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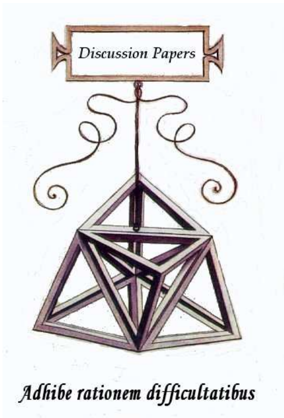

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Abstract

This paper investigates the relation between job search effort and housing tenure by focussing on the impact of the UK Jobseeker's Allowance reform introduced in the UK in 1996. 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 this effect adjusts in size depending on the different housing tenure. Average Treatment Effect estimates confirm that the impact of the reform on the claimant outflow rate is related to housing tenure.

Classificazione JEL: 64, J68, R2
Keywords: Jobseeker's Allowance, Unemployment Benefit, Job Search, Housing Tenure, Oswald Effect

Contents

I. 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 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 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

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

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 the next section we review related literature and we discuss how this paper contributes to it. Section 3 describes the changes JSA introduced and its main characteristics, and provides preliminary evidence on its effects. Section 4 proposes a search model to represent the effect of JSA also considering moving costs and housing costs. Section 5 describes the data used in this paper. Section 6 presents the methodology used to conduct our analysis. Section 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 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 9 concludes.

II. 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 (2000) stated, "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

[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.

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. (1998) found that the estimated effect is quite small.

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 (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

[3] See Johnson and Klepinger (1994).

[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

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 (1996, 1997, 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, Munch et al., 2003), some later tests of the Oswald's hypothesis, with both macro and micro-data, have provided evidence for its reverse.[6] Green and Hendershott (2001) 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 Garca and Rodrguez Hernndez (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, 2002, Flatau et al., 2003, Munch et al., 2003, Battu, Ma and Phimister, 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

[6]Rouwendhal and Nijkamp (2007) provide a survey of studies which tested the Oswald's thesis.

[7]First Oswald (1996, 1999) and later on Green and Hendershott (2001) used OECD countries' and regions' data in which neither Spanish regions nor provinces were included.

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., 2003, Rouwendhal and Nijkamp, 2007).

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.

III. 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 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]

[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.

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 (see Manning, 2009, and 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 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

[10]Pointer and Barnes (1997) provide a detailed description of institutional and administrative aspects of JSA. See Finn et al. (1996) for a description of the previous UB/IS scheme.

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 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.

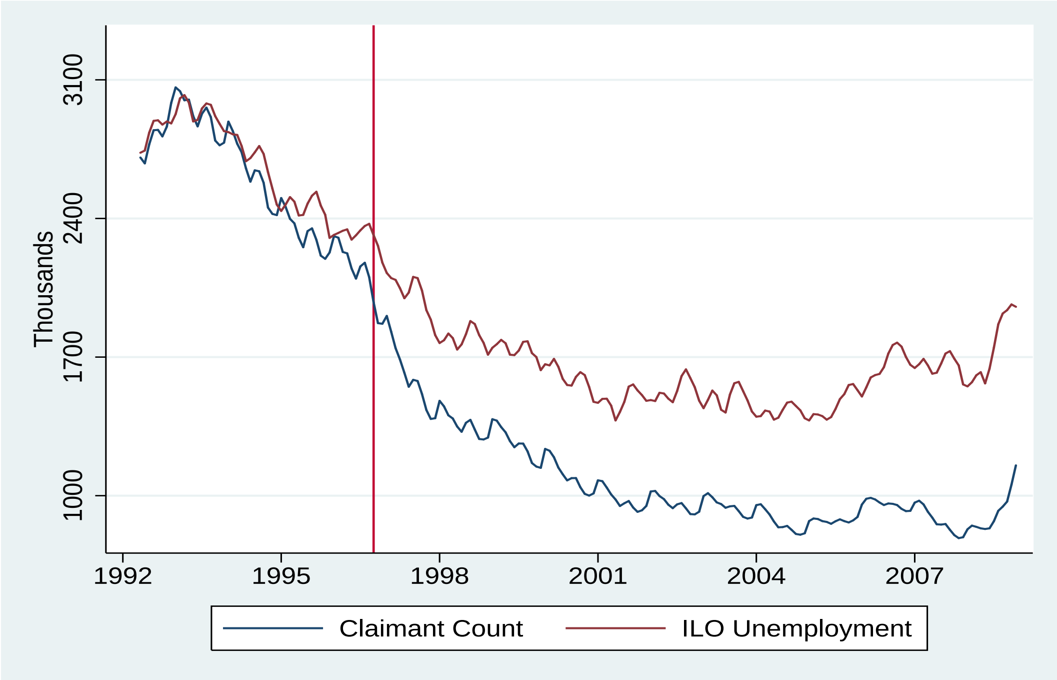

Figure 1: Claimant Count and ILO Unemployment

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.

IV. 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 (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

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

is fixed and independent of the wage, and search for a job with effort s, where s measures the time 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 (1996, 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] 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, 2007, 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 $\lambda(s)$.

[14] Empirical evidence is also provided by Van den Berg and Van Vuuren (1998), and Munch et al. (2003) 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 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 (section ...) 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.

U satisfies the following equation:

$$rU = \max_{s,w_R}\{b - c(s) - H + \lambda(s)\int_{w_R+M}[W(w) - U]\,dF(w)\}, \quad (1)$$

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:

$$rW(w) = w - H + \delta[U - W(w)]. \quad (2)$$

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 (2) we get $W'(w) = 1/(r+\delta)$ so, after integrating by parts, we can rewrite (1), which also implicitly define the reservation wage, as:

$$w_R = rU = \max_s\{b - c(s) - H + \frac{\lambda(s)}{(r+\delta)}\int_{w_R+M}[1 - F(w)]\,dw\}. \quad (3)$$

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

$$c'(s^*) = \frac{\lambda'(s^*)}{(r+\delta)}\int_{w_R+M}[1 - F(w)]\,dw, \quad (4)$$

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 (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:

$$\frac{dw_R}{db} = \frac{r+\delta}{r+\delta+\lambda(s)[1 - F(w_R+M)]} > 0. \quad (5)$$

Differentiating (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:

$$\frac{dw_R}{ds} = \frac{r+\delta}{r+\delta+\lambda(s)[1-F(w_R+M)]}\left\{\lambda'(s)A - c'(s)\right\}, \qquad (6)$$

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 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. 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.

[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.

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

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

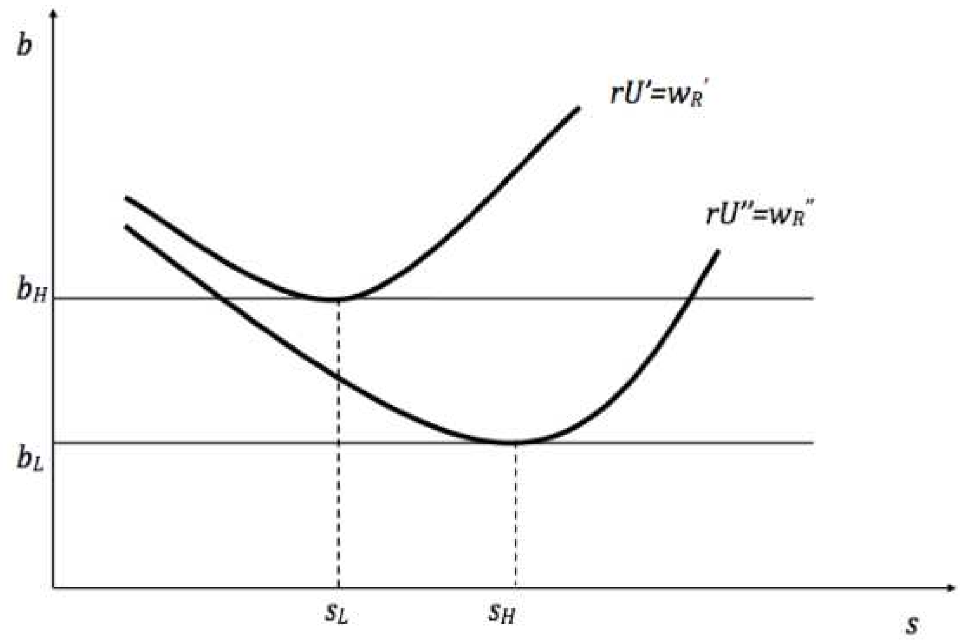

Figure 2: The Choice of Search Intensity

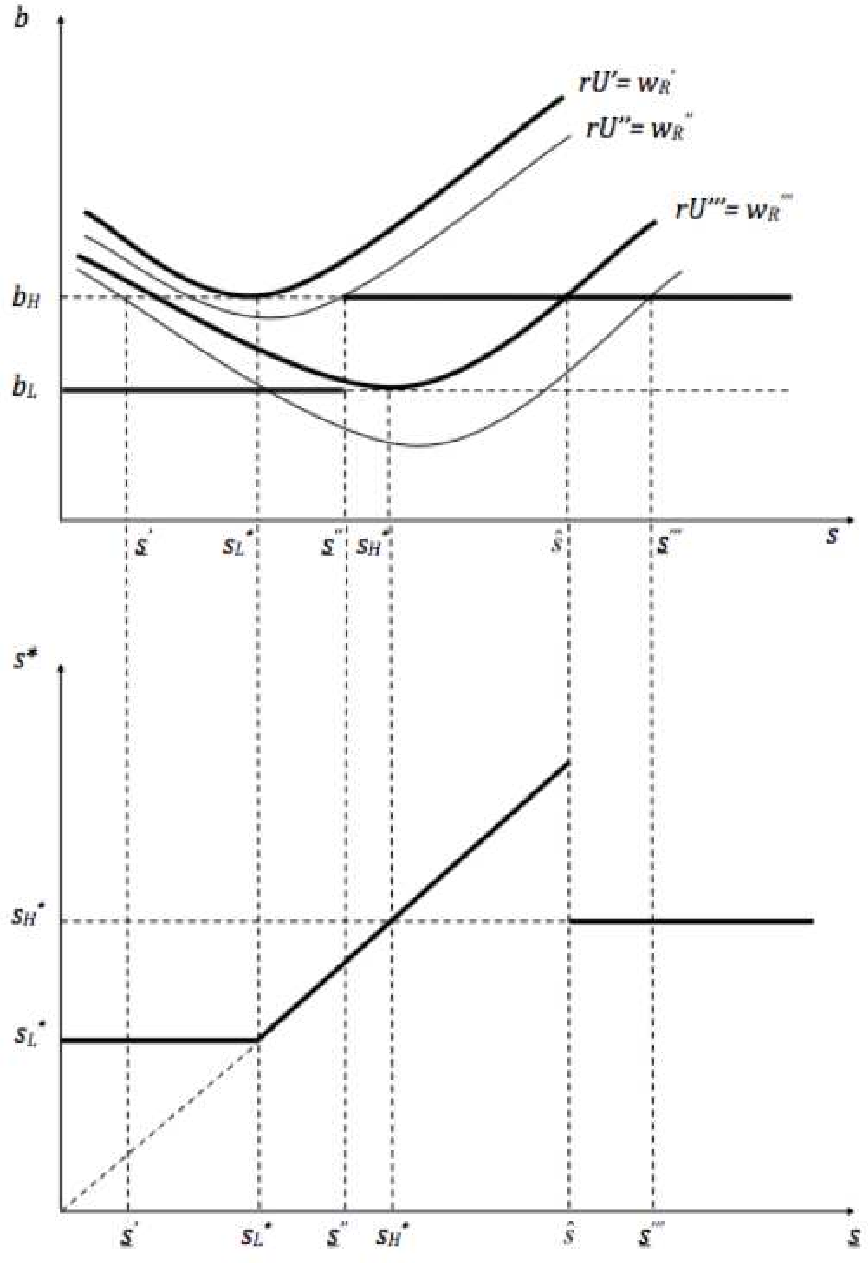

Figure 3: The Impact of Stricter Eligibility Conditions

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 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 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 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 (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:

$$s_m^* - s_o^* = s^*(M_m, H_m) - s^*(M_o, H_o) = \frac{ds^*}{dM}(M_m - M_o) + \frac{ds^*}{dH}(H_m - H_o), \tag{7}$$

$$s_r^* - s_o^* = s^*(M_r, H_r) - s^*(M_o, H_o) = \frac{ds^*}{dM}(M_r - M_o) + \frac{ds^*}{dH}(H_r - H_o), \tag{8}$$

$$s_m^* - s_r^* = s^*(M_m, H_m) - s^*(M_r, H_r) = \frac{ds^*}{dM}(M_m - M_r) + \frac{ds^*}{dH}(H_m - H_r), \tag{9}$$

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 (4) we can study the sign of ds^*/dM and ds^*/dH. From (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]

$$\frac{ds^*}{dH} = -\frac{\phi_H}{\phi_{s^*}} > 0, \tag{10}$$

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

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

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

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

$$\frac{ds^*}{dM} = -\frac{\phi_M}{\phi_{s^*}} < 0. \tag{11}$$

Equations (10) and (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. 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.

V. 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-

[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".

groups. The survey provides information enough to split the sample into three categories according to different housing tenures: owners outright, 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.

VI. 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

[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 ...

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 :

$$y_i = \beta_0 + \beta_1 d96_i + \beta_2 d97_i + \beta_3 jsa_i + \beta_4 jsa_i * d96_i + \delta X_i + u_i, \quad (12)$$

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

[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.

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 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

[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.

the claimant count after the introduction of JSA. This means that our estimated treatment effect may have been even higher.

VII. The Impact of JSA on Claimant Outflows

Results in Table 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 coeffi-

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

Table 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).

cient does not tell anything about people 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 between movers into non-claimant non-employment status (second row of Table 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 em-

seasonal effect).

ployment, 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 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

[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 (2004) 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.

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 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 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 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

[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 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.

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. 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 3 shows similar results when we split the sample by num-

Table 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 apply here.

ber 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 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]

[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 Table 2 and 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.

Table 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 apply here.

VIII. *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 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].

[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.

[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

Table 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 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 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 5 shows the results of this exercise. As we remember from the previous section, JSA had no effect on the flow

referring to people who claim or not housing related benefits, but differences in treatment effects by housing tenure categories are largely unaffected.

Table 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 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.

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 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 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,

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

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.

Table 6 and 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 8. The coefficient of the

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 6: Testing differences in High Search: search categories

Differences in High Search (HS)	Coefficient	Std. Err.	$p > \|t\|$	[95% Conf.	Interval]	obs.
STAYERS						
$HS_m - HS_o$	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*

Differences in High Search (HS)	Coefficient	Std. Err.	$p > \|t\|$	[95% Conf.	Interval]	obs.
MOVERS						
$HS_m - HS_o$	-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*

Differences in High Search (HS)	Coefficient	Std. Err.	$p > \|t\|$	[95% Conf.	Interval]	obs.
ALL						
$HS_m - HS_o$	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 7: Testing differences in High Search: numbers of search methods

Differences in High Search (HS)	Coefficient	Std. Err.	$p > \|t\|$	[95% Conf.	Interval]	obs.
STAYERS						
$HS_m - HS_o$	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*

Differences in High Search (HS)	Coefficient	Std. Err.	$p > \|t\|$	[95% Conf.	Interval]	obs.
MOVERS						
$HS_m - HS_o$	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*

Differences in High Search (HS)	Coefficient	Std. Err.	$p > \|t\|$	[95% Conf.	Interval]	obs.
ALL						
$HS_m - HS_o$	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 6. Statistics allow for sample weights.

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 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 6, 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 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

[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.

Table 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.

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 solution 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

[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.

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 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 9 and 10 we report estimates of the average treatment effect 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

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

Table 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 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 9 apply here.

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 the 3rd section, 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 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 Table 9 and 10 for this case is mild and it is still unexplained why the “weeding out” effect was not higher for outright owners than mortgagers.

IX. 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

[41] 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.

JSA brought many changes 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.

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