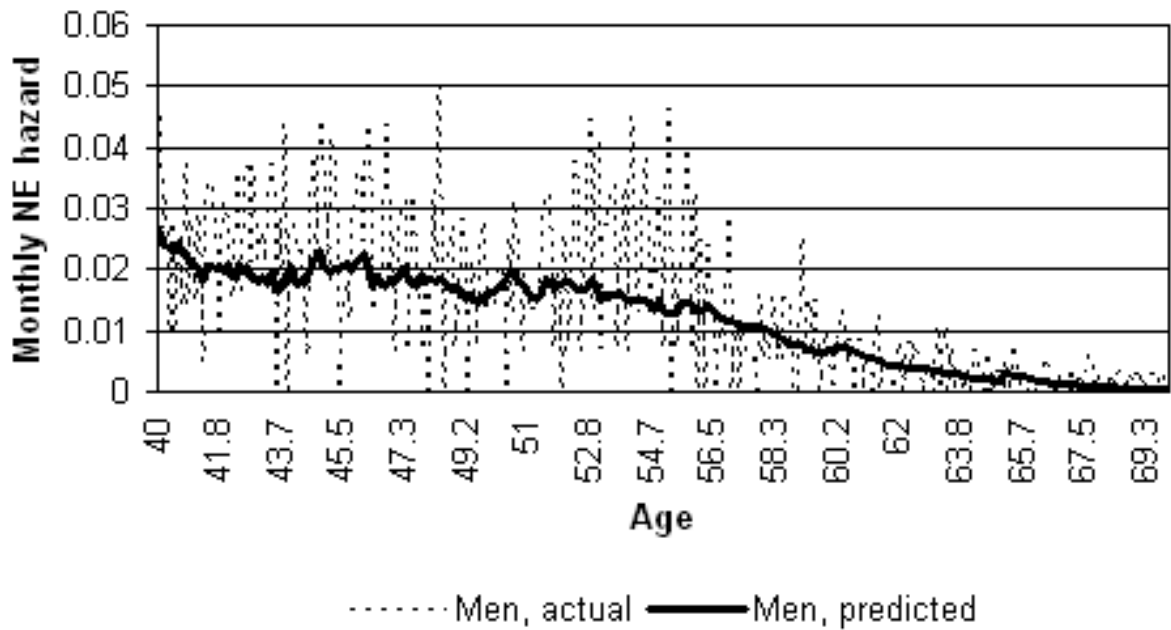
Figure 19: Goodness of fit: actual vs predicted monthly NE hazard for men by age
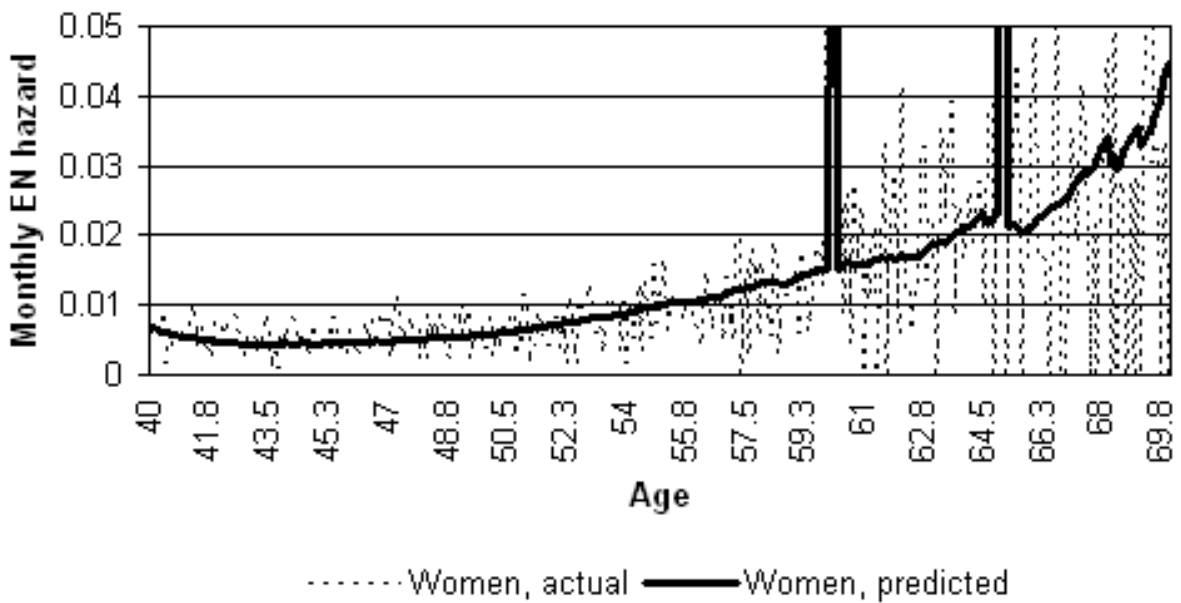
Figure 20: Goodness of fit: actual vs predicted monthly EN hazard for women by age
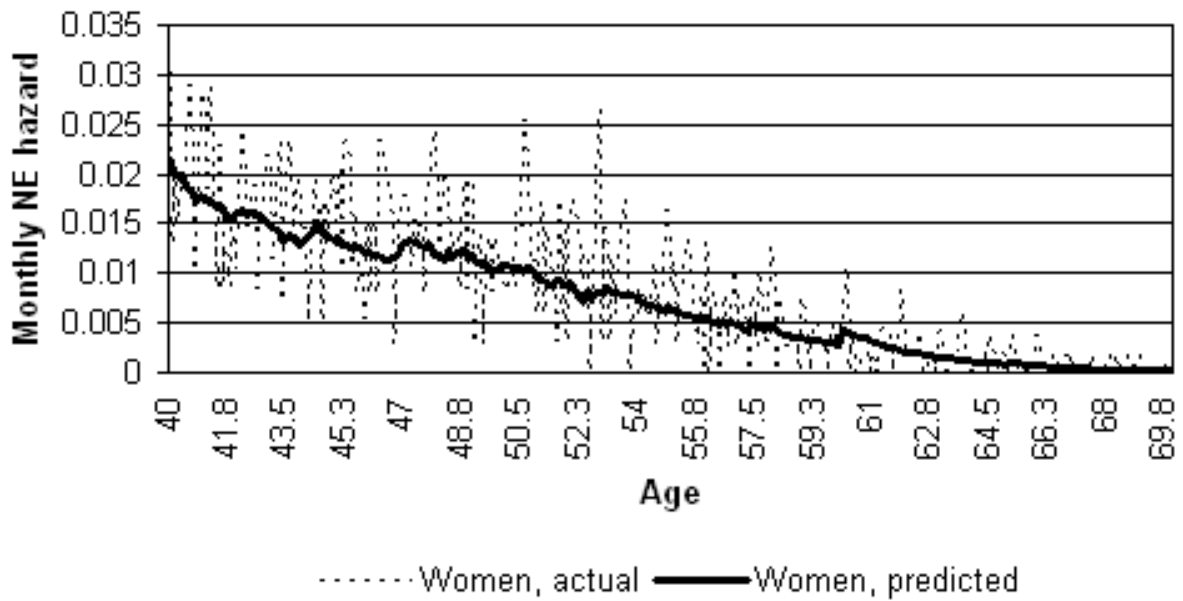
Figure 21: Goodness of fit: actual vs predicted monthly NE hazard for women by age

at ages 60 and 65 are well predicted (although I have two smaller peaks at the birthday month and the subsequent month rather than one big peak at the birthday month which is due to the fact that, as already mentioned earlier on, I was not able to include single-month dummy variables in the full model). When plotting the corresponding survivor functions (not shown in the paper, although this result can be seen implicitly in Table 3), it can be seen that the actual and predicted survivor functions are almost identical, also in replicating the discrete downward jumps at ages 60 and 65. This is a further indication that while unsystematic noise is not picked up by the model, the systematic component of labour market behaviour is well predicted.

5 Summary and conclusions

In this paper, I presented the first comprehensive analysis of older men and women's labour market transitions in the United Kingdom. It is the first study to analyse older UK women's

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labour market transitions and the effects of OP membership in detail, and also the first to assess the exact size of birthday effects, i.e., spikes in the exit rate from employment at certain ages.

When looking at the empirical hazard rates out of and back to employment by age, I found very strong evidence of birthday effects, again particularly for men but also, only somewhat less, for women. The peaks at ages 65 and 60 are markedly bigger than in the literature, both absolutely as well as relatively (i.e., divided by the average of the surrounding two time intervals). For women, both peaks are important (and less marked) while men basically have one major peak at age 65. The importance of these peaks appears to have risen over time also within my own data. Whether one ever had children does not seem to be associated with the empirical transition hazard. Members of OP schemes are more likely to return to work than non-members.

In my regression analysis, I present several interesting findings. First, marital status does not seem to be a very important determinant of older people's labour market transitions in the UK. Second, employment experience in younger years decreases people's exit risk and increases the return probability, but the effects are not that large. Third, women seem to face a generally lower transition risk in times of high regional unemployment. This may call for further investigation. Fourth, the impact of health problems is very important and bigger than found in previous studies. Fifth, OP membership has the expected effects for women, but the size of the effects is not that large. For men, the effects of OP membership are either difficult to explain (exit) or not statistically significant (return). Sixth, having ever had a child is associated with a lower labour market exit risk for women. Seventh, in line with the literature, the earning potential is found to be not that important in influencing one's transition probabilities, but the benefit entitlement is found to have an important impact on the return hazard. This impact is larger than found in previous analyses.

The parameters of my specification of unobserved heterogeneity are all highly statistically

significant, but, similar to earlier results, it does not seem to matter a lot for the general picture of the coefficient estimates. Twice as many women as men are in the group with less stable employment patterns ('movers').

A lot of research remains to be done, and there are many open questions. What can we say about the importance of part-time work and self-employment among older people in the UK? What is the role of earnings, benefits, and assets of other household members? How could we decompose the effect of non-labour income into genuine benefit income and asset income? How can we explain the odd effect of OP membership on men's exit—for instance with a wealth effect? Why do women have fewer transitions in and out of work when regional unemployment is high? Why does the small group of women who never had a child have a higher risk to exit the labour market? Further research is necessary to address these questions.

Appendix A: Income regressions

In Table 4, I present the results of the FES income regressions. These coefficients have then been used on the BHPS to predict income in and out of work for each BHPS respondent.

Natural logarithm of normal total personal gross income	Men		Women	
	Working	Not working	Working	Not working
Calendar time	0.0012	0.0020	−0.0038***	0.0064
Calendar time squared	−0.0022	−0.0074	0.0326***	−0.0145
Before (cf. note)	−0.3351***	−1.5116***	0.0757**	−0.3791***
Age	0.0002	0.0031***	−0.0013***	−0.0017**
Age squared	−0.0020***	−0.0041**	0.0039***	0.0090***
Occ: employers/managers (large)	1.1976***		1.5537***	
Occ: employers/managers (small)	0.8519***		1.1182***	
Occ: professional (self-emp. and emp.)	1.1339***		1.5522***	
Occ: intermediate non-manual worker	0.8337***		1.1512***	
Occ: intermediate non-manual foreman	0.6793***		0.9450***	
Occ: junior non-manual	0.3748***		0.5605***	
Occ: personal service worker	−0.1499***		0.1902***	
Occ: foreman manual	0.6502***		0.8182***	
Occ: skilled manual worker	0.4807***		0.7606***	
Occ: semi-skilled manual/own account worker	0.3591***		0.5447***	
Occ: farmers – employers	0.6252***		0.9359**	
Occ: farmers – own account/agricultural worker	0.2556***		0.3022***	
Occ: HM Forces	1.0651***		1.2334***	
Constant	5.3016***	4.5087***	4.7774***	3.6356***
Number of obs	11,854	6,800	11,669	10,128
R-squared	0.3594	0.1881	0.3218	0.0480

Table 4: FES income predictions for working and non-working men and women. Income is measured in January 2004 pounds.
***: Statistically significant at the 1% level; **: 5% level; *: 10% level (standard errors clustered by household ID).

- Time is measured in months since 1900.

- 'before' is a dummy variable to deal with a structural break in the FES *normalginc* time series between 1993 (up to which the FES referred to calendar years) and 1994–95

(from which on it referred to financial years). In my income predictions, I do not use the coefficient of *before*, i.e., I am predicting on a post-1993 basis.

- Age is measured in months.

- The occupational base category is *unskilled manual.*

- These predictions use data which I adjusted for inflation as described in Section 3 of this paper. The base month is January 2004.

Appendix B: Standard error correction

When estimating a regression model where some of the explanatory variables have been generated through other (so-called first stage) regressions, the standard errors of the so-called second stage will be underestimated. There are several estimators of corrected standard errors. Greene (2003) presents the estimator by Murphy and Topel (1985):

$$V_2^{corr} = V_2 + V_2(CV_1C^T - RV_1C^T - CV_1R^T)V_2 \tag{10}$$

where V_2 is the asymptotic variance matrix of the second stage, V_1 the asymptotic variance matrix of the first stage, and C and R matrices based on derivatives of the first- and second-stage log likelihood function with respect to the first- and second-stage parameter vectors θ_1 and θ_2:

$$C = \left\{ \ \left(\frac{\partial \ln L_2}{\partial \theta_2}\right) \ \left(\frac{\partial \ln L_2}{\partial \theta_1^T}\right) \ \right\} \tag{11}$$

$$R = \left\{ \ \left(\frac{\partial \ln L_2}{\partial \theta_2}\right) \ \left(\frac{\partial \ln L_1}{\partial \theta_1^T}\right) \ \right\} \tag{12}$$

If the first and second stage models are estimated on different data sets, $R = 0$, leading to the shorter expression

$$V_2^{corr} = V_2 + V_2CV_1C^TV_2 \tag{13}$$

In my case, the second-stage models are the hazard regressions (cloglog). I have two OLS first-stage regressions, one for earnings and one for benefits. If there are two generated variables and assuming that the two first-stage regressions are independent from each other,

$$CV_1C^T = \begin{bmatrix} C_1 & C_2 \end{bmatrix} \begin{bmatrix} V_{11} & 0 \\ 0 & V_{12} \end{bmatrix} \begin{bmatrix} C_1^T \\ C_2^T \end{bmatrix} = C_1 V_{11} C_1^T + C_2 V_{12} C_2^T \qquad (14)$$

where C_1 and V_{11} belong to the earnings regression, C_2 and V_{12} to the benefits regression.

The Murphy-Topel standard errors are somewhat larger than the uncorrected standard errors. However, the increases are never huge. I present the Murphy-Topel standard errors for both models, for the one without unobserved heterogeneity as well as for the one with. It is important to note that in the latter case these standard errors are not consistent but since there is no commonly used consistent estimator of the standard errors for models with generated regressors and Heckman-Singer type unobserved heterogeneity (cf. Meghir and Whitehouse 1997: 341), I still present them.

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