# Housing Tenure and Individual Labour Market Outcomes. An Empirical Assessment Based on the UK Labour Force Survey

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**Abstract**

*We analyse the impact of the housing tenure on labour market outcomes using individual data from the UK Labour Force Survey. In defining the residential status, we distinguish between outright owners and mortgage-holders, and between social and private renters. We estimate both a binary model for the probability to be unemployed and a hazard model for exits out of unemployment. In both models we test for endogenity of housing tenure. In the binary model, exogeneity is rejected so we perform endogenous multinomial treatment effects estimates. In the hazard model we find no evidence of unobserved heterogeneity thus estimates are performed assuming exogeneity. Results show that mortgagers have the lowest probability to be unemployed and the highest job finding rates, while social renters exhibit the worst performance. Whether private renters perform better than outright owners is a matter of debate: while we have no evidence in favour of this claim, the evidence in favour of the opposite is only modest.*

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# 1 Introduction

The empirical literature which has investigated the relationship between homeownership and labour market outcomes has plenty of findings at odds with the so-called "Oswald thesis", which would suggest worse employment prospectus for homeowners than renters (Oswald [1996], Oswald [1997], Oswald [1999]). The idea is that residential mobility constraints imposed by homeownership hamper the propensity to move for job reasons. The consequences should be less intense job search and lower job finding rates.

This claim has been further refined allowing for more precise definitions of the residential status. In fact, owners who have to comply with mortgage payments have higher financial constraints than outright owners, that can counteract reduced mobility due to ownership. A similar distinction may hold for private and social renters as the latter should experience lock-in effects similar to those which hamper owners mobility. Below-market rent, long waiting lists, security of tenure and the restricted transferability within social housing may harm relative employment performance of social renters. In this vein, the Oswald thesis can be tested simply comparing outright owners and private renters, as representative of the typical homeowners and renters who one should have in mind for the main mechanism underlying the Oswald thesis to emerge.

With micro data, the Oswald hypothesis has been tested mainly looking at two different dimensions of labour market performance: the probability to be unemployed and, more often, the unemployment duration[1]. The typical approach consists in modeling such outcomes as a function of the residential status, either being a binary variable for homeownership or a multinomial variable which splits both owners and renters in more categories (owners outright or with mortgage, social and private renters, and sometimes also free-renters).

As regards the first dimension, the typical finding is that homeownership reduces the probability to be unemployed, both when it is assumed exogenous (Coulson and Fisher [2002], Arulampalam et al. [2000][2]) and when it is allowed to be correlated with unobserved heterogeneity (Flatau et al. [2003] and Coulson and Fisher [2009]). Flatau et al. [2003] use also a refined definition of residential status and conclude that owners with mortgage are far less likely to be unemployed than owners outright, that the latter are even less

[1]For an excellent survey of all various tests of the Oswald hypothesis see Havet and Penot [2010].

[2]Arulampalam et al. [2000] take individual heterogeneity into account estimating a random effects probit on a sample of British male drawn from the BHPS for the period 1991-1995.

likely than private renters, and that social and free renters have the highest probability instead. Anyway, results of Flatau et al. [2003] can be criticized since they use only age dummies an education dummies as instruments for the residential status, which are likely to be correlated with unemployment outcomes as well. Moreover the statistical method is questionable, as for the two-step approach to produce consistent estimates, one should apply a (complicated) correction which apparently has not been carried out[3].

Empirical investigations of the effect of residential status on the unemployment duration are more controversial. This may be a consequence of the different empirical strategies employed. Most reliable studies have estimated this effect explicitly accounting for endogeneity. There are two basic approaches to deal with it. The traditional approach consists in performing two step procedures in which identification is achieved through exclusion restrictions. In the first step, residential status, either binary or multinomial, is modeled as a function of regressors used in the second step and instruments which affect housing tenure choice but are hopefully not important in explaining unemployment duration once the effect of regressors is partialled out (Green and Hendershott [2001], Flatau et al. [2003], Brunet and Lesueur [2009], Brunet et al. [2007]).

More recent contributions use a simultaneous estimation method in which multiple spells data are exploited to identify the residential status effect (Munch et al. [2006], Battu et al. [2008], Van Vuuren [2009])[4]. The theoretical fundament of these studies is the local versus non-local labour market argument outlined by Munch et al. [2006]. Unemployment spells are distinguished between those who end up in jobs in the local and in jobs in the non-local labour market, where the difference is simply that the latter require a residential move. Then, competing-risk hazard models are jointly estimated with a tenure choice equation to compare the effect of residential status on exits to local jobs and to non-local jobs. While homeownership is expected to hamper exits to jobs which require a move, the underlying theory would suggest a positive effect on hazard to local jobs.

Typically, as for the probability to be unemployed, homeowners have higher hazard rates into employment than renters (Goss and Phillips [1997], Coulson and Fisher [2002], Flatau et al. [2003][5], Munch et al. [2006], Van Vuuren

[3]See Wooldridge [2010], chapter 15, for a textbook discussion and Rivers and Vuong [1988] for the correct method to perform the two-stage.

[4]This approach requires data such that a sufficient number of individuals experience unemployment spells in a different residential status.

[5]Flatau et al. [2003] obtain a significant effect for males, but not for females. These results are based on the assumption of exogenous homeownership since in a first analysis exogeneity of homeownership cannot be not rejected. Exogeneity of housing tenure is not

[2009]), but this is not always the case. For example, Brunet and Lesueur [2009] with French data and Green and Hendershott [2001] with US data, adopting a different estimation method both find that homeownership lengthens the unemployment duration, *i.e* a result in favour of the Oswald hypothesis. In the analysis of Munch et al. [2006] and Van Vuuren [2009] the counter-Oswald effect is anyway driven by a larger effect for exits to local jobs: homeownership hampers exits to non-local jobs but favours exits to local jobs, and the latter effect outweights the former[6].

When more refined definitions of residential status are used, the most robust finding is that mortgagers have the highest probability to escape unemployment. The comparison between outright owners and private renters is ambiguous. Flatau et al. [2003] on US data and Battu et al. [2008] on UK data find no significant differences[7]. Brunet et al. [2007] confirm results of Battu et al. [2008] on UK data, but for French data they find that outright owners reenter employment more slowly than private renters. Social renting instead seems to lengthen unemployment duration relative to private, as found by Flatau et al. [2003], Battu et al. [2008] and Brunet et al. [2007] for the UK. For France, in Brunet et al. [2007] the effect is positive too but not significant.

Our goal in this study is to take simultaneously into account two important issues, in order to assess the empirical effect of housing tenure on both labour market outcomes: the potential endogeneity of residential status, and the refinement in its definition distinguishing in particular between owners with mortgage and outright, and between social and private renters. We carry out the analysis on UK data drawn from the Labour Force Study. This is not the first application on UK data, but the LFS had never been used before for this purpose.

A typical econometric problem which arises in this literature when allowing for endogeneity is that the standard two-stage least squares estimator is strictly only applicable to situations with linear and continuous outcome and endogenous regressors, both of which are not appropriate when studying the effect of housing tenure on labour market outcomes, such as unemployment status or unemployment duration. As for binary models, we opt for simultaneous estimation methods, which allow efficiency gains in esti-

rejected when the use a 5-fold classification of residential status either.

[6] Munch et al. [2006] use data for Denmark and Van Vuuren [2009] for the Netherlands. In the latter both effects are smaller and the negative effect on non-local jobs is even not significant.

[7] Flatau et al. [2003] cannot reject the hypothesis of exogeneity of housing tenure after a formal test, so their results are based on that assumption. Battu et al. [2008] find no significant differences both for exits to local and non-local jobs.

mation and account for unobserved heterogeneity which can correlate with housing tenure. In particular, we make use of an endogenous multinomial treatment effects method developed by Deb and Trivedi (Deb and Trivedi [2006a], Deb and Trivedi [2006b]). As for the unemployment duration, we refer to a discrete time proportional hazard model with normal distributed unobserved heterogeneity. We estimate two main hazard models with exits to employment and to inactivity. Since the hypothesis of absence of unobserved heterogeneity cannot be rejected in both models, we do not even need to control explicitly for potential sources of correlation between housing tenure and the error term, which otherwise would be very complex in this framework.

The paper is organized in four main sections. The second and the third discuss respectively the data and sample used, and the methodology. Each section looks separately at the probability to be unemployed and the unemployment duration. The fourth and the fifth discuss results for the former an for the latter respectively. The last section concludes.

# 2 Data and Preliminary Evidence

We use a data set drawn from the UK Labour Force Survey (LFS), a quarterly national-wide survey which collects address-based interviews of about 60,000 households for each quarter. Each individual is interviewed in five consecutive quarters on a rotating panel basis. The sample we use spans the period Spring 1999 (March to May) to Winter (December to February) 2005 so that we have 28 quarters of observations[8].

For both analysis of labour market outcomes we select a sub-sample of respondent male head of households in working age (aged 16-64). Moreover we drop a small number of observations for people who have never had paid job, or get retirement or old age pension, or are in full-time education or occupy the household rent-free[9].

The reason why we prefer to focus only on head of households is that in order to model an individual tenure choice, we need individuals for whom the residential status is actually the outcome of an individual choice, which is

[8] In accordance with EU regulations, the LFS moved from seasonal (Spring, Summer, Autumn, Winter) quarters to calendar quarters (January-March, April-June, July-September, October-December) in 2006. We use the old seasonal quarters files to avoid major problems which may arise with the calendar ones before 2006 due to discontinuities in some relevant variables.

[9] Given the subjective nature of questions relating unemployment status or duration we prefer to drop proxy responses and whenever possible we use LFS sampling weights which are designed to allow also for non-response.

typically the case for people responsible for the accommodation in the sense that either the accommodation is owned in their name or they pay the housing costs[10]. For some not head of households it may be misleading to seek for a causal link from housing tenure to labour market behaviour given that the former may not reflect the outcome of an individual choice[11]. For example, a young still living in the family home and dependent on their parents in an owner-occupied accommodation can hardly have a labour market behaviour assimilable to the typical homeowner. Of course this may be the case also for young adults (even older than 24) living in the family home even though they are no more notionally dependent on the parents and they are supposed to make an independent tenure choice. In the latter case, we may keep them in the sample and assume they live in a rent-free status (Flatau et al. [2003], Brunet and Lesueur [2009]), but we believe it is somewhat difficult to single out a rule to identify correctly free-renters since the choice would be highly arbitrary. More in general, it is also questionable to include other kinds of not head of households treating their residential status as that of the household, at least so long as we model housing tenure as an individual choice.

## 2.1 Labour Market Status

According to the ILO definitions, we define three labour market statuses: employed, unemployed, inactive. Employed are workers with paid job; unemployed are without paid job but both they have been looking for it in the last four weeks and they are available to start a new job within the following two weeks; inactive are people in working age who do not stick to the unemployment definition.

Tables 1 and 2 report descriptive statistics on the labour market status distribution by housing tenure. The most striking evidence are the high employment rates of mortgagers (92.8%) and private renters (81%), especially if compared to the low rates of outright owners (64.5%) and social renters (48.4%). However, it is clear that the low numbers of the latter are driven by a very high propensity to be out of the labour force, being 32.7% for outright owners and 39.2% for social renters. This means that if we look

[10] This is the LFS definition of household: "A household is defined as a single person, or a group of people living at the same address who have the address as their only or main residence and either share one main meal a day or share the living accommodation (or both)". The LFS uses this definition of head of household: "Head of household (HOH) is defined as either the man or the husband/male partner of the woman in whose name the accommodation was owned or rented. Where two people have equal claim the either the oldest male is selected or, in all female households, the oldest female".

[11] Nor may be an individual choice the residential status of some head of households, but this issue can be handled using controls at the household level in the empirical analysis.

at notional unemployment rates, intended as the percentage of unemployed in the labour force, the relative performance can change significantly. In fact, outright owners have an unemployment rate of 4.2%, which is nearly a half of private renters rate (8.3%), while mortgagers (1.9%) and social renters (20.4%) are at the opposite extremes. It is thus striking that 14.2% of renters are unemployed, against only 2.3% of homeowners.

For binary labour market status models, we are interested, along the line of the related literature, in examining how the housing tenure affects the probability of being unemployed. As we can easily understand from the Tables discussed above, the outcomes of this kind of analysis depend crucially on what definition of unemployed we choose or/and on what sub-sample we condition on to make comparisons. For example, so long as we are interested in studying the chances of a particular worker to have a job *given that he is in the labour force*, it is appropriate to run a binary model (unemployed versus employed) on a restricted sub-sample without inactive people.

Anyway this strategy clearly involves a sample selection issue since the rule by which workers choose to be out of the labour force may be not random, but may depend on individual characteristics such as housing tenure, as it seems very likely according to our sample statistics. In fact both outright owners and social renters do have higher propensity to be inactive. The key point here is that some people are out of the labour force since they do not want to work, but some other drop off the labour force because a weak labour market position discourages them to look for work though they would be willing to have a job. If outright owners and social renters are more likely to be inactive for the latter reason, the procedure outlined above would yield biased estimates, since if they were in the labour force they would lower the employment probability of their category.

For this reason it is important to make a distinction within inactive between workers who would like to have a paid job but do not stick to the ILO unemployment definition, and ex-workers who do not search since basically they do not want to work. The former may be notionally non ILO unemployed since either they are searching for work but are not available to start a job at once, or they are not currently seeking for it since, for example, they are discouraged, temporarily sick or disabled, waiting for results of an application, or just stopped, say, five weeks ago.

The LFS allows us to make this distinction and to look thoroughly into the reason why people do not want to work. Tables 3 and 4 give insights on this point splitting inactive people on the basis of the response to this specific survey question: "Even though you were not looking for work in the 4 weeks ending Sunday the [date], would you like to have a regular paid job at the moment, either a full or part-time job?". Results show clearly that the percentage of inactive who respond to be not interested in paid job is remarkably higher for homeowners (76.5%), especially for outright owners (82.7%). Tables 5 and 6 focus on the main reason why inactive respond to be not interested in paid job. In general, the most important reasons are long-term sickness/disability and retirement from paid work. However, it is interesting to notice that while renters attribute a far larger importance to the first reason, the reverse is true for outright owners. Moreover, 7.1% of outright owners say they do not need a job while the percentage is negligible for renters.

To summarize, we think the most proper strategy to identify the effect of housing tenure on the employment probability is to compare employed versus non-employed conditioning on workers who would like a paid job. Thus as a test of the Oswald hypothesis, we include in the sample also inactive workers willing to work and pool them with unemployed in defining the binary variable.

Figures 1 and 2 show the distribution of labour market status by housing tenure, where we distinguish inactive people according to the question above, yielding a 4-fold categorization for the status. The bar graphs show that the statuses distribution varies remarkably and that we cannot identify even one status with a roughly constant percentage over housing tenure. Mortgagers distinguish themselves for the highest employed percentage, social renters for the highest percentages of unemployed and of inactive who want a paid job, outright owners for the highest percentage of inactive who do not want job. The distribution of private renters is somehow similar to that of mortgagers given a very high employed percentage and small portions of the other statuses. Moreover, these Figures corroborate the view that the collapse of the four residential statuses to yield the classical dichotomy homeowners-renters would be misleading since many features of the housing tenure story would be lost.

Figures 3 and 4 show the distribution of labour market status focusing on the sample we shall use in the analysis, *i.e.* without those unwilling to work. Social renters are by far the least likely to be employed, while mortgagers are the most likely. Employment rates are very similar for outright owners and private renters but it is evident that the former tend to stay more out of the labour force than the latter when without a job. Thus, if we did not include inactive willing to work, employment rates of outright owners would be remarkably higher relatively to private renters. The econometric analysis will yield more refined results on this comparison by controlling for observable and unobservable characteristics.

## 2.2 Unemployment Duration

In order to perform an unemployment duration analysis by means of the LFS, we exploit a survey variable which heavily relies on the information provided by the respondent. This variable reports the minimum of the length of the time the respondent states to have been looking for work and the length of time since his last job[12]. Durations are grouped in 8 time intervals: 0-3 months, 3-6 months, 6-12 months, 1-2 years, 2-3 years, 3-4 years, 4-5 years, 5 years or more. We use as measure of the spell length the value reported in the last interview associated with the unemployed status before a switch. The status in which the spell ends up may be either employment or inactivity, or may be unemployment when the interview is the last, that is the spell is right censored. Regressors are assumed spell constant and their values refer to the last interview before the exit (or the last interview for censored spells)[13].

Apart from the discrete nature of this variable, the choice to refer to the last interview as unemployed leads to an underestimation of the spell since the precise day in which the spell ends can be whatever else up to the day of the next interview. Yet, this underestimation is of minor concern for our analysis since the error derives from an asynchrony between the spell window and the interviews intervals, which is likely to be random[14].

To prevent interferences in the causal link from housing tenure to unemployment duration we focus on a sub-sample with stable housing tenure data over the spell. In particular, we drop spells for individuals who switch housing tenure in the quarter either immediately preceding or following that in which the spells ends. The first correction rules out situations such when long unemployment spells end right after the entrance in a residential status which favours the exit (either into employment or inactivity). The second one rules out also situations when the change in housing tenure takes place right after the end of the spell. However this sample restriction is of minor importance since the decrease in the sample size is negligible. Before the restriction, we have $9,353$ spells of which $3,023$ end in employment, $1,326$ end in inactivity and $5,000$ are right censored. After the restriction we have $9,230$ spells of which $2,973$ end in employment, $1,297$ end in inactivity and

[12] This is the LFS `durun` variable.

[13] In the sample there are some individuals with multiple unemployment spells, but since they are too few to be exploited we treat multiple spells as spells of different individuals.

[14] There may be a second method to generate unemployment spells from the LFS, which consists of adding up 3 months for each consecutive quarter in which the individual is unemployed (Stam and Long [2010]). This method would be more precise in that the status would be checked quarter by quarter instead of relying on the memory of the interviewed, but it would have the drawback of ignoring short spells occurring in between two consecutive quarters.

$4,960$ are right censored.

We analyse separately spells ending up into employment and into inactivity. First we produce some basic evidence for both spells analysis. Figures 5 and 6 report the Kaplan-Meyer estimates of the cumulative density function for exits to employment. Figures 7 and 8 report the Kaplan-Meyer estimates for exits to inactivity. A first evaluation of the hazards without controlling for observable or unobservable characteristics suggests that the cumulative probabilities both of finding a job and of stopping to look for work are always higher for mortgagers and outright owners than for social and private renters. In particular, exits into employment are always more likely for mortgagers than outright owners, while similar for private and social renters. Exits into inactivity are always more likely for outright owners than for mortgagers, while similar for private and social renters.

# 3 Methodology

## 3.1 Labour Market Status

For the purpose of estimating the effect of housing tenure on the probability of having a job, we model a binary outcome equation which compares non-employed against employed. Notional ILO unemployed are pooled with workers no more in the labour force but who would like a paid job.

When trying to estimate the causal effect of housing tenure on the probability to be non-employed, one should keep in mind that housing tenure may be endogenous. In fact some unobserved factors which affect the labour market outcomes are likely to be correlated with housing tenure. In that case it is important to isolate the true impact of housing tenure from that of unobserved factors which are correlated with it.

In binary outcome models endogeneity could be addressed applying non standard two stage approaches, such as those introduced in Rivers and Vuong [1988]. They consist in a first stage equation, which models the housing tenure discrete choice as a function of the exogenous variables and some suitable instruments, and in a second stage equation, by which the binary outcome is modeled as a probit using the exogenous variables and the predicted errors from the first stage as regressors. The Rivers-Vuong approach also turns out to be a very useful and simple tool to test for endogeneity, since the t-statistic of the predicted error terms in the second stage represents a valid test for the null hypothesis of exogeneity.

In the empirical literature, some studies have attempted to allow for this source of bias adopting a very similar method (see Flatau et al. [2003] and

Coulson and Fisher [2009]). Unfortunately this two stage approach is typically less efficient than simultaneous estimation methods and requires complicated calculations to get second step consistent average partial effects and standard errors, which is mostly true when the endogenous regressor is discrete (see Wooldridge [2010], section 15.7). For these reasons we prefer to adopt a joint estimation method of the two sets of equations, though the Rivers-Vuong two-step approach will be employed to test for endogeneity.

In particular, we make use of an endogenous multinomial treatment effect estimation method developed by Deb and Trivedi (Deb and Trivedi [2006a], Deb and Trivedi [2006b]) which turns out to be the most suitable method we are aware of for our case[15]. This method can be used to analyze the effects of an endogenous multinomial treatment on a binary outcome variable. In our framework the treatments are represented by the four housing tenure statuses. More precisely, we set private renting as the control group (*i.e.* base category) and we interpret property owned outright, mortgage holding and social renting as three different kinds of treatment whose differential effect on the probability of being non-employed we aim at estimating.

The model specification comprises an outcome equation with a structural-causal interpretation and other equations that model the generating process of treatment variables (see the appendix for a formal representation). The estimation method relies on the specification of a joint distribution for the outcome and the endogenous treatment choice. Latent factors enter into the outcome and treatments equations in the same way as observed covariates and incorporate unobserved characteristics related both to the housing tenure choice and to the probability of being unemployed. Since the latent factors enter the likelihood function but are unknown, the maximization of the likelihood function is performed through simulation by drawing several random numbers from a standard normal distribution[16]. The housing tenure choice is modeled with a mixed multinomial logit, while the probability density of the outcome variable is assumed to follow a logistic function.

The identification of the parameters of the model is achieved through exclusion restrictions, that is we include in the tenure choice model a set of in-

[15]The method is implemented using the Stata routine `mtreatreg` provided by the reference.

[16]Provided that the number of draws is sufficiently large, maximization of the simulated log likelihood is equivalent to maximizing the log likelihood (Gourieroux et al. [1984]). See Deb and Trivedi [2006a] and Deb and Trivedi [2006b] for a discussion on the choice of the number of draws. In order to save on computing time, the program uses quasi-random draws based on Halton sequences instead of standard methods based on pseudo-random draws. The former have been proved to be more effective for maximum simulated estimation as they can provide the same accuracy with fewer draws (see Bhat [2001] and Train [2003]).

struments which are excluded in the outcome equation[17]. Valid instruments should satisfy two conditions: first, they should be relevant, that is substantially correlated with the endogenous regressors; second, they should be exogenous, that is uncorrelated with the outcome except through their effect on the endogenous regressors. The literature which has attempted to identify the causal link from housing tenure to labour market outcomes has plenty of examples of instruments for housing tenure, such as regional homeownership rates (Munch et al. [2006], Brunet and Lesueur [2009], Van Leuvensteijn and Koning [2004]), father's occupation (Battu et al. [2008], Brunet and Lesueur [2009]), age dummies (Flatau et al. [2003]), the ratio of the user owner cost to the rent (Flatau et al. [2003]), the state marginal tax rate (Coulson and Fisher [2009]), number of families within the household (Coulson and Fisher [2009]), sex of first two children born in the household (Coulson and Fisher [2009]), housing tenure of parents (Munch et al. [2006]), housing tenure in the city of birth (Munch et al. [2006]), price of rents in the neighborhood (Brunet and Lesueur [2009]), vacancy rates (Brunet and Lesueur [2009]), average distance to jobs (Brunet and Lesueur [2009]), age of entry into the housing (Brunet and Lesueur [2009]).

Our data allow us to select a set of three instruments: the number of family units within the household, the sex of the first two children born in the household and an aggregate house price index at regional level.

First, the number of family units should be related to housing tenure, since single-family detached units are more likely to live in owner-occupied dwellings while multifamily dwellings are more likely to live in rented accommodation. Yet there is no reason to expect an influence of the number of family units on labour market outcomes, once controlling for the other regressors.

Second, given parental preferences for a mixed sibling-sex composition, the sex of the first two children is used to create a valid instrument for housing tenure as proposed by Angrist and Evans [1998] and used in Coulson and Fisher [2009]. In particular, we create a dummy which takes one for households in which the sex of the first two children born is the same and zero otherwise. Parents with same-sex siblings are more likely to have an additional child so we expect the dummy to be significant in a housing tenure choice model given that the presence of children is well known to be correlated with a propensity to become owner (see Coulson and Fisher [2009]). Yet, this dummy should be redundant in an unemployment status binary model once housing tenure

[17] In principle the parameters of the model are identified even if the regressors included in the outcome equation are identical to those in the treatment equations. However, Deb and Trivedi [2006a] and Deb and Trivedi [2006b] recommend using traditional exclusion restrictions for more robust identification.

is controlled for.

Third, we use a quarterly house prices real index at regional level which should predict the regional and time variation in the propensity to home-ownership[18]. The choice to buy a home or to live in rented accommodation depends of course on the price of houses. In fact the propensity to become homeowner should drop as the house prices increase across regions and/or over quarters. Yet, there is no a-priori reason to expect an effect of house prices on individual labour outcomes other than that transmitted by housing tenure (or by other covariates).

In principle we may use also regional housing tenure rates (as sometimes is found in the literature) which of course should be strongly correlated with individual housing tenure. Anyway there is no warranty that these rates are even not related with individual labour market outcomes, since, after all, the original formulation of the Oswald hypothesis argues for an aggregate correlation between home ownership rates (most of all at country level) and unemployment rates, whose micro foundation must be found out in the individual causal link from housing tenure towards labour market outcomes.

## 3.2 Unemployment Duration

With regards to the unemployment duration analysis we model two hazard equations, one for exits into employment and one for exits into inactivity, where the duration variable is drawn from a specific question which groups answers in trimester basis time units. We estimate these equations by a discrete time proportional hazard model with piecewise constant baseline hazard.

When duration data comes in discrete time as in this case, the typical approach for estimation is to apply standard binary choice models to stacked data, such as the complementary log-log (clog-log) or the logit regression. We use the clog-log model which represents the discrete-time analogue of the well-known Cox proportional hazards model (Prentice and Gloeckler [1978]). The hazard is assumed to be constant over the duration intervals.

In order to control for unobserved individual heterogeneity ("frailty") the method we use incorporates a random variable which enters the hazard specification as a multiplicative scale factor (see Jenkins [2008], lecture 7). This random variable summarises the impact of omitted variables on the hazard rate and is assumed to follow a normal distribution. However, a crucial assumption in this model is that the random variable is distributed

[18] This is derived from the Halifax House Price Index (Halifax [2010]).

independently of both the regressors and time[19]. As matter of fact, we are estimating a random effects panel model with a clog-log link function.

# 4 Empirical results: The probability of being non-employed

Table 7 reports results of four different models for the probability of non-employment. The first two use the traditional homeownership binary variable, while the subsequent use the more precise 4-fold categorization. For both cases we estimate at first a standard binary model ignoring the potential endogeneity of housing tenure, and then a simultaneous model which explicitly accounts for it. As set of controls we include disability/sickness benefits receipt, marriage status, age dummies, type of last occupation, education, and seasonal, yearly and regional dummies.

When housing tenure takes the form of a simple binary homeownership choice, we address endogeneity using a seemingly unrelated bivariate probit model (see Wooldridge [2010] and Greene [2003]). A probit for unemployment and a probit for homeownership are estimated jointly making use of a set of instruments. Errors in the two equations are potentially correlated and are assumed to follow a bivariate normal distribution. In column (1) and (2) of Table 7 we report estimates from the standard probit and the bivariate probit respectively. Endogeneity of homeownership is supported both by the evidence of correlation in the two error terms of the bivariate probit and by the Rivers-Vuong two stage test[20]. In both columns the negative effect of owning the accommodation on the probability of non-employment

[19] In the literature the consequences of mistakenly ignoring unobserved heterogeneity have been investigated mainly with reference to continuous time proportional hazard model. The main results suggested in terms of parameters estimation are (Jenkins [2008], lecture 6): (1) Overestimation of the degree of negative duration dependence, and underestimation of the degree of positive duration dependence; (2) The proportionate effect of a given regressor on the hazard rate is no longer constant and independent of survival time; (3) Underestimation (overestimation) of the positive (negative) effect of a regressor. However the magnitude of the biases should be attenuated when a fully flexible specification for the baseline hazard is assumed.

[20] The Rivers-Vuong test is carried out running in the first stage a probit model of homeownership in which are used as instruments `famnum` and `hpinsareal` (`samesexhh` is not significant thus it is not included in the final specification). In the second stage a probit regression for non-employment is run including as further regressor the predicted error term from the first stage (coefficients are biased under endogeneity). Under the null hypothesis of exogeneity the coefficient of the error term is zero but the test suggests endogeneity of homeownership since the hypothesis is statistically strongly rejected. Results of this test are available upon request.

is evident[21].

The binary definition of housing tenure is anyway too simplistic and results of models (1) and (2) may be misleading. Model (3) of Table 7 performs a standard logit regression using the three housing tenure dummies as regressors (the base category is private renter). Estimated coefficients of the index function suggest that after controlling for *observable* characteristics, mortgagers and outright owners are less likely to be non-employed than private renters, while social renters are more likely.

However these results may be spurious since they ignore endogeneity due to potential selection into residential status based on unobservables. Endogeneity of housing tenure dummies is tested using the Rivers-Vuong two step method. The coefficients of predicted errors from the first stage housing tenure choice model turn out to be jointly statistically significant, which allow us to reject the hypothesis of exogeneity[22].

Column (4) of Table 7 reports maximum simulated likelihood estimates from the multinomial treatment effect model which accounts for endogeneity[23]. The Table reports coefficients of the logit index function which are informative on the direction of the effects but cannot be readily interpreted in terms of their magnitude. Overall, there is evidence of selection on unobservables since $\lambda$-s coefficients of the latent factors are jointly highly significant, which supports again a rejection of the hypothesis of exogeneity[24]. In particular, both the coefficients $\lambda_{mort}$ and $\lambda_{out}$ are positive suggesting that individuals who are more likely to own the accommodation, either with a mortgage or outright, relative to privately rented dwellings, are also more likely to be non-employed on the basis of their *unobserved characteristics.* Conversely, $\lambda_{soc} < 0$ suggests that individuals who are more likely to occupy the accommodation on social renting basis than private, are less likely to

[21] Homeownership is instrumented using only `famnum` and `hpinsareal` since `samesexhh` is not significant both in the bivariate probit and in the first stage of the Rivers-Vuong test. In the homeownership binary choice model both instruments are significant and negative suggesting that the probability of becoming homeowner is lower for multi-family detached units and decreases with house prices. Results of the homeownership choice probit are available upon request.

[22] In the first stage we estimate a multinomial logit and then plug predicted errors in the outcome equation. Results are available upon request.

[23] Latent factors are simulated drawing $1,200$ random variables from the standard normal distribution. Standard errors are robust in the sense that take simulation error into account (Deb and Trivedi [2006a], Deb and Trivedi [2006b]).

[24] A simple likelihood-ratio test for endogeneity corresponds to the test of the joint significance of the three coefficients. The null hypothesis that the coefficients are simultaneously equal to zero is rejected which is strong evidence in favour of endogeneity (Deb and Trivedi [2006a], Deb and Trivedi [2006b]).

be non-employed on the basis of their unobserved characteristics. In other words, when $\lambda$ is positive (negative), it means that unobserved characteristics that increase the probability of being in that particular treatment relative to the control, also lead to higher (lower) probability of non-employment for treated individuals.

Estimates of the endogenous logit are reliable in terms of the direction of the effects, both as regards the outcome equation and the tenure choice model. In the outcome equation, the probability of being non-employed is enhanced by sickness/disability benefits, by living with spouse without job and by previous occupations as Managers/Senior officials, on Skilled Trades or on Sales, while the probability is reduced by young age (16-34), by higher education, by living with spouse with a job and by previous occupations as Professional, Associate Prof/Technical, Administrator/Secretarial, on Personal Service and as Operative. In the housing tenure choice model (see Table C1), instruments are generally significant and consistent with our expectations: the number of family units within the household is lower in private rented dwellings, siblings of same sex are less likely in private rented dwellings and higher house prices reduce propensity to homeownership.

As regards the treatment effects of housing tenure, they maintain the same sign as in the exogenous case, where mortgagers and outright owners are less likely to be non-employed than private renters, and social renters are more likely. To have an idea on the magnitude of the treatment effects, and how they change after accounting for endogeneity, we report also estimates of the marginal effects for both models. Marginal effects give the percentage points increase in the probability of being unemployed for the change in status between the base (*i.e.* private renter) and the current, given a specific set of values of the regressors[25]. Table 8 report marginal effects calculated at sample means of the regressors, while Tables 10 and 11 report marginal effects at representative values of regressors (see the appendix for a formal representation of marginal effects in the endogenous case).

Interestingly, Table 8 shows that while signs of treatment effects are maintained once endogeneity is accounted for (as we have already pointed out), the magnitude gets smaller in absolute terms and the relative effect of owning the accommodation outright is even no more significant at 5%. In particular, the reduction effect on the non-employment probability for mortgagers

[25] A more syntectic measure of the effect maybe the average partial effect, which averages over individuals the marginal effect of the variable for every individuals using observed values. Anyway, for the treatment effect model in which simulated latent factors are added to the outcome equations, average partial effect would require to recover the actual simulated values, while with marginal effects we can get around it setting them at fixed values such as zero.

shrinks from 6 to 2.1 points, and the incremental effect for social renters shrinks from 3.8 to 1.2 points. How can we interpret these changes in impact? One likely explanation is that in our specification we fail in modeling some sort of skills-gaps which enhance the relative labour market position of mortgagers and outright owners while weaken that of social renters. So, when housing tenure is assumed exogenous, treatment effects are inflated as they capture also the effect of unobserved skills gaps. We believe that this finding is quite relevant, since explanations of rescaling in effects after accounting for endogeneity are quite unsatisfactory in the related literature. In fact, previous studies which attempted to estimate the causal effect of housing tenure on the probability to be unemployed, either did not focus on multinomial tenure (Coulson and Fisher [2009]) or did not tackle rigorously the endogeneity problem (Flatau et al. [2003][26]).

Tables 10 and 11 report marginal effects when the set of regressors values is chosen discretionally instead of at sample means. In these calculations, we always hold fix marriage status (non married), disability benefits receipt (no receipt), season (Winter) and year (2005), while Region varies across Tables (South East or London), and age (16-34 or 45-54), education (GCE or Degree) and occupation (Managers/Senior Off. or Professional) vary within Tables. The rule of selection is representativeness, in the sense that we choose most frequent and relevant values (see Table 9). Again, we observe in these cases that treatment effects for mortgagers and outright owners shrink after accounting for endogeneity. Anyway, unlike marginal effects computed at sample means, the treatment effect for outright owners remains always significant at 5%, and the treatment effect for social renters becomes much larger. As regards the latter, it is interesting to note that the size becomes very large, around 30 points, when age is set to $45-54$, occupation is set to Managers/Senior Officials, and education is set to GCE, *i.e.* all categories that are relative more strongly associated to non-employment. Though these marginal effects partly disagree with those computed averaging over the whole sample, such results cannot be ruled out either.

[26] However, Flatau et al. [2003] find for males that the marginal effect gets larger for mortgagers, remains similar for outright owners and becomes not significant for social renters. The marginal effect for social renters becomes not significant even in the females sample.

# 5 Empirical results: unemployment duration

## 5.1 Exit to employment

Table 12 reports estimates of a discrete time proportional hazards model using the sample of spells which either end into employment or are right censored. Marginal effects measure the impacts of the covariates on the probability to find a job when the set of regressor is evaluated at sample means. We report also hazard ratios for ready interpretation, which for dichotomous variables represent the ratio of hazards between the selected and the base category[27]. The first column reports estimates of the clog-log model without controlling for frialty. These estimates suggest that mortgagers and outright owners have a probability to find a job, respectively, two times larger and 55% larger than private renters. There is not a significant difference in probabilities between social and private renters.

The second column of Table 12 refers to the proportional hazard model in which normally distributed unobserved heterogeneity is allowed for. Estimates are almost identical to those of the first model. In fact the likelihood ratio test suggests that the unobserved heterogeneity is unimportant since the $\rho$ statistic is negligible and not significantly different from zero[28]. Hence we cannot refuse the null hypothesis that heterogeneity is absent. As a robustness check, we estimated different models with alternative specifications. Using a logistic model with Normal distributed errors, unobserved heterogeneity is not significant as well, and using a proportional hazard model with a Gamma distribution for frailty the likelihood does not converge[29]. In conclusion, we have no significant evidence of unobserved individual characteristics which affect the probability of finding a job, thus we do not even need to control for confoundness originating from unobservables.

As the baseline hazard dummies suggest, the hazard function exhibits a non-monotonic behaviour (see model 1). In particular, unemployed have the highest probability to find job in the first three months of job seeking. The probability decreases steadily with duration up to four years, and increases slightly afterwards. This figure is consistent with the commonly perceived wisdom that as the unemployment spell lengthens, unemployed loose skills

[27]For continuous variables the hazard ratio gives the percentage increase (if the ratio is greater than one; decrease if less than one) in the hazard rate for a unit increase in the covariate.

[28]The reported $\rho$ is the ratio of the heterogeneity variance to one plus the heterogeneity variance.

[29]Results of the random effects logit are very similar and are available upon request. The failure to achieve convergence with a gamma distributed frailty may be due to a very small variance of the frailty.

and attachment to the labour market, and/or employed are less willing to hire unemployed due to a stigma effect.

The estimated effect of the other covariates is in line with standard economic interpretations. People claiming unemployed or sickness/disability benefits have longer unemployment spells. Married unemployed are 25.7% more likely to find a job than non married. Least educated unemployed have the lowest chance to escape unemployment. Workers who were previously employed in elementary occupations have lower probabilities to reenter employment than the other types of workers, being the difference significant for Managers and Senior Officials, Administrative and Secretarial occupations, Skilled Trades Occupations, and Process, Plant and Machine Operatives. The probability to find a job decreases with age, though the difference between unemployed aged $16-24$ and aged $25-34$ is not significant.

Fig. 9 and 10 plot hazard estimates for exits to employment by housing tenure. These estimates are out-of-sample predictions computed after running the clog-log model (with no frailty), and refer to two representative unemployed with identical characteristics, but one being an unemployed benefit claimant and the other not. In the second plot the hazards are shifted downwards by a same amount given that claimants have lower exit probabilities[30]. The decay in the hazard looks actually marked though after four years it increases mildly.

## 5.2 Exit to inactivity

Table 13 reports the proportional hazard model estimates when spells can end up into inactivity or are censored. In this case, outright owners have the highest probability of leaving the unemployed state. In particular they are $75,5\%$ more likely than private renters, who have the lowest probability. Mortgagers and social renters behave basically the same way.

Even in this case, unobserved heterogeneity is not significant suggesting that ignoring it is not a major problem[31].

Fig. 11 and 12 show the counterparts of Fig. 9 and 10 for exits into inactivity. The hazard function of stopping looking for work has a similar

[30]The difference in the level of the two plots is larger than the estimated marginal effect of the claimant dummy reported in Table 12. In fact, while the marginal effects are computed at the means of the regressors, the plots refer to a representative unemployed with characteristics chosen at our discretion: aged 25-49, non married, not getting disability benefits, professional occupations, GCSE qualification, resident in Inner London, in Summer, in 2005.

[31]No relevant changes take place if we use a random effects logit model. Converge was not achieved using a proportional hazard model with a Gamma distributed frailty.

U-shape form to that for exits into employment, in that it decays up to the same interval, and it is higher afterwards. In the last interval the failure probability is very high but this result must be interpreted with caution since the interval dummy is not significant. Anyway, the extent of the decay is less marked if compared to exits into employment figures as the hazard ratios of the interval dummies suggest. Moreover, if we look at the marginal effects in Tables 12 and 13 and at the y-axis on the plots, we see that the changes in the hazard are small in absolute terms since the probability of leaving unemployment for a job is, on average, much higher than for inactivity.

As a matter of fact, unemployed people have the highest probability to leave the labour force in the first 3 months window, which suggests that unemployed who decide to drop off the labour force do it mostly soon after the start of the spell. Of course this result does not take into account that unemployment periods can alternate with periods out of the labour force, thus the evidence of most frequent jumps to inactivity in the first time interval can be consistent with soon leavers being more prone to reenter the status at some point. Anyway, in general, it does seem that after four years of unemployment the job seeker is at a crossroads: either finds a job, or drop off the labour force.

As regards the effect of the covariates on the hazard rate Table 13 show some interesting results. Unemployed benefit claimants are 52% less likely to stop being unemployed. This result is easily understood since job seeking is a requirement for benefit eligibility and the utility of the benefit can offset in most cases the disutility of the compliance to the benefit system rules. Instead, unemployed on sickness/disability benefits are 58% more likely to end up out of labour force. Married unemployment are more likely to drop off the labour force. Elementary occupations are associated with the lowest probability of leaving the labour force, which is significantly lower than that associated to Managers and Senior Officials, Administrative and Secretarial occupations, Personal Service, and Process, Plant and Machine Operatives. Age and education dummies are not significant.

# 6 Conclusion

In this paper we perform two tests of the Oswald hypothesis. First we estimate the effect of housing tenure on the probability to be non-employed. Second we estimate the effect on the hazard out of unemployment, both for exits to employment and exits to inactivity. The tests are employed assuming exogeneity of housing tenure and then allowing for unobserved heterogeneity.

In the first exercise, the null hypothesis of exogeneity of housing tenure is strongly rejected. Thus we estimate an endogenous multinomial treatment effects model to account for the effects of unobserved heterogeneity which can be correlated with housing tenure. Marginal effects estimates suggest that mortgagers are less likely and social renters more likely to be non-employed than private renters. Owning the accommodation outright reduces the probability to be non-employed relative to private renting, but the effect is only close to be statistically significant when marginal effects are computed at sample means. The size of treatment effects is larger when housing tenure is assumed exogenous, suggesting that we may have omitted in the specification some unobserved skills which enhance the relative labour market position of mortgagers and outright owners while weaken that of social renters. When marginal effects are computed at representative values, the effect of outright ownership turns out to be statistically significant though quite small. Also, for these particular cases, the incremental effect of social renting turns out to be very large.

In the hazard analysis, unobserved heterogeneity seems unimportant and estimated effects change negligibly once it is explicitly accounted for. Thus we do not even attempt to control for confounding effects. Estimated effects on the proportional hazard rate to employment suggest that mortgagers have the highest probability to escape unemployment and that outright owners are more likely to exit than both private an social renters. Exit rates of private and social renters are not statistically different. As regards exit rates to inactivity, outright owners have the highest probability, while private renters have the lowest. Mortgagers and social renters behave the same way.

Flows from unemployment to inactivity concern in part workers with particularly low employment prospects who give up search, and in part workers who decide to drop off the labour force independently. Unfortunately our empirical strategy in estimating exits to employment cannot account for situations such the first, which may contribute to keep job finding rates high (low) for categories more (less) prone to end up into inactivity. For example, since exit rates to inactivity are higher for outright owners and lower for private renters, it may be the case that job finding rates for outright owners and private renters are, respectively, overestimated and underestimated.

Overall, what is left from these exercises is that mortgagers have typically the best labour market performance, while social renters the worst. As regards private renters and outright owners, whether the former perform better than the latter is a matter of debate. While we have no evidence in favour of this claim, the evidence in favour of the opposite is only modest.

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# Tables and Figures

**Table 1**
SAMPLE STATISTICS: LABOUR MARKET STATUS BY HOUSING TENURE

| | Labour Force status | | | |
|---|---|---|---|---|
| Housing Tenure | **Employed** | **Unemployed** | **Inactive** | Total |
| **owned outright** | 49,061 | 2,151 | 24,907 | 76,119 |
| **mortgage** | 245,333 | 4,818 | 14,342 | 264,493 |
| **rented social** | 29,070 | 7,442 | 23,497 | 60,009 |
| **rented private** | 35,502 | 3,234 | 5,095 | 43,831 |
| Total | 358,966 | 17,645 | 67,841 | 444,452 |
| **owned** | 294,394 | 6,969 | 39,249 | 340,612 |
| **rented** | 64,572 | 10,676 | 28,592 | 103,840 |
| Total | 358,966 | 17,645 | 67,841 | 444,452 |

Notes:

1. The sample is made of respondent male head of households in working age. Observations are quarterly from Spring 1999 (March to May) to Winter (December to February) 2005. A small number of observations is dropped regarding people who have never had paid job, or get retirement or old age pension, or are in full-time education or occupy the household rent-free.

**Table 2**
SAMPLE STATISTICS: LABOUR MARKET STATUS BY HOUSING TENURE (PERCENTAGES)

| | Labour Force status | | | |
|---|---|---|---|---|
| Housing Tenure | **Employed** | **Unemployed** | **Inactive** | Total |
| **owned outright** | 64.5 | 2.8 | 32.7 | 100 |
| **mortgage** | 92.8 | 1.8 | 5.4 | 100 |
| **rented social** | 48.4 | 12.4 | 39.2 | 100 |
| **rented private** | 81.0 | 7.4 | 11.6 | 100 |
| **owned** | 86.5 | 2.0 | 11.5 | 100 |
| **rented** | 62.2 | 10.3 | 27.5 | 100 |

Notes:

1. See note 1 to Table 1.

**Table 3**
Sample statistics of inactive workers: Willingness to work by Housing Tenure

| Housing Tenure | Would like a paid job **No** | **Yes** | Total |
|---|---|---|---|
| **owned outright** | 20,389 | 4,254 | 24,643 |
| **mortgage** | 9,055 | 4,773 | 13,828 |
| **rented social** | 13,156 | 9,793 | 22,949 |
| **rented private** | 2,685 | 2,139 | 4,824 |
| Total | 45,285 | 20,959 | 66,244 |
| **owned** | 29,444 | 9,027 | 38,471 |
| **rented** | 15,841 | 11,932 | 27,773 |
| Total | 45,285 | 20,959 | 66,244 |

Notes:

1. See note 1 to Table 1.
2. The sample is restricted to individuals out of the labour force. Results are derived from this specific survey question: "Even though you were not looking for work in the 4 weeks ending Sunday the [date], would you like to have a regular paid job at the moment, either a full or part-time job?".

**Table 4**
Sample statistics of inactive workers: Willingness to work by Housing Tenure (percentages)

| Housing Tenure | Would like a paid job **No** | **Yes** | Total |
|---|---|---|---|
| **owned outright** | 82.7 | 17.3 | 100 |
| **mortgage** | 65.5 | 34.5 | 100 |
| **rented social** | 57.3 | 42.7 | 100 |
| **rented private** | 55.7 | 44.3 | 100 |
| **owned** | 76.5 | 23.5 | 100 |
| **renter** | 57.0 | 43.0 | 100 |

Notes:

1. See notes to Table 3.

**Table 5**

Sample statistics of inactive unwilling to work: main reason why does not want a regular full/part-time job by housing tenure

| Main Reason | Housing Tenure owned outright | mort-gage | rented social | rented private | Total |
|---|---|---|---|---|---|
| **waiting application** | 0 | 2 | 7 | 1 | 10 |
| **student** | 44 | 77 | 90 | 87 | 298 |
| **look after fam/home** | 589 | 862 | 1,585 | 275 | 3,311 |
| **temp. sick/injured** | 110 | 165 | 422 | 129 | 826 |
| **long-term sick/disabled** | 6,060 | 4,349 | 9,851 | 1,687 | 21,947 |
| **doesn't need work** | 1,461 | 490 | 38 | 38 | 2,027 |
| **retired from paid work** | 11,808 | 2,766 | 822 | 273 | 15,669 |
| **other** | 451 | 455 | 363 | 213 | 1,482 |
| Total | 20,523 | 9,166 | 13,178 | 2,703 | 45,570 |

Notes:

1. See note 1 to Table 1.
2. The sample has been restricted to inactive people who declares not to want a regular paid job, either full-time or part-time. Then results are derived from this specific survey question: "What was the main reason that you did not want work (in the last 4 weeks)?".

**Table 6**

Sample statistics of inactive unwilling to work: main reason why does not want a regular full/part-time job by housing tenure (percentages)

| Main Reason | Housing Tenure owned outright | mort-gage | rented social | rented private |
|---|---|---|---|---|
| **waiting application** | 0.0 | 0.0 | 0.1 | 0.0 |
| **student** | 0.2 | 0.8 | 0.7 | 3.2 |
| **look after fam/home** | 2.9 | 9.4 | 12.0 | 10.2 |
| **temp. sick/injured** | 0.6 | 1.8 | 3.2 | 4.8 |
| **long-term sick/disabled** | 29.5 | 47.4 | 74.7 | 62.4 |
| **doesn't need work** | 7.1 | 5.4 | 0.3 | 1.4 |
| **retired from paid work** | 57.5 | 30.2 | 6.2 | 10.1 |
| **other** | 2.2 | 5.0 | 2.8 | 7.9 |
| Total | 100 | 100 | 100 | 100 |

Notes:

1. See notes to Table 5.

## Table 7

### Probability of being non-employed

| | (1) Probit | | (2) Endog. Probit | | (3) Logit | | (4) Endog. Logit | |
|---|---|---|---|---|---|---|---|---|
| | $\beta$ | *std. err.* | $\beta$ | *std. err.* | $\beta$ | *std. err.* | $\beta$ | *std. err.* |
| **homeowner** | -0.762** | *0.008* | -1.7** | *0.026* | | | | |
| **outright owner** | | | | | -0.579** | *0.028* | -1.986** | *0.280* |
| **mortgager** | | | | | -1.227** | *0.022* | -5.626** | *0.558* |
| **social renter** | | | | | 0.719** | *0.023* | 3.448** | *0.329* |
| **disability benf.** | 1.805** | *0.013* | 1.520** | *0.016* | 3.192** | *0.024* | 6.944** | *0.569* |
| *marriage status* | | | | | | | | |
| **married - spouse in emp.** | -0.322** | *0.009* | -1.400** | *0.010* | -0.687** | *0.018* | -0.764** | *0.066* |
| **married - spouse non emp.** | 0.133** | *0.010* | 0.202** | *0.010* | 0.198** | *0.019* | 0.567** | *0.066* |
| *age dummies* | | | | | | | | |
| **age 35-44** | 0.099** | *0.010* | 0.201** | *0.009* | 0.092** | *0.020* | 0.487** | *0.064* |
| **age 45-54** | 0.200** | *0.010* | 0.326** | *0.010* | 0.221** | *0.021* | 0.712** | *0.082* |
| **age 55-64** | 0.526** | *0.011* | 0.670** | *0.011* | 0.706** | *0.024* | 1.350** | *0.143* |
| *last occupation type* | | | | | | | | |
| **Managers/Senior Off.** | -0.257** | *0.014* | -0.030* | *0.014* | -0.395** | *0.028* | 0.053 | *0.066* |
| **Professional** | -0.377** | *0.017* | -0.160** | *0.018* | -0.649** | *0.036* | -0.538** | *0.087* |
| **Assoc. Prof./Technical** | -0.381** | *0.016* | -0.173** | *0.016* | -0.643** | *0.033* | -0.539** | *0.080* |
| **Admin./Secretarial** | -0.274** | *0.019* | -0.104** | *0.018* | -0.480** | *0.038* | -0.369** | *0.087* |
| **Skilled Trades** | -0.165** | *0.013* | 0.004 | *0.013* | -0.252** | *0.025* | 0.026 | *0.059* |
| **Personal Service** | -0.273** | *0.020* | -0.199** | *0.020* | -0.476** | *0.040* | -0.755** | *0.108* |
| **Sales** | -0.126** | *0.020* | -0.019 | *0.020* | -0.176** | *0.040* | 0.102 | *0.094* |
| **Operatives** | -0.141** | *0.013* | -0.012 | *0.013* | -0.235** | *0.025* | -0.027 | *0.059* |
| *education (highest)* | | | | | | | | |
| **Degree** | -0.203** | *0.014* | -0.066** | *0.014* | -0.391** | *0.029* | -0.293** | *0.064* |
| **Higher educ.** | -0.160** | *0.015* | -0.015 | *0.015* | -0.287** | *0.032* | -0.068 | *0.068* |
| **GCE** | -0.157** | *0.010* | -0.018 | *0.010* | -0.288** | *0.019* | -0.097* | *0.044* |
| **GCSE** | -0.064** | *0.011* | 0.052** | *0.011* | -0.113** | *0.022* | 0.188** | *0.054* |
| seasonal dummies | ✓ | | ✓ | | ✓ | | ✓ | |
| yearly dummies | ✓ | | ✓ | | ✓ | | ✓ | |
| regional dummies | ✓ | | ✓ | | ✓ | | ✓ | |
| $\lambda_{OUT}$ | | | | | | | 0.551* | *0.250* |
| $\lambda_{MORT}$ | | | | | | | 3.608** | *0.415* |
| $\lambda_{SOC}$ | | | | | | | -2.189** | *0.264* |
| number of observations | 382,778 | | 382,778 | | 382,778 | | 382,778 | |

Notes:

1. * significant at 5%; ** significant at 1%. See the Appendix for the base categories of discrete regressors. See note 1 to Table 1 for sample restrictions. See the appendix for a formal representation of model (4). $\beta$s are coefficients of the index function and are informative on the sign of the effects but not on the magnitude.

2. The dependent variable is a dummy for non-employment (unemployed and inactive who would like paid job) versus employment status. Four different estimation methods are used (sampling weights are always used). (1) Probit regression. (2) Bivariate probit, where a probit for non-employment is estimated jointly with a probit for homeownership choice (homeownership is instrumented with `famnum` and `hpinsareal`). (3) Logit regression. (4) Multinomial endogenous treatment effects, where a logit for non-employment is estimated jointly with a mixed multinomial logit for the housing tenure choice (housing tenure is instrumented with `famnum samesexhh` and `hpinsareal`). $\lambda$-s are loading factors of the latent terms and positive (negative) $\lambda_j$ ($j \in OUT, MORT, SOC$) indicates that unobserved characteristics which increase the probability of treatment $j$-th relative to private renting also lead to higher (lower) probability of non-employment relative to that.

**Table 8**

Marginal Effects of Housing Tenure, at means

| Logit - Exogenous Housing Tenure | | | | | |
|---|---|---|---|---|---|
| | *dy/dx* | *std. error* | *P-value* | *[ 95%* | *Conf.Int. ]* |
| **outright owner** | -1.92** | *0.0008* | 0.000 | -2.07 | -1.77 |
| **mortgager** | -6.03** | *0.0013* | 0.000 | -6.29 | -5.77 |
| **social renter** | 3.76** | *0.0016* | 0.000 | 3.44 | 4.07 |
| Logit - Endogenous Housing Tenure | | | | | |
| | *dy/dx* | *std. error* | *P-value* | *[ 95%* | *Conf.Int. ]* |
| **outright owner** | -0.06 | *0.0004* | 0.081 | -0.13 | 0.01 |
| **mortgager** | -2.11** | *0.0058* | 0.000 | -3.23 | -0.98 |
| **social renter** | 1.15* | *0.0047* | 0.014 | 0.23 | 2.07 |

Notes:

1. * significant at 5%; ** significant at 1%. Reported marginal effects are multiplied by 100.
2. Statistics are from the models (3) and (4) of Table 7. Marginal effects are computed at sample means of regressors and latent factors are set to zero.

**Table 9**

Sample means of regressors

| *variables* | *means* | *variables* | *means* |
|---|---|---|---|
| **married - spouse in emp.** | 0.4467 | **2000** | 0.1699 |
| **married - spouse non emp.** | 0.1299 | **2001** | 0.1693 |
| **disability benefits** | 0.0356 | **2002** | 0.1554 |
| **age 35-44** | 0.3275 | **2003** | 0.1157 |
| **age 45-54** | 0.2429 | **2004** | 0.1100 |
| **age 55-64** | 0.1458 | **2005** | 0.1051 |
| **Managers/Senior Off.** | 0.2020 | **East Anglia** | 0.1009 |
| **Professional Occupations** | 0.1441 | **East Midlands** | 0.0728 |
| **Assoc. Professional and Tech.** | 0.1345 | **London** | 0.1095 |
| **Administrative and Secretarial** | 0.0523 | **North West** | 0.1011 |
| **Skilled Trades** | 0.1827 | **North** | 0.0418 |
| **Personal Service** | 0.0357 | **South East** | 0.1469 |
| **Sales and Customer Service** | 0.0325 | **South West** | 0.0888 |
| **Operatives** | 0.1298 | **Scotland** | 0.0902 |
| **Degree** | 0.2200 | **West Midlands** | 0.0880 |
| **Higher education** | 0.0986 | **Wales** | 0.0447 |
| **GCE** | 0.3021 | **Yorkshire & Humberside** | 0.0901 |
| **GCSE** | 0.1657 | **famnum** | 1.06 |
| **Summer** | 0.2503 | **samesexhh** | 0.1161 |
| **Autumn** | 0.2531 | **hpinsareal** | 1485.3 |
| **Winter** | 0.2414 | | |

**Table 10**
Marginal Effects of Housing Tenure, Region of South East

| | Logit | | | endogenous Logit | | |
|---|---|---|---|---|---|---|
| | *dy/dx* | *std. error* | *P-value* | *dy/dx* | *std. error* | *P-value* |
| | *age 16-34 - GCE - Managers/Senior Off.* | | | | | |
| **outright owner** | -2.51** | *0.0015* | 0.000 | -0.68** | *0.0024* | 0.004 |
| **mortgager** | -4.1** | *0.0019* | 0.000 | -0.79** | *0.0029* | 0.007 |
| **social renter** | 5.5** | *0.0028* | 0.000 | 19.28** | *0.0248* | 0.000 |
| | *age 16-34 - GCE - Professional* | | | | | |
| **outright owner** | -1.98** | *0.0013* | 0.000 | -0.38* | *0.0015* | 0.011 |
| **mortgager** | -3.23** | *0.0017* | 0.000 | -0.44* | *0.0018* | 0.017 |
| **social renter** | 4.44** | *0.0025* | 0.000 | 11.77** | *0.0200* | 0.000 |
| | *age 16-34 - Degree - Managers/Senior Off.* | | | | | |
| **outright owner** | -2.28** | *0.0014* | 0.000 | -0.56** | *0.0020* | 0.005 |
| **mortgager** | -3.73** | *0.0018* | 0.000 | -0.65** | *0.0025* | 0.009 |
| **social renter** | 5.05** | *0.0027* | 0.000 | 16.47** | *0.0234* | 0.000 |
| | *age 16-34 - Degree - Professional* | | | | | |
| **outright owner** | -1.8** | *0.0011* | 0.000 | -0.31* | *0.0013* | 0.014 |
| **mortgager** | -2.93** | *0.0014* | 0.000 | -0.36* | *0.0016* | 0.020 |
| **social renter** | 4.06** | *0.0023* | 0.000 | 9.9** | *0.0178* | 0.000 |
| | *age 45-54- GCE - Managers/Senior Off.* | | | | | |
| **outright owner** | -3.05** | *0.0019* | 0.000 | -1.38** | *0.0040* | 0.001 |
| **mortgager** | -5.02** | *0.0023* | 0.000 | -1.6** | *0.0049* | 0.001 |
| **social renter** | 6.58** | *0.0032* | 0.000 | 32.26** | *0.0312* | 0.000 |
| | *age 45-54 - GCE - Professional* | | | | | |
| **outright owner** | -2.43** | *0.0016* | 0.000 | -0.77** | *0.0026* | 0.003 |
| **mortgager** | -3.97** | *0.0021* | 0.000 | -0.89** | *0.0032* | 0.005 |
| **social renter** | 5.35** | *0.0029* | 0.000 | 21.19** | *0.0266* | 0.000 |
| | *age 45-54 - Degree - Managers/Senior Off.* | | | | | |
| **outright owner** | -2.79** | *0.0018* | 0.000 | -1.14** | *0.0035* | 0.001 |
| **mortgager** | -4.57** | *0.0022* | 0.000 | -1.32** | *0.0043* | 0.002 |
| **social renter** | 6.06** | *0.0032* | 0.000 | 28.31** | *0.0302* | 0.000 |
| | *age 45-54 - Degree - Professional* | | | | | |
| **outright owner** | -2.21** | *0.0014* | 0.000 | -0.63** | *0.0022* | 0.004 |
| **mortgager** | -3.61** | *0.0018* | 0.000 | -0.73** | *0.0027* | 0.007 |
| **social renter** | 4.91** | *0.0027* | 0.000 | 18.17** | *0.0242* | 0.000 |

Notes:

1. * significant at 5%; ** significant at 1%. Reported marginal effects are multiplied by 100.
2. Statistics are from the models (3) and (4) of Table 7. Marginal effects are computed for eight different sets of values for regressors chosen discretionally. Age, education and occupation can take on two different values while the other covariates are held fixed across sets of values. Fixed values are: non married, no disability benefits, Winter, 2005 and South East as Region.

**Table 11**
MARGINAL EFFECTS OF HOUSING TENURE, REGION OF LONDON

| | Logit | | | endogenous Logit | | |
|---|---|---|---|---|---|---|
| | *dy/dx* | *std. error* | *P-value* | *dy/dx* | *std. error* | *P-value* |
| | *age 16-34 - GCE - Managers/Senior Off.* | | | | | |
| **outright owner** | -3.25** | *0.0018* | 0.000 | -0.85** | *0.0028* | 0.003 |
| **mortgager** | -5.35** | *0.0022* | 0.000 | -0.98** | *0.0035* | 0.005 |
| **social renter** | 6.95** | *0.0033* | 0.000 | 22.78** | *0.0265* | 0.000 |
| | *age 16-34 - GCE - Professional* | | | | | |
| **outright owner** | -2.59** | *0.0016* | 0.000 | -0.47** | *0.0018* | 0.009 |
| **mortgager** | -4.24** | *0.0020* | 0.000 | -0.54* | *0.0022* | 0.014 |
| **social renter** | 5.67** | *0.0030* | 0.000 | 14.17** | *0.0222* | 0.000 |
| | *age 16-34 - Degree - Managers/Senior Off.* | | | | | |
| **outright owner** | -2.97** | *0.0017* | 0.000 | -0.7** | *0.0024* | 0.004 |
| **mortgager** | -4.87** | *0.0020* | 0.000 | -0.81** | *0.0030* | 0.007 |
| **social renter** | 6.41** | *0.0032* | 0.000 | 19.59** | *0.0251* | 0.000 |
| | *age 16-34 - Degree - Professional* | | | | | |
| **outright owner** | -2.36** | *0.0013* | 0.000 | -0.39* | *0.0015* | 0.011 |
| **mortgager** | -3.86** | *0.0016* | 0.000 | -0.45* | *0.0019* | 0.017 |
| **social renter** | 5.21** | *0.0026* | 0.000 | 11.98** | *0.0198* | 0.000 |
| | *age 45-54- GCE - Managers/Senior Off.* | | | | | |
| **outright owner** | -3.93** | *0.0024* | 0.000 | -1.7** | *0.0047* | 0.000 |
| **mortgager** | -6.5** | *0.0028* | 0.000 | -1.97** | *0.0059* | 0.001 |
| **social renter** | 8.21** | *0.0037* | 0.000 | 36.86** | *0.0326* | 0.000 |
| | *age 45-54 - GCE - Professional* | | | | | |
| **outright owner** | -3.16** | *0.0020* | 0.000 | -0.95** | *0.0031* | 0.002 |
| **mortgager** | -5.19** | *0.0025* | 0.000 | -1.1** | *0.0038* | 0.004 |
| **social renter** | 6.77** | *0.0034* | 0.000 | 24.91** | *0.0286* | 0.000 |
| | *age 45-54 - Degree - Managers/Senior Off.* | | | | | |
| **outright owner** | -3.6** | *0.0022* | 0.000 | -1.41** | *0.0041* | 0.001 |
| **mortgager** | -5.94** | *0.0026* | 0.000 | -1.63** | *0.0051* | 0.001 |
| **social renter** | 7.61** | *0.0036* | 0.000 | 32.67** | *0.0314* | 0.000 |
| | *age 45-54 - Degree - Professional* | | | | | |
| **outright owner** | -2.88** | *0.0018* | 0.000 | -0.79** | *0.0027* | 0.003 |
| **mortgager** | -4.72** | *0.0021* | 0.000 | -0.91** | *0.0033* | 0.005 |
| **social renter** | 6.24** | *0.0031* | 0.000 | 21.52** | *0.0259* | 0.000 |

Notes:

1. * significant at 5%; ** significant at 1%. Reported marginal effects are multiplied by 100.
2. Statistics are from the models (3) and (4) of Table 7. Marginal effects are computed for eight different sets of values for regressors chosen discretionally. Age, education and occupation can take on two different values while the other covariates are held fixed across sets of values. Fixed values are: non married, no disability benefits, Winter, 2005 and London as Region.

**Table 12**

PROBABILITY OF EXITING UNEMPLOYMENT: SPELLS ENDING IN EMPLOYMENT OR CENSORED

| | (1) Hazard with frailty | | (2) Hazard without frailty |
|---|---|---|---|
| | *dy/dx* | *hazard ratio* | *hazard ratio* |
| **outright owner** | 2.69** | 1.552** | 1.584** |
| **mortgager** | 4.51** | 2.045** | 2.057** |
| **social** | -0.31 | 0.942 | 0.964 |
| **claimant** | -1.89** | 0.705** | 0.712** |
| **disability benf.** | -2.70** | 0.506** | 0.524** |
| **married** | 1.22** | 1.257** | 1.247** |
| *baseline hazard dummies* | | | |
| $3-6$ **months** | -1.50** | 0.726** | 0.716** |
| $6-12$ **months** | -3.77** | 0.394** | 0.394** |
| $1-2$ **years** | -5.96** | 0.199** | 0.196** |
| $2-3$ **years** | -5.87** | 0.112** | 0.113** |
| $3-4$ **years** | -6.00** | 0.052** | 0.053** |
| $4-5$ **years** | -5.23** | 0.102** | 0.103** |
| $5-$ **over years** | -4.45** | 0.196** | 0.200** |
| *age dummies* | | | |
| **25-34** | -0.32 | 0.940 | 0.957 |
| **35-49** | -1.42** | 0.757** | 0.783** |
| **50-64** | -3.61** | 0.450** | 0.471** |
| *last occupation type* | | | |
| **Managers/Senior Off.** | 1.33** | 1.263** | 1.258** |
| **Professional** | 0.48 | 1.094 | 1.078 |
| **Assoc. Prof./Technical** | 0.77 | 1.151 | 1.110 |
| **Admin./Secretarial** | 1.66* | 1.327** | 1.327** |
| **Skilled Trades** | 0.95* | 1.190* | 1.187* |
| **Personal Services** | 0.46 | 1.089 | 1.083 |
| **Sales** | 0.32 | 1.063 | 1.122 |
| **Operatives** | 1.24** | 1.249** | 1.237** |
| *education (highest)* | | | |
| **Degree** | 1.49** | 1.297** | 1.310** |
| **Higher** | 0.54 | 1.104 | 1.131 |
| **GCE** | 1.76** | 1.366** | 1.373** |
| **GCSE** | 0.66* | 1.130* | 1.153* |
| seasonal dummies | ✓ | | ✓ |
| yearly dummies | ✓ | | ✓ |
| regional dummies | ✓ | | ✓ |
| number of observations | 31008 | | 31,008 |
| $\rho$ | | | 0.00003 |

Notes:

1. * significant at 5%; ** significant at 1%. See the Appendix for the base categories of discrete regressors.
2. Column (1) reports estimates (marginal effects evaluated at means and hazard ratios) of the clog-log model. Column (2) reports estimates (hazard ratios) of the random effects clog-log model. The dependent variable is a dummy for the failure event: it takes 1 if the spell ends in employment and zero if right censored. Spells ending in inactivity are dropped. Sampling weights are used in estimations only for model (1).
3. See note 1 to Table 1 for sample restrictions. Covariates are time-constant and refers to the last quarter of the unemployment spell. Also people who have a different housing tenure either in previous or in the following quarter are excluded. The $\rho$ statistic is a test for the presence of frailty.

**Table 13**

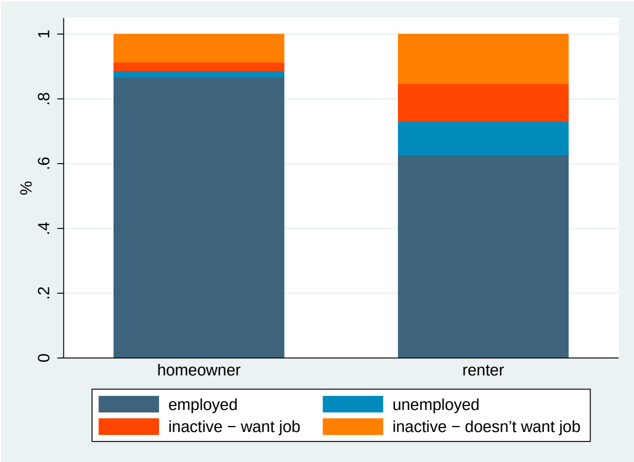
PROBABILITY OF EXITING UNEMPLOYMENT: SPELLS ENDING IN INACTIVITY OR CENSORED

| | (1) Hazard with frailty | | (2) Hazard without frailty |
|---|---|---|---|
| | *dy/dx* | *hazard ratio* | *hazard ratio* |
| **outright owner** | 1.98** | 1.755** | 1.776** |
| **mortgager** | 0.96** | 1.359** | 1.365** |
| **social** | 0.84** | 1.335** | 1.349** |
| **claimant** | -2.01** | 0.517** | 0.518** |
| **disability benf.** | 1.58** | 1.579** | 1.645** |
| **married** | 0.61** | 1.228** | 1.221** |
| *baseline hazard dummies* | | | |
| $3-6$ **months** | -0.68** | 0.769** | 0.762** |
| $6-12$ **months** | -1.36** | 0.573** | 0.552** |
| $1-2$ **years** | -2.25** | 0.375** | 0.367** |
| $2-3$ **years** | -2.28** | 0.311** | 0.310** |
| $3-4$ **years** | -2.28** | 0.281** | 0.274** |
| $4-5$ **years** | -1.98** | 0.348** | 0.329** |
| $5-$ **over years** | -0.40 | 0.860 | 0.854 |
| *age dummies* | | | |
| **25-34** | 0.08 | 1.030 | 1.030 |
| **35-49** | -0.13 | 0.956 | 0.959 |
| **50-64** | 0.06 | 1.023 | 1.026 |
| *last occupation type* | | | |
| **Managers/Senior Off.** | 0.96* | 1.349* | 1.347* |
| **Professional** | 0.51 | 1.180 | 1.159 |
| **Assoc. Prof./Technical** | 0.49 | 1.173 | 1.108 |
| **Admin./Secretarial** | 1.15* | 1.413* | 1.328 |
| **Skilled Trades** | 0.65 | 1.239 | 1.236* |
| **Personal Services** | 1.42* | 1.512* | 1.460* |
| **Sales** | 0.41 | 1.144 | 1.142 |
| **Operatives** | 0.72* | 1.264* | 1.243* |
| *education (highest)* | | | |
| **Degree** | -0.01 | 0.998 | 1.038 |
| **Higher** | -0.10 | 0.964 | 1.001 |
| **GCE** | 0.05 | 1.016 | 1.050 |
| **GCSE** | -0.05 | 0.982 | 1.002 |
| seasonal dummies | ✓ | | ✓ |
| yearly dummies | ✓ | | ✓ |
| regional dummies | ✓ | | ✓ |
| number of observations | 29,041 | | 29,041 |
| $\rho$ | | | 0.00001 |

Notes:

1. * significant at 5%; ** significant at 1%. See the Appendix for the base categories of discrete regressors.
2. Column (1) reports estimates (marginal effects evaluated at means and hazard ratios) of the clog-log model. Column (2) reports estimates (hazard ratios) of the random effects clog-log model. The dependent variable is a dummy for the failure event: it takes 1 if the spell ends in inactivity and zero if right censored. Spells ending in employment are dropped. Sampling weights are used in estimations only for model (1).
3. See note 1 to Table 1 for sample restrictions. Covariates are time-constant and refers to the last quarter of the unemployment spell. Also people who have a different housing tenure either in previous or in the following quarter are excluded. The $\rho$ statistic is a test for the presence of frailty.

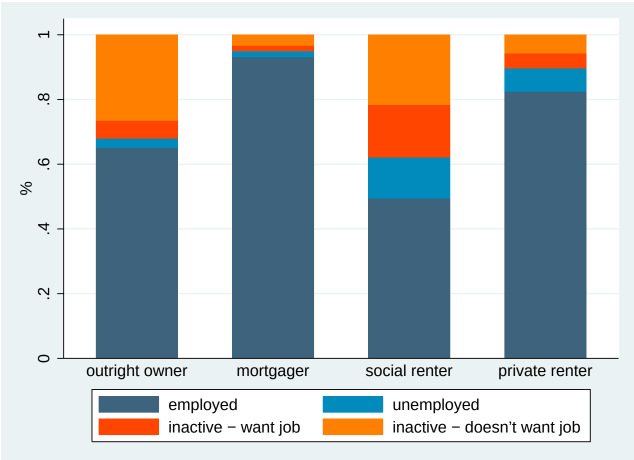
**Figure 1**
Labour Market Status by Housing Tenure




Notes:

1. The sample is made of respondent male head of households in working age. Observations are quarterly from Spring 1999 (March to May) to Winter (December to February) 2005. A small number of observations is dropped regarding people who have never had paid job, or get retirement or old age pension, or are in full-time education or occupy the household rent-free.

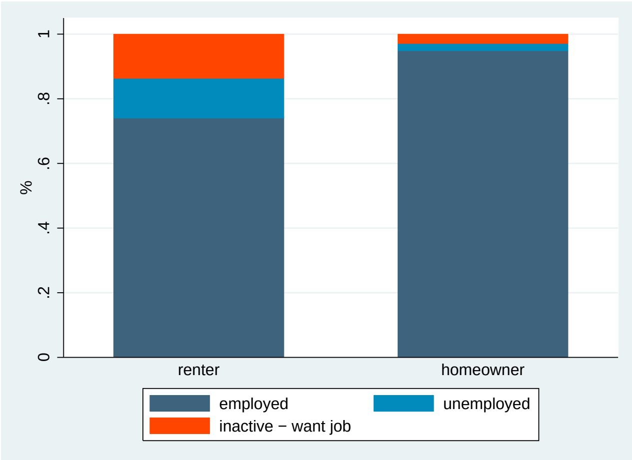
**Figure 2**
Labour Market Status by Housing Tenure




Notes:

1. See note 1 to Fig 1.

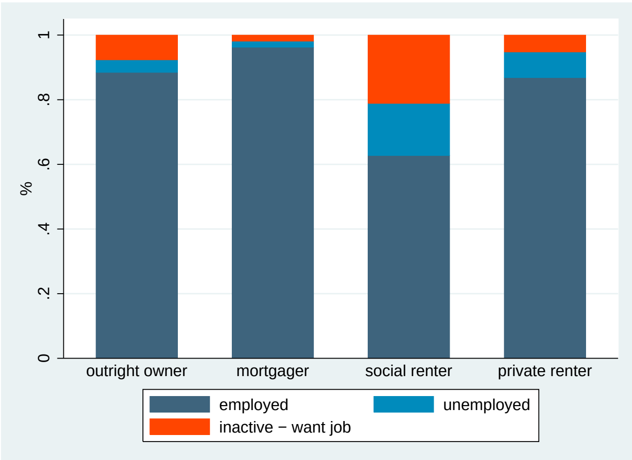
**Figure 3**
Labour Market Status by Housing Tenure - Employed, Unemployed, Inactive want job




Notes:

1. The sample is made of respondent male head of households in working age. Observations are quarterly from Spring 1999 (March to May) to Winter (December to February) 2005. A small number of observations is dropped regarding people who have never had paid job, or get retirement or old age pension, or are in full-time education or occupy the household rent-free.

**Figure 4**
Labour Market Status by Housing Tenure - Employed, Unemployed, Inactive want job

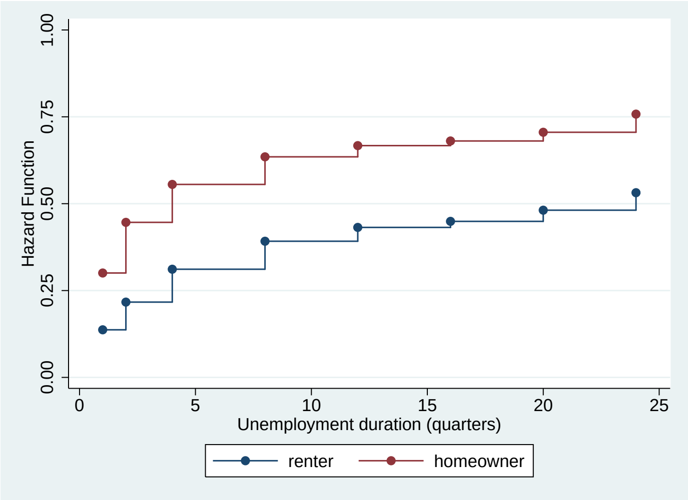



Notes:

1. See note 1 to Fig 3.

**Figure 5**

Cumulative Hazard to employment: Kaplan-Meier estimate

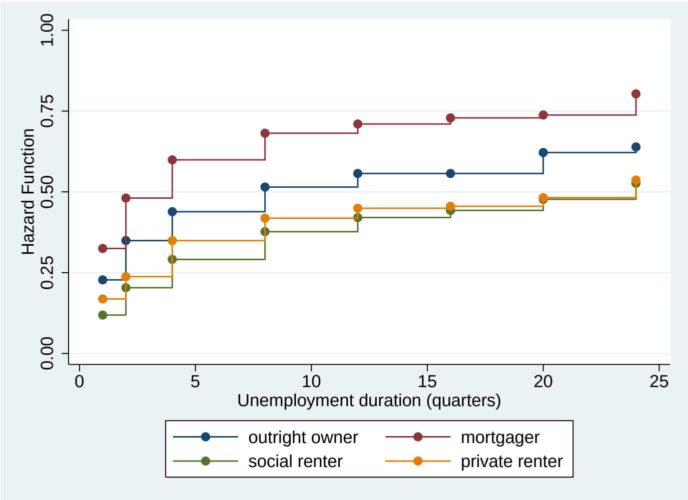



Notes:

1. Non parametric estimate of the cumulative hazard function for exits to employment.

**Figure 6**

Cumulative Hazard to employment: Kaplan-Meier estimate

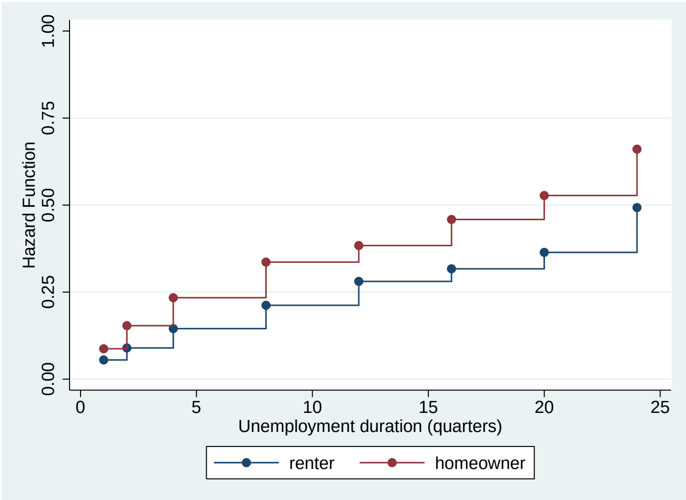



Notes:

1. Non parametric estimate of the cumulative hazard function for exits to employment.

**Figure 7**

Cumulative Hazard to inactivity: Kaplan-Meier estimate

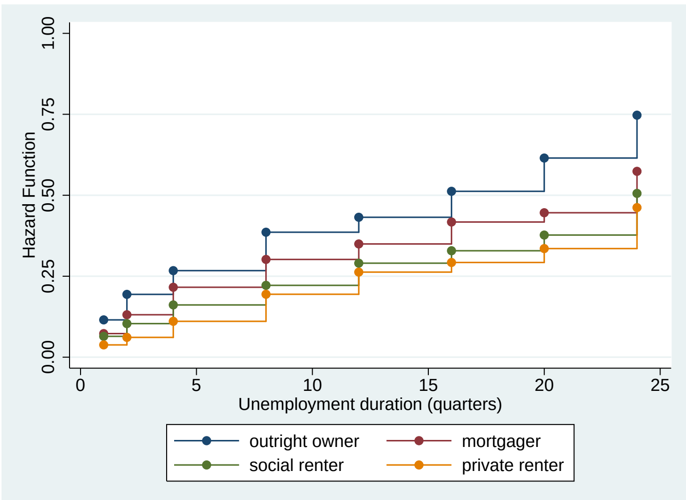



Notes:

1. Non parametric estimate of the cumulative hazard function for exits to inactivity.

**Figure 8**

Cumulative Hazard to inactivity: Kaplan-Meier estimate

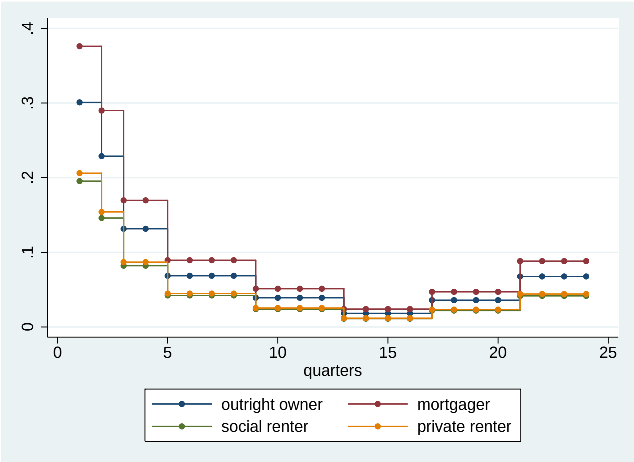



Notes:

1. Non parametric estimate of the cumulative hazard function for exits to inactivity.

**Figure 9**

Hazard to employment. Out-of-sample prediction: non-claimants

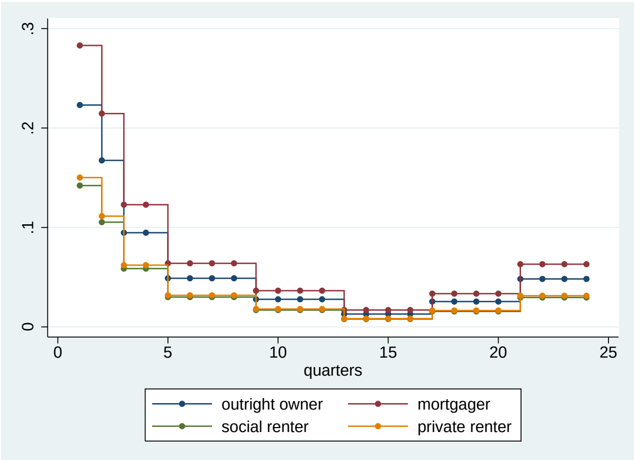



Notes:

1. Predicted hazards have been estimated after the clog-log non-frailty model (Table 12, column 1) attributing specific values to the covariates. Results must be interpreted for a representative head of household unemployed with these features: non claimant, aged 25-49, non married, not getting disability benefits, professional occupations, GCSE qualification, resident in Inner London, in Summer, in 2005.

**Figure 10**

Hazard to employment. Out-of-sample prediction: claimants

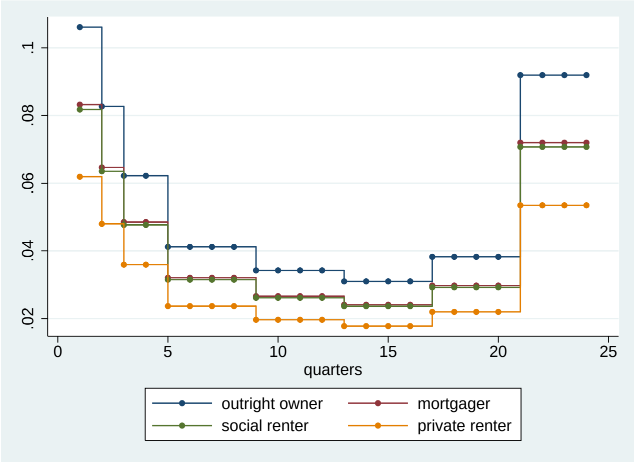



Notes:

1. Predicted hazards have been estimated after the clog-log non-frailty model (Table 12, column 1) attributing specific values to the covariates. Results must be interpreted for a representative head of household unemployed with these features: claimant, aged 25-49, non married, not getting disability benefits, professional occupations, GCSE qualification, resident in Inner London, in Summer, in 2005.

**Figure 11**

Hazard to inactivity. Out-of-sample prediction: non-claimants

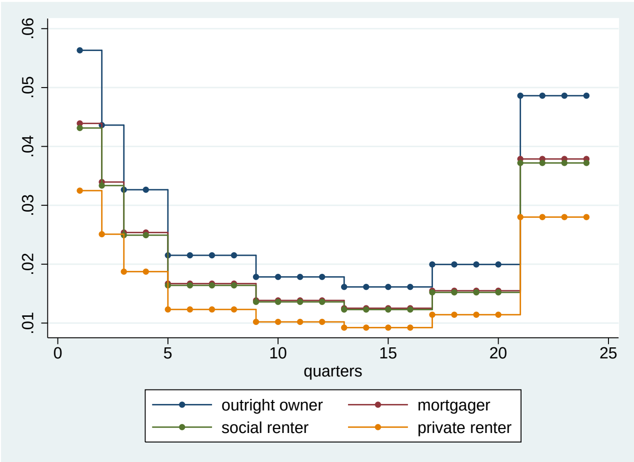



Notes:

1. Predicted hazards have been estimated after the clog-log non-frailty model (Table 13, column 1) attributing specific values to the covariates. Results must be interpreted for a representative head of household unemployed with these features: non claimant, aged 25-49, non married, not getting disability benefits, professional occupations, GCSE qualification, resident in Inner London, in Summer, in 2005.

**Figure 12**

Hazard to inactivity. Out-of-sample prediction: claimants




Notes:

1. Predicted hazards have been estimated after the clog-log non-frailty model (Table 13, column 1) attributing specific values to the covariates. Results must be interpreted for a representative head of household unemployed with these features: claimant, aged 25-49, non married, not getting disability benefits, professional occupations, GCSE qualification, resident in Inner London, in Summer, in 2005.

# Appendix

## A The Endogenous Multinomial Treatment Effect Model

We give a formal representation of the model for the non-employment binary outcome described in the methodological section and whose estimates are reported in Table 7, column (4) (see Deb and Trivedi [2006a] and Deb and Trivedi [2006b]).

Each individual $i$ chooses a residential status $j$ from a set of four choices ($j = 0, 1, 2, 3$), where $j = 0$ is the control group (private renters). Let $EV^*_{ij}$ denotes the utility associated with the $j$-th residential status and

$$EV^*_{ij} = \mathbf{z}'_i \alpha_j + \delta_j l_{ij} + \eta_{ij}, \tag{A1}$$

where $\mathbf{z}_i$ denotes a set of exogenous covariates with parameters $\alpha_j$, $\eta_{ij}$ are $i.i.d$ error terms, and $l_{ij}$ are latent factors which incorporate unobserved characteristics common to the individual $i$'s status choice and outcome. The $l_{ij}$ are assumed to be independent of $\eta_{ij}$. As a normalization $EV^*_{i0} = 0$, so the expected utility of $j$-th status is the differential utility relative to private renters.

Let $d_j$ be binary selection variables representing the observed tenure choice and $\mathbf{d}_i = (d_{i1}, d_{i2}, d_{i3})$. Also let $\mathbf{l}_i = (l_{i1}, l_{i2}, l_{i3})$. Then the mixed multinomial logit structure for the probability of tenure choice can be represented as

$$P(\mathbf{d}_i | \mathbf{z}_i, \mathbf{l}_i) = \frac{\exp(\mathbf{z}'_i \alpha_j + \delta_j l_{ij})}{1 + \sum_{k=1}^{J} \exp(\mathbf{z}'_i \alpha_k + \delta_k l_{ik})}. \tag{A2}$$

Estimates of this model are reported in appendix in Table C1.

The expected binary outcome equation for individual $i$ is formulated as

$$E(y_i) = \mu(\mathbf{x}'_i \beta + \sum_{j=1}^{3} \gamma_j d_{ij} + \sum_{j=1}^{3} \lambda_j l_{ij}), \tag{A3}$$

where $\mathbf{x}_i$ is a set of exogenous variables and $\gamma_j$ denote the treatment effects relative to private renters. The expected probability to be non-employed is a function of the latent factors $l_{ij}$ so that it is affected by unobserved characteristics which affect the selection into housing tenure as well. The function $\mu$ is assumed to have a logit form: $\exp(\cdot)/(1 + \exp(\cdot))$. The interpretation of the factor-loading parameters $\lambda_j$ is the following: when $\lambda_j$ is positive (negative), unobserved factors which increase the probability of selecting $j$-th residential status also increase (reduce) the probability of being non-employed.

In order to estimate parameters of the model, latent factors are assumed to be *i.i.d* draws from the standard normal distribution and simulation-based method are used to maximize the log likelihood. Provided the number of draws is sufficiently large (we select 1,200 draws), maximization of the simulated log likelihood is equivalent to maximizing the log likelihood. Parameters of this model are identified when $\mathbf{z}_i = \mathbf{x}_i$, but Deb and Trivedi recommend including some variables in $\mathbf{z}_i$ which are not included in $\mathbf{x}_i$.

In the text we report estimates of the marginal effects for the housing tenure dummies. Marginal effect of the $s$-th treatment relative to the base category is the difference in the probability of non-employment between individuals in the two statuses. Formally

$$E(y|d_s = 1) - E(y|\mathbf{d} = 0) = \mu(\mathbf{x}'\beta + \gamma_s + \sum_{j=1}^{3} \lambda_j l_j) - \mu(\mathbf{x}'\beta + \sum_{j=1}^{3} \lambda_j l_j). \quad \text{(A4)}$$

Once $\beta$, $\gamma_s$ and $\lambda_j$-s are estimated, point estimates of this difference can be calculated replacing $\mathbf{x}$ and $l_j$-s with appropriate values. In the Tables we report point estimates when $l_j$-s are zero (their expected value) and $\mathbf{x} = \bar{\mathbf{x}}$, where $\bar{\mathbf{x}}$ contains either sample means or representative values of regressors. So we compute

$$\mu(\bar{\mathbf{x}}'\hat{\beta} + \hat{\gamma}_s) - \mu(\bar{\mathbf{x}}'\hat{\beta}), \quad \text{(A5)}$$

which clearly has the same sign of $\hat{\gamma}_s$.

# B Description of variables

**Housing Tenure dummies**

Housing tenure related questions refer to the household. Then the outcome of the household is imputed to all individuals belonging to it at the date of interview.

**homeowner**: selects all individuals whose household owns the accommodation, either outright or with mortgage.

**outright owner**: accommodation outright owned.

**mortgager**: accommodation owned with mortgage.

**social renter**: accommodation rented from Local Authorities or Housing Associations.

**private renter**: accommodation rented from private.

**Unemployment duration**

The variable is derived from the LFS `durun` variable which reports the minimum of the length of time looking for work and the length of time since the respondent's last job. The LFS variable groups durations in 8 time intervals: 0-3 months, 3-6 months, 6-12 months, 1-2 years, 2-3 years, 3-4 years, 4-5 years, 5 years or more.

**Claimant**

This is a dummy for people claiming unemployment-related benefits. On the 7th October 1996 it was introduced the Job Seeker's Allowance who replaced the old unemployment benefit system. With JSA unemployed can claim both cont-JSA, which replaced the old contribution-based Unemployment Benefit (UB), and inc-JSA, which replaced the old retributive element, *i.e.* Income Support for unemployed. The dummy selects all individuals claiming contributory JSA, or income based JSA (or both), or national insurance credits.

**Disability Benefits**

This is a dummy which selects people getting disability or sickness benefits.

**Marriage Status**

People are grouped in legally married (not separated) versus non married. Among currently married we distinguish according to the spouse being in employment or not. Sometimes in the analysis we do not make this distinction since it does not seem to matter. The base category are non married.

**Age**

The sample is made of male in working age (16-64). The base age range in the regressions is always the youngest.

**Last Occupation Type**

Employed workers are grouped according to the current job occupational category, while non-employed workers according to the last job. People who have never had paid job are dropped. Occupational categories are: (1) Managers and Senior Officials; (2) Professional Occupations; (3) Associate Professional and Technical; (4) Administrative and Secretarial; (5) Skilled Trades Occupations; (6) Personal Service

Occupations; (7) Sales and Customer Service Occupations; (8) Process, Plant and Machine Operatives; (9) Elementary Occupations. The default is Elementary Occupations.

**Education**

These are 5 levels of highest qualification attained: (1) Degree or Equivalent; (2) Higher Education; (3) GCE A level or equivalent; (4) GCSE grades A*-C or equivalent; (5) other or no qualification. The base category is the last.

**Seasonal dummies**

These are quarterly dummies for seasons: Spring (March-May), Summer (June-August), Autumn (September-November), Winter (December-February).

**Yearly dummies**

Yearly dummies for 1999, 2000, 2001, 2002, 2003, 2004, 2005.

**Regional dummies**

For the binary outcome models we use this classification: (1) East Anglia; (2) East Midlands; (3) London (4) Northern Ireland (5) North West; (6) North; (7) South East (8) South West; (9) Scotland; (10) West Midlands; (11) Wales; (12) Yorkshire & Humberside. The base category is Northern Ireland.

For the duration analysis we use a deeper classification: (1) Tyne & Wear; (2) Rest of North East; (3) Greater Manchester; (4) Merseyside; (5) Rest of North West; (6) South Yorkshire; (7) West Yorkshire (8) Rest of Yorkshire & Humberside; (9) East Midlands; (10) West Midlands Metropolitan County; (11) Rest of West Midlands; (12) East of England; (13) Inner London; (14) Outer london; (15) South East; (16) South West; (17) Wales; (18) Strathclyde; (19) Rest of Scotland (20) Northern Ireland. The base category is Northern Ireland.

**Famnum**

This variable records the number of family units within a household. According to the LFS definition a "family unit comprises either a single person, or a married or cohabiting couple on their own, or with their never-married children who have no children of their own, or lone parents with such children".

**Samesexhh**

This is a dummy which takes one for households in which the first two children born are same sex.

**Hpinsareal**

This is a quarter-varying region-varying aggregate index for (non seasonally adjusted) real house prices derived from the Halifax House Price Index (HPI). The Halifax HPI is the UK's longest running monthly house price series covering the whole country from January 1983. The Index is derived from mortgage data relative to transactions financed by the Halifax Bank itself, which represents the country's largest mortgage lender and provides a fairly representative sample of the entire UK market (see Halifax [2010] for the methodology and access to data). Regional indices for the 12 standard planning regions of the UK are produced on a quarterly basis. The index groups Regions in this way: (1) East Anglia; (2) East Midlands; (3) Greater London; (4) North Ireland; (5) North West; (6) North; (7) South East; (8) South West; (9) Scotland; (10) West Midlands; (11) Wales; (12) Yorks & Humberside.

We select the non seasonally adjusted index covering all houses and all buyers. The index is then deflated using a quarterly Retail Price Index and expressed in terms of purchasing power of the 4th quarter of 2010[32]. Since the index is produced on a calendar quarter basis and we use seasonal quarters we match each individual (*i.e.* each Region) with the appropriate quarter index observation using the information on the date of interview provided by the LFS.

# C Housing Tenure choice models

[32] We use as RPI the CBZW series provided by the Office for National Statistics (ONS) and available online at `http://www.statistics.gov.uk/cci/nugget.asp?id=21`

**Table C1**
Housing Tenure Choice Model. Mixed Multinomial Logit

| | OUTRIGHT | | MORTGAGER | | SOCIAL R. | |
|---|---|---|---|---|---|---|
| | *RRR* | *p-value* | *RRR* | *p-value* | *RRR* | *p-value* |
| **famnum** | 0.541** | *0.000* | 0.419** | *0.000* | 0.34** | *0.000* |
| **samesexhh** | 1.132** | *0.000* | 1.672** | *0.000* | 2.311** | *0.000* |
| **hpinsareal** | 1.000* | *0.016* | 1.000** | *0.000* | 1.000 | *0.436* |
| **disability benf.** | 0.749** | *0.000* | 0.482** | *0.000* | 3.001** | *0.000* |
| *marriage status* | | | | | | |
| **married, sps. in emp.** | 3.314** | *0.000* | 5.532** | *0.000* | 0.923** | *0.000* |
| **married, sps. no emp.** | 2.621** | *0.000* | 1.872** | *0.000* | 1.437** | *0.000* |
| *age dummies* | | | | | | |
| **age 35-44** | 4.577** | *0.000* | 2.353** | *0.000* | 1.848** | *0.000* |
| **age 45-54** | 19.837** | *0.000* | 3.056** | *0.000* | 2.332** | *0.000* |
| **age 55-64** | 94.392** | *0.000* | 2.150** | *0.000* | 2.507** | *0.000* |
| *last occupation type* | | | | | | |
| **Managers/Senior Off.** | 1.834** | *0.000* | 2.776** | *0.000* | 0.202** | *0.000* |
| **Professional** | 1.470** | *0.000* | 2.265** | *0.000* | 0.161** | *0.000* |
| **Assoc. Prof./Tech.** | 1.257** | *0.000* | 1.948** | *0.000* | 0.214** | *0.000* |
| **Admin./Secretarial** | 1.498** | *0.000* | 1.837** | *0.000* | 0.471** | *0.000* |
| **Skilled Trades** | 1.917** | *0.000* | 2.177** | *0.000* | 0.681** | *0.000* |
| **Personal Service** | 0.760** | *0.000* | 1.027 | *0.483* | 0.578** | *0.000* |
| **Sales** | 1.129* | *0.018* | 1.282** | *0.000* | 0.474** | *0.000* |
| **Operatives** | 1.479** | *0.000* | 2.106** | *0.000* | 0.942* | *0.048* |
| *education (highest)* | | | | | | |
| **Degree** | 2.330** | *0.000* | 1.936** | *0.000* | 0.294** | *0.000* |
| **Higher educ.** | 2.149** | *0.000* | 2.372** | *0.000* | 0.500** | *0.000* |
| **GCE** | 1.964** | *0.000* | 2.221** | *0.000* | 0.679** | *0.000* |
| **GCSE** | 1.750** | *0.000* | 2.078** | *0.000* | 0.891** | *0.000* |
| seasonal dummies | ✓ | | | | | |
| yearly dummies | ✓ | | | | | |
| regional dummies | ✓ | | | | | |
| number of observations | 382,778 | | | | | |

Notes:

1. * significant at 5%; ** significant at 1%.
2. The Table reports relative risk ratios (*RRR*) from the mixed multinomial logit estimated jointly with the binary non-employment equation. Results for the latter are reported in column (3) of Table 7. Notes to that Table apply here.
3. Coefficients must be read in relation to the base category, *i.e.* private rented dwelling. Given the variable $x$ and the residential status $j$ ($j \in \{OUT, MORT, SOC, PRI\}$), where $P_0(y = j|X)$ and $P_1(y = j|X)$ are the probabilities of selecting the $j$-th status respectively when $x$ is equal to a given value and $x$ increments marginally (or shifts from 0 to 1 for dummies), the $RRR$ is defined as $\frac{P_1(y=j|X)}{P_1(y=PRI|X)} / \frac{P_0(y=j|X)}{P_0(y=PRI|X)}$. For the multinomial logit it can be easily showed that the $RRR$ does not depend on $x$. In fact when $y$ is just dichotomous the $RRR$ collapses to the odds ratio. For example, for a one unit increase of a variable (or a shift from 0 to 1 for dummies) in the first column, the risk of being outright owner relative to private renter is $RRR$ times more likely if $RRR > 1$, or $1 - RRR$ times less likely if $RRR < 1$.