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Housing Tenure and Labour Market Outcomes. An Investigation of the Oswald's Hypothesis

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Introduction

Housing tenure structures differ considerably across OECD countries. Typically, the share of owner-occupied housing is very high in Southern European countries (Spain, Italy, Greece), while relatively low in Austria, Germany, France, the Netherlands and in some Nordic countries such as Sweden, Finland and Denmark (see Catte et al. [2004]). This share has increased in most OECD countries during last two decades. Italy, Spain, Portugal, the Netherlands and Belgium registered the largest increases from the eighties to the mid of the first decade of the century. In the UK, the homeonwership rate increased steadily up to 2005, then dropped sharply in the last few years (see figure 1).

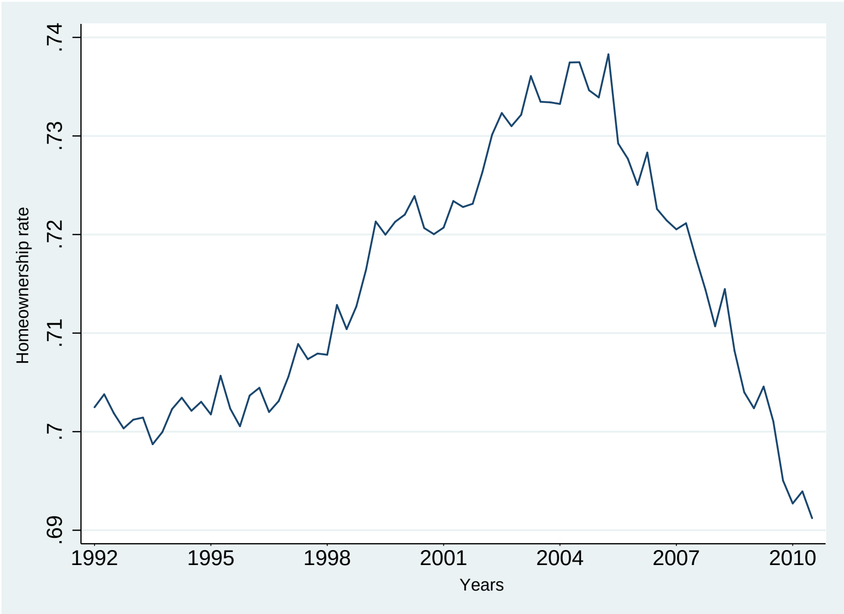
High homeownership rates and their increments have been in part determined by pro-ownership policies implemented throughout most OECD countries. Such policies can take the vest of tax reliefs for mortgage interest payments, exemptions for capital gains taxation applied to residential property, or generous subsidies for low-income families to reduce the costs of homeownership.

Although the orientation of several governments, particularly in Europe, has been to promote homeownership, economists have raised several concerns about the consequences of large homeownership shares on the functioning of the labour market. The analysis of the relation between the housing tenure and the labour market dates back to mid nineties, when Andrew Oswald pinned on high homeownership rates the blame for the high unemployment rates in Europe (Oswald [1996], Oswald [1997], Oswald [1999]). Making use of aggregate data, Oswald found a strong positive statistical association between the two rates at the international and regional level. Thus, his receipt to reduce unemployment was strikingly at odds with the prevailing political wisdom: "We can put Europe back to work ... by reducing homeownership" (Oswald [1999])[1].

Oswald identified five mechanisms that drive the positive impact on unem-

[1]Nickell [1998] and Nickell and Layard [1999] also found a positive and significant impact of the homeownership rate on unemployment rate for a panel of OECD countries.

Figure 1
The Evolution of Homeownership Rates in the UK (1992-2010)



Notes:

1. Data from the UK Quarterly Labour Force Survey. Calendar quarters are used from August-June 1992 to January-March 2010.

2. Rates are computed at the person level. The series is not seasonally adjusted.

ployment. These are mostly related to the idea that the mobility constraints imposed by homeownership introduce frictions in the labour market which harm its efficiency. The first and most important, is a direct effect. Owner-occupiers incur higher transaction costs for selling home and moving to a new accommodation, which can be even higher whenever capital gains for houses are taxed and there are no exemptions. The other four mechanisms can be thought as externalities of homeownership. Second, where the rental market is thin, everyone, regardless of the housing tenure, can have difficulties in finding the accommodation close to the job they aspire to, so that efficient job matchings are hindered. This is the case even for mobile people who would be happy to move for job reasons, such as young still living in the family-home in a kind of free-rent status. Third, owning the accommodation hampers also job-to-job changes and prolong poor job matchings hence harming the whole economic system efficiency. Fourth, owner-occupiers are more likely than renters to lobby for deterring entrepreneurs from investing in their residential area. Fifth, since owners commute much more than renters, and over longer distances, higher homeownership rates in the area lead to higher transport costs and possibly to congestion costs, which make getting to work more costly for everyone and act as a sort of unemployment benefits in increasing the relative attractiveness of not working.

Although the argument underlying a positive relationship between homeownership and unemployment originates from macroeconomic evidence, its theoretical foundations are mainly microeconomic and a vast part of the empirical research carried out subsequently has been based on micro data. The typical test of the so-called "Oswald thesis" consists in estimating the homeownership effect on the the duration of the unemployment spell or, less often, on the probability to be unemployed. The expected sign is positive, along the main idea that higher mobility costs due to property holding reduce the willingness to accept job offers which require a residential move. Fewer studies, and only recently, have focused on the employment duration, testing whether homeownership can reduce job finding rates also for people already employed.

Generally, empirical tests on micro data find no support for the Oswald hypothesis, and in most cases even that homeowners have shorter unemployment spells and lower likelihood to be unemployed, which are exactly counter-Oswald results. However, homeownership seems to increase job stability by hampering job-to-job changes for people already employed.

As regards unemployment outcomes, the existing literature has put forward two possible reasons for the falsification of the Oswald hypothesis. The first one looks at the different effect of mobility costs on job search behaviour in the local and in the non-local labour market. In fact, homeowners may have higher reservation wages for jobs which require a residential move (non-local market), but also lower reservation wages for jobs which do not (local market), so that job finding rates for the latter may be as high as to offset the lower rates for jobs in a distant area. Thus, whether the total job finding rate is lower for homeowners or for renters is just an empirical matter which depends on the magnitude of these two opposite effects.

The second explanation points out the need for a refinement in the definition of the residential status. On the one hand, one should distinguish between owners who have to comply with mortgage payments and outright owners, as housing financial commitments can bear higher pressure to return to work or to keep on with the current job. For mortgage-holders, these financial constraints can counteract the effect of the reduced mobility due to ownership. On the other hand, unemployment outcomes can be different also for private and social renters. In fact, below-market rent, long waiting lists, security of tenure and the restricted transferability within social housing, can harm the relative performance of social renters.

In this work, we aim at investigating the validity of the "Oswald thesis" taking into account the recent refinements by following both a theoretical and an empirical approach. The focus will be on UK micro data. In particular, we will make use of the two leading UK Surveys, the UK Quarterly Labour Force Survey and the British Household Panel Survey.

In the first chapter, we investigate the relation between job search effort of unemployed and housing tenure. We test this relation focusing on the impact of the UK Jobseeker's Allowance reform introduced in the UK in October 1996, whose main aspect was a strengthening of search requirements for eligibility to the unemployment benefit. We revisit a simple model of search in which we introduce moving costs and housing costs to capture the two channels through which the degree of attachment to the accommodation influences search behaviour. Our theory suggests that a tightening in job search requirements, as implied by this reform, raises movements off benefit of non-employed with low search intensity and that this effect should adjust in size depending on the different housing tenure. We draw a dataset from the Labour Force Survey for the period 1995-1997, and by means of a Difference-in-Differences approach we analyze the impact of the reform on the claimant outflow. Average Treatment Effect estimates suggest that the impact of the reform is related to housing tenure. Specifically, renters account for a major portion of claimants who were crowded out of the benefit without finding a job, while the effect on outright owners and mortgagers is lower. Empirical evidence from our dataset clearly confirms that mortgagers search for a job more intensively than renters, as our model predicts. This latter finding is consistent with a higher estimated treatment effect for renters, since a high initial search intensity seems the key to insulate oneself from the impact of the tightening of search requirements.

In the second chapter we attempt to reconcile the empirical evidence with the argument in favour of a negative effect of homeownership on exit rates from unemployment. Taking into account some likely reasons for the falsification of the Oswald's thesis, we provide evidence which supports it. At first, in a theoretical model of endogenous job search adapted to distinguish between local and non-local labour markets, we show that homeowners higher moving costs imply unambiguously lower search and lower job finding rates, even though an opposite effect works for jobs which do not require a move. Then, in the empirical analysis we make use of data drawn from the British Household Panel Survey to compare job search intensity measures by housing tenure. In defining the residential status, we distinguish between outright owners and mortgage-holders, and between social and private renters. We find that, controlling for housing costs and for the four-fold tenure definition, non-employed outright owners have definitely a lower attachment to the labour market than renters, and that this effect is even more evident when we compare them to private renters.

In the third chapter, we analyse the impact of the housing tenure on labour market outcomes using individual data from the UK labour Force Survey. 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.

Chapter 1

Housing Tenure and Job Search Behaviour. A Different Analysis of the Impact of the UK Jobseeker's Allowance*

1.1 Introduction

During recent decades, a lot of research has been carried out about the impact of unemployment benefits on the duration of unemployment, job search effort and re-employment rates, both in the short and in the long term. While the main focus has been on the level and the duration of unemployment benefits, only scant attention has been payed to the role of eligibility criteria, typically job search requirements and administrative burdens, which are to be met in order to be eligible for benefit.

Theoretical models of search (*i.e.* Mortensen [1986]) suggest that stricter search requirements affect search behaviour, lower the reservation wage of unemployed workers and raise the proportion of non-claimants in the non-employment pool. These theoretical predictions have found some empirical confirmation. Meyer [1995] shows from experimental evidence for the US that tighter job search requirements reduce claimant spells, while there is no evidence of any effect on re-employment rates meaning that at least a portion of those who have left the claimant pool are not reintegrated into employment. These early findings have been also confirmed by Card et al. [2007], who found that many workers leave the unemployment pool without

*This chapter is the result of a research conducted with Francesco Arzilli, former Ph.D. Student at the University of Pisa.

returning to work.

Recently, Manning [2009] and Petrongolo [2009] investigated the effects of the introduction of the Jobseeker's Allowance (JSA), which represented a key change into the UK welfare system, and, using two different sources of data and different time horizons, they both found that tighter search requirements were successful in moving individuals off the claimant count but less successful in moving unemployed workers into employment. Specifically, Manning showed that the removal effect was larger for claimants with low initial levels of job search activity. This is known as the "weeding out" effect.

Taking these last two papers as background, we develop our contribution looking for the interaction between the effect of the introduction of JSA and the role of the home ownership. The original question we would like to answer, is whether there is any difference in search behaviour between individuals who are differently attached to their accommodation, and whether this can account for different effects of the tightening of search requirements on the claimant outflow. In fact, there are several contributions that look at these two issues separately, home ownership and tighter search requirements, but none of them have focussed yet on how these could interact each other.

In order to analyze the role of home ownership on the job search behaviour, we enrich the search theoretical model proposed by Mortensen [1986][1] by incorporating moving and housing costs into the analysis. In particular, we identify three different housing tenure categories according to different moving and housing costs (outright owners, mortgagers and renters) and we explore how JSA, with stricter job search requirements, has affected the claimant status of workers who belong to these categories.

We provide empirical evidence using data from the Labour Force Survey, and by means of a Difference-in-Differences approach, we estimate the effect of JSA on the claimant outflow rate. Our results largely confirm the view that a tightening of search requirements implied a strong increase in the claimant outflow but that only a negligible portion of non-employed who left the claimant count ended up in employment. Moreover, this effect is higher for claimants with a low level of search intensity, as Manning [2009] found. We then explore the role, if any, of housing tenure in affecting the size of the treatment effect. Since the treatment operated in a different way according to the search behaviour of non-employed, and since our model predicts different search intensity levels for people with different housing tenure, we aim at testing whether the estimated treatment effect differs by housing tenure. Our results point out that renters account for a major portion of claimants

[1] In particular we refer to the simplified version of Manning [2009] and Petrongolo [2009].

who were crowded out of the benefit without finding a job, while the effect on outright owners and mortgagers is lower. Empirical evidence from our dataset clearly confirms that mortgagers search for a job more intensively than renters, as both our model predicts and earlier evidence pointed out. This latter finding is consistent with a higher estimated treatment effect for renters, since a high initial search intensity seems the key to insulate oneself from the impact of the tightening of search requirements.

The paper is organized as follow. In section 1.2 we review related literature and we discuss how this paper contributes to it. Section 1.3 describes the changes JSA introduced and its main characteristics, and provides preliminary evidence on its effects. Section 1.4 proposes a search model to represent the effect of JSA also considering moving costs and housing costs. Section 1.5 describes the data used in this paper. Section 1.6 presents the methodology used to conduct our analysis. Section 1.7 shows the main findings on the effect of JSA on the claimant outflow rate and how this effect is related to initial search intensity. Section 1.8 is the bulk of our analysis on the role of housing tenure in shaping the impact of JSA, and provides additional evidence on the relation between home ownership and workers' behavior. Section 1.9 concludes.

1.2 Relation to Existing Literature

Our contribution is related to two different strands of literature. The first one deals with welfare reforms and the impact of stricter job search requirements on the behaviour of unemployment benefit claimants. As Grubb stated (Grubb [2000]), "a strong requirement for job search or acceptance of suitable work may in theory offset the disincentive effects that arise when benefits are paid without such criteria"[2].

Most of the empirical evidence about the effect of job search requirements on the time spent on benefit is based on US social experiments carried out in the 70s and 80s. Early studies were conducted by Meyer [1995] who provides a useful survey and evaluation of these experiments. He finds that the combination of tighter search requirements and job assistance reduces claimant spells[3]. More recently, Klepinger et al. [1997] found a negative impact of stricter eligibility criteria on benefit duration while Ashenfelter et al. [2005] found that the estimated effect is quite small.

[2]See Grubb [2000] for a discussion of the expected effects of eligibility conditions, for a brief survey of them and for a general evidence of their impact.

[3]See Johnson and Klepinger [1994].

In the UK there has been one randomized experiment, the Restart Program in 1986, which can be considered as the precursor of the UK JSA. The Restart randomly assigned claimants who had spent at least twelve months on benefits to a treatment program consisting of tighter search requirements and counseling in order to speed up the process of finding a job. Dolton and O'Neill [1996] found that the Restart program increased the exit rate from unemployment, in particular towards employment, but that the effect for women was only short-term (Dolton and O'Neill [2002]).

With regards to previous analysis of the impact of JSA, there is a wide consensus about the effect on the claimant status. According to Trickey et al. [1998], Rayner et al. [2000] and Manning [2009], JSA had a significant impact on the flows out of claimant status, but, as it is argued by Manning [2009], there is no compelling evidence that either movements into employment or search activity were increased with the JSA. His results have been confirmed by Petrongolo [2009] who investigated the long-term effects of the introduction of JSA[4]. In particular, she found that JSA has had a positive impact on the claimant unemployment exit rate, but also a positive effect on exits into other benefits and a negative impact on the probability of working for up to four years after the unemployment spell[5].

The data and the methodological approach we use to assess the impact of JSA on claimant outflows are closely related to Manning [2009]. We differentiate from his estimation technique mainly in the way we deal with the seasonality issue. Then, once his main findings are largely confirmed, we aim at testing whether the effect of a tightening in job search requirements, as implied by JSA, differs by the housing tenure.

The second strand of literature, to which our work refers, looks at the relation between the housing tenure and workers' behaviour. In this context, the most prominent contributions are probably from Oswald (Oswald [1996], Oswald [1997], Oswald [1999]) who provided strong evidence for an aggregate positive relationship between unemployment and the home ownership rate. His key explanation is that home owners face higher transaction costs than renters (to sell and buy housing) when they consider a move to a new location to accept a job offer, so that they should experience longer unemployment spells, at least if compared to private renters. While several empirical studies confirm that home ownership hampers the propensity to move residence for job reasons (Van den Berg and Van Vuuren [1998], Henley [1998],

[4]She used data from the Lifetime Labour Market Database (LLMDB) administered by the Department for Work and Pensions which provides information on labour histories of selected individuals from 1978 onwards.

[5]Moreover, she found that JSA reduced the level of earnings and the number of weeks worked once re-employed

Munch et al. [2006], Battu et al. [2008]), some later tests of the Oswald's hypothesis, with both macro and micro-data, have provided evidence for its reverse[6]. Green and Hendershott [2001a] and Green and Hendershott [2001b] found from the US evidence that unemployment rates of household heads are affected less by tenure than those of the population as a whole; also Barrios García and Rodríguez Hernández [2004] contradict the Oswald thesis stating that the provinces of Spain with lower unemployment rates are associated with higher home ownership rates[7].

The Oswald's hypothesis has been rejected also by several micro-data studies (Goss and Phillips [1997], Coulson and Fisher [2009], Flatau et al. [2003], Munch et al. [2006], Battu et al. [2008]). Their typical finding is that home owners have a shorter duration of unemployment than renters, which is mostly true for mortgagers with a high mortgage debt. This literature points out that the higher are housing costs, the higher is the incentive to become re-employed more rapidly, thus high leveraged owners are supposed to search for a job more intensively than renters. Moreover, since home owners concentrate their search effort in the local labour market, the negative effect of the immobility in the housing market may be offset by higher job finding rates in the local labour market (Munch et al. [2006], Rouwendal and Nijkamp [2008]).

According to the channels between housing tenure and the job search behaviour identified by this literature, we give our contribution both from a theoretical and an empirical perspective. At first, we plug in mobility and housing costs in a standard job search model to analyze their likely effect on the optimal search of unemployed. Then, we use our data to check whether comparisons in job search outcomes of claimants with different housing tenure are consistent with the model's prediction.

1.3 The JSA: Characteristics and Preliminary Evidence

The JSA, which is the current system of welfare for the unemployed in the UK, was introduced on 7 October 1996. Before the JSA, the welfare system for the unemployed consisted of an unemployment insurance scheme called Unemployment Benefit (UB) and an unemployment allowance scheme of Income Support (IS). The JSA has a contributory component, known as

[6] Rouwendal and Nijkamp [2008] and Havet and Penot [2010] provide a survey of studies which tested the Oswald's thesis.

[7] First Oswald and later Green and Hendershott used OECD countries' and regions' data in which neither Spanish regions nor provinces were included.

contJSA, which replaced the UB scheme, and a means tested component, known as incJSA, which replaced the IS element[8]. IncJSA is far the most important component, since many of unemployed have insufficient National Insurance contributions for entitlement to contJSA and some have a level of contribution which requires their contJSA payments to be topped up by incJSA. For example, in December 1996, 76.1% of recipients of JSA were receiving incJSA against 29.3% who were getting contJSA; one year later, in December 1997, 75.5% were receiving incJSA versus 29.8% on contJSA[9].

The relevant changes of this reform can be allocated to two different areas of the whole unemployment benefit system. JSA slightly modified the level and the duration of the contribution-based benefit, but it also implied major changes in the eligibility conditions[10]. With JSA, the entitlement period for the contribution-based benefit was reduced to 6 months from 12 months under the previous system, and the difference in level between UB and IS was eliminated so that both contJSA and incJSA have now exactly the same payment rate and the same conditions as the former IS scheme. The UB and IS payments were very similar except for young people, who received about 20% less under IS than under UB. Therefore, the reduction affected only a small category of people getting the contribution-based benefit. Moreover, since only a modest portion of unemployed claimants receive the contribution-based benefit, it is widely accepted that changes in this area has affected a really small fraction of claimants (Manning [2009], Petrongolo [2009]).

The second and most significant change was represented by the substantial increase in job search requirements for eligibility and in the related administrative burden. All claimants have to sign a Jobseeker's Agreement in which they set out to actively look for a job and they state the period of work and the types of jobs they are available for. Within this agreement, they also commit themselves to undertake certain steps in order to find a job and to increase the chances of finding it, such as how many times at least they are going to contact employers and a Jobcentre. Claimants have to keep a thorough record of the steps taken, and at fortnightly interviews, the Employment Officer checks wether this record complies with what has

[8]The contJSA has a limited duration of 6 months maximum, while the incJSA has potentially unlimited duration.

[9]Data taken from the Labour Force Survey using seasonal datasets. Percentages add up to more than 100%, as claimants can be eligible for both incJSA and contJSA at the same time.

[10]Pointer and Barnes [1997] provide a detailed description of institutional and administrative aspects of JSA. See Finn et al. [1997] for a description of the previous UB/IS scheme.

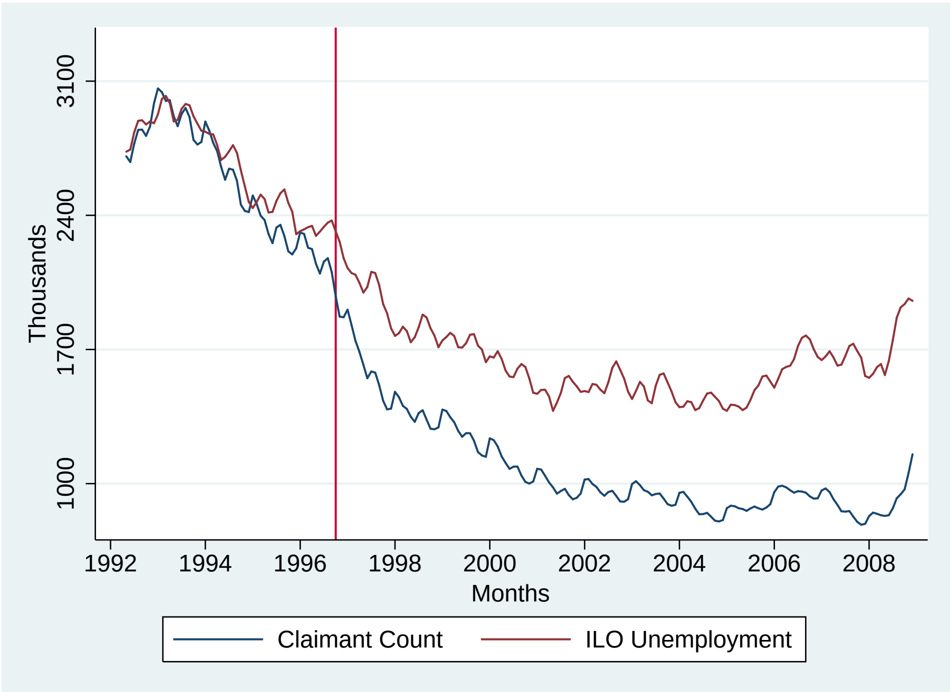
been detailed in the agreement. Furthermore, the Employment Officer can instruct claimants to take certain steps and to apply for specific jobs and, in case of being still unemployed after 13 weeks, they can be subjected to sanctions or disqualification. Regardless of the effectiveness of the new rules, the extra administrative hurdle and a stronger contact with the Employment Service may alone account for a large portion of the observed movements off benefit, as some evidence suggests[11].

Some basic analysis can bear witness to the effect the introduction of JSA had on the claimant count. Figure 1.1 presents a comparison between the series of the claimant count and the number of unemployed according to the ILO definition (ILO unemployed are those who are available to start to work within 2 weeks and have been looking for a job in the past 4 weeks). The claimant count started falling after 1992 and stopped only recently, but the drop has been remarkable on and soon after October 1996, when JSA was introduced[12]. Also the number of ILO unemployed drops soon after JSA though there is an evident overall decreasing trend in the series. This drop may be due to whatever reason, yet as long as we assume that it is, at least partially, explained by JSA, we can not conclude that JSA increased exit rates from unemployment towards employment since some of the claimants who dropped off the register may also have become inactive according to ILO definition. Before 1995 the two lines were following almost the same path, but after that they started to diverge. This gap became very wide right after the introduction of JSA and it has increased more and more since then, which means that, while JSA removed several individuals from the claimant count, most of them did not stop looking for work according to the ILO definition. The lesson we draw is that, given the stricter conditions and administrative hurdles unemployed have to meet in order to be eligible for JSA, ILO unemployment search standards are now far from those required for JSA eligibility. Also, we argue, there may have been a large increase in the number of unemployed who prefer to look for a job independently, without being forced to contact the Employment Service.

[11]For example, evidence from social experiments shows that many claimants who are subjected to treatment involving monitoring and job search assistance drop out of the claimant status since they do not comply with obligations. See Dolton and O'Neill [2002] and Johnson and Klepinger [1994].

[12]Administrative data on claimant flows also show that the decline in the claimant count seems to have been caused by a jump in the outflow rather than by a reduction in the inflow.

Figure 1.1
Claimant Count and ILO Unemployment



Notes:

1. Data for the claimant count series are drawn from administrative records of the welfare system; they can be found at www.nomisweb.co.uk. Data for ILO unemployment series are drawn from Labour Force Survey; they can be found at the ONS website.

2. Both series are monthly and seasonally unadjusted. Numbers are in thousands.

1.4 A Simple Search Model with Housing Tenure

In this section we present a simple job search model which represents a useful tool to investigate the impact of tighter job search requirements. Manning [2009] and Petrongolo [2009] proposed a simplified version of the traditional Mortensen's (Mortensen [1986]) search model with an exogenous wage distribution and endogenous search effort[13]. The relevant change we make in this framework is allowing for a different housing tenure status.

Individuals, who can be unemployed or employed, are infinitely lived and maximize lifetime utility in continuous time. When unemployed, individuals receive b as unemployment compensation, which is fixed and independent of the wage, and search for a job with effort s, where s measures the time

[13] See also Barron and Mellow [1979].

subtracted from leisure for job search activity. We assume that only the unemployed search for jobs, since this is the relevant aspect affected by the JSA reform, and since this simplifies notation without affecting our main theoretical results. Search activity yields a cost $c(s)$ and influences the probability of moving into the employment pool by generating a job offer arrival rate $\lambda(s)$. As typical in this modeling, costs are convex in effort, while returns are concave, so that $c'(s) > 0$ and $c''(s) > 0$, $\lambda'(s) > 0$ and $\lambda''(s) < 0$. The unemployed receive job offers at the rate $\lambda(s)$, where wage offers are sampled from the c.d.f. $F(w)$. The acceptance rule dictates that the unemployed will accept any job offer whose wage is at least equal to the reservation wage.

First, in this standard search framework, we add moving costs by assuming that they affect the job finding probability. Namely, moving costs act as a wedge between the reservation wage and the wage level the unemployed would be actually willing to accept. The idea we want to capture is that job offers differ not only in wage level, but also in location. Some jobs are located further than others from the unemployed's accommodation, so that accepting an offer may require a moving. People who are less mobile will reject some job offers that others may accept, and this implies a lower job finding probability. As long as individuals may bear different moving costs depending on the housing tenure, this idea can simply highlight the channel through which the degree of attachment to the accommodation affect search behaviour.

Owners, either outright or mortgagers, have a higher degree of attachment towards the property than renters. Also, it seems reasonable that outright owners have a stronger attachment to the accommodation than mortgagers since time spent in the current accommodation should be longer on average and since transaction costs for moving home may be higher. So, we assume that $M_o > M_m > M_r$, where M are moving costs, *i.e.* a proxy for mobility. This is also consistent with Oswald (Oswald [1996], Oswald [1999]), according to whom owners occupiers are supposed to be less mobile than renters since they are less prone to accept a job offer far from their current accommodation[14].

Secondly, we bring into the model housing costs. When looking for work the unemployed faces the cost function $c(s)$, where c translates hours devoted to search in utility loss, that is, in its monetary cost given the standard risk neutrality hypothesis. We assume that the unemployed has also to bear a housing cost H, whose amount depends on the housing tenure status[15].

[14] Empirical evidence is also provided by Van den Berg and Van Vuuren [1998], Munch et al. [2006] and Battu et al. [2008] who suggest that homeowners are less likely to change residential location in order to accept a job outside the local labour market because of their higher moving costs.

[15] This cost is not related to the unemployment status, since also employed people have

Housing costs matter in this framework since people have a higher pressure to find a job the higher are these costs. In particular, we assume that $H_m > H_r > H_o$, which is consistent with empirical evidence supporting the view that people who bear the cost of a mortgage have higher housing expenditure than either outright owners and renters (i.e Rouwendal and Nijkamp [2008], Goss and Phillips [1997], Flatau et al. [2003])[16]. Moreover, this is also a likely explanation for the repeated finding that high leveraged owners have lower unemployment spells than renters[17].

Let U and W denote the present-discounted value of expected income stream of, respectively, an unemployed and an employed worker, included the imputed return from non market activities. The unemployed worker enjoys the benefit b, bears the cost $c(s) + H$ and he expects to move into the employment pool at the rate $\lambda(s)$. U satisfies the following equation:

$$rU = \max_{s,w_R}\{b - c(s) - H + \lambda(s) \int_{w_R+M} [W(w) - U]\, dF(w)\}, \tag{1.1}$$

where r is the discount factor. The job finding rate is $\lambda(s)[1 - F(w_R + M)]$ and is decreasing in moving costs M. Employed workers earn a wage w, they bear the cost for the house tenure and they face an exogenous risk of job loss δ; W satisfies the following:

$$rW(w) = w - H + \delta[U - W(w)]. \tag{1.2}$$

Since U is the present value of the expected utility stream of an unemployed, rU represents (given also risk neutrality) the instantaneous income derived from that. The reservation wage w_R is defined as the wage level such that employment and unemployment are equally valuable, *i.e.* $W(w_R) = U$. Thus, since the present value of a future income stream given a wage equal to x is $W(x) = x/r$, the reservation wage will be equal to the instantaneous income of the unemployed rU $(rW(w_R) = rU = w_R)$. Differentiating 1.2 we get $W'(w) = 1/(r + \delta)$ so, after integrating by parts, we can rewrite 1.1, which also

to bear it. We will plug this cost in the employed's value function, but this will not have any role since we rule out on-the-job search.

16 We will provide below further results in support of this assumption comparing out-of-pocket housing costs of mortgagers and renters with data drawn from the British Household Panel Survey.

17 Plugging in the parameter H as a fixed cost flow is an easy way to allow for differentiation in income flows. We are basically making the *ad hoc* assumption that the only source of variation in the income related to housing tenure is due to housing costs, while one can argue that owners could have a higher income than renters despite a lower housing cost. In other words, there could be different channels by which this income effect can operate, but here we want just to focus on the likely effect of housing costs.

implicitly define the reservation wage, as:

$$w_R = rU = \max_s\{b - c(s) - H + \frac{\lambda(s)}{(r+\delta)}\int_{w_R+M}[1 - F(w)]\,dw\}. \tag{1.3}$$

The unemployed worker will chose the optimal search effort s^* such that:

$$c'(s^*) = \frac{\lambda'(s^*)}{(r+\delta)}\int_{w_R+M}[1 - F(w)]\,dw, \tag{1.4}$$

where marginal costs of search effort are equal to marginal benefits, which are represented by the gain from employment weighted for the higher job offers arrival rate.

Using the implicit function of w_R we can determine the shape of indifference curves in the space (s, b). Differentiating 1.3 with respect to w_R and b we have $dw_R = db - (r+\delta)^{-1}\lambda(s)[1 - F(w_R + M)]dw_R$, so the effect of b on w_R is clearly positive as usual:

$$\frac{dw_R}{db} = \frac{r+\delta}{r+\delta+\lambda(s)[1 - F(w_R+M)]} > 0. \tag{1.5}$$

Differentiating 1.3 with respect to w_R and s we get $dw_R = -c'(s)ds + \{(r + \delta)^{-1}\lambda'(s)\int_{w_R+M}[1 - F(w)]\,dw\}ds - (r+\delta)^{-1}\lambda(s)[1 - F(w_R + M)]dw_R$, so the effect of s on w_R depends on the level of s:

$$\frac{dw_R}{ds} = \frac{r+\delta}{r+\delta+\lambda(s)[1 - F(w_R+M)]}\left\{\lambda'(s)A - c'(s)\right\}, \tag{1.6}$$

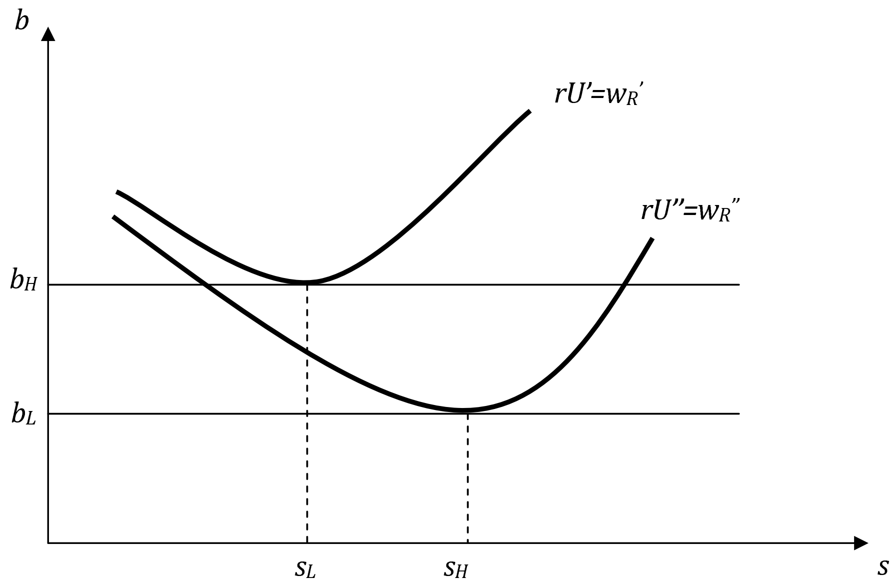
where we set $A = (r+\delta)^{-1}\int_{w_R+M}[1-F(w)]\,dw$. The effect of s on the reservation wage is zero at the optimal level s^*, since the term in braces is zero, while is positive (negative) for $s < (>)s^*$. When $s > (<)s^*$ a further increase in s lowers (increases) w_R so the worker requires an increase (decrease) in b to keep the reservation wage constant. The indifference curves are thus as drawn in figure 1.2 for two different levels of b, where we point out that an increase in b lowers the optimal search effort and increases the reservation wage[18].

Given this theoretical framework we can now investigate the effect of tighter eligibility rules on optimal search and on the claimant outflow. This framework can be slightly modified to allow for eligibility rules by conditioning the receiving of unemployment benefits on the keeping of these rules.

[18]The relationship between s^* and b is negative as an increase in b makes unemployment relatively more attractive than employment and thus reduces the return to searching.

$$\frac{ds^*}{db} = -\frac{\lambda'(s^*)[1 - F(w_R+M)]}{r+\delta+\lambda(s^*)[1 - F(w_R+M)]}\left[c''(s^*) - \lambda''(s^*)A\right]^{-1} < 0.$$

Figure 1.2
THE CHOICE OF SEARCH INTENSITY

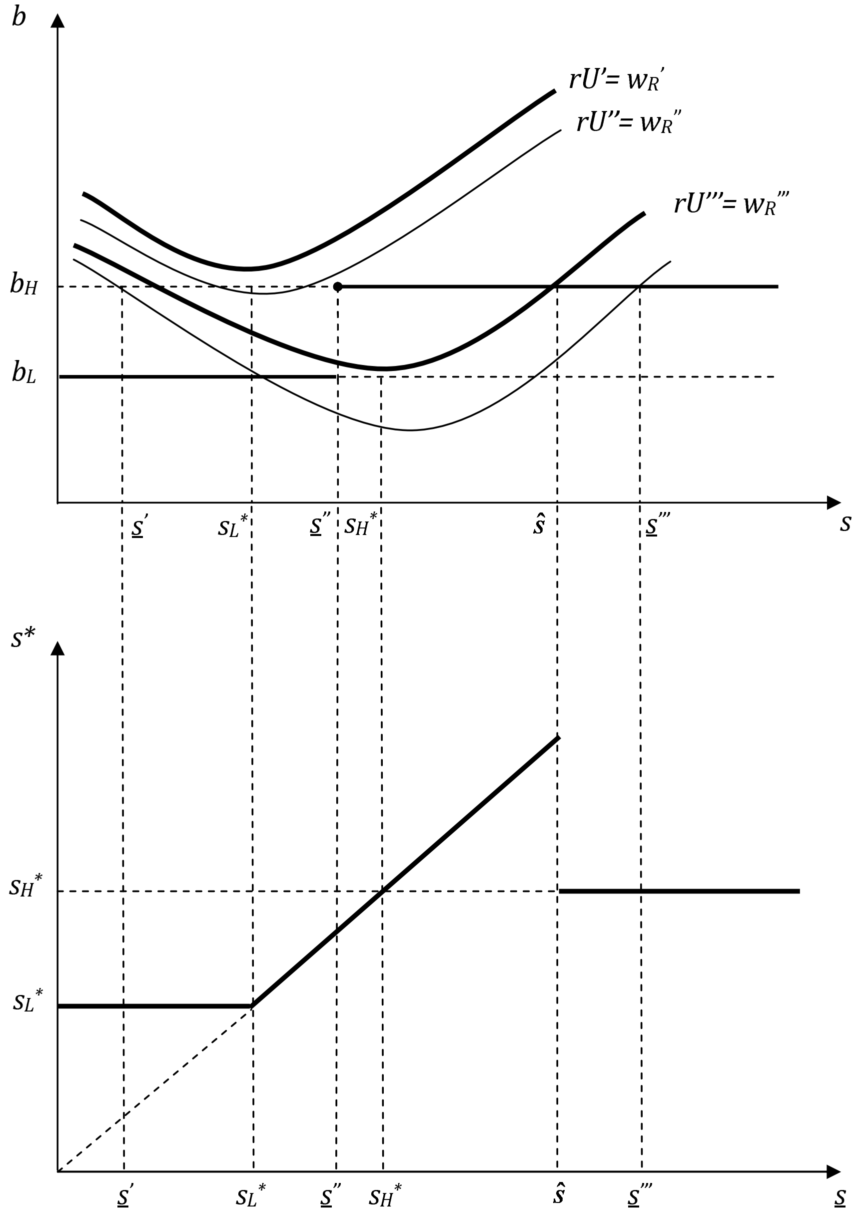


Following Manning [2009] and Petrongolo [2009], we study this element by introducing a threshold level of search activity $\underline{s}$ which has to be exerted in order to be entitled to claim the benefit. Unemployed workers whose search effort is equal or greater than $\underline{s}$ are classified as claimants, while individuals who exhibit a search effort below $\underline{s}$ are considered non-claimants and they receive an income lower than the claimants' one[19]. We can thus define two level of benefits b_H and b_L whose difference is the search related benefit, *i.e.* the income that the worker receives if he chooses a search effort above the threshold.

In this context we can simulate the effect of the JSA reform just by looking at the effect of an increase in the threshold level $\underline{s}$ as in figure 1.3. When the threshold is set at $\underline{s}'$ it does not bind and the worker will chose the interior solution s_L^*, which is associated to the utility level rU'. The increase of search requirements from a low level $\underline{s}'$ to a higher level $\underline{s}''$ affects the optimal search effort which moves from s_L^* to the corner solution $\underline{s}''$, and lowers the indifference curve where the individual will be positioned from rU' to rU'', which is characterized by a lower reservation wage (the discontinue bold line represents the benefit rule whenever the threshold is $\underline{s}''$). Further increases

[19]As specified in Petrongolo [2009], the income of non-claimants is not necessarily zero since they may receive other not search related benefits (*i.d.* health-related benefits).

Figure 1.3
The Impact of Stricter Eligibility Conditions



in the search threshold will be followed by one-for-one increases in optimal search, at least up to the level $\hat{s}$, where the unemployed is indifferent between meeting the rules and leaving the claimant status, since the pairs $(b_H, \hat{s})$ and (b_L, s_H^*) lie on the same indifference curve. Yet, any increase in the threshold from below to above $\hat{s}$ would actually lead to a drop in the optimal search back to the level s_H^*, because the marginal costs the unemployed would incur to meet the higher requirements would be higher than the marginal benefits in terms of higher unemployment income and job offers arrival rate (this effect of discouraging unemployed people to provide search level for a job has been considered as the "unintended" consequences of the JSA).

The economics of this model is thus not able to predict the sign of the effect of a tightening in search requirements on the average search activity of the unemployed. The lowest graph in figure 1.3 plots the optimal search activity against the search requirements as implied by this model and clearly shows that changes in these requirements may either not affect or affect in both ways the actual search intensity. A tightening of the rules would not affect the optimal search intensity for workers who have very high $(s^* \geq \hat{s})$ or very low search effort $(s^* < s_L^*)$. In fact, the former will continue to be claimants despite the change in the policy, while the latter will be non-claimants both before and after such a change. The targeted workers who are affected by the introduction of the JSA are those who exert a search intensity in the middle range $s_L^* < s < \hat{s}$: all of these are initially claimants but, after the introduction of the JSA, some of them will find optimal to increase search effort to continue to be claimant while others will be better off by reducing it and thus they will stop to claim.

In order to shed some light on the role of housing tenure on optimal search intensity, we refer now to equation 1.4, which holds in equilibrium, and, by means of the envelope theorem, we study the sign of the differences in optimal search of the three housing tenure categories. Indicating with s_o^*, s_m^* and s_r^* the optimal search levels of respectively outright owners, mortgagers and renters, we obtain the following differences:

$$s_m^* - s_o^* = s^*(M_m, H_m) - s^*(M_o, H_o) = \frac{ds^*}{dM}(M_m - M_o) + \frac{ds^*}{dH}(H_m - H_o), \tag{1.7}$$

$$s_r^* - s_o^* = s^*(M_r, H_r) - s^*(M_o, H_o) = \frac{ds^*}{dM}(M_r - M_o) + \frac{ds^*}{dH}(H_r - H_o), \tag{1.8}$$

$$s_m^* - s_r^* = s^*(M_m, H_m) - s^*(M_r, H_r) = \frac{ds^*}{dM}(M_m - M_r) + \frac{ds^*}{dH}(H_m - H_r), \tag{1.9}$$

where $(M_m - M_o) < 0$, $(M_r - M_o) < 0$, $(M_m - M_r) > 0$, $(H_m - H_o) > 0$, $(H_r - H_o) > 0$, $(H_m - H_r) > 0$ by assumption.

Applying the implicit function theorem to equation 1.4 we can study the sign of ds^*/dM and ds^*/dH. From 1.4 we define $\phi(s^*, H, M) = c'(s^*) - (r + \delta)^{-1}\lambda'(s^*)\int_{w_R(s^*,H,M)+M}[1 - F(w)]\,dw = 0$, thus we have[20]:

$$\frac{ds^*}{dH} = -\frac{\phi_H}{\phi_{s^*}} > 0, \tag{1.10}$$

$$\frac{ds^*}{dM} = -\frac{\phi_M}{\phi_{s^*}} < 0. \tag{1.11}$$

Equations 1.10 and 1.11 show clearly the relation between housing tenure and search behaviour of the unemployed. This relation operates through two different channels. First, the higher are housing costs the higher is the need for income, so the unemployed will increase the time subtracted from leisure for search purpose in order to raise the probability of finding a job. Secondly, the higher are moving costs the lower are returns to search since the probability of accepting a job offers is lower, thus the unemployed will reduce search intensity. The expression $(ds^*/dM)\Delta M$ picks up the "mobility effect", which is negative (positive) whenever $\Delta M > (<)0$, while $(ds^*/dH)\Delta H$ picks up the "housing cost" effect, which is positive (negative) whenever $\Delta H > (<)0$.

The mobility effect alone suggests that the optimal search activity should be lower the higher is the degree of attachment to the accommodation, thus renters should exhibit a higher search intensity than both mortgagers and renters, and mortgagers higher than renters. Anyway, if we account also for the housing cost effect these outcomes may be reinforced or weakened, if not reversed. If we compare mortgagers with outright owners, the housing cost effect would simply reinforce the former leading to the conclusion that mortgagers should unambiguously exhibit higher search intensity than outright owners ($s^*_m - s^*_o > 0$): the rationale of this outcome is that owners who are still paying the accommodation look for work in a wider area and have to find a job more quickly in order to sustain the cost of the mortgage. We obtain the same outcome also for renters with respect to outright owners, at least for renters who bear higher housing costs ($s^*_r - s^*_o > 0$). If we compare mortgagers with renters, the housing cost effect has opposite sign with respect to the mobility effect, instead, so the sign of $s^*_m - s^*_r$ depends on the balancing of both.

[20]We use the following derivatives, where we set $A = (r + \delta)^{-1}\int_{w_R+M}[1 - F(w)]\,dw$, which is positive:

$$\phi_{s^*} = c''(s^*) - \lambda''(s^*)A > 0,$$

$$\phi_H = -\frac{\lambda'(s^*)[1 - F(w_R + M)]}{r + \delta + \lambda(s^*)[1 - F(w_R + M)]} < 0,$$

$$\phi_M = \frac{\lambda'(s^*)[1 - F(w_R + M)]}{r + \delta + \lambda(s^*)[1 - F(w_R + M)]} = -\phi_H > 0.$$

As long as we assume (consistently with empirical literature cited above and our following results) that mortgagers face higher housing costs, the issue whether mortgagers have higher search activity than renters is basically an empirical matter.

1.5 Data

We draw our data set 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, and we exploit this panel component building categorical variables which report flows among different labour market status. Even though our econometric methodology does not rely on a panel analysis, the panel structure of the survey allows us to follow cases for two subsequent quarters, so that our outcome variable is typically whether or not an individual leaves a particular status, as unemployment benefits claimant.

From 1992 to 2006 the LFS has been conducted on a seasonal-quarter basis, that is interviews were referred to Spring (March-May), Summer (June-August), Autumn (September-November) and Winter (December-February). From 2006 onwards, however, the LFS is being conducted on a calendar-quarter basis and interviews refer to Quarter 1 (January-March), Quarter 2 (April-June), Quarter 3 (July-September) and Quarter 4 (October-December)[21]. Since JSA reform was introduced on Monday the 7th October 1996, we postpone all calendar quarters by one week in order to set this date as the starting point of both the treatment and the 4th quarter of 1996.

Each LFS's quarter contains hundreds of variables which cover many features of the UK labour market and provide detailed pieces of information on individual characteristics. We focus mainly on variables sets which refer to individual labour market status, search behaviour and housing tenure. The survey provides a specific variable which reports whether or not an individual is claiming unemployment related benefits. The questions about housing tenure form the basis for our analysis of different treatment effects by sub-groups. The survey provides information enough to split the sample into three categories according to different housing tenures: owners outright,

[21]The switching from seasonal to calendar quarters has introduced several discontinuities in the data files up to 2006, since they were all rearranged in order to fit the calendar pattern. This major change affected many of the variables over the relevant period for our analysis, so that we preferred to deal with the old seasonal quarters files. Then we reallocated cases in order to fit the calendar pattern. Sampling weights refer to the old person weight variable `pwt03`.

owners still paying with mortgage or loan, and renters. The survey gives also further details about renters that we exploit to test whether differences in some relevant features matter in explaining different responses within this group.

1.6 Methodology

Our aim is to estimate the Average Treatment Effect (ATE) of the JSA reform on a number of outcome variables, typically flows out of the claimant status[22]. In order to do this we use claimants interviewed in the 3rd quarter of 1996 (which we will call wave 1) as treatment group, and we look at their status in the next quarter (which we will call wave 2). In the 3rd quarter they are not treated yet but in the 4th they are, so the choice to move or not from the initial status is affected by the new rules. Of course we cannot impute all of these moves to the reform as these may also have been observed in the counterfactual settings, that is without the treatment. Thus, to identify the causal effect we use claimants in the 2nd quarter of 1996 (wave 1) as control group, and we look at their status in the next quarter (wave 2). Treatment and control groups are close enough in date to allay fears that differences in their behaviour could be affected by aggregate factors[23].

Differences in response between treatment and control groups we build in this way are what we expect to be due to JSA reform, at least so long as these groups are similar in observable characteristics, as this is the case. Anyway, since treatment and control groups differ in quarters, ATE estimates would be biased if claimant outflows had any seasonal pattern. In order to control for seasonality we generate treatment and control groups in the same way by means of two new cohorts drawn from the adjoining years 1995 and 1997, and we difference out the average seasonal effect using a Difference-in-Differences technique. The baseline equation we estimate appears like this:

$$y_i = \beta_0 + \beta_1 d96_i + \beta_2 d97_i + \beta_3 jsa_i + \beta_4 jsa_i * d96_i + \delta X_i + u_i, \tag{1.12}$$

[22] Our approach is very close to that of Manning [2009].

[23] We emphasize that there could be some overlapping between treatment and control groups since some claimants interviewed in 3rd quarter can belong either to wave 1 of the treatment group or to wave 2 of the control group. When we compute our regressions the outcome variable and the regressors refer typically to the 2nd quarter for the control group and to the 3rd quarter for the treatment group: this means that for every claimant interviewed in the 3rd quarter who belongs to both treatment group (in wave 1) and control group (in wave 2), we use two distinct observations which refer to two different variables sets, at least regarding variables which can vary over time, as the flow outcome variable and regressors such duration since last job, age, education, region and so on.

where y_i is the outcome variable, jsa_i is a dummy that takes 1 if i belongs to treatment group and 0 if i belongs to control group, $d96_i$ and $d97_i$ are year dummies. The vector X_i contains variables we can plug in to control for observable characteristics. Including controls anyway hardly changes treatment effect estimates and this is exactly what we expected since treatment and control groups are very similar in these observables. The coefficient of the interaction term, β_4, is the Difference-in-Differences coefficient and captures the causal effect of the program. The outcome variable y_i represents typically whether the claimant, either being part of the treatment group or the control group within the cohort, stops claiming at wave 2 and we run regressions pooling the three cohorts for 1995, 1996 and 1997.

The series of the claimant outflow rate typically exhibits some seasonality in that the rate of claimants in the 3rd quarter who move off in the 4th is usually higher than the rate of those in the 2nd quarter who move off in the 3rd. If we run two separate regressions just only for the 1995 cohort (here no one is receiving treatment), and just only for the 1997 cohort (here all are receiving treatment), we estimate a difference in the outflow rate between treatment and control groups of 3% and 1.9%, respectively (the latter is not significant)[24]. This means that the way by which we create treatment and control groups is by itself prone to deliver a positive difference in claimant outflows regardless of the treatment; so if we did not account for seasonality we would probably overestimate the causal effect.

These coefficients also suggest us that we would probably overrate the true seasonal effect if we accounted for only 1995. In theory one extra cohort would be enough to identify the causal effect, as Manning does using only 1995. But, if we use one only extra cohort, the seasonal effect estimate is too sensitive to the choice of the particular year for the comparison, so we prefer to use both cohorts as we think this can better remove the seasonal effect. Moreover, we focus on just the two adjoining years to exploit the persistence in the series.

One could be concerned about some anticipatory effects of the JSA, especially on the basis of its retroactive nature[25]. The LFS collects weekly interviews, so it does not seem unreasonable that some people whose reference week is very close to 7th October 1996 behaved in a different way of

[24] We emphasize that we distinguish treatment and control groups just by the quarters they refer to, regardless of being actually treated or not, since, obviously, all individuals in 1995 are not treated and all individuals in 1997 are treated. So, for example, the coefficient for 1995 cohort is the estimated difference in the outflow rate between 4th and 3rd quarter.

[25] All existing UB and IS claimants as of 7th October 1996 are automatically transferred to the JSA system, and new rules are enforced also in the meantime until they fill a Jobseeker's Agreement, which is supposed to be done soon after 7th October.

what they may have done without the awareness of the imminent change of rules. Anyway, we think this concern should not apply to registered claimants but only to people who face the decision to claim just few weeks before the JSA introduction. In fact, people who are already claiming and may be unwilling to meet new imminent stricter rules should not have any reason to stop claiming before their introduction. This seems to be confirmed by our sample, since if we estimate a "fictitious" treatment effect for claimants belonging to the last week or to the last two weeks before the JSA introduction, we get a negligible and insignificant coefficient. However, non-claimants who would be willing to claim under ongoing rules but not under the new ones, may have some disincentives to sign up just for few weeks. If this is the case, the anticipatory effect should have worked by dropping potential claimants in the wave 1 of the treatment group who have never signed up and who otherwise would have been crowded out of the claimant count after the introduction of JSA. This means that our estimated treatment effect may have been even higher.

1.7 The Impact of JSA on Claimant Outflows

Results in table 1.1 show both the magnitude and the way the treatment operated. This reports probit estimates of the effect of JSA on the flows out of claimant status into different economic activity status. Claimants who stop claiming can end up in either employment or non-employment, where non-employment means either unemployment or inactivity. The first row of the table refers to the flow out of claimant status whatever is the destination, while 2nd and 3rd split up the total outflow between non-employment and employment destinations, and 4th and 5th split outflows into non-employment between unemployment and inactivity destinations. Columns 1 and 3 report estimates of the gross treatment effect, while columns 2 and 4 report DiD estimates. Columns 3 and 4 correspond to 1 and 2 but they show whether ATE estimates are sensitive to the inclusion of the vector of variables X_i. First of all, as we already pointed out, we notice that adding controls to the baseline regression hardly affects the treatment effect estimates, so for simplicity we will focus on just the first two columns.

The 1996 sample alone suggests a 10.3% treatment effect on the total claimant outflow (see column 1), but this exercise is blurring the true causal effect of JSA since it does not control for seasonality. When adding 1995 and 1997 cohorts, this coefficient drops to 7.7%, revealing a seasonal effect of around 2.6%[26]. However, this coefficient does not tell anything about people

[26]The coefficient from the 1996 sample is simply the difference (weighted for sam-

Table 1.1
IMPACT OF JSA ON CLAIMANT OUTFLOWS: FROM CLAIMANT IN WAVE 1

Average Treatment Effect on:	**1**	**2**	**3**	**4**
Flow out of Claimant Status	0.1028	0.0765	0.1025	0.0761
	(0.000)	(0.000)	(0.000)	(0.000)
Flow into non-employment	0.0737	0.0696	0.0710	0.0656
	(0.000)	(0.000)	(0.000)	(0.000)
Flow into employment	0.0291	0.0071	0.0299	0.0094
	(0.004)	(0.571)	(0.002)	(0.441)
Flow into unemployment	0.0468	0.0558	0.0429	0.0511
	(0.000)	(0.000)	(0.000)	(0.000)
Flow into inactivity	0.0269	0.0172	0.0245	0.0151
	(0.000)	(0.064)	(0.000)	(0.079)
Difference-in-Differences	No	✓	No	✓
Controls	No	No	✓	✓
Number of observations	5958	16836	5904	16289

Notes:

1. Reported coefficients are marginal effects of a probit model for a change of the dummy from 0 to 1; p-value in brackets. Observations are weighted by survey sampling weights. The DiD coefficient is obtained differencing out the seasonal effect obtained with both 1995 and 1997 cohorts.
2. The basic sample are claimants in wave 1. The outcome variable takes 1 only if claimant stops claiming in wave 2. In computing flows into economic activity status, the outcome variable takes 1 if and only if claimant stops claiming in wave 2 and at the same time moves into the relevant economic activity status.
3. We use as controls age, age squared, sex, race (white, black, asian, other), education, regional dummies and dummies for degree of attachment to the labor market (that is duration since last job and whether ever worked).

who were moved off the claimant count, so we cannot actually conclude at this point that JSA was able to fulfill both its purposes, basically to move off the claimant count cheating claimants and people who were not assiduous in searching a job, and to increase flows into employment by encouraging greater search activity among claimants. The second intended effect seems far from having worked, indeed. When splitting up claimant outflows be-

pling weights) between the claimant outflows of 3rd and 2nd quarters, that is the percentage of claimants in July-September quarter who became non claimant in October-December (38.88%) minus the percentage of claimants in April-June quarter who became non claimant in July-September (28.6%). This 10.28% difference cannot be totally put down to JSA, as we observe an increase in claimant outflows between 3rd and 4th quarters, though far smaller, also for both 1995 and 1997, highlighting a seasonal pattern. The fictitious treatment effect is 3% for 1995 and 1.9% for 1997, so if we subtract the seasonal effects' weighted average (2.63%) from the gross treatment effect of 1996 we get precisely a causal effect of 7.65%. This means that the claimant outflow rate in October-December 1996 was higher than that we might have observed without JSA by 1/4 times (*i.e.* (38.9-31.2)/31.2, where 31.2% is the outflow rate for wave 1 plus the average seasonal effect).

tween movers into non-claimant non-employment status (second row of table 1.1) and into non-claimant employment status (third row), ATE estimations reveal the whole story: first transitions are far more important with a DiD coefficient of 7%, which accounts for almost all of claimant outflows, while outflows into employment are basically negligible. The 1996 sample suggests a significant increase of 2.9% in the outflow to employment, but this is mainly due to seasonality as the ATE drops to a small and not significant 0.7% when using the whole sample. So, our results strongly confirm the view that JSA reform had a sizeable impact on the claimant outflows, but it did not operate by addressing these into employment. Interestingly, we also notice that seasonality in claimant outflows almost entirely concerns flows into employment, as the coefficients in second row of table 1.1 are very similar.

The large estimated impact on claimants who end up in non-employment suggests that JSA has been very effective in moving off the claimant count people "who were not assiduous in their job search or were claiming fraudulently" (Rayner et al. [2000]). This "weeding out" effect may have accounted for large savings in the welfare expenditure, but it is also arguable whether the state of people who lost this benefit should not be of any concern[27]. Many of them may just have a search activity level high enough to be registered as ILO unemployed but not as high as to meet the stricter eligibility restrictions. The rationale of any unemployment benefit, which is also even stronger for the JSA, is to sustain search effort of unemployed who do want a job, not to sustain people with low income. Thus we think it is worth trying to distinguish job seekers who are really willing to work from people who exert the minimum effort called for to receive benefit.

If we look at rows 4 and 5, we can tell more about people who exit the claimant status and end up in non-employment. The ATE on the outflow into unemployment is significant and 5.58% is a very large size if we consider that the estimated outflow rate for the treatment group is 8.91%, *i.e.* our model predicts that the outflow in the counterfactual setting would have been only 8.91-5.58=3.33%. The treatment effect on the outflow into non claimant-inactivity is anyway not significant at a 5% level and quite small

[27]Regarding the fate of people who drop off the register, Petrongolo [2009] and Machin and Marie [2004] provide two different pieces of evidence. Petrongolo finds a positive effect of JSA on exit rates from unemployment into other benefits, such as Incapacity Benefits, so that savings in the welfare expenditure may not have been as high as believed. Machin and Marie study the relationship between crime and the introduction of JSA and they find that crime rates rose more in areas most affected by JSA, that is where the increase of outflow rate was higher. Moreover, they observe an overall increase of the outflow rate to destination "nowhere", which refers to people who drop off the register but do not end up into employment, into full time education or training, or into other benefits.

as it is 1.72 percentage points out of an estimated outflow for the treatment group of 10.51%. Basically, most of claimants who dropped off the register kept on seeking for a job and this gives a picture of how tighter have become entitlement criteria than those which have to be met in order to be registered as ILO unemployed. This is consistent with figure 1.1 which clearly shows that JSA reduced the proportion of claimants in the unemployment pool. We interpret these findings as supportive of the view that expenditure savings were not the only implication of the "weeding out" effect[28].

Another way to check the operating of the "weeding out" effect is to estimate the treatment effect for different groups by search activity dimensions. The LFS provides information both about last time the interviewed searched for work and about the number of search methods he experienced in the last 4 weeks. As our theoretical conclusions suggest, claimants who self-report as exerting a low level of search effort are supposed to be the most affected by the JSA. Following Manning [2009] we split the claimant non-employed sample into 4 categories according to the last time they searched for work and to their willingness to work[29]. Table 1.2 shows the results when we apply our usual technique to these 4 groups separately, where search activity levels refer to wave 1. Comparisons between these groups cannot be very precise since the size of group 1 (search in last week) is far higher than that of the others. Anyway, DiD estimates in column 2 clearly suggest that the smallest treatment effect regards people who have searched in the past week, and this is exactly what we expected on the ground of our theoretical predictions. A pejorative reading of the table may awaken some worries about the reliability of these results, since we can also notice that the treatment effect for group 2 is far larger than that of both groups with the smallest search intensity, and the coefficients of the latter lose significance when controls are added.

[28]The analysis so far uses as a basic sample people who are claimant in wave 1 without any restrictions in their activity status. For the purpose of disentangling claimant outflows by economic activity destination within the not-in-employment category, a more refined analysis should focus on just claimant unemployed in wave 1. We argue that unemployed who lose the right to claim, but still keep on looking for job as unemployed instead of ending up inactive, can be a good proxy of people who embark job search not just for the purpose of exploiting the benefit. When restricting the sample to claimant-unemployed in wave 1 we drop employed and inactive people who account for a small part of the claimant pool, thus if we replicate the exercise of table 1.1 results are very similar.

[29]The LFS provides search measures only for non-employed people, but this is no concern of ours since we are not dealing with on-the-job search. Therefore, the sample we use for this analysis picks up only individuals not in employment in wave 1. We have already pointed out that a portion of these individuals, even though small, end up in employment in wave 2. We drop these observations as here our purpose is to focus on just the "weeding out" effect.

Table 1.2
IMPACT OF JSA ON CLAIMANT OUTFLOW BY SEARCH ACTIVITY IN WAVE 1

Average Treatment Effect on:	**1**	**2**	**3**	**4**
(4) Do not want work	0.0826	0.1283	0.0992	0.1112
	(0.128)	(0.056)	(0.088)	(0.115)
observations	*348*	*1012*	*346*	*945*
(3) Want work, no search in past 4 weeks	0.1506	0.1026	0.1466	0.0843
	(0.002)	(0.097)	(0.004)	(0.193)
observations	*426*	*1094*	*418*	*1029*
(2) Search in past 4 weeks	0.1980	0.2031	0.2461	0.2100
	(0.001)	(0.011)	(0.000)	(0.011)
observations	*226*	*650*	*223*	*634*
(1) Search in last week	0.0839	0.0751	0.0834	0.0721
	(0.000)	(0.000)	(0.000)	(0.000)
observations	*3401*	*9706*	*3365*	*9400*
LOW SEARCH vs HIGH SEARCH				
(2,3,4) Low Search	0.1374	0.1416	0.1348	0.1248
	(0.000)	(0.000)	(0.000)	(0.002)
observations	*1000*	*2756*	*987*	*2608*
(1) High Search	0.0839	0.0751	0.0834	0.0721
	(0.000)	(0.000)	(0.000)	(0.000)
observations	*3401*	*9706*	*3365*	*9400*
Difference-in-Differences	No	✓	No	✓
Controls	No	No	✓	✓

Notes:

1. The basic sample are claimants non-employed in wave 1. People who end up in employment in wave 2 (whether they stop claiming or not) are dropped. Notes to table 1.1 apply here.

Anyway, if we run a regression pooling observations of groups 1, 2 and 3 we get a DiD of 14.2%, which is almost twice as large as the coefficient for group 1 and statistically different (also regressions with controls reveal a significant difference in the coefficients).

Table 1.3 shows similar results when we split the sample by number of search methods used in the past 4 weeks. When looking at column 2 we observe a significant and quite large treatment effect for the 5 groups with lowest numbers, while it gets smaller and not significant at a 5% level for claimants who exerted 5, 6 or 7 search methods. Surprisingly, the treatment effect for claimants who reported the highest number of search methods is significant and quite large, and this definitely clashes with our theoretical predictions. This result may be partially explained by misleading responses when interviewed. In fact, people are asked to report out of 12 search methods which ones they adopted, so claimants who answer they adopted most, if not all, of them, may just trying to emphasize their search effort while

Table 1.3
Impact of JSA on claimant outflow by number of search methods in wave 1

Average Treatment Effect on:	**1**	**2**	**3**	**4**
0	0.1230	0.1249	0.1221	0.1099
	(0.001)	(0.006)	(0.001)	(0.021)
observations	*774*	*2106*	*764*	*1974*
1	0.0848	0.2611	0.1066	0.2465
	(0.211)	(0.008)	(0.101)	(0.016)
observations	*161*	*407*	*143*	*348*
2	0.1585	0.1379	0.1604	0.1297
	(0.003)	(0.037)	(0.002)	(0.054)
observations	*263*	*731*	*257*	*669*
3	0.1118	0.1244	0.1220	0.1398
	(0.001)	(0.009)	(0.000)	(0.003)
observations	*450*	*1357*	*446*	*1263*
4	0.0926	0.0864	0.0847	0.0777
	(0.002)	(0.025)	(0.004)	(0.041)
observations	*581*	*1682*	*574*	*1635*
5	0.0711	0.0167	0.0735	0.0179
	(0.007)	(0.586)	(0.002)	(0.545)
observations	*706*	*2009*	*697*	*1977*
6	0.1041	0.0580	0.1044	0.0587
	(0.000)	(0.071)	(0.000)	(0.060)
observations	*771*	*2159*	*765*	*2138*
7	0.0706	0.0660	0.0613	0.0482
	(0.022)	(0.091)	(0.024)	(0.178)
observations	*478*	*1356*	*473*	*1337*
8+	0.0397	0.1366	0.0680	0.1375
	(0.376)	(0.037)	(0.126)	(0.026)
observations	*216*	*655*	*199*	*649*
LOW SEARCH vs HIGH SEARCH				
(0,1,2,3,4) Low Search	0.1168	0.1208	0.1155	0.1148
	(0.000)	(0.000)	(0.000)	(0.000)
observations	*2229*	*6283*	*2197*	*5899*
(5,6,7,8+) High Search	0.0796	0.0544	0.0810	0.0528
	(0.000)	(0.004)	(0.000)	(0.004)
observations	*2171*	*6179*	*2154*	*6109*
Difference-in-Differences	No	✓	No	✓
Controls	No	No	✓	✓

Notes:

1. The basic sample are claimants non-employed in wave 1. People who end up in employment in wave 2 (whether they stop claiming or not) are dropped. Notes to table 1.1 apply here.

this may have not actually been as high as reported. Even in this case, we get very convincing results if we run two regressions pooling observations of groups with, respectively, the lowest and the highest search numbers and we compare treatment effects estimates: the DiD calculated on groups with reported numbers from 0 to 4 is more than two times larger than that computed on groups from 5 to 8+ numbers, and statistically different.

Both of these tables show results consistent with those Manning obtained in similar exercises, and overall they seem to support the view that JSA removed from the claimant count especially people with low levels of search activity[30].

1.8 Search Behaviour and Treatment Effect by Housing Tenure

The message we draw by the empirical analysis of the effect of JSA seems clear cut. The tightening of search requirements had a sizeable impact in moving off benefit non-employed people, but only a negligible portion of them entered employment. Moreover, this "weeding out" effect involved especially those with a low level of search intensity. Now, our purpose is to check whether these results fit our theoretical predictions about both search behaviour and the effect of JSA on the claimant status, with regard to different housing tenure categories. Since the treatment operated in a different way according to search intensity of non-employed, and since our model predicts different search levels for people with different housing tenure, it is natural to test whether the estimated treatment effect differs by housing tenure.

In order to run the empirical analysis on housing tenure we restrict the basic sample dropping few individuals who get housing related benefits[31]. In our model, housing related benefits would work as a reduction of housing costs, implying a lower optimal search intensity, therefore, this element would bias differences in treatment effect among housing tenure categories, should

[30] Of course, both variables we use represent a crude measure of the actual search effort of an unemployed, so it is not surprising that our regressions are not able to capture a continuous relationship between them and the treatment effect. Anyway, we think that these measures are reliable enough, in that it looks like existing a correlation between these measures and not only the abstract concept of the probability of meeting search requirements, as tables 1.2 and 1.3 point out, but also between them and the probability of finding a job. For example, if we split up our sample of non-employed claimants between people who remain in non-employment and people who end up in employment, we observe very different distributions over these search effort's measures. Tables are available upon request.

[31] The LFS provides information about housing related benefits, like housing benefit, which applies to only renters, and council tax benefit or rebate, which can apply to owners too.

not the distribution of benefits be uniform over these categories. In our sample the percentage of individuals getting housing related benefit is anyway very small, around 3%, and renters account for a huge 85% of this quota. Moreover all of these observations regard 1997, so we have observations for neither 1996 nor 1995. Since in our sample mostly renters claim housing related benefits, if we kept these observations we would introduce a bias in differences in treatment effect between renters and both other groups, operating through the 1997 seasonal effect[32].

Table 1.4 shows treatment effect estimates when we run separate regressions for each housing tenure sample. These findings are interesting. Even though the JSA reform had in general a sizeable impact on the claimant outflow, it had no effect on the outright owners' sample. Only mortgagers and renters were affected by the reform and their impact was large: 8 and almost 10 percentage points, respectively. The lower part of the table shows whether differences in treatment effect are statistically significant. While both effects on mortgagers and renters are statistically different from that on owners outright, there is no difference between them.

These figures cannot distinguish claimants who exit the claimant status and remain non-employed, from those who end up in employment. It is interesting to disentangle the general effect accounting for the two flows, and to split up each of them with regard to the housing tenure. Table 1.5 shows the results of this exercise. As we remember from the previous section, JSA had no effect on the flow into employment, on average; we learn now that this effect is not even significant for any one of these categories, neither are there differences between them. According to these results we would expect to find a similar pattern for flow into non-employment and flow into any status, but the left part of the table shows remarkably different results from those general of table 1.4. In fact, we notice an outstanding change in differences in treatment effect between mortgagers and both other categories. The difference in treatment effect between mortgagers and outright owners shrinks from 11.6% to 3.6% and it is no more significant, while the difference between renters and mortgagers increases from 2.8% to 4.9% and it becomes significant at a 10% level. The most striking change regards the decomposition of the effect on mortgagers, which appears quite balanced in size between flow into non-employment and into employment. Even if the effect on flow into employment is not significant, the coefficient of flow into

[32]Given the small number of observations and that these apply only to 1997, we think the most suitable way to prevent this bias is to drop these individuals instead of allowing for a dummy for housing related benefits. We also tried to include these cases plugging in a dummy referring to people who claim or not housing related benefits, but differences in treatment effects by housing tenure categories are largely unaffected.

Table 1.4

TREATMENT EFFECTS ON CLAIMANT OUTFLOW BY HOUSING TENURE: FROM CLAIMANTS NOT-IN-EMPLOYMENT TO ANY ECONOMIC ACTIVITY

DiD by housing tenure	**DiD**	$p > \|z\|$	**obs**
DiD_o	-0.0329	(0.489)	*1762*
DiD_m	0.0802	(0.010)	*4445*
DiD_r	0.0966	(0.000)	*7964*
Differences by housing tenure	**Coefficient**	$p > \|z\|$	**obs**
$DiD_m - DiD_o$	0.1157	(0.053)	*6207*
$DiD_r - DiD_o$	0.1331	(0.010)	*9726*
$DiD_r - DiD_m$	0.0279	(0.437)	*12409*

Notes:

1. $DiD_o = DiD$ over the sample of outright owners, $DiD_m = DiD$ over the sample of mortgagers, $DiD_r = DiD$ over the sample of renters. The basic sample are claimants non-employed in wave 1. The sample contains both "stayers", *i.e.* those who remain non-employed in wave 2, whether they move off the claimant status or not, and "movers", *i.e.* those who end up in employment in wave 2, whether they move off the claimant status or not. Notes to table 1.1 apply here.

2. The upper part of the table reports Difference-in-Differences estimates from three different regressions by housing tenure, where controls are always included.

3. The lower part reports differences in DiD estimates between two housing tenure categories. These estimates come from three regressions which pool observations of two by two categories. Every regression includes all usual variables for the DiD, and also interactions between each of them and a dummy for housing tenure: the difference between DiDs we report is just the coefficient of the triple interaction term between the dummy for housing tenure and the interaction term $jsa * d96$.

Table 1.5

TREATMENT EFFECTS ON CLAIMANT OUTFLOW BY HOUSING TENURE: FROM CLAIMANTS NOT-IN-EMPLOYMENT TO NOT-IN-EMPLOYMENT AND TO EMPLOYMENT

DiD by housing tenure	**To not-in-employment**			**To employment**		
	DiD	$p > \|z\|$	**obs**	**DiD**	$p > \|z\|$	**obs**
DiD_o	0.0059	(0.863)	*1762*	-0.0293	(0.396)	*1762*
DiD_m	0.0417	(0.056)	*4445*	0.0395	(0.149)	*4445*
DiD_r	0.1009	(0.000)	*7964*	-0.0034	(0.789)	*7964*
Differences by housing tenure	**Coefficient**	$p > \|z\|$	**obs**	**Coefficient**	$p > \|z\|$	**obs**
$DiD_m - DiD_o$	0.0364	(0.378)	*6207*	0.0786	(0.146)	*6207*
$DiD_r - DiD_o$	0.0940	(0.031)	*9726*	0.0195	(0.533)	*9726*
$DiD_r - DiD_m$	0.0491	(0.090)	*12409*	-0.0288	(0.215)	*12409*

Notes:

1. Notes to table 1.4 apply here. The outcome variable for the analysis of outflows into non-employment takes 1 only if the individual is neither claimant nor employed in wave 2; it takes 0 in all other cases. The outcome variable for the analysis of outflows into employment takes 1 only if the individual is both non-claimant and in employment in wave 2; it takes 0 in all other cases.

non-employment is still significant, but this latter is now too small to yield a significant difference from that for outright owners.

A first interesting reading can be given of the findings above. Mortgagers account for a relevant portion of movers[33]. Of course, the sample of movers is too small to yield significant differences in treatment effects between mortgagers and both other categories, yet this result may be a symptom of mortgagers being crowded in the upper part of the search intensity distribution. This would not let us conclude that mortgagers exert more effort than others, on average, since if we look at the stayers sample we notice that outright owners are the least prone to be weeded out, suggesting that these may actually have the highest search intensity. We look now into this point more thoroughly, providing descriptive statistics about search intensity.

Tables 1.6 and 1.7 show whether differences in High Search percentages by housing tenure are significant[34]. They represent a broad-brush test of search behaviour's outcomes predicted by our theoretical model. Our model predicts a higher optimal search effort, the higher are housing costs, and the lower is the degree of attachment to the accommodation, so we expect that mortgagers and renters exhibit higher search intensity than outright owners. Moreover, mortgagers should exhibit higher search intensity than renters so long as the "housing cost effect" is larger than the "mobility cost" effect. The latter prediction relies on a specific assumption about housing costs' size of the different housing tenure.

Unfortunately, the LFS lacks measures on housing costs so we cannot test this assumption over the sample we use. Yet we can provide some evidence in support of it using data from the British Household Panel Survey (BHPS)[35]. We draw individual data from the sixth wave, which regards the period from 1st September 1996 to the end of April 1997[36] and we regress the net housing costs variable on a dummy which takes 1 for mortgagers and zero for renters. Results are reported in table 1.8. The coefficient of the dummy *owner* reveals a difference in means of £105, which drops to £60 if we control for differences in the gross household income between mortgagers and renters. In the third regression we plug in the dummy *noemp* and its interaction with *owner* to

[33]The raw percentage of mortgagers in the movers' sample is 46.7%, while they account for a smaller 30.9% in the whole sample.

[34]Search measures refer to wave 1.

[35]The BHPS is an ongoing annual survey which follows any individuals of the original sample collected in 1990, which accounted for about 5,000 British households, making a total of approximately 10,000 adult members (16+). The same individuals are re-interviewed in successive waves and, if they split-off from original households, all adult members of their new households are also interviewed.

[36]Most of the interviews are carried out by the end of December.

Table 1.6
Testing differences in High Search: search categories

STAYERS						
Differences in High Search (HS)	Coefficient	Std. Err.	$p > \|t\|$	[95% Conf.	Interval]	obs.
$HS_m - HS_o$	0.0362	0.0130	0.005	0.0109	0.0617	*4763*
$HS_m - HS_r$	0.0514	0.0084	0.000	0.0348	0.0679	*10584*
$HS_r - HS_o$	-0.0151	0.0121	0.214	-0.0389	0.0087	*8705*

MOVERS						
Differences in High Search (HS)	Coefficient	Std. Err.	$p > \|t\|$	[95% Conf.	Interval]	obs.
$HS_m - HS_o$	-0.0077	0.0171	0.653	-0.0411	0.0258	*1544*
$HS_m - HS_r$	0.0163	0.0132	0.220	-0.0097	0.0422	*2158*
$HS_r - HS_o$	-0.0239	0.0178	0.180	-0.0589	0.0111	*1346*

ALL						
Differences in High Search (HS)	Coefficient	Std. Err.	$p > \|t\|$	[95% Conf.	Interval]	obs.
$HS_m - HS_o$	0.0324	0.0109	0.003	0.0111	0.0537	*6307*
$HS_m - HS_r$	0.0602	0.0072	0.000	0.0461	0.0743	*12742*
$HS_r - HS_o$	-0.0278	0.0104	0.008	-0.0483	-0.0074	*10051*

Notes:

1. The basic sample are claimants non-employed in wave 1. “Stayers” are those who remain non-employed in wave 2, whether they move off the claimant status or not, and “movers” are those who end up in employment in wave 2, whether they move off the claimant status or not. The “All” part of the table pools the two samples. Statistics allow for sample weights.

Table 1.7
Testing differences in High Search: numbers of search methods

STAYERS						
Differences in High Search (HS)	Coefficient	Std. Err.	$p > \|t\|$	[95% Conf.	Interval]	obs.
$HS_m - HS_o$	0.0992	0.0159	0.000	0.0681	0.1303	*4763*
$HS_m - HS_r$	0.0883	0.0105	0.000	0.0677	0.1089	*10584*
$HS_r - HS_o$	-0.0109	0.0146	0.455	-0.0177	0.0394	*8705*

MOVERS						
Differences in High Search (HS)	Coefficient	Std. Err.	$p > \|t\|$	[95% Conf.	Interval]	obs.
$HS_m - HS_o$	0.0506	0.0271	0.062	-0.0025	0.1039	*1544*
$HS_m - HS_r$	0.0492	0.0194	0.011	0.0112	0.0872	*2158*
$HS_r - HS_o$	0.0015	0.0281	0.958	-0.0536	0.0565	*1346*

ALL						
Differences in High Search (HS)	Coefficient	Std. Err.	$p > \|t\|$	[95% Conf.	Interval]	obs.
$HS_m - HS_o$	0.0996	0.0139	0.000	0.0723	0.1269	*6307*
$HS_m - HS_r$	0.1099	0.0092	0.000	0.0919	0.1279	*12742*
$HS_r - HS_o$	-0.0103	0.0131	0.430	-0.0361	0.0154	*10051*

Notes:

1. See table 1.6. Statistics allow for sample weights.

Table 1.8
Difference in net housing costs of mortgagers and renters

Net monthly housing costs	(1)	(2)	(3)	(4)
Owner	105.1 (*6.7*)	60.3 (*8.0*)	34.9 (*10.3*)	32.0 (*11.0*)
Gross household income		0.049 (*0.005*)	0.046 (*0.005*)	0.041 (*0.005*)
Noemp			-63.1 (*10.0*)	-51.7 (*9.9*)
Noemp*Owner			65.4 (*12.7*)	53.8 (*12.4*)
Controls (age, household size, education, region)	No	No	No	✓
Constant	167.0 (*5.9*)	101.8 (*6.8*)	132.0 (*9.9*)	153.8 (*22.2*)
n	6305	6305	6299	6275
R^2	0.05	0.15	0.15	0.23

Notes:

1. Standard errors are in brackets
2. The dependent variable is the monthly housing expenditure: for renters is the gross rent minus rent rebates or allowances, for mortgagers is the last instalment on the mortgage or loan. Owner is a dummy which takes 1 for mortgagers and takes 0 for renters, noemp is a dummy which takes 1 for non-employed and 0 for employed. Controls are age, household size, educational dummies, regional dummies.
3. The gross household income is the sum of incomes from all sources perceived in the last month, before tax and other deductions. This sum is then divided for the McClements scale factor, which allows for the effects of household size and composition on needs when making income comparisons. We use the particular scale factor built for comparisons in incomes before housing costs are deducted.

check wether the difference in housing costs holds also for non-employed people, who represent our main focus in this paper (*noemp*=1 if non-employed, *noemp*=0 if employed)[37]. The out-of-pocket housing expenditure for non-employed mortgagers is on average £100 higher than non-employed renters, and this difference slightly drops to £86 if we add further controls for age, household size, education and region. This finding confirms for Britain that mortgagers face on average significantly higher out-of-pocket housing costs than renters, and supports our assumption with regard to the sample of non-employed we are dealing with. Incidentally, we also note that the difference in housing costs is mostly evident for non-employed, while it is far lower for employed, which is only £32. In relative terms this result is even more strong since housing costs are on average higher for employed than non-employed.

Statistics on search intensity measures of claimants are not wholly consistent with our predictions, yet. The "All" part of tables 1.6 and 1.7 shows the relevant statistics for this analysis. Both of our measures suggest that mortgagers exert, on average, higher search intensity than outright owners and than renters. The former finding confirms theoretical expectations. In order

[37] When we control for the unemployment benefit claimants the difference in housing costs is the same for claimants and non-claimants, but this test is not very reliable since the subset of claimants within the BHPS sample is too small.

to be consistent with the theory, the latter calls for a larger "housing cost" than "mobility cost" effect. Contrary to model's predictions, search measures are not higher for renters than outright owners: both are even slightly lower for renters, though only the first one shows a significant difference.

At this stage, we have all empirical results we need to discuss our view about the housing tenure puzzle. The discussion will evolve by means of two-fold comparisons between the three housing tenure categories: (1) mortgagers versus renters, (2) mortgagers versus outright owners, (3) renters versus outright owners. We think our theoretical framework is able to explain differences in outcomes between mortgagers and renters we observe in the data. Indeed, the latter show that differences in both search intensity measures are significant both among stayers and within the overall sample, while only differences in search methods' numbers are significant among movers. This is straight evidence for mortgagers exerting higher search effort than renters. As a consequence, we argue, the introduction of JSA moved off benefit more renters than mortgagers among those who remained non-employed. Mortgagers were able to insulate themselves from the impact of tighter search requirements either because their search effort was already above the new threshold, or because they found worthwhile to increase it in order to keep on claiming. Regardless of the way, the reason has been the same: housing costs of mortgagers are so much higher than renters that they cannot afford to lose the unemployment benefit. We point out that the difference in housing costs is supposed to be high enough to offset the impact of moving costs, which works in the reverse direction by lowering returns to search for mortgagers[38].

The comparison in outcomes between mortgagers and outright owners is consistent with our theory when looking at search behaviour, but it is not when looking at differences in treatment effect on the claimant outflow. Reported search intensity measures are in fact significantly higher for mortgagers, while the estimated treatment effect on stayers is not higher for outright owners. Since search intensity is significantly different also in the stayers sample, we would expect to observe a higher "weeding out" effect for outright owners. In brief, mortgagers search more than outright owners because they have higher housing costs to cope with and they are more mobile, but this is not reflected into lower probability to be crowded out when search requirements are tightened[39]. Our theoretical model provides a possible solu-

[38]Of course, nothing can assure that housing costs are actually higher for mortgagers in our sample, but existing empirical evidence does support this assumption in general.

[39]One possible explanation for this contradiction could appeal to a different distribution in search intensity between mortgagers and renters whenever we observed thicker tails for mortgagers, but the evidence we have come up with does not support that.

tion to this puzzle in that it allows for a different search behaviour response to the treatment. Insofar, we have ignored the occurrence of a treatment effect also on search activity, but, according to our theoretical model, we expect to observe a group of claimants who react to tightening of search requirements by modifying their optimal search: within this group some claimants will find optimal to increase search in order to meet the higher threshold, while others will find optimal to reduce it. Thus, our comparison in the "weeding out" effect of mortgagers and outright owners would be fully consistent with our theoretical predictions as long as we observed a higher (and positive) treatment effect on search activity of outright owners. We will explore this later.

Finally, our theoretical model fails in predicting a higher search intensity for renters than owners outright. Differences in search intensity measures between these groups are never significant but in the overall sample for the first variable, where search is even higher for outright owners. Moreover, coefficients are generally larger, though not significant, for this category. Both housing costs' and moving costs' effects look like not operating in this comparison, since they should push for an increase in search incentives of renters. Anyway, in spite of a similar search effort, renters have been strongly affected by the stricter search rules, while outright owners avoided entirely their impact (see table 1.5, left part)[40]. We may sort it out if in turn we observed a higher treatment effect on search activity of outright owners. This issue may account for the large observed differences in the "weeding out" effect, but it could not explain why renters and outright owners exert a similar search activity although the former have to cope with higher housing and moving costs.

According to comparisons between DiD estimates of different housing tenure categories, outright owners seem more able at avoiding the effect of JSA than it may be gathered by the search activity distribution in wave 1. In fact, outright owners search less than mortgagers but this is not reflected in a higher treatment effect for the former, and the treatment effect on renters is far higher than that on outright owners despite no differences in search activity. One explanation of these findings could appeal to a different variation in search activity as a response to JSA. For example, outright owners may have stronger incentives than other categories to increase their search efforts in order to keep on claiming, and this should show in a higher estimate of the treatment effect on search intensity.

In tables 1.9 and 1.10 we report estimates of the average treatment effect

[40] As in the previous comparison, no major distributional effects seem explain this contradiction.

Table 1.9
THE IMPACT OF JSA ON SEARCH ACTIVITY: SEARCH CATEGORIES

Samples	Coefficient	$p > \|z\|$	obs.
Whole Sample	0.01	0.723	*11971*
Outright Owners	0.10	0.280	*1436*
Mortgagers	0.07	0.242	*3296*
Renters	-0.03	0.365	*7239*

Notes:

1. The base sample is made of only stayers since we cannot observe search measures for people who are employed. We consider claimants who both leave and remain in the claimant pool.

2. The dependent variable is the variation in the search variable between wave 2 and wave 1 for each group. Since the search variable can take four values which are the integers in the range from 1 to 4, the dependent variable can take all integers in the range from -3 to 3. The values of the search variable are recoded so that higher numbers mean higher search intensity, thus a positive coefficient means an increase in search intensity.

3. Results come from an ordered probit model. Given that the DiD coefficient is very robust to the inclusion of controls, these are not included in order to avoid the problem of choosing proper values of them to compute coefficients. We estimate the parameters of the index model by means of an ordered probit and then we compute the expected values of the dependent variable conditioning for being part either of the treatment group or of the control group and for each of the three years. The matrix of regressors here is $X_i = [d96_i \ d97_i \ jsa_i \ jsa_i * d96_i]$, so, for example, the expected values of y_i for the treatment group in 1996 is computed conditioning on $X_i = [1 \ 0 \ 1 \ 1]$. We estimate the average treatment effect of JSA subtracting from the difference in expected values for 1996 the weighted average of differences for 1995 and 1997.

Table 1.10
THE IMPACT OF JSA ON SEARCH ACTIVITY: NUMBER OF SEARCH METHODS

Samples	Coefficient	$p > \|z\|$	obs.
Whole Sample	0.16	0.029	*11971*
Outright Owners	0.40	0.054	*1436*
Mortgagers	0.37	0.013	*3296*
Renters	0.02	0.821	*7239*

Notes:

1. Notes to table 1.9 apply here.

of JSA on both search measures. Methodology is identical to the previous analysis, except that the dependent variable is now the difference between search intensities in both waves. For example, when we use the four-fold categorization of search activity, we build a variable whose range is made of all integers between -3 and 3, where 3 indicates a transition from "do not want work" in wave 1 to "search in last week" in wave 2. We estimate the coefficients of the index model by means of an ordered probit, then we compute the effect of JSA as Difference-in-Differences in the expected values of the dependent variable for different groups. When we focus on the whole sample, JSA seems to have a positive effect only on the number of search methods, though the coefficient is tiny. Anyway, as we pointed out in section 1.3, the expected effect on the average search intensity of claimants in wave 1 is ambiguous: some may increase search intensity in order to stick to new rules, while others may reduce it and stop claiming[41]. When we focus on specific sub-samples by housing tenure, we do not obtain any significant coefficient using the first variable, but results for the number of search methods are in part consistent with what we expected. In fact, table 1.10 shows that the effect for renters is zero, while it is positive and significant for outright owners and mortgagers[42]. Even though the estimated effects are small, this table suggests that JSA increased the number of search methods of both owners' categories while it had no effect on renters. These results are in line with the estimated difference in the "weeding out" effect for outright owners and renters, since they suggest that the former may have been able to avoid the effect of new requirements just by increasing their search effort. Anyway, the evidence provided overall by tables 1.9 and 1.10 for this case is mild and it is still unexplained why the "weeding out" effect was not higher for outright owners than mortgagers.

1.9 Conclusion

This paper has investigated the relation between the optimal search intensity and the housing tenure exploiting the variation from the UK Jobseeker's Allowance reform of 1996. The introduction of JSA brought many changes

[41]Manning Manning [2009] explores more in depth this issue first focusing on claimants in wave 2 and then looking at distributional effects. Anyway he does not find compelling evidence for a clear effect of JSA on the search activity of anyone within the distribution.

[42]We recall that we are using non-employed claimants in wave 1 as base sample, so some of them may have moved off benefit and thus reduced their search intensity in wave 2. What we are interested in, is not that the average effect was positive for specific categories, but that it was *larger* for some of them.

to the unemployment benefits scheme but the most significant was represented by the substantial increase in job search requirements for the eligibility. Existing evaluation of this reform has accounted for a strong "weeding out" effect, which means that a major impact of the reform was directed to claimants with low search effort who moved off benefit without finding a job. Our empirical analysis largely confirms this view and on top of this it points out that housing tenure matters in shaping the effect of this reform.

To investigate the impact of tighter job search requirements we use a simple search model, where we introduce moving and housing costs in order to capture the two different channels through which the degree of attachment to the accommodation affects search behaviour. We make use of this theoretical framework to compare outcomes of three distinct housing tenure categories: outright owners, mortgagers and renters. The existing literature we refer to has usually focussed on comparisons between renters and owners in general, while only sometimes it has pointed out some peculiarities of mortgagers. We provide a general framework within which it is possible to analyze separately and then to compare behaviour and outcomes of these three distinct groups.

Using a Difference-in-Differences approach we investigate these insights by means of a dataset drawn from the Labour Force Survey. Treatment effect estimates on the claimant outflows are strongly related to housing tenure, and we argue that this result is driven by differences in search behaviour, which in turn is affected by housing costs and mobility. Our analysis sheds further light on the comparison between mortgagers and renters as it reveals that higher search intensity has prevented mortgagers to be crowded out of the claimant stock as much as renters has been.

The role of outright owners seems less clear cut, instead. Search intensity measures provided by our dataset report higher numbers for outright owners than we may expect given both moving and housing costs' effects. Also, they are the category with the lowest estimated treatment effect on claimant outflow, but we would expect, according to their reported search intensity, an impact higher than that for mortgagers and similar to that for renters. Anyway, we do not think that these failings undermine the validity of our theoretical foundations. Rather, we interpret these as signals of a missing element of the puzzle, whose investigation is left for further research.

Chapter 2

The Puzzle of Job Search and Housing Tenure: a Reconciliation of Theory and Empirical Evidence

2.1 Introduction

During the nineties Andrew Oswald[1] studied the relationship between the unemployment rate and the homeownership rate for OECD countries and he argued that homeownership was one of the major culprits of high unemployment given the observed strong positive correlation. The most influential microeconomic interpretation of this finding has focused on the supposed lower job finding rates of unemployed people who own the home. In fact, since homeownership hampers propensity to move for job reasons, homeowners should have longer unemployment durations than otherwise comparable renters[2]While there is abundant evidence supporting the first element of this rationale[3], several empirical studies have found no support for the second, and in most cases even the opposite, that is lower unemployment durations for homeowners[4].

[1]See Oswald [1996], Oswald [1997] and Oswald [1999].

[2]Other interpretations explored in the literature refer to the effect of homeownership on the probability to be employed and on the employed's probability to become unemployed.

[3]See Van den Berg and Van Vuuren [1998], Henley [1998], Munch et al. [2006], Van Vuuren [2009] and Battu et al. [2008].

[4]See Goss and Phillips [1997], Coulson and Fisher [2002], Flatau et al. [2003], Munch et al. [2006], Van Vuuren [2009] and Battu et al. [2008].

The existing literature[5] has provided two possible explanations for this paradox. The first one looks at the different effect of mobility costs on job search behaviour between the local and the non local labour market. The second has focused on the importance of making distinctions among homeowners, in particular between mortgage-holders and outright owners, and among renters, in particular between social and private renters, since results can be very different by subgroups.

With regards to the first explanation, Munch et al. [2006] point out that the lower propensity of owner-occupiers to move for job reasons does not necessarily imply that they have lower exit rates from unemployment. In fact, homeowners may have higher reservation wages for jobs which require a residential move, but also lower reservation wages for jobs which do not, so that job finding rates in the local labour market may be as high as to offset the lower rate for jobs in a distant area. Whether or not the total job finding rate is lower for homeowners is just, they argue, an empirical matter which depends on the magnitude of these two opposite effects. However, Van Vuuren [2009] shows that in the model of Munch et al. [2006] the hazard rate out of unemployment should be always lower for homeowners, but in a special case, that is when homeowners can receive unemployment benefit only for a fixed period, while renters never run out of it. Even in this case, the theoretical effect of homeownership is ambiguous.

The second explanation takes into account some controversies with the original Oswald's definitions of residential status. At first, when comparing homeowners and renters one should account for the role of mortgage payments in confounding the relation between mobility costs and job search behaviour. Committed housing expenditures such as the mortgage and the rent should increase exit rates from unemployment through higher pressure to return to work. Given that outright owners do not cope with these expenses and that rent payments should be on average lower than mortgage payments, this interpretation is consistent with the observed unemployment duration for mortgage-holders being typically the lowest among different residential statuses[6]. Second, also social and private renters do not behave the same way[7]. Social renters may have lower mobility due to lock-in effects sim-

[5]See Rouwendal and Nijkamp [2008], and Havet and Penot [2010] for a survey of this literature.

[6]Rouwendal and Nijkamp [2008] and Arzilli and Morescalchi [2011] provide evidence for mortgagers having higher housing costs than renters on average.

[7]Social housing is a form of housing tenure in which the property is owned by Local Authorities or by Housing Associations, usually with the aim of providing accommodation at below-market rent or even rent-free.

ilar to those which hamper homeowners mobility[8]. For example, long waiting lists, security of tenure and the restricted transferability within social housing may cause social renters to be less prone to move for job reasons than private renters[9]. Moreover, also the housing costs effect differs by tenant status since social renters pay below-market rent.

Our contribution consists in refining the empirical analysis by taking into account simultaneously housing costs and the distinctions among all the relevant residential statuses, with the purpose of bringing out the empirical effect of the main mechanism underlying the Oswald's hypothesis[10]. We start by building a theoretical framework with endogenous search which models the Oswald's effect as in Munch et al. [2006]. Homeownership rises the reservation wage and reduces the optimal search in the non local labour market, but has opposite effects locally. In line with the findings of Van Vuuren [2009], our model overcomes the theoretical ambiguity of the model of Munch et al. [2006] in that the negative effect of homeownership on non local outcomes turns out to be unambiguously stronger. In particular, the endogenisation of search allows us to evaluate the marginal impacts of homeownership on both local and non local optimal search and then to compare them. As a net effect, homeownership reduces the optimal search and the job finding rate, which means that the Oswald's hypothesis is still verified.

In the theoretical model we ignore the role of housing costs but we are aware that the effect of housing costs may revert the Oswald's outcome empirically. Hence, the comparison in the model is intended to be between owners with no mortgage payments and private renters. We do allow for housing costs in the empirical analysis, where observed outcomes are confounded by this effect.

We carry out our empirical analysis using data from the British Household Panel Survey (BHPS), which provides job search effort's measures for unemployed. We use these categorical measures as dependent variables and we regress them on the housing tenure dummies using a random effects probit and controlling for several individual (or household) characteristics. The BHPS provide us with a measure for housing costs, which we plug in the regression in order to purge search differentials of the housing costs effect. Moreover, we control for different tenure statuses among homeowners, be-

[8] See Flatau et al. [2003], Battu et al. [2008], McCormick [1983], Hughes and McCormick [1981] and Hughes and McCormick [1987].

[9] Hughes and McCormick [2000] in a later work argue that these effects may have lessened.

[10] Munch et al. [2006], Van Vuuren [2009] and Battu et al. [2008], which represent the most updated analysis of the Oswald's hypothesis have disregarded one or both of these issues.

tween mortgage and outright, and among renters, between social and private.

Our focus is on the job search intensity, unlike the empirical literature, which typically has tested the Oswald's hypothesis looking at the hazard rate. The probability that an unemployed worker will find a job can be decomposed as the product of two probabilities: the probability of receiving a job offer and the probability of this offer being accepted. Job search effort is positively related to the first, and to the second through the negative relation with the reservation wage and maybe also through the positive effect of search on the quality of job offers. Whatsoever, the relationship between search and the hazard rate is always positive, so nothing is lost whenever we state the Oswald's hypothesis in terms of a negative effect of homeownership on the search intensity.

The empirical evidence we provide is consistent with the theory outlined. In fact, the Oswald's hypothesis is confirmed in the sense that the search differential between outright owners and private renters, the two extremes of mobility, is negative, even whenever the housing costs effect is netted out. More in details, though search intensity is usually higher for homeowners than renters, this difference is no more meaningful when we refine the residential status definitions, since it is clear that mortgagers search more than outright owners and private renters more than social. Anyway, the negative search differential between outright owners and private renters manifests itself through the owners' lower attachment to the labour market, while no statistical difference in search intensity measures is found looking only at non-employed workers who state to have been looking for work in the last four weeks. Moreover, housing costs work in the expected direction albeit with weak impact, and never revert the sign of search differentials.

Our findings are very similar to those of Flatau et al. [2003] who, examining data for Australia, provide strong evidence that the counter-Oswald results are due to the behaviour of leveraged owners and social tenants. In fact, while they find that homeowners have higher probability to exit unemployment than renters, when they compare outright owners with private renters they find strong evidence, particularly for females, that outright owners are slower to become reemployed than private renters.

2.2 The Theoretical Model

In this section we present a simplified model of job search with endogenous search effort and exogenous wage offer distribution[11]. In order to investi-

[11]See Mortensen [1986] for the background of search modeling and Manning [2009] for a similar version.

gate the Oswald's explanation for the impact of homeownership on search behaviour, we ignore housing costs and we just focus on moving costs[12].

The effect of homeownership is captured by allowing for two labour markets which differ geographically as in Munch et al. [2006]. By definition, the local labour market is the region in which a worker can take a job without moving. The non local labour market is the rest of the economy. Workers can have a job only in the region they live in, so if they want to accept a job offer in the other region they have necessarily to move, *i.e.* we do not allow commuting. This setup captures the idea that there exist two distinct reservation wages, one for the local labour market and one for jobs outside, which will diverge when moving entails a cost.

To model the lifetime utility of the employed as simple as possible without affecting our main results, we rule out on-the-job search and we set to zero the separation rate, that is:

$$V^E(w) = \frac{w}{\rho}, \tag{2.1}$$

where w is the wage and ρ is the discount rate.

The unemployed can increase the job offer arrival rate through search activity at the cost of an utility loss of C^s. This cost and the job offers arrival rate differ between the local and non local labour market uniquely for the search effort exerted in each of them, which we call s_l and s_n respectively. We assume that the total cost of search function is additive in the costs of search in the two labour markets, *i.e.* $C^s = c(s_l) + c(s_n)$, where $c' > 0$ and $c'' > 0$[13]. The arrival rate of job offers in the local and non local labour markets are respectively $\alpha(s_l)$ and $\alpha(s_n)$, where $\alpha' > 0$ and $\alpha'' < 0$. Wage offers are sampled from the *c.d.f.* $F(w)$ which we assume identical for both markets. When choosing how to distribute search effort in the two labour markets, the unemployed must take into account the cost of moving, that is the cost he would incur if he found and accepted a job in the other region. The difference in this cost for homeowners and renters is precisely what captures the Oswald's effect in the model. For simplicity, we set this cost to zero for renters since we need only that it is higher for homeowners. The value equation for the unemployed renter is:

$$\rho V^U = b - c(s_l) - c(s_n) + (\alpha(s_l) + \alpha(s_n)) \int_{w_r^*} (V^E(w) - V^U)\, dF(w), \tag{2.2}$$

[12]See Arzilli and Morescalchi [2011] for an endogenous search model which includes also the housing costs effect. When comparing search outcomes of homeowners (outright owners) and renters in this set-up, housing costs simply reinforces the negative effect of homeownership due to moving costs.

[13]The assumptions on c yield a standard convex total cost of search function. The model may be enriched by allowing higher costs of search in the non local labour market, but this is irrelevant for the comparison between search behaviours of homeowners and renters.

where w_r^* is the reservation wage for the renter and b is the unemployment benefit. The unemployed sets the reservation wage and the search effort levels in order to maximise his lifetime utility. As the renter can move without costs, he is indifferent between accepting a job in the local labour market or outside, so the reservation wage is the same in both. Given risk neutrality, the reservation wage will be such that $w_r^* = \rho V^U$. Replacing this and equation 2.1 in equation 2.2, and rearranging we have:

$$w_r^* = b - c(s_l) - c(s_n) + \frac{(\alpha(s_l) + \alpha(s_n))}{\rho} \int_{w_r^*} (w - w_r^*) \, dF(w). \tag{2.3}$$

Differentiating 2.3 with respect to s_l and s_n we get the first order conditions for the maximum:

$$c'(s_l^*) = \alpha'(s_l^*)A, \tag{2.4}$$

$$c'(s_n^*) = \alpha'(s_n^*)A, \tag{2.5}$$

where we set $A = \rho^{-1} \int_{w_r^*} (w - w_r^*) \, dF(w)$. It is easy to show that the unemployed renter will exert the same search effort in both markets. In fact, from 2.4 and 2.5 we get $c'(s_l^*)/\alpha'(s_l^*) = c'(s_n^*)/\alpha'(s_n^*)$, which is true only when $s_l^* = s_n^*$[14]. In this simple setup, with no additional costs of search far from home and no costs of moving, the renter is indifferent between search locally and in a distant area.

If the unemployed is homeowner he has to consider the cost m which he would run into if he accepted a job in the non local labour market[15]. The discounted lifetime utility for the unemployed homeowner is:

$$\rho\tilde{V}^U = b - c(s_l) - c(s_n) + \alpha(s_l) \int_{w_l^*} \left(\frac{w}{\rho} - \tilde{V}^U \right) dF(w) + \alpha(s_n) \int_{w_n^*} \left(\frac{w}{\rho} - \tilde{V}^U - m \right) dF(w), \tag{2.6}$$

where we have already replaced $V^E(w) = w/\rho$. Now, we have two distinct levels of the reservation wage, one for each of the two markets. The reservation wage for the local labour market is $w_l^* = \rho\tilde{V}^U$, while the reservation wage for the job offers outside the local labour market is $w_n^* = \rho\tilde{V}^U + \rho m$: to accept a job offer which requires a move, the unemployed homeowner needs a compensation

[14] Alternatively, since s_l and s_n have the same impact on the cost and the revenue for the unemployed renter, we may maximise with respect to the total search effort s subject to the constraint $s = s_l + s_n$.

[15] The cost of moving will be the same whether the homeowner moves to another owner-occupied housing or to a rental accommodation, hence, this model captures only the lower mobility due to the cost of selling a home. We may enrich the model by differentiating between moves to a rental and to an owner-occupied accommodation (with higher costs for the latter), but this higher complexity will not come with any benefits for our purposes.

for the cost of moving. Equation 2.6 can be rewritten as

$$w_l^* = b - c(s_l) - c(s_n) + \frac{\alpha(s_l)}{\rho}\int_{w_l^*}(w - w_l^*)\ dF(w) + \frac{\alpha(s_n)}{\rho}\int_{w_n^*}(w - w_l^* - \rho m)\ dF(w). \tag{2.7}$$

The optimal search levels in the two markets are determined by the following first order conditions:

$$c'(s_l^*) = \alpha'(s_l^*)B, \tag{2.8}$$

$$c'(s_n^*) = \alpha'(s_n^*)C, \tag{2.9}$$

where we set $B = \rho^{-1}\int_{w_l^*}(w - w_l^*)\,dF(w)$ and $C = \rho^{-1}\int_{w_n^*}(w - w_n^*)\,dF(w)$. Since $w_l^* < w_n^*$, $B > C$ holds for any w. From 2.8 and 2.9, $B > C$ implies $c'(s_l^*)/\alpha'(s_l^*) > c'(s_n^*)/\alpha'(s_n^*)$. Given that c is convex and α is concave, the latter inequality implies $s_l^* > s_n^*$. Unlike the renter, for the homeowner is optimal to search harder in the local labour market than outside.

Up to this point we have found that the renter chooses the same level of optimal search in both markets, which we indicate as s_r^*, while the homeowner sets $s_l^* > s_n^*$. To identify the effect of housing tenure we compare now the search effort of the renter and the homeowner in both markets. A first result is stated in the following proposition:

Proposition 1 $s_l^* > s_r^* > s_n^*$.

Proof:

(a) $s_l^* > s_r^*$. To prove this we calculate the derivative of s_l^* with respect to m by means of the implicit function theorem. At first, we need to calculate dw_l^*/ds_l and dw_l^*/dm and evaluate these functions at the optimum.

Differentiating equation 2.7 with respect to w_l^* and s_l we obtain:

$$\frac{dw_l^*}{ds_l} = \frac{\rho^{-1}\alpha'(s_l)\int_{w_l^*}(w - w_l^*)\,F'(w)dw - c'(s_l)}{1 + \rho^{-1}\alpha(s_l)[1 - F(w_l^*)] + \rho^{-1}\alpha(s_n)[1 - F(w_n^*)]}. \tag{2.10}$$

It can be easily shown that this derivative is zero for $s_l = s_l^*$, since the numerator is zero as follows directly from the first order condition for s_l^*. Moreover $dw_l^*/ds_l > (<)0$ if $s_l < (>)s_l^*$. Differentiating equation 2.7 with respect to w_l^* and m we obtain:

$$\frac{dw_l^*}{dm} = -\frac{\alpha(s_n)[1 - F(w_l^* + \rho m)]}{1 + \rho^{-1}\alpha(s_l)[1 - F(w_l^*)] + \rho^{-1}\alpha(s_n)[1 - F(w_n^*)]} < 0. \tag{2.11}$$

This derivative is negative for any value of s_l. Intuitively, as the moving cost increases, the reservation wage in the local labour market for the homeowner drops since the acceptation of a job far from home comes with a lower expected surplus. We rewrite now the first order condition for s_l^* as

$$\Phi(s_l^*, m) = c'(s_l^*) - \frac{\alpha'(s_l^*)}{\rho}\int_{w_l^*(s_l^*,m)}[w - w_l^*(s_l^*, m)]F'(w)dw = 0. \tag{2.12}$$

Applying the implicit function theorem we have[16]

$$\frac{ds_l^*}{dm} = -\frac{\Phi_m}{\Phi_{s_l^*}} = -\frac{\rho^{-1}\alpha'(s_l^*)\int_{w_l^*}\left(\frac{dw_l^*}{dm}\right)F'(w)dw}{c''(s_l^*)-\rho^{-1}\alpha''(s_l^*)\int_{w_l^*}(w-w_l^*)F'(w)dw} > 0. \tag{2.13}$$

As expected, the higher is the moving cost, the higher is the search of the homeowner in the local labour market. Since the relation between s_l^* and m is positive for any value of m, this will be true in particular when $m = 0$, that is when the optimal search locally (and non locally) is $s_r^* = s_l^*$. Thus, when m increases from zero to a positive number, which captures a shift from tenant to owner status, the unemployed will increase search in the local labour market from s_r^* to s_l^*.

(b) $s_n^* < s_r^*$. As in the previous case we calculate the derivatives dw_n^*/ds_n and dw_n^*/dm and we study the sign of ds_n^*/dm. Differentiating the equation $w_n^* = w_l^* + \rho m$ with respect to w_n^* and s_n we obtain:

$$\frac{dw_n^*}{ds_n} = \frac{\rho^{-1}\alpha'(s_n)\int_{w_n^*}(w-w_n^*)F'(w)dw - c'(s_n)}{1+\rho^{-1}\alpha(s_l)[1-F(w_l^*)]+\rho^{-1}\alpha(s_n)[1-F(w_n^*)]}. \tag{2.14}$$

Given the first order condition for s_n^*, this derivative is zero when $s_n = s_n^*$. Moreover $dw_n^*/ds_n > (<) 0$ if $s_n < (>) s_n^*$. Differentiating with respect to w_n^* and m we obtain:

$$\frac{dw_n^*}{dm} = \frac{\rho+\alpha(s_l)[1-F(w_l^*)]}{1+\rho^{-1}\alpha(s_l)[1-F(w_l^*)]+\rho^{-1}\alpha(s_n)[1-F(w_n^*)]} > 0. \tag{2.15}$$

This derivative is positive for any value of s_n. A rise in the moving cost requires a higher wage to induce the homeowner to move for a job. We rewrite the first order condition for s_n^* as

$$\Psi(s_n^*, m) = c'(s_n^*) - \frac{\alpha'(s_n^*)}{\rho}\int_{w_n^*(s_n^*,m)}[w - w_n^*(s_n^*,m)]F'(w)dw = 0. \tag{2.16}$$

[16] When computing $\Phi_{s_l^*}$ we remark that

$$\Phi_{s_l^*} = c''(s_l^*) - \frac{\alpha''(s_l^*)}{\rho}\int_{w_l^*}(w-w_l^*)F'(w)dw + \frac{\alpha'(s_l^*)}{\rho}\int_{w_l^*}\left(\frac{dw_l^*}{ds_l}\right)F'(w)dw =$$
$$= c''(s_l^*) - \frac{\alpha''(s_l^*)}{\rho}\int_{w_l^*}(w-w_l^*)F'(w)dw > 0,$$

where the simplification is allowed given that $dw_l^*/ds_l = 0$ when $s_l = s_l^*$. This derivative is clearly positive since $c'' > 0$, $\alpha'' < 0$, $F' > 0$. Also, Φ_m is negative since $\alpha' > 0$, $F' > 0$ and $dw_l^*/dm < 0$.

Applying the implicit function theorem we have[17]

$$\frac{ds_n^*}{dm} = -\frac{\Psi_m}{\Psi_{s_n^*}} = -\frac{\rho^{-1}\alpha'(s_n^*)\int_{w_n^*}\left(\frac{dw_n^*}{dm}\right)F'(w)dw}{c''(s_n^*) - \rho^{-1}\alpha''(s_n^*)\int_{w_n^*}(w - w_n^*)F'(w)dw} < 0. \tag{2.17}$$

The relation between s_n^* and m is negative for any value of m, thus, when m increases from zero to a positive number the unemployed will reduce search in the non local labour market from s_r^* to s_n^*.

The rationale of this proposition is straightforward. When the unemployed has to face a cost of moving to accept a job offer far from home, he search less outside and centres his effort on the local area to reduce the probability of incurring this cost[18]. Whether or not the homeowner search in general less than the renter depends on the balance of these two opposite effects, whose net result can be identified within this framework, unlike in Munch et al. [2006]. Before to tackle this point, we show the relations between the reservation wages of homeowners and renters in both markets, which is the counterpart of Proposition 1. That is stated in the following proposition[19]:

Proposition 2 $w_l^* < w_r^* < w_n^*$.

Proof: We only need to look at the first order conditions 2.4, 2.5, 2.8, 2.9 and to remark the result of Proposition 1 and that $c'(\cdot)/\alpha'(\cdot)$ is an increasing function.

$$w_l^* < w_r^* \longleftrightarrow B > A \longleftrightarrow \frac{c'(s_l^*)}{\alpha'(s_l^*)} > \frac{c'(s_r^*)}{\alpha'(s_r^*)} \longleftrightarrow s_l^* > s_r^*,$$

$$w_n^* > w_r^* \longleftrightarrow C < A \longleftrightarrow \frac{c'(s_n^*)}{\alpha'(s_n^*)} < \frac{c'(s_r^*)}{\alpha'(s_r^*)} \longleftrightarrow s_n^* < s_r^*.$$

In order to compare the total search level of the homeowner and the renter, that is the impact of housing tenure on search, we just have to compare the sum of search levels in the local and in the non local market for both. The search level of the homeowner will be higher, equal or lower than that of the renter as far as $s_l^* + s_n^* \gtreqless 2s_r^*$. The only thing which differentiates

[17] In the computation of $\Psi_{s_n^*}$ we make use of the fact that $dw_n^*/ds_n = 0$ when $s_n = s_n^*$. The sign of Ψ_m is positive since $dw_n^*/dm > 0$.

[18] Commuting would be another mechanism which implies that homeowners may search locally more than renters. Given the higher costs of moving, homeowners would be willing to commute longer so that their local labour market would be larger. Anyway, no major changes would take place for our purposes if we allowed for commuting in this set-up.

[19] This result is the same of what Munch et al. [2006] obtain in a model of search very similar to ours except that they do not endogenise search effort.

the homeowner from the renter is the moving cost, so we may expect that an increase of the moving cost from zero to a positive number, which represents just a shift from renter to owner tenure, comes with a reduction of the total search. The rationale is that, although this cost is incurred only if the homeowner actually moves, it increases the *expected* cost of search, which in turn makes unemployment more valuable. Thus, despite the incentive to search harder locally, this expected cost has to be covered by an extra reduction in the non local search (from s_r^* to s_n^*) with respect to what would be needed to compensate the increase in the local search (from s_r^* to s_l^*). The following proposition supports this insight:

Proposition 3 $2s_r^* > s_l^* + s_n^*$.

Proof: Since we cannot derive a closed form for the optimal search levels, the device of the demonstration is to study the derivatives of s_l^* and s_n^* with regards to m evaluated at $m = 0$. In fact, when $m = 0$ the optimal search is identical in both the local and non local markets, so by deriving the optimal search levels with respect to m we can compare the magnitude of the (opposite) marginal variations, which can be interpreted simply as "marginal" differences in each market's search levels between homeowner and renter. Then we just need to show that the magnitude of the marginal decrease in the non local search is higher, in absolute terms, than the marginal increase in the local search. Let's look at equations 2.13 and 2.17 which represent the marginal variations of the homeowner's local and non local search respectively. When $m = 0$, we have $s_l^* = s_r^* = s_n^*$ thus the two derivatives are identical expect for the derivatives of the reservation wage in the numerator, which have opposite sign:

$$\frac{ds_l^*}{dm}(m=0) = -\frac{\rho^{-1}\alpha'(s_r^*)\int_{w_r^*}\left(\frac{dw_l^*}{dm}(m=0)\right)F'(w)dw}{c''(s_r^*) - \rho^{-1}\alpha''(s_r^*)\int_{w_r^*}(w-w_r^*)F'(w)dw}, \tag{2.18}$$

$$\frac{ds_n^*}{dm}(m=0) = -\frac{\rho^{-1}\alpha'(s_r^*)\int_{w_r^*}\left(\frac{dw_n^*}{dm}(m=0)\right)F'(w)dw}{c''(s_r^*) - \rho^{-1}\alpha''(s_r^*)\int_{w_r^*}(w-w_r^*)F'(w)dw}. \tag{2.19}$$

Making use of equations 2.11 and 2.15 we can evaluate the derivatives of the reservation wages at the optimal values of search when $m = 0$:

$$\frac{dw_l^*}{dm}(s_r^*, m=0) = -\frac{\alpha(s_r^*)[1-F(w_r^*]}{1+\rho^{-1}\alpha(s_r^*)[1-F(w_r^*)] + \rho^{-1}\alpha(s_r^*)[1-F(w_r^*)]}, \tag{2.20}$$

$$\frac{dw_n^*}{dm}(s_r^*, m=0) = \frac{\rho+\alpha(s_r^*)[1-F(w_r^*)]}{1+\rho^{-1}\alpha(s_r^*)[1-F(w_r^*)] + \rho^{-1}\alpha(s_r^*)[1-F(w_r^*)]}. \tag{2.21}$$

It is easy to show that $\rho > 0$ implies $\frac{dw_n^*}{dm}(s_r^*, m = 0) > |\frac{dw_l^*}{dm}(s_r^*, m = 0)|$, which in turn implies $|\frac{ds_n^*}{dm}(m = 0)| > \frac{ds_l^*}{dm}(m = 0)$. This means that, for small m, the difference in the non local search between homeowner and renter is higher, in absolute value, than the difference in the local search, that is $s_r^* - s_n^* > s_l^* - s_r^*$, which rearranging is identical to the proposition. This holds for every m so the proposition is proved.

Unlike the model of Munch et al. [2006], but like Van Vuuren [2009], we can make clear predictions also on the whole job finding rates of the homeowner and the renter[20]. The renter's job finding rate is two times $h_r = \alpha(s_r^*)\,[1 - F(w_r^*)]$, which is the common job finding rate for both markets, while the owner's job finding rate is the sum of $h_l = \alpha(s_l^*)\,[1 - F(w_l^*)]$ and $h_n = \alpha(s_n^*)\,[1 - F(w_n^*)]$, which refer respectively to the local and to the non local market. In order to compare job finding rates, we first point out that, given Propositions 1 and 2, $h_l > h_r > h_n$. Thus, unemployed living in owner-occupied accommodation are expected to have a higher exit rate from unemployment towards jobs which require a move, but a lower exit rate towards employment in the local labour market. The main mechanism of the Oswald's hypothesis works in this setup, since homeownership reduces the chances to find an acceptable job far from home by hampering residential mobility. Can we also state that renters have a higher exit rates than homeowners *in general*? This is the case if $2h_r > h_l + h_n$, which again can be showed to be true within this framework. The logic of the demonstration is similar to that of Proposition 3 and relies on its results.

Proposition 4 $2\alpha(s_r^*)\,[1 - F(w_r^*)] > \alpha(s_l^*)\,[1 - F(w_l^*)] + \alpha(s_n^*)\,[1 - F(w_n^*)]$.

Proof: We just need to prove the negative sign of the derivative of $(h_l + h_n)$ with respect to m at the optimal values of search when $m = 0$. Defining

[20] Van Vuuren [2009] shows that in the model of Munch et al. [2006] the hazard rate to exit unemployment for homeowners is unambiguously lower than that for renters. Anyway, in the generalized version of the model Van Vuuren [2009] allows for differences in the unemployment benefit duration between homeowners and renters which brings non-stationarity into the model of Munch et al. [2006]. In particular they assume that homeowners receive benefits for only T periods of unemployment, while renters receive benefits for the whole unemployment spell. This implies a reduction in the relative reservation wages of homeowners from T onwards. As a consequence, though homeowners have higher exit rates from unemployment than renters in the local labour market, the reverse is no more necessarily the case in the non local labour market. Thus, as long as the benefit exhaustion assumption holds, the model of Van Vuuren [2009] yields ambiguous results as in Munch et al. [2006] pointing out the need for empirical research. But whit no unemployment benefit exhaustion, as in the stationary framework of Munch et al. [2006] and in the ours, this indeterminacy is eliminated.

$\frac{dw_l^*}{dm}(s_r^*, m=0) = L^w$, $\frac{dw_n^*}{dm}(s_r^*, m=0) = N^w$, $\frac{ds_l^*}{dm}(m=0) = L^s$ and $\frac{ds_n^*}{dm}(m=0) = N^s$, we have:

$$\begin{aligned}\frac{d(h_l+h_n)}{dm}(s_r^*, m=0) &= \alpha'(s_r^*)\left[1-F(w_r^*)\right]L^s - \alpha(s_r^*)F'(w_r^*)L^w + \\ &\quad + \alpha'(s_r^*)\left[1-F(w_r^*)\right]N^s - \alpha(s_r^*)F'(w_r^*)N^w = \\ &= \alpha'(s_r^*)\left[1-F(w_r^*)\right](L^s+N^s) - \alpha(s_r^*)F'(w_r^*)(L^w+N^w) < 0,\end{aligned} \tag{2.22}$$

where the latter inequality holds since $(L^s + N^s) < 0$ and $(L^w + N^w) > 0$, which are results of Proposition 3.

To conclude, the theoretical section delivers us a clear message: mobility costs imply lower search and exit rates from unemployment for homeowners, thus the local versus non local search explanation is not able *alone* to revert the argument underlying the Oswald effect. In the empirical section 2.4 we will provide some evidence for this by abstracting from the role of housing costs.

2.3 Methodology and Data

We draw our data set from the British Household Panel Survey (BHPS), a nationally representative annual panel survey which has been carried out continuously since 1991. A random sample of around 5,500 households, accounting for around 10,000 adult members, was drawn at the start of the survey, then all residents of those households were traced and re-interviewed each year up to now. Each wave there are flows in and out of the survey, so the panel is highly unbalanced[21]. At time of writing the last wave released refers to 2007, so we have 17 waves at our disposal.

The BHPS contains detailed information about the economic activity status of an individual. We focus on the group of non-employed, that is people who state not to have a job, and among these we distinguish between unemployed and people out of the labour force, the first being essentially job seekers and the second non-job seekers. Our definition of unemployment is similar but not identical to the standard ILO definition. We classify as

[21] If a member of the original sample drops off the original household, all adult members of his new households will also be interviewed. Moreover, the original sample has been supplemented with a number of "boost" groups, including major additional subsamples from Wales and Scotland (1999 onwards), and Northern Ireland (2001 onwards). Many people out of the original sample and out of those who are subsequently added, may drop off the survey due to various reasons such as move abroad, death, co-residents of original sample members who no longer live with a sample member and so on.

unemployed those without a job who have been looking for work in the last four weeks, which is one of the two ILO's requirements, but, unlike the second, we do not drop out of the pool people who are not available to start a new job within the following two weeks[22]. The BHPS provides two different measures of job search intensity for unemployed. On the one hand, they are asked whether they searched for work in the last week or in the last four. On the other hand, they are asked which search methods they used so we can derive the total number of methods (from 0 to 5). We use these categorical measures as dependent variables and we regress them on the housing tenure dummies controlling for several individual (or household) characteristics[23].

Since our dependent variables are categorical, either binary or ordinal, we run non linear panel regressions, using in particular a random effects probit model[24]. Unfortunately it is very problematic to perform a fixed effects analysis for non linear models such as probit or logit. This is due to the so called "incidental parameters problem" which prevents to consistently estimate the parameters of the index function along with the individual effects when the number of cross sections is small[25]. While a solution to get consistent estimates for fixed effects logit models has been found (see for example Chamberlain [1980]), this is not the case for the fixed effects probit. The usual device is to find a sufficient statistic for the unobserved effects which allows these to be conditioned out of the likelihood function. Such a sufficient statistic does not exist for the fixed effects probit. An unconditional fixed effects probit maybe estimated just plugging in a large number of individual dummies, but estimates would be biased[26].

Though fixed-T-consistent estimators have been derived for panel logit models, these methods have some drawbacks. In particular, given the way the sufficient statistic is build, only observations for individuals who switch status between two subsequent periods can be kept in the computation. Then they

[22]The problem is that the related question is asked only since wave 6.

[23]See Wadsworth [1991] for a reduced form estimation of search intensity measures on data drawn from the UK Labour Force Survey. He used as dependent variable the number of search methods too, but in a simpler econometric set-up. See also Schmitt and Wadsworth [1993] and Gregg and Wadsworth [1996]

[24]The motivation for using non linear rather than linear models for categorical dependent variables is typically that in the linear model the predicted probabilities are not guaranteed to lie in the unit interval. Moreover, in a panel setting the linear model would also require an unnatural assumption on the unobserved heterogeneity (see Wooldridge [2010]).

[25]See Wooldridge [2010] and Baltagi [2008] for an exhaustive treatment of estimation techniques and problems in applying panel models with a discrete dependent variable.

[26]See Fernándes-Val [2009] for a discussion of the magnitude of the fixed effects probit's bias. He argues that the magnitude of the bias of the marginal effects' estimates may be small for a range of distributions of regressors and individual effects, even for small T.

do not provide estimates for individual effects, thus precluding estimation of other quantities of interest such as marginal effects. Also, unlike the probit approach, these fixed effects logit models require a conditional serial independence assumption for consistency, which may be even less appealing when several time periods are available (see Wooldridge [2010], pag. 492). In the end, the random effects probit turned out to be the most reliable technique we have come up with, although of course it puts restrictions on the relation between the regressors and the unobserved effects. The choice of a panel rather than a pooled analysis is motivated by the better properties of the random effects model in the presence of unobserved heterogeneity, which is always detected by the likelihood ratio test of ρ in our regressions.

2.4 Empirical Results

2.4.1 Unemployed Sample

In our sample we can observe search measures only for unemployed, that is for those who searched for work in the last four weeks[27]. At first, we report estimates of random effects probit models for both of search measures within the unemployed sample. Then we discuss the sample selection bias issue which arises when inactive people, for whom search intensity is zero by definition, are excluded from the sample. In fact we can think of inactive people as workers who have chosen a degree of search intensity equal to zero due to the same set of variables which affect the search intensity of unemployed, at least after controlling for other characteristics which may be crucial in determining the choice of being out of the labour force, such as full-time education, retirement or disability. The final specification will thus include also inactive and the fairly larger sample will allow us to get more precise results[28].

Tables 2.1, 2.2 and 2.3 report estimates for the unemployed sample. The dependent variable used in table 2.1 is a dummy which takes 1 if the unemployed searched in the last week and 0 if searched in the last 4 weeks but not in the last. Tables 2.2 and 2.3 report estimates which focus on the number of search methods, the former using an ordinal variable (from 0 to 5 methods), the latter grouping these numbers in a dummy which takes 1 for high numbers (3, 4 and 5 methods), and takes 0 for low numbers (0, 1 and 2 methods). The first two columns of each table refer to models which include

[27] We restrict the sample to people in working age, *i.e.* males in age range 16-64 and females in age range 16-59.

[28] See appendix A for a description of the variables selected.

Table 2.1
UNEMPLOYED: SEARCH LAST WEEK OR LAST 4 WEEKS

	(1)	(2)	(3)	(4)
owner (out. or mort.)	9.1**	7.6*		
mortgager			11.2**	9.1*
outright owner			1.7	3.3
housing costs		3.1*		2.5
equivalized hh income	1.1	-0.0	0.7	-0.1
hh size	0.3	-0.1	0.3	-0.1
dep. child	-0.3	-0.3	-0.7	-0.6
claimant	18.3***	18.9***	18.4***	18.8***
financial sit.	7.3***	7.0***	7.2***	7.0***
female	-8.8**	-9.0***	-8.6**	-8.9**
young (16-24)	-7.1	-7.7	-7.3	-7.8
elderly (50-64)	7.7	8.3	9.3	9.0
disability benf.	-26.0***	-25.4***	-25.9***	-25.4***
pension	46.0	46.8	46.4	46.8
full-time education	-35.4***	-36.9***	-35.7***	-36.8***
relation with HoH				
spouse or live-in partner	-0.6	-0.4	-0.8	-0.5
child	12.8**	14.4**	13.7**	14.6**
other	2.8	-3.7	3.1	0.3
duration since last job				
6-12 months	-15.2**	-15.2**	-15.2**	-15.2**
1-3 years	-21.3***	-20.6***	-21.0***	-20.5***
3 years or more	-30.0***	-29.4***	-29.9***	-29.4***
never had job	-11.5*	-11.0*	-11.4*	-10.9*
education				
1st degree or higher	21.9***	22.2***	22.5***	22.8***
hnd, hnc, teaching qf	14.1	12.9	14.4	13.3**
a level	12.1*	11.0*	12.3**	11.3*
o level	14.4***	14.5***	14.5***	14.6***
cse	5.3	5.0	5.0	4.9
other qlf	2.3	1.7	2.3	1.8
regional dummies	✓	✓	✓	✓
time dummies	✓	✓	✓	✓
number of observations	7124	7062	7124	7062
ρ	9.8***	9.1***	9.8***	9.1***

Notes:

1. * significant at 10%; ** significant at 5%; *** significant at 1%.
2. The Table reports marginal effects (at regressors means) from the random effects probit. Coefficients are expressed in percentage points, expect for housing costs and equivalized household income for which we report semi-elasticities.
3. Sample: people in working age without job who searched in last four weeks. Dependent variable: search in the last week or in the last four but not in the last. The last row reports the likelihood ratio test for the presence of unobserved heterogeneity; the ρ statistic is the portion of variance of the composite error due to the variance of the unobserved heterogeneity.
4. For dummies which capture the relation with the head of household, the omitted category is just the head of household; for duration since last job dummies it is from 0 to 6 months ago; for education dummies it is no qualification.

Table 2.2
UNEMPLOYED: SEARCH METHODS

	(1)	(2)	(3)	(4)
owner (out. or mort.)	14.3***	13.1***		
mortgager			17.3***	15.3***
outright owner			3.8	6.7
housing costs		0.0**		0.0
equivalized hh income	0.0	0.0	-0.0	-0.0
hh size	-0.5	-0.5	-0.6	-0.5
dep. child	-5.9	-5.5	-6.5	-5.9
claimant	26.9***	28.2***	27.2***	28.1***
financial sit.	10.2***	10.1***	10.1***	10.0***
female	-24.7***	-24.4***	-24.5***	-24.3***
young (16-24)	11.4**	11.1**	11.2**	11.0**
elderly (50-64)	-17.9***	-15.9**	-15.7**	-14.8**
disability benf.	-35.3***	-34.1***	-35.1***	-34.1***
pension	54.6**	55.0**	56.3**	55.8**
full-time education	-44.6***	-45.2***	-44.9***	-45.1***
relation with HoH				
spouse or live-in partner	5.9	5.4	5.7	5.4
child	-11.4*	-10.5*	-10.2*	-10.1
other	-3.1	-6.1	-2.6	-5.2
duration since last job				
6-12 months	-26.4***	-25.9***	-26.4***	-26.1***
1-3 years	-31.2***	-31.0***	-31.0***	-31.0***
3 years or more	-56.3***	-55.2***	-56.1***	-55.3***
never had job	-29.3***	-28.7***	-29.3***	-28.6***
education				
1st degree or higher	24.9***	24.8***	26.0***	25.7***
hnd, hnc, teaching qf	15.3	13.5	16.1	14.2
a level	9.2	7.9	9.4	8.4
o level	11.0**	10.2*	11.2**	10.4*
cse	-13.7**	-14.2*	-13.8*	-14.3*
other qlf	2.1	1.9	2.2	2.0
regional dummies	✓	✓	✓	✓
time dummies	✓	✓	✓	✓
number of observations	4651	4591	4651	4591
ρ	19.0***	18.0***	19.1***	18.1***

Notes:

1. * significant at 10%; ** significant at 5%; *** significant at 1%.
2. The estimated model is a random effects ordered probit. The calculation is run by using the `reoprob` Stata package. Reported coefficients are those of the index function thus do not have a straight economic interpretation.
3. Sample: people in working age without job who searched in last four weeks. Dependent variable: number of search methods, 6 categories (0-5). Since the question on search methods is asked only since 1996, the sample shrinks to 12 waves, that is from 6 to 17 waves. The last row reports the likelihood ratio test for the presence of unobserved heterogeneity; the ρ statistic is the portion of variance of the composite error due to the variance of the unobserved heterogeneity.
4. For dummies which capture the relation with the head of household, the omitted category is just the head of household; for duration since last job dummies it is from 0 to 6 months ago; for education dummies it is no qualification.

Table 2.3
UNEMPLOYED: DUMMY FOR SEARCH METHODS

	(1)	(2)	(3)	(4)
owner (out. or mort.)	11.6**	10.5**		
mortgager			12.2**	9.6*
outright owner			9.8	13.3
housing costs		3.6*		4.0*
equivalized hh income	3.0	1.9	2.9	1.9
hh size	-1.2	-1.0	-1.1	-1.0
dep. child	-10.2*	-10.4*	-10.3*	-10.2*
claimant	34.6***	35.5***	34.6***	35.5***
financial sit.	9.6***	9.4***	9.6***	9.5***
female	-26.8**	-26.0***	-26.8***	-26.1***
young (16-24)	6.0	6.3	6.0	6.3
elderly (50-64)	-16.0**	-14.2*	-15.6*	-14.6*
disability benf.	-31.1***	-31.4***	-31.1***	-31.4***
pension	60.1*	60.6*	60.3*	60.4*
full-time education	-49.1***	-50.6***	-49.2***	-50.6***
relation with HoH				
spouse or live-in partner	7.3	6.3	7.3	6.4
child	-7.8	-7.8	-7.6	-8.0
other	-1.9	-5.4	-1.8	-5.9
duration since last job				
6-12 months	-38.5***	-38.3***	-38.5***	-38.3***
1-3 years	-34.5***	-34.1***	-34.5***	-34.0***
3 years or more	-61.0***	-60.4***	-60.9***	-60.4***
never had job	-31.6***	-30.6***	-31.6***	-30.6***
education				
1st degree or higher	26.3***	26.6***	26.5***	26.2***
hnd, hnc, teaching qf	21.1*	19.5	21.2*	19.3
a level	11.4	10.0	11.4	9.8
o level	15.0**	14.8**	15.1**	14.7**
cse	-18.1**	-19.3**	-18.2**	-19.3**
other qlf	-0.1	-0.5	-0.1	-0.5
regional dummies	✓	✓	✓	✓
time dummies	✓	✓	✓	✓
number of observations	4651	4591	4651	4591
ρ	17.5***	16.5***	17.6***	16.5***

Notes:

1. * significant at 10%; ** significant at 5%; *** significant at 1%.
2. The table reports marginal effects (at regressors means) from the random effects probit. Coefficients are expressed in percentage points, expect for housing costs and equivalized household income for which we report semi-elasticities.
3. Sample: people in working age without job who searched in last four weeks. Dependent variable: dummy which takes 1 for numbers of methods between 3 and 5 and takes 0 for numbers between 0 and 2. As in table 2.2 the sample includes only from 6 to 17 waves. The last row reports the likelihood ratio test for the presence of unobserved heterogeneity; the ρ statistic is the portion of variance of the composite error due to the variance of the unobserved heterogeneity.
4. For dummies which capture the relation with the head of household, the omitted category is just the head of household; for duration since last job dummies it is from 0 to 6 months ago; for education dummies it is no qualification.

the dummy *owner* for housing tenure, which pools both outright owners and mortgagers, while in models (3) and (4) we use two housing tenure dummies, *mortgager* and *outright owner*, in order to distinguish between these two categories: the coefficient of each of these dummies captures the difference in search between the category which names the dummy and the reference category, *i.e.* renters. In models (1) and (3) there is no control for housing costs, while in models (2) and (4) we add the housing costs variable in order to check how it affects the coefficients of the housing tenure dummies.

Table 2.1 shows that owners as a whole have a higher probability than renters to search in the last week. Anyway, in models which distinguish between the two owners categories we see that this difference is driven by the higher probability of mortgagers, while there is no significant difference between outright owners and renters. Housing costs have a positive impact on search as expected, but the effect is not significant when we allow for the *mortgager* dummy (see column 4). Moreover, when we add the housing costs variable to model (3), the coefficient of *mortgager* slightly drops while the coefficient of *outright owner* slightly increases. These results are even more evident when we look at search methods. Table 2.2 gives perhaps more precise results since it exploits all the variation in the number of search methods. In fact *owner* and *mortgager* dummies are highly significant confirming that the difference in search between owners and non owners is explained only by the higher search of mortgagers. Housing costs are again significant in model (2) but not in model (4).

Unfortunately, since coefficients reported in table 2.2 are just the coefficients of the probit's index function, they do not have a straight meaning in terms of magnitude, although they give information on the direction and on the statistical importance of the effect. For ordered probit models like this one might report the marginal effects on the probability of being in each of 6 statuses but it would be confusing, so we prefer to look at table 2.3 to have an idea of the magnitude of the effects. In column (4) of table 2.3 we see that the probability of using a large number of methods for mortgagers is around 10 percentage points higher than renters, which is very close to the effect we get when we use the last-week/last-4-weeks variable in table 2.1. Moreover, a 1% rise in housing costs increases this probability by 4 points, while the effect in table 2.1 is 2.5 points (for money variables such as housing costs and equivalized income we always report semi-elasticities)[29]. There are also several significant and interesting effects from other variables which we prefer to discuss later when we deal with a larger sample.

[29]For model (4), the housing costs variable is significant only in table 2.3, though mildly, while in table 2.1 and 2.2 it is very close to significance.

2.4.2 Whole Sample

The analysis so far focuses on the unemployed sample, but we argue that estimates are biased unless we allow also for inactive people. Search behaviour does not concern only the choice of the degree of search, but also the choice of searching or not in the first place. For some individuals the outcome of the set of variables we have allowed for may be no search in the last four weeks, which means that these drop out of the unemployed sample. This is a clear example of non-random sample selection, which results in biasing estimates on the sub-sample. In order to account for individuals who self-select in inactivity we replicate the same analysis as above for the overall sample, that is unemployed plus inactive.

Anyway, this strategy must be tackled carefully since the choice of being inactive depends also on other reasons than those which influence the search optimization process, such as housing tenure for example. In fact, for individuals in working age who are simply not interested in work, the choice between unemployment and inactivity cannot be interpreted as a matter of choice about the intensity of search. In order to distinguish within the inactive sample between these individuals and those who would be interested in working but have set their search level to zero, we use controls for retirement, full-time education and incapability to work, which should account for most of the reasons for individuals not being involved in a search choice process.

Table 2.4 reports estimates of the baseline models when we use the larger sample of unemployed plus inactive and the dependent variable is a dummy for the status. Since these models include also the three controls, coefficients of the other variables should actually capture the effect on the search choice for individuals who are actually involved in the search choice process. As expected, all of these controls are highly significant and have a very strong impact. Interestingly, outright owners are far less likely to search than renters, while typically there are not significant differences in search intensity when unemployed as shown before. Thus the mobility effect of homeownership seems to work by reducing the attachment to the labour market of outright owners rather than by reducing their search intensity relative to renters.

One alternative strategy to allow for purely inactive individuals, may be to drop people who are in full-time education, who get retirement pension or get disability benefits. Anyway this strategy does not seem very promising since the percentage of people in each of these statuses who are actually inactive is not as high as one may expect. As table 2.5 shows, more than a half of people in full-time education or getting a retirement pension can be job seekers or even employed, while for people on disability benefits this is the case for at least 15%. Since a non negligible portion of these people

Table 2.4
THE CHOICE OF SEARCH

	(1)	(2)	(3)	(4)
owner (out. or mort.)	5.6*	5.0		
mortgager			15.2***	14.4***
outright owner			-23.2***	-21.2***
housing costs		3.6***		1.8**
equivalized hh income	-4.7***	-5.6***	-6.1***	-6.3***
hh size	-5.3***	-5.9***	-5.5***	-5.8***
dep. child	-5.9*	-5.1	-8.2***	-7.5**
claimant	51.6***	52.8***	51.1***	52.0***
financial sit.	18.6***	18.5***	18.1***	18.1***
female	-69.6***	-69.0***	-69.7***	-69.2***
young (16-24)	35.4***	35.7***	34.5***	35.1***
elderly (50-64)	-77.7***	-75.3***	-70.5***	-69.6***
disability benf.	-94.7***	-94.8***	-95.4***	-95.7***
pension	-42.9***	-41.3***	-38.4***	-37.7***
full-time education	-98.7***	-100.9***	-100.4***	-101.8***
relation with HoH				
spouse or live-in partner	5.5	4.6	3.9	3.3
child	65.0***	67.1***	67.9***	68.7***
other	31.5***	27.0***	31.6***	29.4***
duration since last job				
6-12 months	-28.6***	-28.6***	-28.4***	-28.5***
1-3 years	-70.9***	-70.7***	-69.8***	-70.0***
3 years or more	-99.6***	-99.2***	-98.9***	-98.8***
never had job	-65.9***	-65.8***	-65.0***	-65.1***
education				
1st degree or higher	47.3***	45.9***	49.5***	48.6***
hnd, hnc, teaching qf	32.2***	33.1***	33.7***	34.9***
a level	21.8***	20.1***	22.5***	21.7***
o level	31.8***	30.9***	31.5***	31.0***
cse	18.0***	17.8***	17.2***	17.4***
other qlf	8.5	8.9	8.7	9.2
regional dummies	✓	✓	✓	✓
time dummies	✓	✓	✓	✓
number of observations	40184	39876	40184	39876
ρ	40.7***	40.6***	40.6***	40.6***

Notes:

1. * significant at 10%; ** significant at 5%; *** significant at 1%.
2. The table reports marginal effects (at regressors means) from the random effects probit. Coefficients are expressed in percentage points, expect for housing costs and equivalized household income for which we report semi-elasticities.
3. Sample: all people in working age without job. Dependent variable: dummy which takes 1 if searched in last four weeks and 0 if not. The last row reports the likelihood ratio test for the presence of unobserved heterogeneity; the ρ statistic is the portion of variance of the composite error due to the variance of the unobserved heterogeneity.
4. For dummies which capture the relation with the head of household, the omitted category is just the head of household; for duration since last job dummies it is from 0 to 6 months ago; for education dummies it is no qualification.

Table 2.5
FT EDUCATION, RETIREMENT PENSION, DISABILITY BENEFITS

	all sample		FT educ		pension		disab. ben.	
	freq.	%	freq.	%	freq.	%	freq.	%
employed	124,699	74.8%	4,893	40.3%	542	42.6%	1,089	11.9%
unemployed	7,411	4.5%	1,130	9.3%	34	2.7%	346	3.8%
inactive	34,532	20.7%	6,113	50.4%	695	54.7%	7,730	84.3%
Total	166,642	100%	12,136	100%	1,271	100%	9,165	100%

Notes:

1. The sample refers to all 17 waves and is made of all respondents in working age.

seem to be able to look for work or even to have a job, it is reasonable that their search behaviour may be influenced by also other reasons than being in that status, which arguably are the same which affect the degree of search intensity of job seekers, such as housing tenure. We thus include in the sample all inactive people setting their search effort equal to zero[30].

Tables 2.6, 2.7 and 2.8 report results when we pool unemployed and inactive. In models of table 2.6 we just add the inactive category to the two used in the dependent variable of table 2.1: the dependent variable has now three categories so an ordered probit is required for estimation. Table 2.7 and 2.8 correspond to table 2.2 and table 2.3 respectively, except that in the 0 methods group we include also inactive people.

These three tables conceive the same information on the differences in search activity by housing tenure, confirming part of the results found within the unemployed sample. The difference in search between homeowners as a whole and renters is positive and not strongly significant, but this is clearly the result of the balancing of two opposite effects as the models in column (3) and (4) highlight: on the one side, mortgagers search more than renters, but on the other side, outright owners search less. In fact, the coefficient of *outright owner* is negative and strongly significant in table 2.6 and 2.7[31], which is in line with results of table 2.4 but at odds with results drawn from the unemployed sample. Thus it appears that among unemployed there is not a relevant difference in search intensity between outright owners and renters,

[30]People who have not searched in the last four weeks may have searched, for example, 5 or 6 weeks before, but since we cannot observe these measures we set search to zero for all of them.

[31]In model (4) of table 2.8 this coefficient is significant only at a 10% level, but here the effect cannot be captured as precisely as when all the variation in the number of search methods is exploited

Table 2.6
Non-employed: search last week, search last 4 weeks or no search

	(1)	(2)	(3)	(4)
owner (out. or mort.)	6.7**	5.9**		
mortgager			15.9***	14.8***
outright owner			-21.2***	-19.2***
housing costs		0.0***		0.0***
equivalized hh income	-0.0***	-0.0***	-0.0***	-0.0***
hh size	-4.9***	-5.5***	-5.1***	-5.5***
dep. child	-4.9	-4.1	-7.0**	-6.3**
claimant	50.3***	51.6***	49.9***	50.8***
financial sit.	18.5***	18.4***	18.0***	18.0***
female	-65.6***	-65.1***	-65.6***	-65.1***
young (16-24)	31.0***	31.1***	30.2***	30.6***
elderly (50-64)	-73.5***	-71.1***	-66.4***	-65.6***
disability benf.	-92.0***	-92.1***	-92.7***	-93.0***
pension	-40.0***	-38.4***	-35.6***	-35.0***
full-time education	-94.7***	-96.9***	-96.2***	-98.0***
relation with HoH				
spouse or live-in partner	4.2	3.3	2.7	2.2
child	61.0***	63.2***	63.7***	64.6***
other	28.0***	23.3***	28.0***	25.5***
duration since last job				
6-12 months	-27.7***	-27.6***	-27.5***	-27.6***
1-3 years	-68.1***	-67.8***	-67.0***	-67.1***
3 years or more	-97.0***	-96.4***	-96.1***	-96.0***
never had job	-61.8***	-61.6***	-61.0***	-60.9***
education				
1st degree or higher	48.1***	46.8***	50.2***	49.4***
hnd, hnc, teaching qf	33.5***	34.0***	34.9***	35.8***
a level	23.8***	22.2***	24.4***	23.5***
o level	32.8***	31.9***	32.4***	31.9***
cse	17.8***	17.7***	17.0***	17.2***
other qlf	9.0	9.3	9.3	9.7
regional dummies	✓	✓	✓	✓
time dummies	✓	✓	✓	✓
number of observations	40236	39927	40236	39927
ρ	39.2***	39.0***	39.0***	39.1***

Notes:

1. * significant at 10%; ** significant at 5%; *** significant at 1%.
2. The estimated model is a random effects ordered probit. The calculation is run by using the `reoprob` Stata package. Reported coefficients are those of the index function thus do not have a straight economic interpretation.
3. Sample: all non-employed people in working age. The dependent variable is ordinal with three categories: search in the last week, search in the last four but not in the last, no search in the last four. The last row reports the likelihood ratio test for the presence of unobserved heterogeneity; the ρ statistic is the portion of variance of the composite error due to the variance of the unobserved heterogeneity.
4. For dummies which capture the relation with the head of household, the omitted category is just the head of household; for duration since last job dummies it is from 0 to 6 months ago; for education dummies it is no qualification.

Table 2.7

Non-employed: search methods

	(1)	(2)	(3)	(4)
owner (out. or mort.)	7.2**	6.2*		
mortgager			16.8***	15.4***
outright owner			-21.4***	-18.1***
housing costs		0.0***		0.0**
equivalized hh income	-0.0**	-0.0***	-0.0***	-0.0***
hh size	-5.1***	-5.8***	-5.4***	-5.8***
dep. child	-4.9	-4.0	-6.9**	-6.0*
claimant	45.8***	48.1***	45.4***	47.0***
financial sit.	19.5***	19.3***	18.9***	18.9***
female	-62.1***	-61.4***	-62.2***	-61.4***
young (16-24)	35.4***	35.0***	34.2***	34.4***
elderly (50-64)	-72.2***	-68.2***	-64.8***	-63.4***
disability benf.	-88.5***	-88.2***	-89.0***	-89.1***
pension	-45.9***	-43.2***	-40.9***	-40.0***
full-time education	-85.5***	-87.5***	-86.6***	-87.4***
relation with HoH				
spouse or live-in partner	3.5	2.3	2.0	1.5
child	45.2***	47.7***	47.8***	48.4***
other	22.1***	15.3*	22.2***	19.0**
duration since last job				
6-12 months	-29.4***	-28.9***	-29.0***	-28.8***
1-3 years	-68.2***	-67.4***	-67.1***	-67.0***
3 years or more	-101.7***	-100.3***	-100.9***	-100.1***
never had job	-68.6***	-67.8***	-67.4***	-67.0***
education				
1st degree or higher	53.2***	51.3***	55.4***	54.3***
hnd, hnc, teaching qf	31.1***	30.8***	32.6***	32.9***
a level	23.9***	21.7***	24.4***	23.3***
o level	33.8***	32.6***	33.8***	33.1***
cse	10.5*	10.5*	10.3*	10.5*
other qlf	8.8	9.1	9.1	9.5
regional dummies	✓	✓	✓	✓
time dummies	✓	✓	✓	✓
number of observations	30639	30344	30639	30344
ρ	38.0***	37.7***	37.7***	37.6***

Notes:

1. * significant at 10%; ** significant at 5%; *** significant at 1%.
2. The estimated model is a random effects ordered probit. The calculation is run by using the `reoprob` Stata package. Reported coefficients are those of the index function thus do not have a straight economic interpretation.
3. Sample: all non-employed people in working age. Dependent variable: number of search methods, 6 categories (0-5). As in table 2.2 and 2.3 the sample includes only from 6 to 17 waves. The last row reports the likelihood ratio test for the presence of unobserved heterogeneity; the ρ statistic is the portion of variance of the composite error due to the variance of the unobserved heterogeneity.
4. For dummies which capture the relation with the head of household, the omitted category is just the head of household; for duration since last job dummies it is from 0 to 6 months ago; for education dummies it is no qualification.

Table 2.8

NON-EMPLOYED: DUMMY FOR SEARCH METHODS

	(1)	(2)	(3)	(4)
owner (out. or mort.)	10.2**	8.9**		
mortgager			18.3***	16.2***
outright owner			-13.5**	-10.0*
housing costs		5.5***		3.3**
equivalized hh income	-2.3	-4.2*	-3.4	-4.3*
hh size	-5.4***	-6.0***	-5.7***	-6.0***
dep. child	-11.5***	-11.0***	-13.1***	-12.5***
claimant	54.7***	56.5***	54.4***	55.7***
financial sit.	19.5***	19.3***	19.0***	19.0***
female	-65.1***	-64.1***	-65.1***	-64.2***
young (16-24)	28.1***	-28.1***	27.0***	27.6***
elderly (50-64)	-63.1***	-59.2***	-56.9***	-55.4***
disability benf.	-87.4***	-87.4***	-87.8***	-88.1***
pension	-31.0*	-28.5*	-26.7	-25.8
full-time education	-96.7***	-99.7***	-97.6***	-99.5***
relation with HoH				
spouse or live-in partner	7.8*	6.5	6.6	5.8
child	36.7***	38.7***	39.1***	39.5***
other	27.2***	20.2**	27.5***	23.3**
duration since last job				
6-12 months	-43.1***	-42.5***	-42.6***	-42.4***
1-3 years	-72.1***	-71.5***	-71.3***	-71.1***
3 years or more	-114.1***	-112.8***	-113.4***	-112.8***
never had job	-73.3***	-72.0***	-72.2***	-71.4***
education				
1st degree or higher	53.9***	52.4***	55.8***	54.8***
hnd, hnc, teaching qf	33.3***	33.3***	34.5***	34.8***
a level	26.9***	24.8***	27.3***	25.9***
o level	34.7***	33.9***	34.9***	34.4***
cse	3.0	2.4	3.0	2.6
other qlf	9.0	9.1	9.4	9.5
regional dummies	✓	✓	✓	✓
time dummies	✓	✓	✓	✓
number of observations	30639	30344	30639	30344
ρ	32.4***	32.0***	32.1***	31.9***

Notes:

1. * significant at 10%; ** significant at 5%; *** significant at 1%.
2. The table reports marginal effects (at regressors means) from the random effects probit. Coefficients are expressed in percentage points, expect for housing costs and equivalized household income for which we report semi-elasticities.
3. Sample: all non-employed people in working age. Dependent variable: dummy which takes 1 for numbers of methods between 3 and 5 and takes 0 for numbers between 0 and 2. As in table 2.2, 2.3 and 2.7 the sample includes only from 6 to 17 waves. The last row reports the likelihood ratio test for the presence of unobserved heterogeneity; the ρ statistic is the portion of variance of the composite error due to the variance of the unobserved heterogeneity.
4. For dummies which capture the relation with the head of household, the omitted category is just the head of household; for duration since last job dummies it is from 0 to 6 months ago; for education dummies it is no qualification.

but once we allow for the effect of housing tenure on the choice to search, the difference gets negative and significant. To have an idea of the magnitude of the effect, in column (4) of table 2.8 is shown that outright owners are less likely than renters by 10 percentage points to use a high number of search methods.

Are these differences in search intensity driven by mobility or by housing costs? Something can be said by analysing the housing costs variable. Its coefficient is always positive and significant, which means that the higher the housing costs the higher the pressure to find a job thus the higher the search intensity. In particular, a 1% increase in housing costs implies 3.3 points more in the probability of using a high number of search methods (see table 2.8, model 4). Typically, the housing costs effect decreases and is fairly less significant when we split the *owner* dummy in two dummies[32]. In fact, when mortgagers and outright owners are pooled as in model (2), the variation in search due to the difference in their individual characteristics is left unexplained, and this effect is captured by the housing costs variable. Moreover, when we add the housing costs variable in the model with two housing tenure dummies, the coefficient of *mortgager* drops while that of *outright owner* increases; clearly, since mortgagers and outright owners have, respectively, higher and lower housing costs than renters, if we omit the housing costs variable the coefficient of the former is biased upwards while the coefficient of the latter is biased downwards, given the positive effect of housing costs on search. Anyway, the change in these coefficients whenever we add the control for housing costs is always slight and the signs are never reverted: this means that housing costs matter but also that much of the search differentials are still unexplained.

To sum up, it appears that renters search more than outright owners since they have housing costs to cope with, but whenever we control for this effect, the differential in search is still high which is probably due to renters' higher mobility. This negative differential in search between outright owners and renters is precisely what the Oswald effect calls for. Right here comes the reconciliation between theory and evidence argued in the title. In fact, on the one hand, our theoretical model confirms the old idea that the lower mobility of homeowners implies lower search effort, *once we abstract from*

[32]This is clear both in the unemployed sample, as discussed above, and in the non-employed sample. Reported coefficients In table 2.6 and 2.7 are just those of the index function, so we cannot state how large is an effect though we can state if the effect of a variable is larger in a specific model. The coefficient of housing costs is always 0.0 in these tables (since we are measuring the effect of an only one pound increase in the variable) thus the lowering of the size from model (2) to model (4) cannot be identified. Anyway, if we look at omitted decimals we can state that this is the case for both tables.

the role of housing costs. On the other hand, we observe in the data, even after controlling for housing costs, higher search measures for renters than outright owners, for whom the mobility effect is the only one operating, and even at its maximum.

2.4.3 Social vs Private Renters

Anyway, given that mobility matters, we would not expect also that mortgagers search more than renters whenever we net out the housing costs effect. One partial explanation for this counterintuitive finding can be sought in the nature of rent. In fact, when comparing renters to the other categories we should be aware that renters' search outcome may be different between social and private renters, since the former may actually be less prone to move for job reasons due to lock-in effects similar to those which hamper homeowners' mobility[33]. In our whole sample of non-employed, 73% of renters occupy social housing, thus our analysis so far may be seriously confounded unless social housing does not hamper mobility. In order to control if the "lock-in" effect of social housing really matters, we replicate the previous estimations splitting the renter sample in two distinct groups. In particular, we add a dummy for social renters to the specification with the housing costs variable and with dummies for outright owners and mortgagers. The omitted group consists of private renters, thus the coefficient of the groups included is to be interpreted as the search differential with respect to them.

Table 2.9 reports results for five of the regressions previously discussed: each regression is identical to that of the correspondent table except for the social renter dummy. The first two models use the unemployed sample and refer to the dummy for search last week or last 4 weeks, and to the number of search methods variable, respectively (see table 2.1 and table 2.2 respectively). Again, when unemployed, mortgagers search more than every other category notwithstanding the control for social housing, while there are no significant differences among the other categories. But if we look at regression III, which models the probability of being a job seeker, we notice that the difference between mortgagers and private renters disappears, while both social renters and outright owners are less likely to be unemployed than private renters. These results are confirmed in the subsequent two models which account also for the different degree of search among unemployed. Moreover, the hypothesis that the coefficients of the three housing tenure dummies are identical in pairs are strongly rejected, which means that we

[33]Moreover, social renting comes at below market rent or even rent-free. In our whole sample of non-employed, 48% of social renters pay zero rent. Anyway this effect on search differentials should be captured by the housing costs variable.

Table 2.9
Mortgagers vs Outright vs Social Renters vs Private Renters

	unemployed		non-employed		
	I	II	III	IV	V
	last week, last 4 weeks	search methods	the choice of search	last week, last 4 weeks, no search	search methods
mortgager	12.3**	17.0***	4.0	5.5	6.3
outright owner	7.0	8.6	-32.4***	-29.1***	-28.2***
social renters	4.9	2.6	-16.1***	-14.4***	-14.3***
housing costs	2.7	0.0*	1.4*	0.0**	0.0**
equivalized hh income	-0.1	-0.0	-6.2***	-0.0***	-0.0***
hh size	-0.1	-0.6	-5.8***	-5.4***	-5.7***
dep. child	-1.1	-6.2	-5.9*	-4.9	-4.7
claimant	18.8***	28.1***	52.4***	51.1***	47.3***
financial sit.	7.0***	10.1***	18.2***	18.0***	18.9***
female	-8.8**	-24.2***	-69.2***	-65.2***	-61.5***
young (16-24)	-7.4	11.1**	34.0***	29.6***	33.5***
elderly (50-64)	8.6	-15.0**	-68.6***	-64.7***	-62.6***
disability benf.	-25.6***	-34.2***	-95.3***	-92.5***	-88.7***
pension	46.8	55.7**	-37.9***	-35.0***	-40.2***
full-time education	-36.1***	-44.8***	-103.7***	-99.2***	-88.9***
relation with HoH					
spouse or live-in partner	-0.6	5.3	3.4	2.3	1.6
child	13.8**	-10.5*	71.5***	67.1***	50.6***
other	0.7	-5.0	27.3***	23.7***	17.5**
duration since last job					
6-12 months	-15.3**	-26.1***	-28.5***	-27.6***	-28.8***
1-3 years	-20.5***	-31.0***	-70.0***	-67.1***	-67.0***
3 years or more	-29.5***	-55.3***	-98.4***	-95.7***	-99.9***
never had job	-10.9*	-28.6***	-65.2***	-61.1***	-67.2***
education					
1st degree or higher	23.6***	26.2***	45.9***	46.9***	52.0***
hnd, hnc, teaching qf	13.8	14.5	33.0***	34.1***	31.1***
a level	11.8**	8.6	19.5***	21.6***	21.5***
o level	14.8***	10.6*	29.9***	31.0***	31.1***
cse	5.1	-14.2**	16.9***	16.8***	10.2
other qlf	1.9	2.1	8.5	8.5	8.9
regional dummies	✓	✓	✓	✓	✓
time dummies	✓	✓	✓	✓	✓
number of observations	7062	4591	39876	39927	30344
ρ	9.0***	18.1***	40.6***	39.0***	37.5***

Notes:

1. * significant at 10%; ** significant at 5%; *** significant at 1%.
2. For model I and III, coefficients reported are marginal effects evaluated at means, for the other models they are straight coefficients of the index function.
3. Model I: see table 2.1; model II: see table 2.2; model III: see table 2.4; model IV: see table 2.6; model V: see table 2.7.

can identify a clear pattern of search effort levels: (1) private renters search as much as mortgagers; (2) mortgagers and private renters search more than both social renters and outright owners; (3) social renters search more than outright owners. Anyway, we remark that this pattern of search behaviour work through a different degree of attachment to the labour market, while the results within the unemployed sample are not clear cut.

In conclusion, the distinction between social and private renters can at least in part explain why the difference in search between mortgagers and renters as a whole is so high. But it is still unexplained why mortgagers, who are homeowners, are the category with the highest search measures. In the first place, we should take into account that the mobility effect for some mortgagers may be not as strong as that for outright owners (and social renters). In fact it seems reasonable to believe that outright owners have a stronger attachment to the accommodation since time spent in the current accommodation should be longer on average and since they may have higher transaction costs for moving home. With regards to the housing cost effect, some possible explanations may be that the housing costs variable of the BHPS has some measurement errors, or that it cannot capture what increases the search of mortgagers relative to renters who pay the rent. However, the most likely explanation is that the pressure to pay the mortgage is far higher than the pressure to pay the rent and this different pressure may not be simply captured by treating housing costs in the same way for both.

2.4.4 The Effect of the Other Covariates

In order to shed some light on the reliability of our conclusions, we discuss now the estimated effects of the economic and demographic variables on search effort. Estimates are consistent with economic interpretation, which is a valid signal of the goodness of our specification. We refer to results of table 2.9 as models reported here distinguish among all of the housing tenure categories, and allow for housing costs. In particular we select model V since the dependent variable is the most precise. Moreover, if we look at model V in combination with models II and III, we can also understand whether the impacts work through an influence on labour force participation, on the intensity of search, or on both.

Variables which increase the reservation wage imply lower search, hence, as expected, the effect of the household income is negative and the effect of the variable which captures the perceived individual's financial situation is positive, where a higher value means a worse situation. More educated workers search more than less educated or with no qualifications, as education increases the probability of finding a job and the return from employment.

Females search less than males due to the lower participation to labour market, but this effect is clear even within the unemployed sample. Individuals who live in households within which there is at least one dependent child search less, but the effect is not significant: in theory, the need to look after children may affect labour force participation of at least one household member, who is typically a female (no wonder, this effect is significant in model III). The fact that this effect should work mainly for females and not for males, may explain why the coefficient is not significant given that we include a dummy for gender[34].

Search intensity drops as the number of household members rises. In theory the effect of the household size on search activity should work through the influence on the household income, but as our measure of household income is scaled controlling for the number of members, this effect should no more appear in the household size variable. So this variable is probably capturing a residual effect not accounted for by the scaling of the household income or by other variables related to the household size.

The effect of age on search is negative and monotonic, given that prime-age workers search more than older and less than younger ones. The rationale is that the return from search is lower the lower the time horizon. However, while the effect of the elderly dummy is very strong, the difference between young and prime-age workers is typically smaller (see table 2.8), given that workers at the start of their career may experience some inactivity spells[35]. The duration effect is very clear: the higher the duration since last job the lower search effort due to the deterioration of skills and the discouragement effect[36]. People who have never had a job search less than those with low duration, but their coefficient is not as high as that of the last category since some of them may be young workers about to enter the labour market with strong motivations.

The dummies which capture the relationship with the head of household show that children search more than the head of household and his/her partner, and that between the latter there is no difference (see appendix A for the groupings in these dummies). In theory we should expect that more responsible members, *i.e.* those with stronger commitments to the home or

[34]Wadsworth [1991] found a negative and significant effect on search in the sample of females, while the effect was not significant in the sample of men.

[35]These findings are similar to those of Wadsworth [1991]. He found that old workers search significantly less than their prime-age counterparts, and that young workers search more if males and less if females, though the latter two effects are not significant.

[36]These findings partly disagree with those of Wadsworth [1991], who found a hump-shaped relation in the sense that search effort increases in the initial stages of unemployment and then drops.

the household, search more, thus it appears strange that children are more active in the job search. However, this does not imply that the commitment effect is not operating once we take into account that a not negligible percentage of head of households live alone within the household (11% within the non-employed sample), for whom commitments are definitely lower, and that the *child* dummy may be capturing part of the negative effect of age. Moreover, it is worth pointing out that in the unemployed sample the sign of the *child* dummy is negative, which means that the positive coefficients in the whole sample reflects a higher attachment to the labour market for children rather than a stricter job search behaviour.

Unemployed benefit claimants search more than non-claimants and the effect is strongly significant in all models we tested. It can be argued that claimants are able to keep closer ties with the labour market due to the financial support (see Wadsworth [1991]). However, there is also a reverse causality issue: non-employed who search more are more likely to meet the requirements for the benefit eligibility. This channel is even more important from 1996, when the UK Jobseeker's Allowance was introduced[37].

To conclude, one concern about the reliability of the estimated difference in search between renters and outright owners may arise. In the literature it has been argued that housing tenure may be endogenous since it may be correlated with unobservable factors which in turn help determine labour market outcomes. For example, individuals may self-select housing tenure on the ground of their intrinsic mobility, as well as of their greater desire to retain proximity to family members or friends, so that people who are more mobile may prefer to chose rented accommodations. Moreover, housing tenure choice may be influenced by unaccounted wealth effects or unobserved skill gaps which as well increase search intensity. Depending on which are the most important sources of endogeneity, the true differences in search between residential statuses may be larger or smaller than what we estimated. Unfortunately, if one is willing to exploit the panel dimension of data at hand, there is not a simple way to control for endogeneity in this econometric set-up given that the fixed effects analysis for discrete response models is not well developed.

[37]The Jobseekers' Allowance involved notable tightenings of search criteria which have to be met for the unemployment benefit eligibility. See Manning [2009], Petrongolo [2009] and Arzilli and Morescalchi [2011]

2.5 Conclusion

This paper has investigated the old argument that homeownership reduces exit rates from unemployment by hampering residential mobility, which is known under the name of Oswald's effect. While the empirical literature on this point has confirmed that unemployed homeowners are less prone than renters to move for job reasons, the typical finding is that homeowners have lower unemployment duration, which is exactly the reverse of the Oswald hypothesis. By exploring the reasons for these falsifications of the Oswald hypothesis, we provide a more refined empirical evidence which is consistent with the underlying theory.

At first, we develop a search theory which is in accordance with the Oswald effect. In particular this model overcomes an ambiguity which may arise in models which distinguish between local and non local labour markets. In these models homeownership reduces the job finding rate in the non local labour market but it increases the local job finding rate, so that the latter effect may dominate empirically. By allowing for endogenous search we show that, as net result, homeownership unambiguously reduces the optimal search and the job finding rate.

Then, in the empirical analysis we allow for housing costs and for the different nature of tenure, both among owners and among renters, in order to point out the true effect of owners' lower mobility. In particular, within the homeowners' group we distinguish between outright owners and owners who pay the mortgage, and within the renters' group we distinguish between social and private. Once the housing costs effect is controlled for, the results show that, while homeowners search more than renters as found in the earlier literature, outright owners alone search less than renters, and the difference is even higher when we compare them to private renters. This difference works mainly through a lower attachment to the labour market of outright owners, while there are not significant differences within the unemployed sample.

Housing costs work in the expected direction, though the effect is not as strong as expected. Moreover, once we control for housing costs, mortgagers have a similar search to the supposed more mobile private renters, which means that the higher level of housing costs cannot explain alone why mortgagers search so much. The most likely explanation is that the pressure to pay the mortgage is far higher than the pressure to pay the rent and this pressure cannot simply be captured by treating housing costs in the same way for both.

In brief, we argue that tests of the Oswald hypothesis which compare homeowners and renters as if they were two distinct groups are misleading, since some individuals who belong to one of these share common features with some individuals belonging to the other. If we group individuals in only two different categories the empirical effect is confounded, but if we allow for proper distinctions, some evidence in favour of the Oswald hypothesis can be provided.

Chapter 3

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

3.1 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 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 [2001a], Flatau et al. [2003], Brunet and Lesueur [2009], Brunet et al. [2007]).

[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.

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

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 [2009]), but this is not always the case. For example, Brunet and Lesueur [2009] with French data and Green and Hendershott [2001a] 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 rent-

[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 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.

ing 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 estimation 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. Sections 3.2 and 3.3 discuss respectively the data and sample used, and the methodology. Each section looks separately at the probability to be unemployed and the unemployment duration. Sections 3.4 and 3.5 discuss results for the former an for the latter respectively. Section 3.6 concludes.

3.2 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 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

[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.

[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.

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.

3.2.1 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 3.1 and 3.2 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 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

Table 3.1
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 3.2
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 3.1.

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 3.3 and 3.4 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 3.5 and 3.6 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 3.1 and 3.2 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

Table 3.3
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 3.1.
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 3.4
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 3.3.

Table 3.5

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 3.1.
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 3.6

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

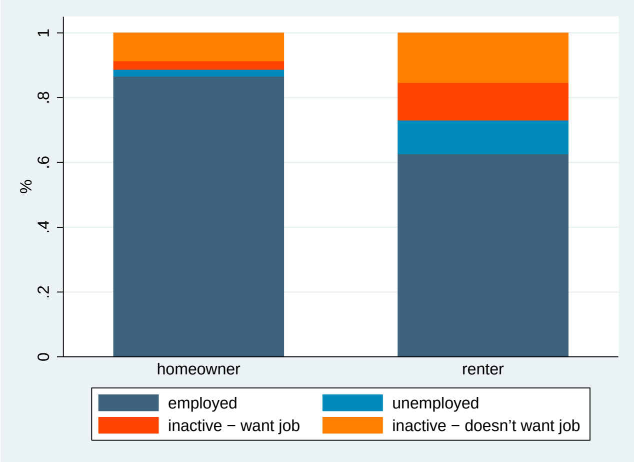
Main Reason	Housing Tenure: owned outright	mort-gage	rented social	rented private
waiting application	0.0	0.0	0.1	0.0
student	0.2	0.8	0.7	3.2
look after fam/home	2.9	9.4	12.0	10.2
temp. sick/injured	0.6	1.8	3.2	4.8
long-term sick/disabled	29.5	47.4	74.7	62.4
doesn't need work	7.1	5.4	0.3	1.4
retired 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 3.5.

Figure 3.1

Labour Market Status by Housing Tenure

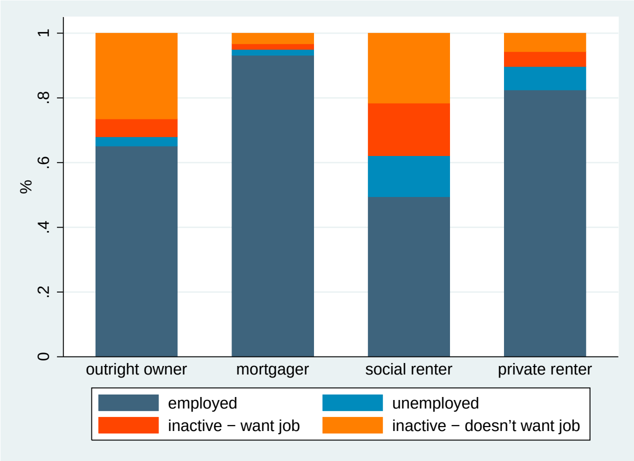


Notes:

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 3.2

Labour Market Status by Housing Tenure

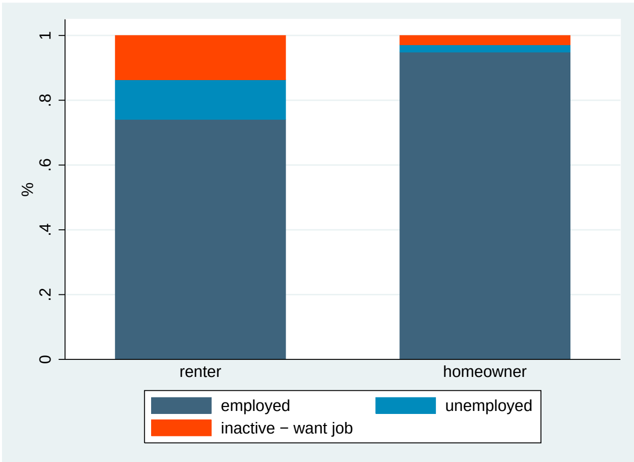


Notes:

1. See note 1 to figure 3.1.

Figure 3.3

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

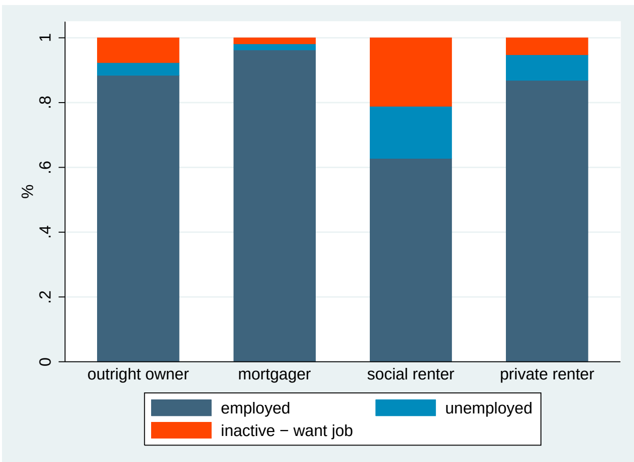


Notes:

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 3.4

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



Notes:

1. See note 1 to figure 3.3.

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 3.3 and 3.4 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.

3.2.2 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].

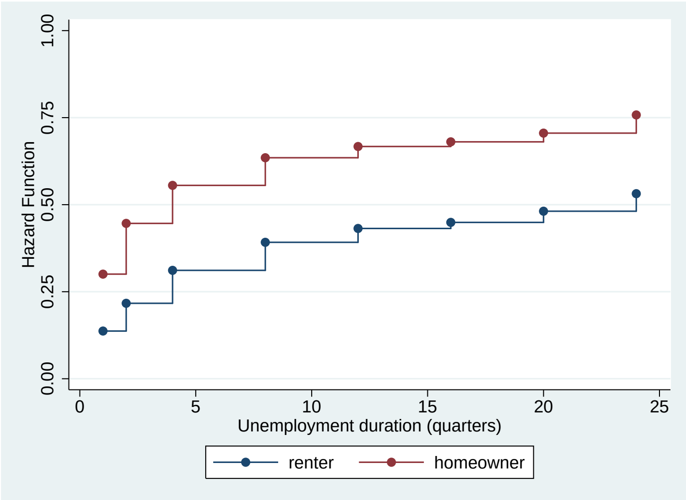
Apart from the discrete nature of this variable, the choice to refer to the last interview as unemployed leads to an underestimation of the spell since the precise day in which the spell ends can be whatever else up to the day of the next interview. Yet, this underestimation is of minor concern for our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

[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.

Figure 3.5

Cumulative Hazard to employment: Kaplan-Meier estimate

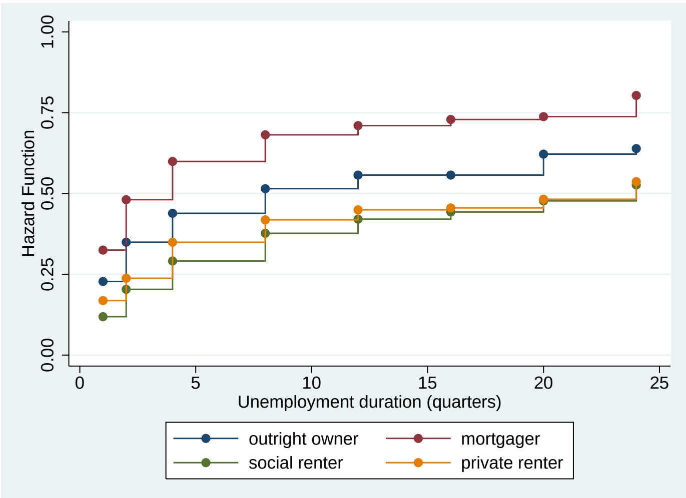


Notes:

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

Figure 3.6

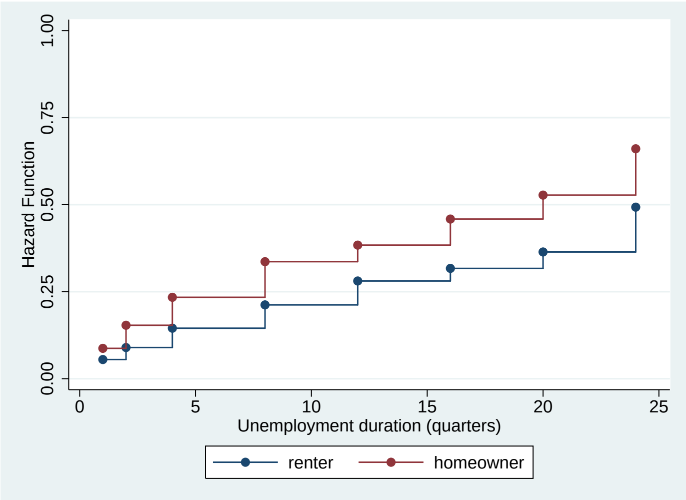
Cumulative Hazard to employment: Kaplan-Meier estimate



Notes:

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

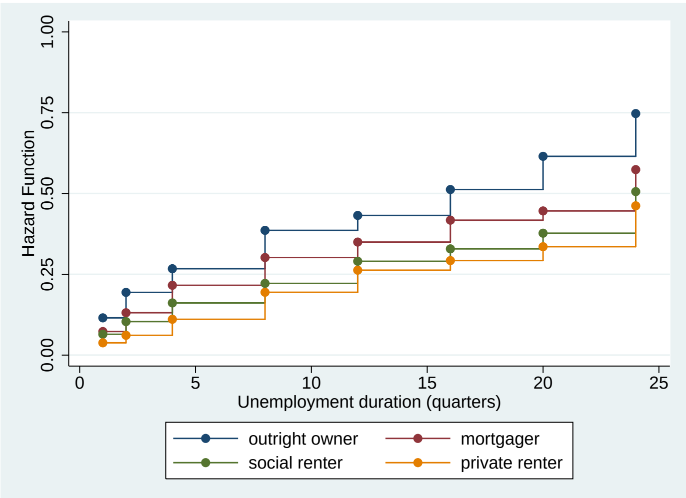
Figure 3.7
Cumulative Hazard to inactivity: Kaplan-Meier estimate



Notes:

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

Figure 3.8
Cumulative Hazard to inactivity: Kaplan-Meier estimate



Notes:

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

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 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 3.5 and 3.6 report the Kaplan-Meyer estimates of the cumulative density function for exits to employment. Figures 3.7 and 3.8 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.

3.3 Methodology

3.3.1 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

[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.

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-

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

causal interpretation and other equations that model the generating process of treatment variables (see appendix B 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 instruments 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

[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]).

[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.

(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 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 homeownership[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.

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

3.3.2 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 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.

3.4 Empirical Results: The probability of Being Non-Employed

Table 3.7 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

[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.

Table 3.7
PROBABILITY OF BEING NON-EMPLOYED

	(1) Probit		(2) Endog. Probit		(3) Logit		(4) Endog. Logit	
	β	*std. err.*	β	*std. err.*	β	*std. err.*	β	*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	✓		✓		✓		✓	
λ_{OUT}							0.551*	*0.250*
λ_{MORT}							3.608**	*0.415*
λ_{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 appendix C for the base categories of discrete regressors. See note 1 to table 3.1 for sample restrictions. See appendix B for a formal representation of model (4). βs 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`). λ-s are loading factors of the latent terms and positive (negative) λ_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.

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 3.7 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 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 3.7 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 3.7 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

[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.

[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

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 λ-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 λ_{mort} and λ_{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 be non-employed on the basis of their unobserved characteristics. In other words, when λ 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 D.1 in appendix D), 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

(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]).

of values of the regressors[25]. Table 3.8 report marginal effects calculated at sample means of the regressors, while tables 3.10 and 3.11 report marginal effects at representative values of regressors (see appendix B for a formal representation of marginal effects in the endogenous case).

Interestingly, table 3.8 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 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 3.10 and 3.11 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 3.9). Again, we observe in these cases that treatment effects for mortgagers and outright owners shrink after accounting for endogeneity. Anyway, unlike marginal

[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.

[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.

Table 3.8
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 3.7. Marginal effects are computed at sample means of regressors and latent factors are set to zero.

Table 3.9
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 3.10
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 3.7. 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 3.11
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 3.7. 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.

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.

3.5 Empirical Results: Unemployment Duration

3.5.1 Exit to Employment

Table 3.12 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 3.12 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 ρ 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

[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 ρ is the ratio of the heterogeneity variance to one plus the heterogeneity variance.

Table 3.12

Probability of exiting unemployment: spells ending in employment or censored

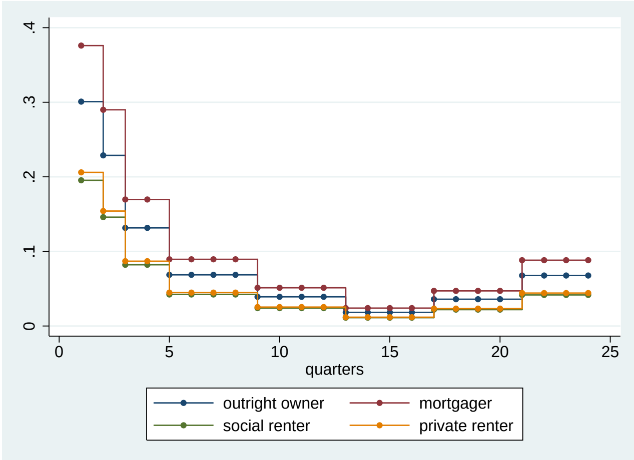
	(1) Hazard with frailty		(2) Hazard without frailty
	dy/dx	*hazard ratio*	*hazard ratio*
outright owner	2.69**	1.552**	1.584**
mortgager	4.51**	2.045**	2.057**
social	-0.31	0.942	0.964
claimant	-1.89**	0.705**	0.712**
disability benf.	-2.70**	0.506**	0.524**
married	1.22**	1.257**	1.247**
baseline hazard 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
ρ			0.00003

Notes:

1. * significant at 5%; ** significant at 1%. See the appendix C 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 3.1 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 ρ statistic is a test for the presence of frailty.

Figure 3.9

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

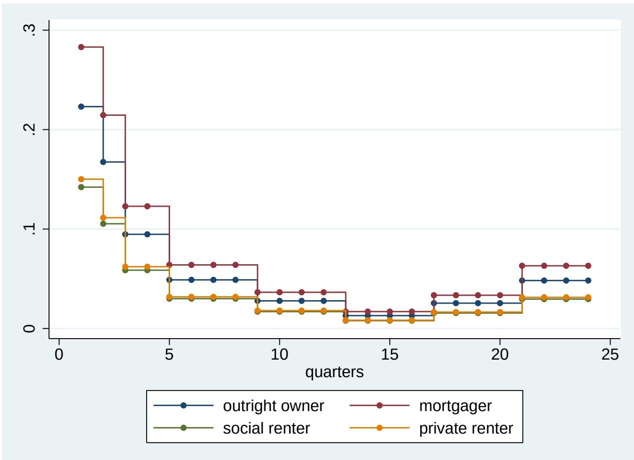


Notes:

1. Predicted hazards have been estimated after the clog-log non-frailty model (table 3.12, 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 3.10

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



Notes:

1. Predicted hazards have been estimated after the clog-log non-frailty model (table 3.12, 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.

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 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.

Figures 3.9 and 3.10 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.

[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.

[30]The difference in the level of the two plots is larger than the estimated marginal effect of the claimant dummy reported in table 3.12. 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.

3.5.2 Exit to Inactivity

Table 3.13 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].

Figures 3.11 and 3.12 show the counterparts of figures 3.9 and 3.10 for exits into inactivity. The hazard function of stopping looking for work has a similar 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 3.12 and 3.13 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 3.13 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

[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.

Table 3.13

Probability of exiting unemployment: spells ending in inactivity or censored

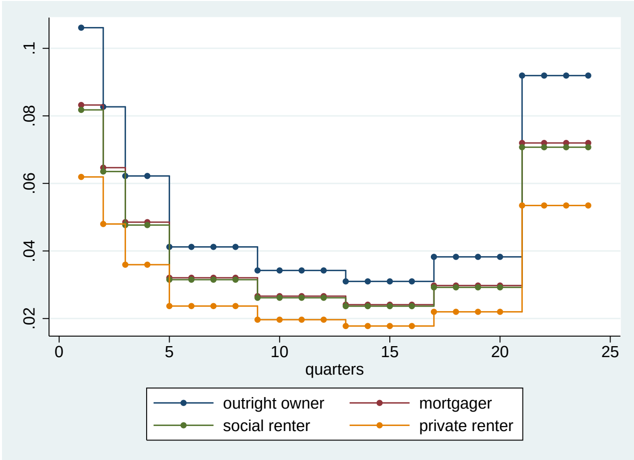
	(1) Hazard with frailty		(2) Hazard without frailty
	dy/dx	*hazard ratio*	*hazard ratio*
outright owner	1.98**	1.755**	1.776**
mortgager	0.96**	1.359**	1.365**
social	0.84**	1.335**	1.349**
claimant	-2.01**	0.517**	0.518**
disability benf.	1.58**	1.579**	1.645**
married	0.61**	1.228**	1.221**
baseline 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
ρ			0.00001

Notes:

1. * significant at 5%; ** significant at 1%. See the appendix C 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 3.1 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 ρ statistic is a test for the presence of frailty.

Figure 3.11

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

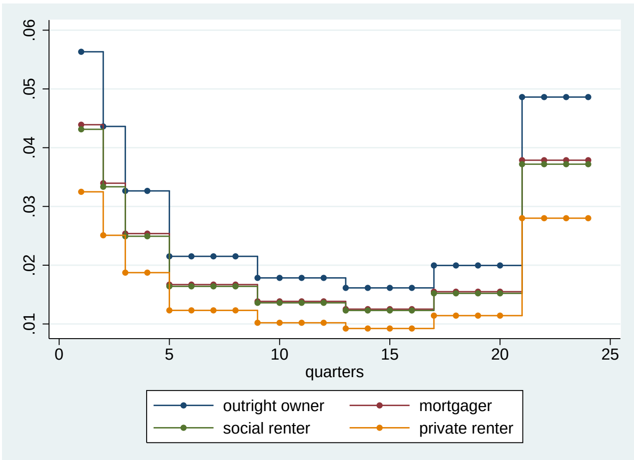


Notes:

1. Predicted hazards have been estimated after the clog-log non-frailty model (table 3.13, 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 3.12

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



Notes:

1. Predicted hazards have been estimated after the clog-log non-frailty model (table 3.13, 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.

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.

3.6 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.

Appendix A

Description of Variables (Chapter 2)

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.

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

mortgager: accommodation owned with mortgage.

outright owner: accommodation outright owned.

renter: accommodation rented.

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

private renter: accommodation rented from private.

Housing costs

Measures net monthly mortgage or rent costs as paid in the last month instalment. For renters who receive housing benefit, either partial or complete, includes the rent after the rebate. Variable is zero for houses rent free or owned outright. This is an household variable, so the housing costs for the household are imputed to all individuals within it at the date of interview. As all monetary variables, this is adjusted for inflation using a Retail Prices Index (the czbh series at the ONS web site).

Equivalized household income

As household income we use the sum of the gross individual incomes perceived last month. It is a gross variable in that we refer to incomes before deductions for income tax, NI, pension contributions and local taxes have been made. For each household income we apply the McClements' scale factor to account for the effects of household size and composition on needs in making income comparisons. Thus the effect of our equivalized income measure should not be sensitive either to the household size or to its composition.

Household size

Number of people living within the household at the time of interview.

Dependent child

This is a dummy for households with at least one dependent child, that is those up to 18, if they are still in (non-advanced) full-time education.

Claimant

This is a dummy for people claiming unemployment-related benefit. 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. While after 1996 there is a clear question which asks directly if the respondent is claiming JSA (whether cont-JSA or inc-JSA), before that it is more complicated to identify claimants for the retributive element, since the question on income support benefits is general and does not specify the reasons for the claim. The strategy is thus to count in the unemployment benefit claimants pool also people who get income support, both before and after JSA. Of course this has the shortcoming that also people who get IS for reasons other than unemployment (for example since they have just low income) are counted as unemployment benefit claimants. But it is not so problematic for our analysis, given that the effect on search effort of the unemployment benefit may be similar to that of income-related benefits, which may be lost as well when the unemployed finds a job and his income rises.

Financial Situation

This variable captures a subjective evaluation of respondent financial situation. Precisely it refers to the question: "How well would you say you yourself are managing financially these days?". The responses may be five, where the higher is the number, the worse is the financial situation: (1) Living comfortably; (2) Doing alright; (3) Just about getting by; (4) Find it quite difficult; (5) Find it very difficult.

Age

The base age range in the regressions is 25-49, then we use dummies for young (16-24) and elderly (50-64) people. The sample is restricted to all people in working wage, that is 16-64 for males and 16-59 for females.

Disability Benefits

This is a dummy which selects people getting incapacity benefits or severe disablement allowance. In 1996 the incapacity benefit assembled under a unified system invalidity and sickness benefits.

Pension

This dummy takes one if respondent gets NI retirement pension or private pension or annuity.

Full-time education

This dummy identifies people who are in full-time education.

Relation with head of household

This identifies the relation between the respondent and the head of household. The BHPS uses the concept of Household Reference Person (HRP), defined as the person legally or financially responsible for the accommodation, or the eldest of two people equally responsible. The head of household (hoh) is defined in general (for example by the General Household Survey) as the principal owner or renter of the property, where (if there is more than one) the male takes precedence, and (if there is more than one potential hoh of the same sex) the eldest takes precedence. The BHPS HRP definition is similar except that only the age criterion is used to distinguish multiple potential HRPs. In our analysis we use the hoh definition which requires only minor replacements by sex with respect to the HRP definition. We identify three other categories: (1) lawful spouse or live-in partner; (2) children: hoh's child (natural, adopted, foster, step-child), partner's child, daughter/son-in-law, any grand-child, any nephew; (3) other living within the household, whether or not member's relatives. The base group in the regressions is made of head of households, then we include 3 dummies in the regression to control for the other categories.

Duration since last job

We created five categories depending on time past since last occupation: from now to 6 months ago, from 6 to 12 months, from 1 to 3 years, from 3 years or more, and the last refers to people who have never had a job. The base category in the regressions is the first.

Education

These are 7 levels of education attained: (1) first or higher degree; (2) HND, HNC, teaching qualification; (3) GCE a level; (4) GCE o level; (5) cse; (6) other qualification; (7) no qualification. The base category is the last.

Regional dummies

Regions are 19: (1) Inner London; (2) Outer London; (3) Rest of South East; (4) South West; (5) East Anglia; (6) East Midlands; (7) West Midlands Conurb (8) Rest of West Midlands; (9) Greater Manchester; (10) Merseyside; (11) Rest of North West; (12) South Yorkshire; (13) West Yorkshire; (14) Rest of Yorks & Humber; (15) Tyne & Wear; (16) Rest of North; (17) Wales; (18) Scotland; (19) Northern Ireland.

Appendix B

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 (Subsection 3.3.1) and whose estimates are reported in table 3.7, 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

$$EV_{ij}^* = \mathbf{z}_i' \alpha_j + \delta_j l_{ij} + \eta_{ij}, \tag{B.1}$$

where $\mathbf{z}_i$ denotes a set of exogenous covariates with parameters α_j, η_{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 η_{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

$$P(\mathbf{d}_i | \mathbf{z}_i, \mathbf{l}_i) = \frac{\exp(\mathbf{z}_i' \alpha_j + \delta_j l_{ij})}{1 + \sum_{k=1}^{J} \exp(\mathbf{z}_i' \alpha_k + \delta_k l_{ik})}. \tag{B.2}$$

Estimates of this model are reported in appendix D in table D.1.

The expected binary outcome equation for individual i is formulated as

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

where $\mathbf{x}_i$ is a set of exogenous variables and γ_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 μ is assumed to have a logit form: $\exp(\cdot)/(1+\exp(\cdot))$. The interpretation of the factor-loading parameters λ_j is the following: when λ_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

$$E(y|d_s = 1) - E(y|\mathbf{d} = 0) = \mu(\mathbf{x}'\beta + \gamma_s + \sum_{j=1}^{3}\lambda_j l_j) - \mu(\mathbf{x}'\beta + \sum_{j=1}^{3}\lambda_j l_j). \qquad \text{(B.4)}$$

Once β, γ_s and λ_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

$$\mu(\bar{\mathbf{x}}'\hat{\beta} + \hat{\gamma}_s) - \mu(\bar{\mathbf{x}}'\hat{\beta}), \qquad \text{(B.5)}$$

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

Appendix C

Description of Variables (Chapter 3)

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[1]. 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.

[1]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`.

Appendix D

Housing Tenure Choice Model

In table D.1 we report estimates of the mixed multinomial logit discussed in appendix B. This model is estimated jointly with a binary non-employment equation by a multinomial endogenous treatment effect technique. Results of the binary outcome equation are reported in column (3) of table 3.7.

We report Relative Risk Ratios (RRR) for ready interpretation. Coefficients must be read in relation to the base category, *i.e.* private rented dwelling. Given the variable x and the residential status j ($j \in \{OUT, MORT, SOC, PRI\}$), the RRR is defined as $\frac{P_1(y=j|X)}{P_1(y=PRI|X)} / \frac{P_0(y=j|X)}{P_0(y=PRI|X)}$, 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). 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 $RRR < 1$.

Table D.1
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. Notes to table 3.7 apply here.

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