Too much stick for the carrot? Job search requirements and search behaviour of unemployment benefit claimants☆,☆☆

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Abstract

This paper investigates the effects of stricter job search-related conditions for Unemployment Insurance (UI) eligibility on the job search behaviour of claimants. Estimation makes use of exogenous variation introduced by the UK Jobseeker's Allowance. A significant share of claimants is found to leave the register increasing search intensity as well as request for job search assistance from Public Employment Centres. Exits with increased search are performed predominantly by liquidity constrained individuals. This evidence suggests that tighter search conditionality can lead committed jobseekers to un-subsidized search, reducing the capacity of UI to protect workers against the risk of unemployment.

Keywords: Unemployment benefit, job search requirements, job search effort, job search methods, impact evaluation.

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1. Introduction

Over the last three decades, a large body of literature has been dedicated to analysing the impact of unemployment insurance (UI) systems on the labour market (Holmlund, 1998; Krueger and Meyer, 2002). While this literature has initially focused on the level and the duration of benefits, a growing strand of more recent research has focused on eligibility conditions related to job search, such as minimum job search requirements, monitoring and sanctioning. A common belief is that this "stick" component can mitigate the moral hazard effect on job search caused by generous and long-lasting UI payments (Grubb, 2001; Fredriksson and Holmlund, 2006; McVicar, 2014). For this reason most OECD countries have tightened the enforcement of such measures since the mid-1990s (Langenbucher, 2015).

A wealth of studies has documented that search conditionality reduces the time spent on benefit, generating large savings in welfare payments. However, although earlier exits from benefit often correspond to transitions to jobs, several studies have found limited or no effects on employment inflows (see Caliendo and Schmidl, 2016; McVicar, 2014; Meyer, 1995, for surveys on the evidence). Moreover, more recent studies have pointed out some unintended consequences of search-conditionality measures, leading to some criticism on their utility (Raffass, 2017; Cockx et al., 2014). In fact tighter conditions can increase UI outflows to lower paid and/or more unstable jobs (see e.g. Van den Berg, Hofmann and Uhlendorff, 2016), to other income support benefits (see e.g. Avram, Brewer and Salvatori, 2018), to irregular jobs (Wolff et al., 2016) or criminal activities (Machin and Marie, 2006). They can also lead to a substitution from effective search methods to ineffective ones (Van den Berg and Van der Klaauw, 2006) and to higher rates of non-compliance and benefit sanctions (Arni and Schiprowski, 2019).

Despite the large interest in the employment effects of search conditionality, the literature has dedicated only limited attention to the effects on job search behaviour (Arni and Schiprowski, 2019, 2015; Manning, 2009; Van den Berg and Van der Klaauw, 2006; Keeley and Robins, 1985). More specifically, no studies have so far investigated the effects on the job search of claimants leaving the UI system for unsubsidized unemployment. This appears a relevant gap, since it has been often reported that a sizeable fraction of claimants drop off the register without returning to work (Avram et al., 2018; Wolff et al., 2016; Arni et al., 2013; McVicar, 2010; Manning, 2009; Petrongolo, 2009; Card et al., 2007).

Furthermore, a primary objective of search conditionality is "drawing a much closer link between the receipt of benefit and the claimant's demonstrated willingness to look for work" (Clarke, 1993).[1] This objective has two implications, which may be often in conflict. On the one hand, subsidised

[1]This statement was made by the UK Minister of Finance Ken Clarke during the announcement of the Jobseeker's Allowance (JSA) held at the House of Lords on 30 November 1993.

unemployed are expected to engage in active search, but also, on the other hand, unemployed engaged in active search are expected to be subsidized. While search conditionality may fulfill the first goal by lessening the moral hazard, it may limit the capacity of the UI to insure workers against the risk of unemployment.

In some cases, jobseekers committed to return to work may opt for un-subsidized search because they feel excessive bureaucratic pressure, or because they are bounded to accept downgrading jobs. In line with this insight, Arni et al. (2013) found that sanctioning increased exits to unregistered unemployment but reduced its duration, suggesting that leavers might be highly active in their search. Moreover, some studies report evidence that mandatory re-employment services aiming to support claimant's job search can act as a threat of extra administrative burden, despite they are meant to enhance their job search abilities and job prospects. In fact, a substantial portion of claimants assigned to programs combining monitoring and job search assistance leave the register, hence renouncing both assistance and UI payments, just before the start of these mandatory programs, or even by simply receiving notification to attend (Black et al., 2003; Dolton and O'Neill, 2002; Johnson and Klepinger, 1994; Klepinger et al., 2002). Job search assistance services are known as the "carrot" component of the UI system.

The present paper analyzes the impact of tighter search-related conditions on the job search behaviour of UI recipients exploiting exogenous variation introduced by the UK Jobseeker's Allowance (JSA). The JSA was introduced on 7 October 1996 and brought about a key innovation into the UK welfare system by means of a substantial tightening of job search-related eligibility rules. Manning (2009) and Petrongolo (2009) found that JSA increased outflows of claimants remarkably but was not successful in moving them into jobs. Manning (2009) found that UI drop-outs generated by the JSA were larger for groups of claimants with low initial level of search. This might suggest that claimants leaving the UI were mostly not engaged in active search. However, Manning (2009) did not investigate the job search response of leavers, and hence one cannot exclude that they possibly increased search when leaving the UI.

The present analysis contributes to the literature by investigating separately the effects on search of claimants staying in the UI pool ("stayers") or leaving it without returning to work ("leavers"). UK Quarterly Labour Force Survey data are used to estimate the JSA impact on two categorical variables capturing changes in the search intensity ($\Delta s^* \gtreqless 0$, where s^* is the number of search methods) and in the use of the Public Employment Centre (PEC) as a search method (dismissal, adoption, stable use/non-use) respectively. The effects on the two variables are estimated overall and decomposed by stayers and leavers. Changes in outcomes are measured between two consecutive quarters, using claimant transitions between the quarters around the start date to form a treatment group (flows between 1996q3 and 1996q4), and comparing these changes with claimant transitions between the preceding quarters (flows between 1996q2 and 1996q3). Similar cohorts of treated and control claimants are drawn from other years to eliminate the seasonal effect with a Diff-in-diff approach.

The purpose of this analysis is to measure the success of the JSA reform in conditioning benefit receipt to commitment to look for work, and viceversa. On the one hand, the focus on stayers can inform whether subsidized unemployed enhanced their job search in line with the intended goal of moderating moral hazard. In this respect, the JSA appears an interesting case study since previous evaluations found no impact on job inflows. Poor employment outcomes might be explained by failure to adjust search to tougher requirements or by larger use of less effective search methods. The latter result may arise if, for example, tighter rules led to more frequent contact of PEC, which has been often blamed to be ineffective for finding a job (Holzer, 1988; Blau and Robins, 1990; Addison and Portugal, 2002; Longhi and Taylor, 2011).

On the other hand, the focus on leavers can inform whether claimants weeded out from the UI system were actually those less motivated to get back to work. Since many unemployed individuals have liquidity constraints (Chetty, 2008), it is possible that a relevant fraction of leavers may be unable to absorb the income loss. These individuals should be highly motivated to find a job because they need to finance their consumption. Therefore, in order to preserve the UI insurance function, the policy should aim to minimize outflows to un-subsidized unemployment of liquidity constrained individuals. The present work shows that change in search intensity is a strong predictor of the liquidity status and hence can be used to measure the success of the policy in this respect. The JSA appears an interesting case study also from this perspective. First, it generated a large outflow into unregistered unemployment hence providing a relevant sample to study the behaviour of leavers. Second, tighter eligibility rules were not accompanied by extra search assistance service possibly counteracting the increase in drop-outs (Petrongolo, 2009).

The rest of the paper is organized as follows. Section 2 relates the present contribution to the relevant literature and Section 3 introduces the JSA. Section 4 presents the economic background relevant for the study of the impact of stricter search-related eligibility criteria. Section 5 describes data and methods. Section 6 presents and discusses the results of the JSA impact evaluation. Section 7 concludes. Additional evidence and information are reported in the Supplementary Materials (SM).

2. Related literature

The present work is related to the large body of literature investigating the effects of search-related eligibility conditions for UI. Search conditionality aims to restore incentives to search of benefit recipients by setting minimum search requirements, by monitoring search activity and by sanctioning non-compliers with benefit cuts or suspensions. Such measures, either in combination or isolation, are typically found to reduce the time spent on benefit (Johnson and Klepinger, 1994; Klepinger et al., 2002; Abbring et al., 2005; Lalive et al., 2007; Svarer, 2011; Van der Klaauw and Van Ours, 2013; Arni and Schiprowski, 2019). However, evidence that shorter spells on benefit correspond to new hires is less conclusive. In fact, sometimes the impact on employment inflows is null or at best weak (Meyer, 1995; Ashenfelter et al., 2005; Van den Berg and Van der Klaauw, 2006; Card et al., 2007; Micklewright and Nagy, 2010; Engström et al., 2012; Cockx et al., 2018).[2] Manning (2009) and Petrongolo (2009), using Labour Force Survey and administrative data respectively, found that tighter search conditionality introduced by the JSA increased remarkably claimant outflows but was not successful in moving them into jobs, both in the short run (Manning, 2009) and in the long run (Petrongolo, 2009).[3]

Although search-conditionality measures can generate savings in welfare expenditure by reducing the time spent on UI, and sometimes enhance job inflows, they are also found to create a number of unintended consequences. First, transitions to work induced by tighter rules are often found to correspond to less paid and/or more unstable jobs (Van den Berg et al., 2016; Arni and Schiprowski, 2019, 2016, 2015; Van den Berg and Vikström, 2014; Arni et al., 2013; Petrongolo, 2009).[4] Second, UI outflows can be associated to an increase in inflows into other income support benefits, typically health-related ones (Avram et al., 2018; Lammers et al., 2013; Fok and McVicar, 2013; Petrongolo, 2009). Third, claimants leaving the register may drop off the formal labour market and start irregular jobs or criminal activities (Wolff et al., 2016; Machin and Marie, 2006). Fourth, higher search requirements can increase rates of non-compliance and benefit sanctions (Arni and Schiprowski, 2019). Furthermore, theoretical evaluations show that search conditionality can be even socially inefficient if requirements are too strict (Cockx et al., 2014). Overall the available evaluations have raised concerns on the utility of search conditionality measures, leading to some criticism (Raffass, 2017).

Theoretical models analysing search-related conditions show that the effects on exits to employment are mediated by the intensity and the methods of search used (Abbring et al., 2005; Van den Berg and Van der Klaauw, 2006; Manning, 2009). However, only few studies have evaluated these effects empirically. Keeley and Robins (1985) found for the US that unemployed subject to search requirements had higher number of search methods and weekly hours, but no better job hazards, pointing to a substitution from more effective to less effective methods.

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[2] Earlier evidence on the impact of these measures is based on a number of social experiments, especially in the US, often combining search conditionality ("the stick") and job search assistance ("the carrot"). See Meyer (1995) for an extensive survey and evaluation of experiments in the US. More recent studies have been able to isolate the effect of the stick component and to separate the effects of monitoring and sanctions within that. See McVicar (2014) and Caliendo and Schmidl (2016) for surveys of more recent studies.

[3] For the Northern Ireland, McVicar (2008) found a reduction in claimant outflows as well as in employment inflows during exogenous temporary suspensions of JSA eligibility checks due to Benefit Office refurbishments.

[4] Arni et al. (2013) and Van den Berg and Vikström (2014) even find that income losses due to lower earnings and stability outweigh income gains associated to earlier employment, leading to a net loss. These negative effects can persist in the long run suggesting that the initial occupational downgrading can lead to human capital losses (Arni et al., 2013; Van den Berg and Vikström, 2014). Arni and Schiprowski (2019) find that an increase in search requirements by one application has a small negative effect on the duration of re-employment spells and no effect on re-employment wages.

A similar argument has been formalized by Van den Berg and Van der Klaauw (2006) in a theoretical model distinguishing between formal and informal search. Formal search is by definition under monitoring of the Benefit's agency, while informal search is not monitored. In their model, increased monitoring leads to substitution from informal to formal methods, with ambiguous effect on the job finding rate depending on the relative effectiveness of methods. Using data from a Dutch randomized experiment, they found evidence of this substitution effect, specifically an increase in use of Employment Offices/Agencies and job advertisements, but overall no effect on the job entry rate.

Manning (2009) found that JSA had no effect on search intensity, as measured by the reported number of search methods and by an indicator of the time elapsed since the last search. Arni and Schiprowski (2015) using Swiss register data found that binding requirements increased the number of applications as well as the job finding rate of claimants, but non-binding requirements had opposite effects. Search requirements were defined binding (respectively non-binding) if the difference between the required and observed ex-ante number of applications was positive (respectively negative). Using the same data set, Arni and Schiprowski (2019) found a positive but less than proportional effect of search requirements on search efforts of claimants, suggesting the presence of imperfect compliance.[5]

Among the few studies investigating the impact on search behaviour, none has considered so far the search behaviour of claimants leaving the UI system for un-subsidized unemployment. Manning (2009) found that UI drop-outs generated by the JSA were larger for groups of claimants with low initial level of search. This raises the possibility that claimants leaving the JSA were mostly not engaged in active search and hence their removal can be considered an intended effect. However, Manning (2009) did not investigate the job search response of UI leavers, and hence it is possible that they may have switched to un-subsidized search with increased effort.

Arni et al. (2013) is the only study investigating job search outcomes of leavers. They found that a combination of warning and enforcement of a sanction doubled exits to unregistered unemployment, and reduced the duration of post-claim unregistered unemployment as well as post-unemployment earnings. An increase in the hazard to employment may suggest that leavers were committed to look for a job, although of lower quality. Moreover, the negative effect of the treatment on post-unemployment earnings was larger for claimants experiencing a period of unregistered unemployment, relative to claimants leaving UI for a job. This means that leavers paid a price in terms of un-subsidized search that was not compensated by future earnings. A limitation of this study is that only final employment outcomes were analyzed, with no evidence on the job search response. A positive

[5] Arni and Schiprowski (2019) used caseworker's stringency as an instrument to estimate the impact of search effort and search requirements on labour outcomes, measuring caseworker's stringency by the average search requirement assigned by the caseworker to claimants other than the focal one. In the 1st stage equations they found that an additional application required by the caseworker to other job seekers, increased the search effort and search requirement of the focal claimant by 0.7 and 0.47 applications, respectively, which implies a 0.67 (= 0.47/0.7) increase in the number of applications provided by the individual for an additional required application.

employment impact can be the result of increased search intensity and/or substitution from ineffective to effective methods. The present work contributes to the literature by analysing the impact of JSA on search intensity and on the use of PEC, decomposing effects by stayers and leavers.

3. The JSA

The JSA was introduced in the UK on 7 October 1996. Before the JSA, the UI system consisted of a contributions-based benefit, called Unemployment Benefit (UB), and a means-tested allowance, called Income Support (IS). The JSA replaced both schemes maintaining a contributory component, known as cont-JSA, and a means-tested component, known as inc-JSA. The JSA had a retrospective nature since the new rules were applied also to claimants starting an UB or IS spell before 7 October 1996 (Finn et al., 1996).

The cont-JSA has a limited duration of 6 months maximum, while the inc-JSA has potentially unlimited duration. Inc-JSA is by far the most important component, since many unemployed do not have enough contributions for entitlement to cont-JSA and can get inc-JSA as full payment in place of or as a top up of cont-JSA. For example, considering LFS data for the 4th quarter in 1996, 76.4% of JSA recipients were receiving inc-JSA, of which 5.3% as a top up of cont-JSA, while 23.6% were receiving only cont-JSA.

The relevant changes introduced by this reform can be allocated to two different areas.[6] First, the JSA reduced the level and the duration of the contribution-based benefit. The payment rate of both components was made identical to the former IS scheme level. This amounted to a 20% cut of the contribution-based component for young people (aged 18-24) and to a negligible reduction for older people. In the 4th quarter of 1996, individuals below 25 on cont-JSA were 4.7% of the claimant count. The maximum duration for the contribution-based benefit was reduced from 12 to 6 months; this had no impact on the actual length of the claim because these payments were followed by the means-tested payments both before and after JSA. Since changes in this area affected only a modest portion of claimants, their impact has been judged negligible (Manning, 2009; Petrongolo, 2009).

The second and most significant change was a substantial tightening of job search related eligibility rules. The motivation for this major change was clearly stated in the White Paper accompanying the reform's introduction: "The taxpayer has a right to expect the commitment of unemployed people to make every effort to get back to work" (Department for Social Security and Department for Employment, 1994). With the JSA scheme, benefit receipt is conditioned upon signing a Jobseeker's Agreement in which claimants commit to active search. In this agreement, claimants have to state the types, days and hours of work they are available for, and to report a detailed breakdown of their

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

activities to find a job.

After signing the Jobseeker's Agreement, claimants have to keep a thorough record of their activities, and at fortnightly interviews, the caseworker checks whether this record complies with the agreement. In case claimants fail to comply with the agreed commitments, they are subject to sanctions consisting in a temporary suspension of JSA payments. Unlike the old rules for IS, no JSA at all is paid during the sanction period unless the claimant qualifies for a hardship payment. Depending on the reason for the sanction, the sanction period may be fixed of two or four weeks, or between one week and 26 weeks.

While JSA implied a higher threat of sanctions, the actual sanctioning rate was rather low both before and after its introduction. McKay et al. (1997) and Smith et al. (2000) analysed two separate but nationally representative cohorts of approximately 5000 claimants surveyed before and after the JSA. In the follow-up interview 4.4% of respondents in the pre-JSA cohort (McKay et al., 1997) and 4.6% in the post-JSA cohort (Smith et al., 2000) reported they received a sanction in the previous six months. Sanction data are made available by the UK Department for Work and Pensions since April 2000. The monthly rate of sanctions on the claimant count has been on average 2.3% from April 2000 to December 2006, with a peak at 2.8%.

One precursor of the JSA, the Restart Program, was introduced nationally in April 1987. Restart consisted of a 15–25 minutes interview that unemployed people were asked to attend every six months of unemployment. During the interview, unemployed people were assessed in their search history and were offered job search assistance and advice. Failure to attend the interview was followed by threat of sanctions. These Restart interviews were maintained unaltered under the JSA (Pointer and Barnes, 1997).

At the time JSA was introduced, the enforcement of eligibility rules (the "stick") was maintained by the Benefit's Agency, while job search assistance services (the "carrot") were offered by the Jobcentre, who was in charge of maintaining the register of vacancies and supporting jobseekers in the work placement. Although the two offices were placed in the same area, they were functionally and physically separated (McVicar, 2008). Some elements of job search counseling were also provided in the "new jobseeker interview", during which the Jobseeker's Agreement was set up. In this interview, normally lasting 30 minutes, the claimant is instructed about the steps to take in order to increase the chance of finding job and might be referred to a vacancy immediately. However, since the functioning of the Jobcentre was not directly affected when JSA was put into force, it is thought that the job search assistance service was not significantly altered by this reform (Petrongolo, 2009).

Subsequently, during a series of programs gradually introduced since 1999, the two types of services, namely enforcement of eligibility rules and job search assistance, were integrated in one single office, called "Jobcentre Plus", with the aim of strengthening further the two components (Karagiannaki, 2007; Bagaria et al., 2016). Along the same spirit, a second major reform of the UK welfare system, called "New Deal", was gradually introduced since 1998 with a strong emphasis on activation measures towards targeted unemployed people, such as intensive job search counseling, subsidised full-time training/education, wage subsidies paid by employers, or government provided employment (Blundell et al., 2004). While New Deal strengthened remarkably the "carrot" component, JSA was mainly concerned with the "stick" component. New Deal was first introduced in January 1998 only for young people aged 18–24 who had been on JSA for 6 months. New Deal for Young People was initially introduced as a pilot phase involving a limited set of areas, and it was rolled out nationally in April. In July 1998, the program was extended to long-term unemployed, i.e. all those unemployed for over 2 years. After further extensions, New Deal became a permanent feature of the UK unemployment benefit system in 2001 (Van Reenen, 2004). No other relevant policy changes were present around the time JSA was introduced possibly confounding its effects (Machin and Marie, 2006).

4. Search-related eligibility criteria in job search models

This section covers the relevant theoretical background to study the effects of stricter search-related criteria for UI eligibility. Following Manning (2009), a variant of the traditional search model à la Mortensen (1986) is considered as a reference to analyze the role of search requirements. The focus here is on the impact on search intensity of claimants staying in the UI pool ("stayers") or leaving it without returning to work ("leavers"). The Section is organized as follows. Section 4.1 discusses the relation between optimal search intensity and the strictness of search requirements. Section 4.2 identifies possible intended and unintended effects of stricter search requirements.

4.1. The relation between search intensity and search requirements

In the reference model, the unemployed worker receives an unemployment compensation b in case she/he fulfills search requirements, captured by a non-stochastic level of search $s = \underline{s}$, such that the unemployed is claimant (non claimant) if $s \geq \underline{s}(< \underline{s})$. A tightening of search requirements is represented by an increase in $\underline{s}$. The solution to this model corresponds to selecting the optimum level of search s^* that maximises the lifetime utility of the unemployed worker. The solution implies a relationship between s^* and the current rule $\underline{s}$, which is qualitatively depicted in Fig. 1 (see also Manning, 2009). Reference points A–F indicate possible configurations. The derivation of this relation is discussed graphically in Section SM1.

Three points are relevant to draw this relation, corresponding to values of $\underline{s}$ indicated here as s_L, s_H and s_c: s_L and s_H are the optimal search effort levels when eligibility is unconstrained for the claimant and non-claimant unemployed, respectively; s_c is the maximum level of search requirement the claimant abides by since it makes compliance equally valuable to the alternative option of ceasing

Figure 1: Relationship between optimal search intensity s^* and search requirement $\underline{s}$

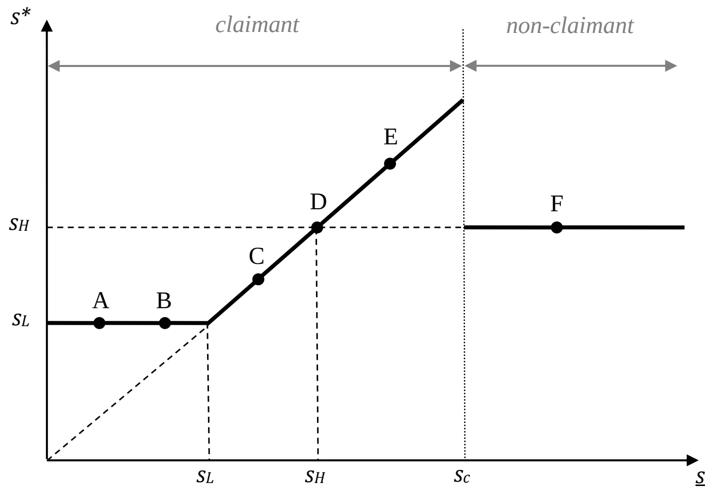

Table 1: Possible scenarios of search requirement tightening. The focus is on claimants in wave 1 (before JSA) still unemployed in wave 2 (after JSA). $C = 1$ indicates permanence in the claimant pool and $C = 2$ indicates exit. See Fig. 1 for the corresponding reference points.

cases	Wave 1	Wave 2	C	Δs^*
	(points in Fig. 1)			
(i)†	–	–	1	< 0
(ii)	A	B	1	$= 0$
(iii)	A	C	1	> 0
(iv)	E	F	2	< 0
(v)	D	F	2	$= 0$
(vi)	C	F	2	> 0

†Although the relation between s^* and $\underline{s}$ is never decreasing when $C = 1$, case (i) is included in the list for consistency with the empirical model, where outcomes are modeled in probabilistic terms and hence a decrease in the probability of case (i) might be observed as an effect of the reform.

claim and lowering search to s_H. One has that $s_L < s_H$ because $\partial s^*/\partial b < 0$. The result that $\partial s^*/\partial b$ is negative derives from the fact that an increase in b reduces the relative price of leisure. When $\underline{s} < s_L$ the eligibility rule is not binding and the claimant chooses the interior solution s_L (for example, points A or B). When $s_L \leq \underline{s} < s_c$ the rule is binding and the claimant chooses the corner solution $s^* = \underline{s}$ (points C, D, or E). When $\underline{s} = s_c$ the unemployed is indifferent between meeting the rules and leaving the claimant pool. When $\underline{s} > s_c$ the unemployed chooses $s^* = s_H$ and leave the claimant status because the marginal cost of compliance is higher than the marginal benefits in terms of higher unemployment income and job offer arrivals (point F).

Fig. 1 can be used to predict the effects of a tightening in search-related criteria on search intensity as well as on claimant state. Possible scenarios are summarised in Table 1, which identifies six combinations of change in search intensity (Δs^*) and in claimant state ($C = 1$ in case of permanence and $C = 2$ in case of drop out) for claimants in wave 1, following an increase in $\underline{s}$ in wave 2. Scenarios can be illustrated graphically using reference points A–F in Fig. 1.

Fig. 1 shows that permanence in the claimant state ($C = 1$) can be observed for any increase in $\underline{s}$ below s_c. Some individuals can maintain the claimant state without adjusting search intensity (case (ii)), while others have to increase it to comply with higher requirements (case (iii)). Case (ii) corresponds to a transition from point A to B in Fig. 1, where $\underline{s}$ is not binding in either waves. Case (iii) corresponds to a transition such as from A to C, where $\underline{s}$ is binding only in wave 2. The distinction between these two cases lies in the fact that $\underline{s}$ is necessarily binding in wave 2 for case (iii) but not for case (ii) (in wave 1 $\underline{s}$ is not necessarily binding for case (iii)). In case (iii), the marginal cost to increase s^* is small relative to gains in terms of benefit receipt (and more job offers), therefore it is optimal to adjust s^* to higher requirements. When the marginal cost becomes too large it is optimal to cease claim (cases (iv)-(vi)), which occurs when $\underline{s}$ exceeds s_c in wave 2, such as in point F. When claimants leave the register, they can possibly decrease search intensity (case (iv)), keep it stable (case (v)), or increase it (case (vi)). These cases are illustrated by transitions from points like E, D, and C, respectively.

Overall, while the expected effect of the reform on the claimant outflow is positive, the effect on the average search intensity is ambiguous. In fact, the impact on search intensity should be positive (or at least non-negative) for stayers, but it is undetermined for leavers. The distinction in search intensity changes for stayers and leavers can be used to identify possible intended and unintended effects of the policy. This topic is discussed in the next section.

4.2. Intended and unintended effects of search requirements

A primary goal of a policy tightening search requirements is to contrast the moral hazard induced by the unemployment benefit, which reduces job search intensity to a socially suboptimal level. Tighter search requirements can diminish moral hazard costs by raising search intensity of claimants. This effect corresponds to case (iii) among the possible scenarios summarised in Table 1. Therefore, an increase in type (iii) transitions can be considered an intended effect of the policy.

In designing optimal UI systems, the moral hazard cost has to be traded-off with consumption smoothing gains: UI protects workers against the risk of unemployment guaranteeing them stable consumption levels should they end up unemployed, at the cost of lower search intensity. While tighter search requirements may diminish the moral hazard cost, they may alter the consumption smoothing function at the same time. In fact, many unemployed individuals have limited liquid wealth and their consumption is highly sensitive to cash on hand (Chetty, 2008). Therefore, since tighter requirements are predicted to increase flows to un-subsidized unemployment, they may also increase the share of unemployed individuals who are unable to smooth consumption.

The success of the policy in mitigating this trade-off can be assessed examining the search behaviour of individuals leaving the UI payment rolls. Search effort adjusts to variations in the unemployment compensation depending on a "moral hazard effect" as well as a "liquidity effect" (Chetty, 2008).

The former effect implies a negative relation via substitution between leisure and consumption. The latter effect can reinforce this negative relation via relaxation of liquidity constrains and hence reduced pressure to find a job. The distinction between the two effects is relevant because they have opposite welfare implications: the moral hazard effect is a socially suboptimal response to a distortion in relative prices; the liquidity effect is a socially beneficial correction to credit and insurance market failures (Chetty, 2008). The present analysis aims to establish a relation between the change in search intensity and the liquidity status of leavers, pointing out possible intended and unintended outcomes of the policy.

The theoretical framework presented in Section 4.1 can be used to derive a correspondence between the sign of Δs^* and the liquidity status of leavers. As a first step, the sign of Δs^* can be examined in relation to the parameters s_H, s_L, s_c, which determine the function $s^*(\underline{s})$ in Fig. 1. A formal analysis of these relations is reported in Section SM2 in the Supplemental Materials. The main conclusion derived from this analysis is the following: the sign of Δs^* for leavers is only affected by the parameter s_H, reflecting the optimal search intensity of an unemployed person without benefit. Specifically, claimants with higher s_H are more likely to drop off with increased search relative to decreased search.

The parameter s_H should be higher for liquidity constrained individuals because they are less able to smooth consumption without benefit. Therefore, given the relation between Δs^* and s_H, the following predictions can be formulated: on the one hand, liquidity constrained individuals are more likely to increase search when they exit UI, which corresponds to case (vi) in Table 1; on the other hand, unconstrained individuals are more likely to reduce search when they exit UI, which corresponds to case (iv). Note that the correspondence between type (vi) (respectively (iv)) transitions and liquidity constrained (respectively unconstrained) individuals can be stated only in probabilistic terms. However this correspondence can be checked with available data and evidence is presented in the empirical analysis below.

In light of the established correspondence, a possible increase in type (iv) transitions can be considered an intended effect of JSA. The search behaviour of these claimants should be influenced by moral hazard only; therefore their exit corresponds to savings in moral hazard costs without prejudice of their consumption smoothing capacity. Conversely, an increase in type (vi) transitions can be considered an unintended effect. In fact, since type (vi) individuals are likely to be liquidity constrained, they may experience significant consumption cuts when leaving the UI, reflecting a weakening of the UI insurance function. Clearly, a moral hazard effect may be present also for these workers, so whether an increase in type (vi) transitions is socially inefficient cannot be ascertained in absolute terms. However, minimization of these transitions is relatively efficient, provided an increase in exits to un-subsidized unemployment is observed as an effect of the policy. Moreover, since the moral hazard effect can be significantly less important than the liquidity effect (Chetty, 2008), savings associated to these transitions may be small relative to losses.

5. Data and Methods

5.1. Data

The data source is the UK Labour Force Survey (LFS) which collects quarterly interviews for about 60,000 households. Each individual is interviewed in five consecutive quarters on a rotating panel basis, so approximately 20% of the sample is replaced each quarter by a cohort of new entrants. Because the JSA reform was introduced on Monday, 7 October 1996, all calendar quarters are postponed by one week in the data, considering the first week of any quarter as the last week of the previous quarter. This change does not impair the LFS sampling design since each individual is interviewed in the same week across quarters and weekly samples are nationally representative (see Office for National Statistics, 2016).

The questions in the LFS about job search methods are used to track changes in job search intensity. Following the empirical literature, a proxy for the job search intensity can be constructed by counting the number of methods the respondent used (Holzer, 1988; Blau and Robins, 1990; Wadsworth, 1991; Schmitt and Wadsworth, 1993; Gregg and Wadsworth, 1996; Boeheim and Taylor, 2001; Addison and Portugal, 2002; Weber and Mahringer, 2008; Manning, 2009; Bachmann and Baumgarten, 2012; Morescalchi, 2016). Considering a total of 14 methods, this variable ranges from 0 to 14 according to the number of positive answers (see Section SM3 for the list of methods).

The number of search methods can be an imperfect measure of search intensity, as it assumes identical level of effort associated to each method. However, previous evidence suggests that this variable is a somewhat reliable proxy for search intensity, because it is normally found to be strongly related to job hazards (Holzer, 1988; Gregg and Wadsworth, 1996; Boeheim and Taylor, 2001; Morescalchi, 2016), notably in British data (Gregg and Wadsworth, 1996; Boeheim and Taylor, 2001; Morescalchi, 2016), and to other variables coherently with expectations (Holzer, 1988; Blau and Robins, 1990; Schmitt and Wadsworth, 1993; Gregg and Wadsworth, 1996; Addison and Portugal, 2002; Weber and Mahringer, 2008; Morescalchi, 2016). Moreover, considering the change in this variable for the same individual between two following quarters reduces remarkably the risk of measurement error arising by possible subjective over/under-reporting behaviour.

5.2. Identification strategy

The reference population considered in the analysis is made of non-employed claimants in a given quarter, called wave 1, and their status transition is tracked in the following quarter, called wave 2. Following Manning (2009), a treatment group is formed by collecting all claimants interviewed in

1996$q3$ (the 3rd quarter of 1996), who are also interviewed in 1996$q4$, (the 4th quarter of 1996). In 1996$q3$ claimants are not treated as they are still under the old rules, but they are treated in 1996$q4$ and movements from the initial status are affected by the new rules.

Since 7 October 1996, the date when JSA came into force, all new and ongoing claimant spells were subject to the new regime (Finn et al., 1996). In particular, all existing spells were immediately handed over to the JSA system and claimants were treated as having made a Jobseekers' Agreement even for the period before its actual completion, recovering information from their initial claim form (Finn et al., 1996, pp. 64). Given this retroactive feature, there were no overlaps in claimant spells subject to the old and new regimes. Therefore it is not possible to create a control group of claimants unaffected by the reform for which outcomes are measured at the same time of treated claimants.

A control group is formed using the closest pair of quarters available before the JSA introduction; namely, all claimants in 1996$q2$ and followed through 1996$q3$ are considered as a control group. This choice allows to minimize possible differences in cyclical factors between treated and untreated claimants. Seasonal effects are eliminated with a Diff-in-diff strategy drawing similar cohorts of claimants from other years (Manning, 2009).

Four cohorts of claimants in the two years preceding (1994, 1995) and following (1997, 1998) the introduction of JSA are used to implement the Diff-in-diff. Similarly to 1996, claimant transitions from $q3$ to $q4$ ($q3q4$) and from $q2$ to $q3$ ($q2q3$) serve as treatment and control group respectively. Note that the terminology is kept identical to 1996 for simplicity, although all (respectively no) groups after (respectively before) 1996 receive the JSA treatment. Since eligibility rules are stable in all years considered except for 1996, the comparison between $q3q4$ and $q2q3$ transitions in any of these years isolates a seasonal effect. These years can be used to form valid comparison groups under the assumption that the seasonal effect is not influenced by the (presence/absence of) JSA in these years. Under the assumption that the seasonal effect is stable across years, one extra cohort (year) would be enough to identify the effect of JSA. However, additional yearly cohorts are used to account for possible yearly fluctuations and hence better pin down the seasonal effect (Petrongolo, 2009). Years before and after 1996 are used at the same time to account for a possible trend in the seasonal effect. In existing JSA evaluations conducted by Manning (2009) and Petrongolo (2009) comparison groups are drawn from either years before or after 1996, respectively, and hence do not take into account possible persistence in the outcome variables. Moreover, to assess robustness of results across alternative combinations of yearly cohorts, the four following comparison groups are considered here: (1) 94-95-97-98; (2) 95-97-98; (3) 94-95-97; (4) 95-97.

The impact of the policy change can be captured by response differences between the treatment and control groups as long as they are similar in observable and unobservable characteristics. Randomness of the survey sampling design makes it quite unlikely that they differ systematically unless in case of sampling errors or non-response bias. Non response-bias might originate in the present set-up by differences in response patterns across quarters and years. However these effects should be eliminated by the Diff-in-diff approach. Table 2 shows Diff-in-diff in weighted averages of observable characteristics calculated comparing treated and untreated claimants in 1996 and then comparing the 1996 cohort with alternative comparison cohorts. Significant Diff-in-diff statistics are not found for any of the control variables, suggesting that the 1996 cohort and alternative comparison groups are well balanced once seasonal effects are eliminated. Possible non-response bias is also taken into account by using sampling weights in estimations. Although treatment and control groups appear well balanced, it is worthwhile to perform additional checks on the validity of control groups as counterfactuals. These checks are discussed in Section 6.4 and overall provide support on their validity.

Table 2: Sample statistics for claimants in 1996q3 and Difference-in-differences in variable means calculated comparing between q3 and q2 and between 1996 and control years. *** significant 0.1%, ** significant 1%, * significant 5%. Standard errors in parenthesis.

	96		94-95-97-98		95-97-98		94-95-97		95-97	
	mean	se	DiD	se	DiD	se	DiD	se	DiD	se
Female	0.267	(0.442)	-0.004	(0.014)	-0.003	(0.015)	-0.007	(0.015)	-0.007	(0.016)
Age	35.206	(12.316)	0.044	(0.41)	-0.011	(0.434)	-0.055	(0.417)	-0.197	(0.453)
Married	0.308	(0.462)	0.001	(0.015)	-0.001	(0.016)	0.000	(0.016)	-0.003	(0.017)
Nr. kids 0—18 y	0.678	(1.119)	-0.029	(0.038)	-0.033	(0.04)	-0.021	(0.038)	-0.021	(0.042)
Degree or equiv.	0.101	(0.301)	0.014	(0.009)	0.012	(0.01)	0.016	(0.009)	0.015	(0.01)
Higher education	0.046	(0.208)	-0.001	(0.007)	0.002	(0.007)	-0.002	(0.007)	0.000	(0.008)
GCE, A or eq.	0.208	(0.406)	0.012	(0.013)	0.009	(0.014)	0.013	(0.014)	0.01	(0.015)
GCSE A*–C or eq.	0.219	(0.413)	0.004	(0.013)	0.004	(0.014)	0.004	(0.014)	0.003	(0.015)
Other qual.	0.170	(0.375)	-0.01	(0.013)	-0.013	(0.014)	-0.008	(0.013)	-0.011	(0.014)
No qual.	0.257	(0.437)	-0.019	(0.015)	-0.014	(0.016)	-0.022	(0.015)	-0.018	(0.016)
Last job < 3 m	0.109	(0.312)	-0.016	(0.01)	-0.012	(0.011)	-0.017	(0.011)	-0.013	(0.012)
Last job 3—6 m	0.114	(0.318)	0.001	(0.011)	0.001	(0.011)	-0.002	(0.011)	-0.005	(0.012)
Last job 6—12 m	0.154	(0.361)	-0.01	(0.012)	-0.02	(0.013)	-0.004	(0.012)	-0.013	(0.013)
Last job 1—2 y	0.149	(0.356)	-0.006	(0.012)	-0.005	(0.013)	-0.005	(0.012)	-0.004	(0.013)
Last job 2—3 y	0.090	(0.286)	0.011	(0.009)	0.011	(0.01)	0.011	(0.01)	0.012	(0.01)
Last job 3—4 y	0.067	(0.249)	0.002	(0.008)	0.004	(0.009)	0.003	(0.008)	0.006	(0.009)
Last job 4—5 y	0.059	(0.236)	0.002	(0.008)	0.004	(0.008)	0.000	(0.008)	0.001	(0.009)
Last job > 5 y	0.166	(0.373)	0.009	(0.012)	0.014	(0.013)	0.006	(0.012)	0.011	(0.013)
No job before	0.092	(0.29)	0.006	(0.009)	0.003	(0.01)	0.007	(0.009)	0.004	(0.01)
North East	0.056	(0.23)	0.002	(0.007)	0.004	(0.008)	0.002	(0.008)	0.005	(0.008)
North West	0.116	(0.32)	-0.001	(0.011)	-0.003	(0.011)	-0.003	(0.011)	-0.008	(0.012)
York. & Humber.	0.087	(0.283)	-0.001	(0.009)	-0.004	(0.01)	-0.002	(0.009)	-0.006	(0.01)
East Midlands	0.065	(0.247)	-0.001	(0.008)	0.000	(0.009)	0.000	(0.008)	0.002	(0.009)
West Midlands	0.090	(0.287)	-0.013	(0.01)	-0.014	(0.01)	-0.012	(0.01)	-0.012	(0.011)
Eastern	0.086	(0.281)	0.012	(0.009)	0.013	(0.009)	0.012	(0.009)	0.013	(0.01)
London	0.176	(0.381)	-0.006	(0.013)	-0.003	(0.014)	-0.006	(0.013)	-0.004	(0.014)
South East	0.096	(0.294)	-0.003	(0.01)	-0.004	(0.01)	-0.002	(0.01)	-0.003	(0.011)
South West	0.074	(0.261)	0.006	(0.008)	0.006	(0.009)	0.008	(0.009)	0.01	(0.009)
Wales	0.055	(0.229)	-0.003	(0.007)	-0.004	(0.008)	-0.003	(0.007)	-0.004	(0.008)
Scotland	0.099	(0.299)	0.008	(0.01)	0.008	(0.01)	0.006	(0.01)	0.005	(0.011)
Nobs	2383		17670		11419		14968		8717	

Given the nature of treatment and control groups and the LFS design, potential attrition bias needs to be investigated. Attrition may introduce bias if differences between attriters and non-attriters are systematically related to the treatment status. Section SM12 reports attrition statistics comparing claimant attriters and non-attriters after differencing out across relevant quarters and years. Since no significant differences are found for any of the observed characteristics, attrition does not appear to be a concern.

5.3. Outcome variables

The impact of JSA is estimated on two sets of multinomial outcomes. The first set is based on a measure of change in search intensity and the second set on transitions in the use of Public Employment Centre (PEC) as search method. Both sets distinguish effects by claimants staying in and leaving the register, where the discrete variables C indicates transitions from the claimant status. Because search behaviour for individuals finding a job in wave 2 is not observed, all variables are complemented with an additional outcome corresponding to transitions to employment ($C = 3$).

The first set of outcome variables makes use of the variables C, Δs^*, and $(C, \Delta s^*)$, where Δs^* indicates change in job search intensity between wave 1 and wave 2, and s^* is proxied by the count of search methods (see Section SM3 for the list of search methods). According to this definition the average search intensity is 4.3 methods for claimants in 1996q3. Outcomes are defined in line with the outcomes discussed in Table 1 of Section 4 as follows:

(a) C captures claim and job transitions, with three different possible cases: $\{C = 1\}$ if still non-employed and claimant in wave 2, $\{C = 2\}$ if still non-employed but no longer claimant, $\{C = 3\}$ if employed.

(b) Δs^* is represented by the following four possible outcomes: $\{\Delta s^* < 0\}$; $\{\Delta s^* = 0\}$; $\{\Delta s^* > 0\}$; $\{C = 3\}$.

(c) $(C, \Delta s^*)$ is constructed as the joint discrete distribution of C and Δs^*, giving rise to the following seven possible outcomes: $\{C = 1, \Delta s^* < 0\}$; $\{C = 1,\ \Delta s^* = 0\}$; $\{C = 1,\ \Delta s^* > 0\}$; $\{C = 2,\ \Delta s^* < 0\}$; $\{C = 2,\ \Delta s^* = 0\}$; $\{C = 2,\ \Delta s^* > 0\}$; $\{C = 3\}$.

As concerns the variable Δs^*, the precision of the proposed measuring approach may be affected by discreteness of the search effort proxy and by non-homogeneity of search methods. In particular, the indicator may be less precise if the distribution of search effort is highly non-homogenous across methods and over time. In case of longitudinal fluctuations in effort but homogenous distribution across methods, relatively small variations in effort may occur even with unchanged count of methods and hence may be improperly imputed to $\Delta s^* = 0$. When effort is stable longitudinally but heterogeneous across methods, precision is highly dependent on search methods switching. Without switching, the sign of Δs^* can be estimated more precisely. Conversely, measurement may be less precise with switching because small changes in the count may be associated to larger volatility in effort; even in this case $\Delta s^* = 0$ may be overestimated.

The second set of outcome variables considers the use of Public Employment Centres (PEC) as search method in wave 1 and/or wave 2. Use of PEC is defined as use of any of the following search methods: (1) Visit a Jobcentre, (2) Visit a Careers office, (3) Visit a Jobclub. Following this definition, the share of claimants using PEC in 1996q3 was 76.2%. Almost all of these claimants approached a Jobcentre (75.8%), the office in charge of offering the work placement/job-seeking service, while only 15.8% used the other two offices. PEC was also quite frequently used by non-claimants (53.6%), suggesting that this search method was not a prerogative of unemployed workers on UI payment rolls. The following outcome variables are hence defined:

(d) M_{PEC} equal -1 when PEC is abandoned from wave 1 to 2 (i.e. it is used in wave 1 but not in wave 2); $M_{PEC} = 1$ corresponds to adopting PEC from wave 1 to 2 (i.e. it is not used in wave 1 but it is in wave 2); $M_{PEC} = 0$ corresponds to the residual cases, i.e. PEC is always or never used. The outcome variable is hence represented by the following four states: $\{M_{PEC} = -1\}$; $\{M_{PEC} = 1\}$; $\{M_{PEC} = 0\}$; $\{C = 3\}$.

(e) (M_{PEC}, C) is constructed as the joint discrete distribution of M_{PEC} and C, giving rise to the following seven possible outcomes: $\{C = 1,\ M_{PEC} = -1\}$; $\{C = 1,\ M_{PEC} = 1\}$; $\{C = 1,\ M_{PEC} = 0\}$; $\{C = 2,\ M_{PEC} = -1\}$; $\{C = 2,\ M_{PEC} = 1\}$; $\{C = 2,\ M_{PEC} = 0\}$; $\{C = 3\}$.

Outcomes related to search behaviour defined in this Section requires discussion on possible selection issues. Since search is not observed for individuals finding job ($C = 3$), and these individuals are likely to represent a specific population with better employment prospects, the resulting sample of jobseekers ($C = 1, 2$) may be negatively selected on employability. Two possible selection issues may follow. First, external validity of results may be limited because inference cannot be extended to this population. Second, internal validity may be affected too if the composition between treated and untreated groups becomes unbalanced after eliminating this group. This risk is more pronounced when employment inflows are significantly larger in the treatment group, because the negative selection would be stronger in the treatment group. Since earlier studies reported a null impact of JSA on employment inflows, this issue might be of minor concern in the present analysis.

5.4. Econometric model

The outcome variables defined in Section 5.3 are categorical, therefore, unlike Manning (2009) and Petrongolo (2009), non-linear multinomial models are used to estimate the effect of JSA. Specifically, the generic outcome variable y is modelled by means of a multinomial logistic distribution, with probability of a given outcome j equal to

$$P(y = j|Z) = g_j(\beta_j' Z) \qquad j = 1, \ldots, J, \tag{1}$$

where J is the number of outcomes, g_j equals the multinomial probability and Z is the matrix of regressors. In eq. (1) and hereafter the subscript i indexing individuals is omitted for simplicity. Estimation of the model is performed by maximum likelihood.

The linear index $\beta' Z$ is defined for a generic individual as follows:

$$\beta' Z = \beta_0 + \beta_1\, d96 + \beta_2\, dq3q4 + \beta_3\, d96 \cdot dq3q4 + \delta' X, \tag{2}$$

where $dq3q4$ is equal to 1 for claimants interviewed in $q3$ and followed through $q4$ in any year, and it is equal to 0 for claimants interviewed in $q2$ and followed through $q3$, $d96$ is a year dummy, and the vector X contains additional year dummies and variables controlling for observable characteristics in wave 1. The set of controls comprises: gender, age, quadratic age, marriage status, number of kids below 19 in household, highest education dummies, duration since last job and regional dummies, (see Table 2).

In a linear case, the parameter β_3 associated to the interaction term $d96 \cdot dq3q4$ would capture the Diff-in-diff estimate of the JSA average treatment effect (ATE). In the present non-linear model, the following $J \cdot 4$ probabilities are needed to construct estimates of ATEs:

$$P^j_{t,q}(X^\circ) = P(y = j | d96 = t, dq3q4 = q, X = X^\circ), \tag{3}$$

where $t \in \{0, 1\}$ and $q \in \{0, 1\}$ determine the four possible combinations of time and treatment status in the Diff-in-diff, $j \in \{1, \dots, J\}$ represent possible outcomes, and appropriate values are used for X°. J Diff-in-diff coefficients are identified as $DiD^j(X) = (P^j_{1,1}(X) - P^j_{1,0}(X)) - (P^j_{0,1}(X) - P^j_{0,0}(X))$, where $DiD^j(X^\circ)$ is the JSA effect on the probability of experiencing transition j with values of covariates X°. The probabilities $P^j_{t,q}(X^\circ)$ are estimated replacing parameters with maximum likelihood estimates.

The ATE is then estimated as the average of the DiD^j for each individual within the sample, i.e. $ATE^j = N^{-1} \sum_{i=1}^{N} DiD^j(X_i)$ (see Wooldridge, 2010, Section 15.6). Analytical standard errors for these coefficients are computed using the Delta method. Remark that ATE^j-s must sum up to zero because they capture changes in probabilities. As a consequence, each of them is compensated by other effects reflecting the same behavioural change. For example, a decrease in the claimant permanence ($P(C = 1)$) necessarily corresponds to an increase in claimant outflows ($P(C = 2)$) and/or in employment inflows ($P(C = 3)$). Interpretation of results will focus on outcomes associated to behavioural change, considering the following outcomes as their counterparts: $P(C = 1)$ in (a); $P(\Delta s^* = 0)$ in (b); $P(C = \cdot, \Delta s^* = 0)$ in (c); $P(M_{PEC} = 0)$ in (d); $P(C = \cdot, M_{PEC} = 0)$ in (e).

6. Results

This section presents evidence on the JSA ATE. Section 6.1 and 6.2 report estimates of ATE on various transition probabilities, as captured by the outcome variables defined in Section 5.3. Section 6.3 reports estimates of ATE on employment outcomes of claimants leaving the register without returning to work. Finally Section 6.4 reports robustness checks and complementary evidence.

6.1. Search intensity and claimant flows

Table 3 reports ATEs on the change of claimant status, the change in search intensity and their interaction. Panel (a) shows that JSA increased transition in non-claimant non-employed status ($C = 2$) in the range of 5.3–5.8 p.p., without increasing transitions to job ($C = 3$). These results confirm earlier evidence of a JSA sizeable impact on claimant outflows, without causing an influx into jobs.

Panel (b) shows the JSA effect on job search patterns of non-employed (Δs^*) jointly with transitions into employment ($C = 3$). $P^\star(\cdot)$ indicates $P(\cdot|C \in \{1, 2\})$, i.e. results for non-employed people in both waves. The effects on the probabilities of increasing the level of search and of reducing it are respectively positive and negative in all models. However, while the effect on $P^\star(\Delta s^* > 0)$ is significant in three models and close to significance in the fourth model, the effect on $P^\star(\Delta s^* < 0)$ never reaches significance. These results imply that in three cases out of four a significant share of individuals had a higher level of search with JSA than the level they would have had in the absence of JSA. This evidence appears in contrast with Manning (2009) who found no effect on the average search intensity. Remark that Manning (2009) used a cardinal measure of search intensity while the dependent variable chosen here is categorical for consistency with the theoretical background. Results using a cardinal measure of search intensity using the number of search methods are reported in Section SM4; the JSA effect on this variable is significant in three cases but overall is quite small. Overall, it is found that a significant portion of individuals increased search because of JSA, although by a small magnitude on average.

One may wish to investigate whether the effect on search was different between claimants staying in ("stayers") and leaving the register ("leavers"), as this may have very different policy implications as discussed in Section 4. Results are reported in panel (c) of Table 3, which shows estimates for the case when the sign of Δs^* is considered jointly with C, giving rise to the six possible outcomes defined in Table 1 (see Section SM6 for estimates of the multinomial logit model).

Three main findings can be singled out. Firstly, while JSA had no significant impact on the probability of keep claiming with higher search level (case (iii) of Table 1), it reduced the probability of decreasing search (case (i)). A negative impact on $\Delta s^* < 0$ means that under the JSA some claimants had higher search than they would have had without the JSA, perhaps by avoiding to drop

Table 3: JSA ATE on claimant behaviour with alternative control years.

	Treatment = 96			
	94-95-97-98	95-97-98	94-95-97	95-97
(a) C				
$P(C=1)$	-0.074^{***}	-0.065^{***}	-0.076^{***}	-0.065^{***}
	(0.015)	(0.016)	(0.015)	(0.016)
$P(C=2)$	0.055^{***}	0.053^{***}	0.058^{***}	0.056^{***}
	(0.010)	(0.011)	(0.011)	(0.012)
$P(C=3)$	0.019	0.012	0.019	0.009
	(0.012)	(0.013)	(0.012)	(0.013)
(b) Δs^*				
$P^\star(\Delta s^* < 0)$	-0.020	-0.009	-0.024	-0.013
	(0.013)	(0.014)	(0.014)	(0.015)
$P^\star(\Delta s^* = 0)$	-0.028	-0.026	-0.029	-0.028
	(0.016)	(0.017)	(0.016)	(0.017)
$P^\star(\Delta s^* > 0)$	0.031^{*}	0.026	0.037^{**}	0.034^{*}
	(0.014)	(0.014)	(0.014)	(0.015)
$P(C=3)$	0.017	0.010	0.017	0.007
	(0.012)	(0.013)	(0.012)	(0.013)
(c) $(C, \Delta s^*)$				
$P(C=1, \Delta s^* < 0)$	-0.034^{**}	-0.024	-0.039^{**}	-0.029^{*}
	(0.012)	(0.013)	(0.012)	(0.013)
$P(C=1, \Delta s^* = 0)$	-0.050^{**}	-0.044^{**}	-0.052^{**}	-0.046^{**}
	(0.016)	(0.016)	(0.016)	(0.017)
$P(C=1, \Delta s^* > 0)$	0.009	0.003	0.014	0.009
	(0.013)	(0.014)	(0.013)	(0.014)
$P(C=2, \Delta s^* < 0)$	0.014^{*}	0.015^{*}	0.014^{*}	0.016^{*}
	(0.007)	(0.007)	(0.007)	(0.008)
$P(C=2, \Delta s^* = 0)$	0.020^{**}	0.016	0.022^{**}	0.017^{*}
	(0.007)	(0.008)	(0.008)	(0.009)
$P(C=2, \Delta s^* > 0)$	0.021^{***}	0.023^{***}	0.022^{***}	0.025^{***}
	(0.005)	(0.005)	(0.005)	(0.006)
$P(C=3)$	0.019	0.012	0.019	0.009
	(0.012)	(0.013)	(0.012)	(0.013)
Nobs	22399	16148	19697	13446

Notes: *** significant 0.1%, ** significant 1%, * significant 5%. Robust standard errors are reported in parenthesis. $P^\star(\cdot) := P(\cdot|C \in \{1,2\})$. The outcome variables (a), (b) and (c) make use of the variables C and Δs^*. C identifies transitions from claimant status in wave 1 as follows: $C=1$ if still non-employed and claimant in wave 2, $C=2$ if still non-employed but no longer claimant, $C=3$ if employed. Δs^* is an indicator of change in job search intensity ($\Delta s^* \gtreqless 0$) between wave 1 and wave 2. Reported ATEs are computed from multinomial logit models with Diff-in-diff specification and correspond to changes in the probability of performing the specified transition. See Table SM7 for multinomial logit estimates for case (c), using 94-95-97-98 as control years. The following control variables are used in the multinomial logit specification: gender, number of kids (< 19), age, age^2, education, duration since last job, marriage status, region. See Table 2 for descriptive statistics.

off active search.

Secondly, JSA had positive and significant impact on the probability of abandoning the register with reduced search (case (iv)). The framework proposed in Section 4 suggests that case (iv) claimants are unlikely to be liquidity constrained and hence their search behaviour may be influenced by moral hazard only. Since these exits should be associated to savings in moral hazard costs without altering consumption smoothing, this finding suggests that JSA was successful in removing targeted individuals from the claimant register.

Thirdly, JSA had positive and significant impact on the probability to increase search while leaving the register (case (vi)). Case (vi) claimants correspond to around 2% of the sample, therefore an estimated ATE of above 2% implies that JSA more than doubled the size of this category. Considering the ATE reported in panel (a), this effect accounts for a portion of the total claimant outflow generated by JSA in the range of 38-45%. The framework proposed in Section 4 suggests that case (vi) claimants are likely to be liquidity constrained and hence they may experience significant consumption cuts when exiting the claimant pool. Since these claimants are supposed to be highly motivated to look for job, unlike type (iv) claimants, this evidence suggests that JSA removed from the claimant register also individuals who were not targeted; the administrative burden required to maintain eligibility under the JSA may have induced several claimants to search for a job outside the Benefit's Agency monitoring.

The combined evidence of positive impact on case (iv) and case (vi) suggests that JSA had an ambiguous impact on search intensity of those abandoning the register. However the relative risk ratio (RRR) between outcomes (vi) and (iv) estimated by the multinomial logit is significant and ranges between 1.999 ($p < 0.01$) and 2.050 ($p < 0.01$) (see Section SM6 for results corresponding to the first model). This implies that the removal effect was relatively larger for workers who were not targeted. Results in Section SM4 show that the effect on the cardinal measure of search intensity is positive for leavers, suggesting that incremental effects on type (vi) transitions dominated decrease effects on type (iv) transitions on average.

The hypothesized correspondence between type (vi) (respectively (iv)) transitions and liquidity constrained (respectively unconstrained) claimants has been checked against available data. Statistics and technical details are reported in Section SM5. JSA relative ATEs between liquidity constrained and unconstrained individuals have been estimated with a Diff-in-diff-in-diff strategy, augmenting the linear index with a liquidity status indicator and interactions terms. Following Chetty (2008), liquidity constrains are proxied by two indicators for having to make mortgage payments, and for single- versus dual-earner status. A third indicator is constructed as their product. Similarly to Chetty (2008), these indicators suggest that a substantial portion of unemployed individuals might be liquidity constrained. Results show that the JSA impact on type (vi) transitions was significantly larger for liquidity constrained claimants for all the three indicators employed. Conversely, the JSA impact on type (iv) transitions was lower for constrained claimants, with an insignificant effect only for the single-earner indicator. Both results are hence consistent with the hypothesized correspondence. Furthermore, in both cases the effect size is generally larger than the absolute effects in Table 3, suggesting that the correspondence may be very precise.

6.2. Search via PEC and claimant flows

Table 4 reports JSA ATEs on the use of PEC (panel (d)), also in association with the decomposition by claimant flows (panel (e)). $P^{\star}(\cdot)$ indicates $P(\cdot|C \in \{1,2\})$. Looking at the baseline transitions (panel (d)), there is no evidence of a JSA impact on the dismissal of PEC. The JSA impact on PEC inflows is positive, but never reaches statistical significance either. However these results hide potential differences in behaviour between stayers and leavers.

Table 4: JSA ATE on use of PEC with alternative control years.

	Treatment = 96			
	94-95-97-98	95-97-98	94-95-97	95-97
(d) M_{PEC}				
$P^{\star}(M_{PEC} = -1)$	−0.007	−0.004	−0.009	−0.008
	(0.008)	(0.009)	(0.009)	(0.009)
$P^{\star}(M_{PEC} = 1)$	0.011	0.009	0.013	0.011
	(0.006)	(0.007)	(0.007)	(0.007)
$P^{\star}(M_{PEC} = 0)$	−0.022	−0.016	−0.021	−0.012
	(0.014)	(0.015)	(0.015)	(0.016)
$P(C = 3)$	0.018	0.011	0.017	0.008
	(0.012)	(0.013)	(0.012)	(0.013)
(e) (M_{PEC}, C)				
$P(C = 1, M_{PEC} = -1)$	−0.011	−0.008	−0.013	−0.011
	(0.007)	(0.007)	(0.007)	(0.007)
$P(C = 1, M_{PEC} = 1)$	0.003	0.000	0.005	0.002
	(0.006)	(0.006)	(0.006)	(0.007)
$P(C = 1, M_{PEC} = 0)$	−0.067***	−0.058***	−0.069***	−0.058***
	(0.016)	(0.017)	(0.016)	(0.017)
$P(C = 2, M_{PEC} = -1)$	0.004	0.004	0.004	0.004
	(0.006)	(0.006)	(0.006)	(0.006)
$P(C = 2, M_{PEC} = 1)$	0.008**	0.009**	0.008**	0.010**
	(0.003)	(0.003)	(0.003)	(0.004)
$P(C = 2, M_{PEC} = 0)$	0.044***	0.040***	0.047***	0.043***
	(0.009)	(0.010)	(0.009)	(0.011)
$P(C = 3)$	0.020	0.013	0.019	0.009
	(0.012)	(0.013)	(0.012)	(0.013)
Nobs	22310	16092	19617	13399

Notes: *** significant 0.1%, ** significant 1%, * significant 5%. Robust standard errors are reported in parenthesis. $P^{\star}(\cdot) := P(\cdot|C \in \{1,2\})$. The outcome variables (d) and (e) make use of the variables C and M_{PEC}. C identifies transitions from claimant status in wave 1 as follows: $C = 1$ if still non-employed and claimant in wave 2, $C = 2$ if still non-employed but no longer claimant, $C = 3$ if employed. M_{PEC} identifies transitions involving the search method PEC for claimants in wave 1, such as abandoning PEC ($M_{PEC} = -1$) in wave 2, adopting PEC ($M_{PEC} = 1$) in wave 2, or consistently using or not PEC in both waves ($M_{PEC} = 0$). Reported ATEs are computed from multinomial logit models with Diff-in-diff specification and correspond to changes in the probability of performing the specified transition. The following control variables are used in the multinomial logit specification: gender, number of kids (< 19), age, age^2, education, duration since last job, marriage status, region. See Table 2 for descriptive statistics.

Panel (e) shows results decomposing the total effect by claimant flows. Considering stayers, a null impact on the use of PEC may be interpreted as a sign that stayers were already making use of this method.

Considering leavers, JSA had no effect on dismissal of PEC, but led a significant share to adopt it. Since claimants signing off and adopting PEC correspond to around 0.5% of the sample, an estimated ATE in the range of 0.8–1% implies that JSA more than doubled this category. The increase in PEC inflows is associated with the increase in the share of claimants dropping off benefit with higher search documented in Table 3.

What appears relevant here is that, despite a substantial portion of claimants abandoning the JSA are found to search more, revealing commitment to search, there is no evidence of escapes from PEC, but rather of increased PEC inflows. Therefore, while these committed jobseekers are reluctant to search through the administrative steps dictated by the new rules, they may be willing at the same time to receive more informal assistance from the public job search service. They appear to shift demand of public service from formal assistance toward informal assistance.

6.3. Employment outcomes of leavers

Since one of the most relevant effects of JSA was to increase UI drop-outs of unemployed people, including those motivated to re-enter employment, it appears of interest to investigate the employment outcomes of this category. A linear Diff-in-diff approach was implemented to estimate JSA ATEs on employment outcomes for the sample of leavers. Outcomes are observed in $q1$ of the following year for the treatment group, and in $q4$ of the same year for the control group. Although outcomes for the 1996 control group are observed right after the introduction of JSA, they should not be affected by the change of regime because leavers should be off the UI system since the previous quarter. Remark that the sample used in this analysis contains individuals observed in three consecutive quarters, and observed as unemployed at least in the first two; therefore it is quite restricted and made of relatively long-term unemployed. These characteristics of the sample should be kept in mind when interpreting estimates.

Table 5 reports estimates of JSA ATEs. Results show that the impact of JSA one quarter after leaving the UI pool is positive but statistically insignificant for employment status, hours workers, and full-time employment. No significant effects are found for permanent/temporary contract either. Since JSA led to a significant increase in the share of leavers increasing search and in their average intensity, the absence of subsequent employment effects for this category could suggest that they increased the use of ineffective methods. Since PEC is often found to be ineffective for employment, this interpretation is consistent with the estimated increase in transitions to PEC. However, the failure to observe a significant effect on the probability to find job may be also due to the small sample size.

Table 5: JSA ATE on employment outcomes of leavers with alternative control years.

	Treatment = 96			
	94-95-97-98	95-97-98	94-95-97	95-97
Employment				
ATE	0.032	0.030	0.037	0.038
	(0.029)	(0.031)	(0.029)	(0.032)
Nobs	2058	1539	1842	1323
Permanent				
ATE	0.012	0.007	0.009	0.000
	(0.020)	(0.022)	(0.020)	(0.022)
Nobs	2011	1502	1800	1291
Full-time				
ATE	0.017	0.015	0.022	0.024
	(0.022)	(0.024)	(0.022)	(0.025)
Nobs	2058	1539	1842	1323
Weekly hours				
ATE	1.453	1.332	1.655	1.632
	(0.996)	(1.096)	(1.002)	(1.125)
Nobs	2047	1530	1831	1314

Notes: *** significant 0.1%, ** significant 1%, * significant 5%. Robust standard errors are reported in parenthesis. The variables Employment, Permanent and Full-time are binary, while Weekly hours is continuous. Estimates are carried out by OLS. The sample is made of claimants in wave 1 leaving the register without job in wave 2 and still observed in the following quarter. The following control variables are used: gender, number of kids (< 19), age, age^2, education, duration since last job, marriage status, region.

6.4. Robustness checks and complementary evidence

Several robustness checks and complementary analyses were performed. First, the presence of JSA anticipatory effects was tested. If some individuals anticipate the policy change, reported estimates may capture causal effects only in part. Section SM7 reports anticipatory effects tests with respect to ATE-s reported in Table 3 and Table 4. If individuals tended to modify their behaviour before the date JSA came into force, one would expect to observe changes between $q2$ and $q3$ in the year JSA was introduced but not in others. Therefore the tests consider claimant flows between $q2$ and $q3$ ($q2q3$) in 1996 as a fictitious treatment group and compare them to transitions between $q1$ and $q2$ ($q1q2$), under the assumption that the latter transitions are not affected by anticipatory effects. Similar cohorts are drawn from other years to implement the usual Diff-in-diff set-up. In this case, significant coefficients would point out anticipatory effects. Since no significant tests are found for relevant outcomes in Table 3 and Table 4, ATEs reported in these Tables do not seem to be invalidated by JSA anticipatory effects. A possible shortcoming of the proposed check is that anticipatory effects may not be detected if the policy is anticipated well in advance, so that $q1q2$ and $q2q3$ transitions would be influenced similarly. While it seems quite unlikely to observe such an early reaction, this possibility was taken into account re-estimating models without $q1q2$ transitions. In this case, the Diff-in-diff boils down to a simple cross-year comparison of $q2q3$ transitions. No evidence of anticipatory effects is found in this case either.[7] Remark that also Manning (2009) and Petrongolo (2009) found no evidence of JSA anticipatory effects on claimant flows.

Second, Section SM8 reports falsification tests on "placebo" JSA reforms. The tests are based on the same sample used in estimation with the exclusion of 1996. Placebo treatment groups are formed using comparison years one at a time, and then compared to the group of residual comparison years within the usual Diff-in-diff setting. Resulting estimates test the equality of seasonal effects between the focal year and the others. Evidence of no significant deviations would indicate that the seasonal effect is stable across years, supporting the validity of the identification strategy. Conversely, instability in seasonal effects would indicate that ATEs reported in Table 3 and Table 4 may reflect yearly fluctuations, raising concerns on their interpretation as causal effects. Results in Section SM8 show that statistically significant deviations are found only occasionally in a limited set of outcomes. Moreover, such fluctuations appear to influence only marginally the corresponding ATEs reported in Table 3 and Table 4; in fact, these coefficients are quite stable across alternative combinations of comparison years in terms of sign, magnitude and significance. Overall this evidence supports to the validity of the identification strategy.

Third, a possible influence of unobserved heterogeneity was checked. Whenever assignment to treatment is random, the straight comparison in outcomes between treated and untreated identifies the treatment effect. In this case estimation of the treatment effect is not affected by including control variables in regression models. Section SM9 reports additional estimates without inclusion of control variables, showing remarkably similar results to the ones reported in Table 3 and Table 4. This evidence suggests that, once seasonal and cyclical factors are eliminated with the Diff-in-diff, residual differences in observed individual characteristics have limited impact on results, and hence omission of unobservable predictors is unlikely to affect estimation significantly. The validity of the Diff-in-diff strategy appears further supported.

Fourth, a check was performed on the possible impact of the New Deal program on the validity of 1998 as comparison year. New Deal was first introduced in 1998 only for specific categories (see Section 3), and scaled up in later years. Since the program offers a variety of activation measures that may affect the search behaviour and employment chances of claimants, outcomes for the 1998 sample might be altered by its introduction, possibly confounding JSA treatment effect estimates. However, the fraction of claimants reportedly on New Deal in the 1998 sample is only 3.3%, suggesting that this group is unlikely to introduce bias. Nevertheless, the sensitivity of results to exclusion of these individuals was also assessed. Estimates after eliminating these cases are reported in Section SM10, and they are almost identical to corresponding estimates in Table 3 and Table 4. While the share of

[7]These results are made available by the authors upon request.

individuals on New Deal was too small to affect estimates, it is possible that the policy affected the behaviour of a larger share of claimants. This could be the case for individuals who were close to the duration cutoff for New Deal eligibility and might eventually enter the program. However, placebo tests reported in Section SM8 show no significant seasonal effects for 1998, bearing no evidence that New Deal might have affected the outcomes of interest. Likewise, Table 3 and Table 4 show that results are not sensitive to inclusion of 1998 in the control group. Overall, available evidence shows no effects of the policy change that might question the validity of 1998 as comparison year.

Fifth, results on switches in the use of PEC were complemented by a similar analysis considering a more restrictive definition of method use; namely, whether PEC is used as the main method of search. This analysis can reveal additional information on changes in the relative importance of PEC in search strategies. ATE estimates, anticipatory effects and placebo tests based on this definition are reported in Section SM11. ATE estimates are very similar to the ones based on the more general definition (see Table 4). Specifically, no effect is found on the probability to abandon PEC as main method when signing off, but a positive effect is found on the probability to adopt it when signing off. Tests for anticipatory effects show no evidence of anticipation. However, placebo tests reveal an anomalous negative seasonal effect in 1995 for transitions to PEC associated to claim exit ($P(C = 2, M_{PEC} = 1)$). By re-estimating JSA ATEs without the 1995 cohort, the effect on $P(C = 2, M_{PEC} = 1)$ shrinks and becomes insignificant with all comparison groups, suggesting that the estimated positive impact may just reflect the anomalous fluctuation in 1995. Therefore, the most robust available evidence seems to indicate that the JSA impact on $P(C = 2, M_{PEC} = 1)$ was null when PEC is considered as main method. Remark that no deviations are observed when the more general definition is considered (see Table SM11), therefore the positive impact on $P(C = 2, M_{PEC} = 1)$ appears a robust finding in this case. The combined evidence on the two definitions suggests the following interpretation: on the one hand, JSA did not induce claimants to abandon the use of PEC when leaving the register, either as simple use or as main method; on the other hand, JSA significantly increased the share of leavers newly adopting PEC, although not selecting it as main method.

Finally, the interpretation of the ATE on type (vi) transitions observed in Table 3 requires additional discussion. The interpretation is based on the assumption that JSA was neutral to the search intensity in the status of non-claimant; therefore an increase in the search of leavers is attributed to the loss of benefit. However if the assumption does not hold, an increase in type (vi) transitions may simply reflect a positive JSA effect on the search of non-claimants. Section SM4 reports estimates of JSA effects on the search intensity of non-claimants. Failure to observe significant effects supports the validity of the assumption and hence the interpretation.

Another problem with the interpretation may be caused by reporting bias. For example, claimants leaving the register may feel they need to convince the LFS interviewer they are highly search active. This over-reporting behaviour does not seem plausible for individuals ceasing claim voluntarily. However, it might be possible if claiming is stopped because of a sanction. Since sanctioning rates were rather low both before and after JSA (see Section 3), the extent of this possible reporting bias appears negligible.

7. Concluding remarks

This paper has investigated the impact of job search related conditions on the search behaviour of UI recipients. Exogenous variation arising by the introduction of the UK Jobseeker's Allowance (JSA) has been used to identify the effect of stricter job search requirements. Previous impact evaluations found that JSA moved off the claimant register many unemployed without reintegrating them to jobs. Unlike existing studies analysing the impact of stricter conditionality, the present work has focused on the differential impact on job search between claimants staying in the register ("stayers") and claimants leaving it without returning to work ("leavers"). A theoretical framework was presented to assess possible intended and unintended effects of the policy distinguishing between a "moral hazard" and a "liquidity" effect. This analysis aims to evaluate whether tighter conditionality can establish a closer link between UI receipt and commitment to return to work.

Making use of Labour Force Survey data, JSA Average Treatment Effects (ATE) have been estimated on two multinomial outcome variables, measuring the change in the intensity of search ($\Delta s^* \gtreqless 0$, where s^* is the number of search methods) and the switch in the use of the Public Employment Centre (PEC) as search method (dismissal, adoption, stable use/non-use). These effects have been further decomposed by stayers and leavers.

Results confirm that JSA increased remarkably the share of claimants leaving the register, in the range of 5.3–5.8 percentage points, but had no impact on employment inflows. On the one hand, JSA increased the share of unemployed keeping UI claim with higher search. This suggests that a significant group of claimants adapted to the higher search requirements, in line with the intended goal of mitigating moral hazard.

On the other hand, two opposite effects have been found for claimants leaving the UI pool. First, JSA increased the share of claimants leaving the register with reduced search. Theoretical expectations suggest that this group of workers are unlikely to be liquidity constrained and hence their search may be influenced by a moral hazard component only. This result is considered an intended outcome because these exits should be associated to decreased moral hazard costs without prejudice of the consumption smoothing capacity of unemployed workers. Second, JSA increased also the share of claimants leaving the register with increased search. Theory suggests that these individuals are likely to be liquidity constrained, and hence they may be unable to absorb the income loss and be highly motivated to look for a job. This effect is considered unintended because failure to insure committed jobseekers would lessen the UI insurance function. The hypothesised relation between change in search intensity of leavers and liquidity status was largely supported by empirical checks.

The second impact is larger, accounting for a portion of the total claimant outflow in the range of 38–45% against 24–28%. The substantial extra administrative hurdle introduced by the JSA may have induced several jobseekers committed to get back into job to escape the monitoring of the Benefit Agency, renouncing the UI coverage at the same time. This is consistent with evidence from social experiments suggesting that mandatory re-employment services can be perceived by claimants as threat of extra administrative burden, despite they are meant to support them. This threat effect may be particularly strong for the JSA, which did not provide any further active job search assistance service (Petrongolo, 2009). Moreover, since JSA increased search intensity of unemployed people without increasing their job finding rate, it appears that JSA mainly enhanced the use of more ineffective search channels. The rise in ineffective search might have been more pronounced for leavers, because they increased search intensity by a larger amount. In addition, the null employment impact for this category was found to persist even in the quarter following exit.

It was also found that JSA increased the share of claimants leaving the register and switching to the use of the Public Employment Centre (PEC). Since monitoring and job search assistance services were performed by functionally and physically separated offices at the time of the reform (McVicar, 2008), this finding suggests that stricter requirements may have induced several jobseekers to switch from monitored to un-monitored search assistance services. Therefore it appears that the threat effect of search requirements did not extend to services of the Employment Office. This may be interpreted as a positive outcome of the policy given that more unemployed people were using dedicated support services, possibly because they were perceived to be of some utility. However, in light of the null employment impact of JSA, it also indicates that this method might have been ineffective for escaping unemployment, especially for leavers. This interpretation is in line with evidence that PEC is often ineffective for finding a job (see Holzer, 1988, inter alia). It is also consistent with evidence that stricter monitoring redistributes search activities toward formal methods like PEC, possibly with no effect on the job entry rate (Van den Berg and Van der Klaauw, 2006). Remark that no significant impact was found on the switch rate when PEC was considered as the main search method; this suggests that leavers were not dedicating the largest effort to PEC and hence other methods may have contributed to the ineffectiveness of JSA.

In conclusion, the present evidence raises concerns about the success of search conditionality policies in establishing a tighter link between benefit receipt and commitment to return to work. While many claimants can enhance their search intensity to comply with higher requirements, at the same time many unemployed individuals with liquidity constraints and hence motivated to re-enter employment may abandon the UI system opting for un-subsidized search. The latter finding suggests that these policies can severely diminish the capacity of the UI to protect workers against the risk of unemployment. This result adds up to a number of unintended consequences of search conditionality already found in the literature, and points out an additional dimension to evaluate the welfare effect of such policies (Cockx et al., 2014).

Finally, possible limitations of the analysis need to be mentioned. Two possible selection issues may arise because search behaviour is not observed for exits to job. In fact, since this group may possess better labour market characteristics, the resulting sample of consecutive jobseekers may be negatively selected on employability. First, one should bear in mind that inference cannot be extended to this population, possibly limiting the external validity of results presented in this paper. A second selection issue can arise if the composition between treated and untreated groups becomes unbalanced after eliminating those who have found a job. In the present case, this issue appears of minor concern because the JSA treatment effect on employment inflows was found to be null. However, one cannot exclude that the composition between those who found a job and those who did not was different between treatment and control groups. This caveat should be taken in consideration when interpreting findings in terms of causal effects. Third, the precision of the measurement approach to search outcomes may be limited by discreteness of the search effort proxy and by non-homogeneity of search methods. Such limitations may lead to over-representation of stable levels of search, since in some cases the count of methods may remain unchanged even when effort actually changes. This issue could be mitigated with availability of homogeneous indicators of search effort, such as the number of job applications, or, more significantly, continuous indicators such as the time spent on search. Future research is encouraged to deepen the understanding of welfare consequences of search conditionality policies, taking the aforementioned issues into account.

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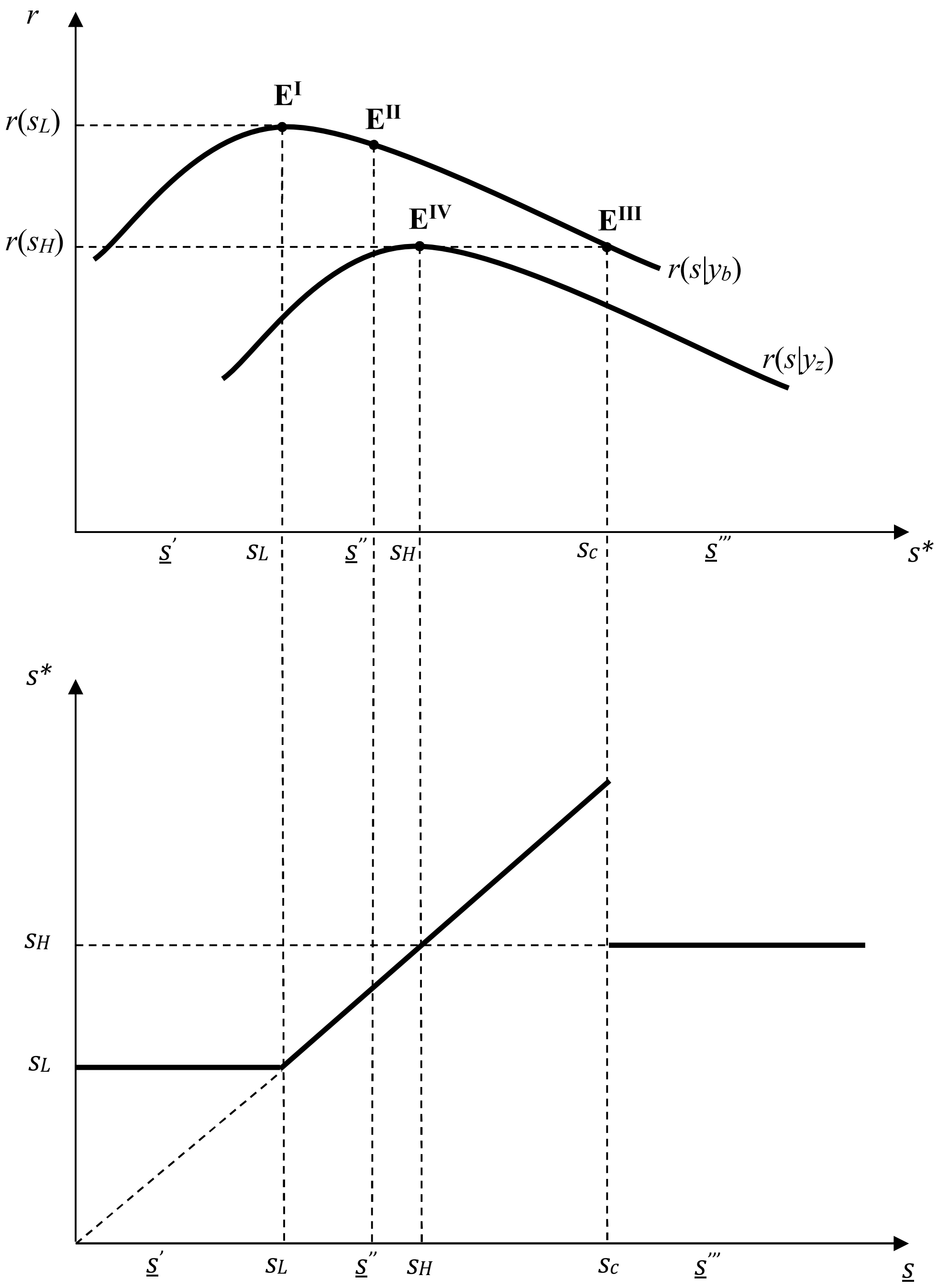
Supplemental Materials: Too much stick for the carrot? Job search requirements and search behaviour of unemployment benefit claimants

SM1. Derivation of the relation between search intensity and search requirements

This Section discusses the derivation of the relation between optimal search intensity (s^*) and search requirements ($\underline{s}$) from the underlying first-order conditions. Fig. SM1 shows on the upper part the curves $r(s|y_u)$ representing combinations between the reservation wage r and s^* that satisfy first-order conditions for given unemployment income y_u, and on the lower part the relation between s^* and $\underline{s}$. The graphical derivation of the curves $r(s|y_u)$ follows the analysis of Cockx et al. (2014, see Fig. 1) for the case of an agent with exponential time preferences. In any point of $r(s|y_u)$ the benefit of continuing search rather than accepting a job at the reservation wage is equal to the cost of continued search in terms of effort and forgone employment income (see Cockx et al., 2014, eq. 11). Such curves are hump-shaped and their maximum corresponds to (s, r) pairs where the second first-order condition holds, i.e. marginal benefits and marginal costs of search are equal (see Cockx et al., 2014, Proposition. 1). The net marginal return to search is a decreasing function of s for given r, under the assumption of decreasing return in the job offer arrival rate and increasing marginal costs of search. Therefore, marginal costs become greater (respectively lower) than marginal benefits moving from the maximum to the right (respectively left) along the curves. Increments in the unemployment income y_u shift the curve upwards. UI recipients receive an unemployment income equal to y_b, while non-recipients receive a lower income equal to y_z, which may not be necessarily equal to zero. Curves corresponding to such levels are defined as $r(s|y_b)$ and $r(s|y_z)$, respectively, and reported in Fig. SM1.

In the absence of search requirements, the unemployed worker would be positioned on the maximum of the curve $r(s|y_b)$ in Fig. SM1, characterized by the pair $(s_L, r(s_L))$ (point E^I). With the introduction of search requirements, the unemployed worker would maintain the interior solution until $\underline{s} = s_L$, for example in a point corresponding to $\underline{s}^{'}$. When the requirement increases from $\underline{s}^{'}$ to $\underline{s}^{''}$, the optimal search effort increases from s_L to the corner solution $\underline{s}^{''}$, and the individual will move along the curve $r(s|y_b)$ to the right of the maximum, that is in point E^{II}. Further increases will be followed by one-for-one increases in s until point E^{III}, where the claimant is indifferent between staying in and ceasing claim with search intensity equal to s_H (point E^{IV}). Point E^{III} corresponds to the pair $(s_c, r(s_H))$, where s_c is formally defined by the following equality: $r(s_c|y_b) = r(s_H|y_z)$. Note that the lifetime utility is equivalent in E^{III} and in E^{IV}, i.e. the maximum of the curve $r(s|y_z)$, because it is proportional to the reservation wage (see Cockx et al., 2014, eq. 8). Any increase in the threshold from below to above s_c would lead to exit the claimant pool and to move to E^{IV}, where search intensity is set equal to s_H. The optimal search without claim (s_H) is larger than the unconstrained optimal search with claim (s_L) because the relative price of leisure is larger in the first case.

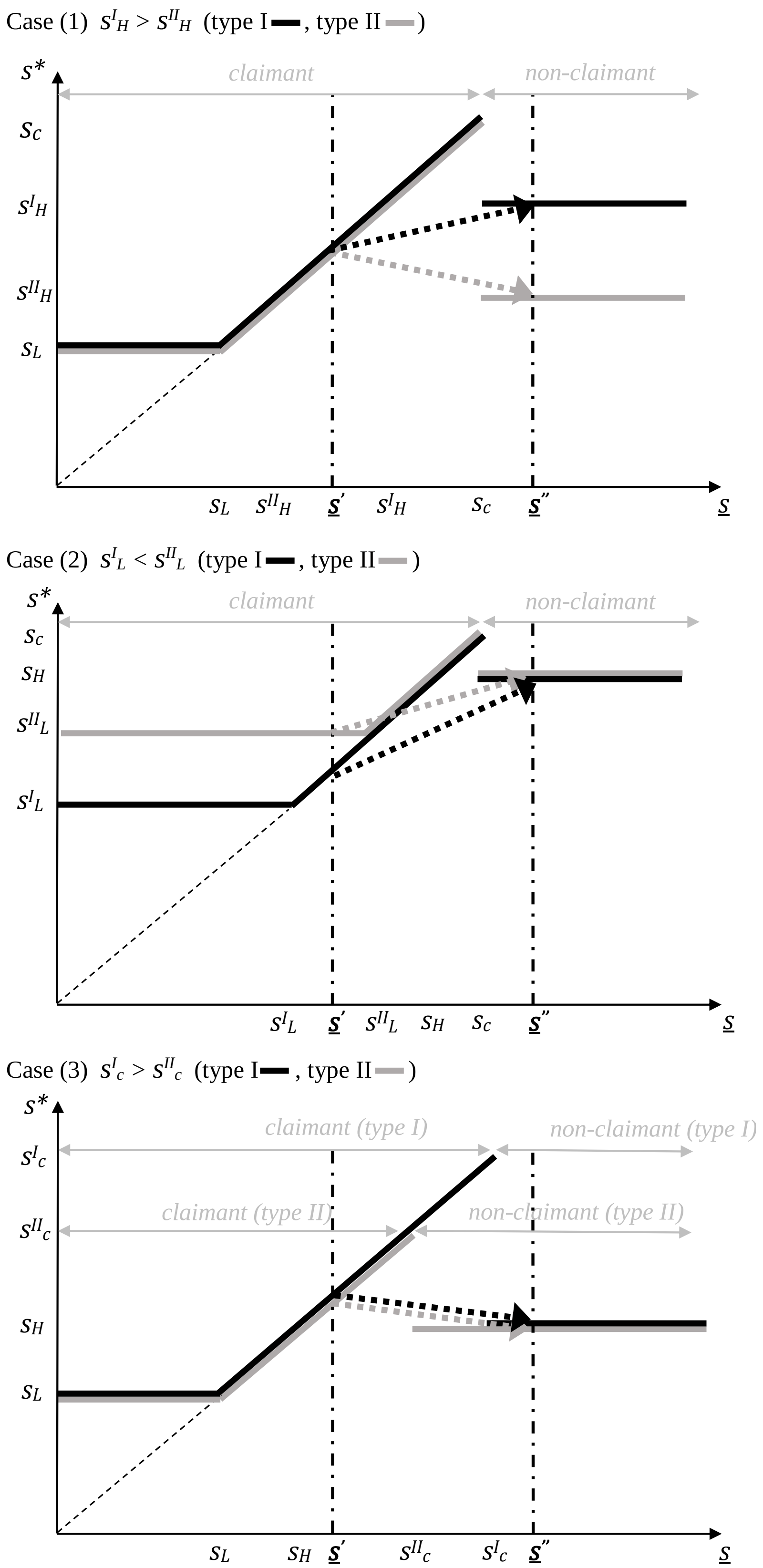
Figure SM1: Derivation of the relation between search intensity s^* and search requirements $\underline{s}$



SM2. Determinants of Δs^* for claimants exits to un-subsidized unemployment

This Section examines determinants of changes in search intensity (Δs^*) for claimants leaving the register. Since the relation between search intensity (s^*) and requirements ($\underline{s}$) depends only on the three parameters s^H, s^L, and s_c, variation in individual responses can be identified by changes in these parameters. Fig. SM2 shows effects of tighter search requirements ($\underline{s}^{'} \rightarrow \underline{s}^{''}$) highlighting three possible cases. Each case shows responses for two types of leavers with different values of s^H (case (1)), s^L (case (2)), or s_c (case (3)).

Figure SM2: Tighter search requirements ($\underline{s}' \to \underline{s}''$) and change in search intensity of claim leavers. Changes are compared between types of claimants with different values of s^H, s^L, or s_c.



In case (1) type I leaver (in black) has higher value of s_H than type II leaver (in grey) ($s_H^I > s_H^{II}$). The graph shows that an increase in search requirements from $\underline{s}^{'}$ to $\underline{s}^{''}$ leads both claimants to drop off, but type I leaver will continue searching with higher intensity, while type II will reduce search. In the example of the graph, an opposite change occurs because $s_H^{II} < \underline{s}^{'} < s_H^I$, but other outcomes may occur for other combinations of s_H^I and s_H^{II}. In general, a higher s_H increases the probability of leaving the claimant pool with increased search intensity relative to decreased search intensity.

In case (2), type I has lower s_L implying that the first segment of the relation is shifted downward-left along the 45° line. In the case illustrated in the graph, an increase in requirements from $\underline{s}^{'}$ to $\underline{s}^{''}$ leads both claimants to drop off with increased search, but type I leaver will perform a larger increase. Note that as long as $\underline{s}^{'} < s_H$, like in the graph, both claimants will perform an increase in search intensity irrespective of the particular values of s_L^I and s_L^{II}. s_L can only affect the size of the increment when $\underline{s}^{'} < s_L^{II}$, but s_L becomes irrelevant when $s_L^{II} \leq \underline{s}^{'} < s_H$. When $\underline{s}^{'} \geq s_H$ the two relations are identical and hence s_L is not relevant. Therefore while s_L can influence the magnitude of increments in search intensity in some cases, it has no impact on the probability of observing any of the scenarios associated to claim exit in Table 1.

Finally, case (3) shows that variations in s_c have no impact on the search intensity of leavers. s_c can only reduce the probability of leaving the claimant pool. For example, in case $\underline{s}^{''} < s_c^I$, the reform would lead type I claimants to keep claiming by adjusting search to higher standards. However note that in this case s_c does not influence search of leavers via sample selection, because in both periods search intensity of type I claimant would be the same of leavers should $\underline{s}^{''}$ be above s_c^I.

The analysis conducted in Fig. 1 delivers a clear conclusion: the direction of change in search intensity of leavers is only affected by the parameter s_H, reflecting the optimal search intensity of an unemployed person without benefit. Specifically, claimants with higher s_H are more likely to drop off with increased search relative to decreased search.

SM3. List of search methods

This section reports the list of possible search methods. Survey respondents who searched for a job in the last four weeks can select any of the followings: (1) Visit a Jobcentre, (2) Visit a Careers office, (3) Visit a Jobclub, (4) Have your name on the books of a private employment agency, (5) Advertise for jobs in newspapers or journals, (6) Answer advertisements in newspapers and journals, (7) Study situations vacant in newspapers or journals, (8) Apply directly to employers, (9) Ask friends, relatives, colleagues or trade unions about jobs, (10) Wait for the results of an application for a job, (11) Look for premises or equipment, (12) Seek any kind of permit, (13) Try to get a loan or other financial backing for a job or business (14) Do anything else to find work. The search methods (1), (2), and (3) have been considered for the analysis of transitions in the use of Public Employment Centre (PEC).

SM4. JSA ATE on cardinal search intensity

This section reports estimates of the JSA effect on the cardinal change in the count of methods. Table SM1 reports results for the sample of claimants in wave 1 remaining non-employed in wave 2. Table SM2 considers unemployed non-claimants in wave 1 remaining non-claimants without job in wave 2.

Table SM1: JSA ATE on search intensity with alternative control years. Sample of claimants in wave 1 remaining non-employed in wave 2.

	Treatment = 96			
	94-95-97-98	95-97-98	94-95-97	95-97
Full sample				
ATE	0.166*	0.140	0.195**	0.182*
	(0.073)	(0.077)	(0.074)	(0.081)
Nobs	18418	13195	16230	11007
Stayers				
ATE	0.158*	0.125	0.185*	0.160
	(0.075)	(0.079)	(0.076)	(0.082)
Nobs	15427	10976	13548	9097
Leavers				
ATE	0.521*	0.505*	0.566**	0.571*
	(0.209)	(0.224)	(0.213)	(0.236)
Nobs	2991	2219	2682	1910

Notes: *** significant 0.1%, ** significant 1%, * significant 5%. Robust standard errors are reported in parenthesis. The dependent variable is change in s^* from wave 1 to wave 2. ATE is estimated by OLS. The following control variables are used: gender, number of kids (< 19), age, age^2, education, duration since last job, marriage status, region.

Table SM2: JSA ATE on search intensity with alternative control years. Sample of unemployed non-claimants in wave 1 remaining non-claimants without job in wave 2.

	Treatment = 96			
	94-95-97-98	95-97-98	94-95-97	95-97
ATE	0.132	0.128	0.102	0.082
	(0.110)	(0.114)	(0.114)	(0.121)
Nobs	10359	8164	8438	6243

Notes: *** significant 0.1%, ** significant 1%, * significant 5%. Robust standard errors are reported in parenthesis. The dependent variable is change in s^* from wave 1 to wave 2. ATE is estimated by OLS. The following control variables are used: gender, number of kids (< 19), age, age^2, education, duration since last job, marriage status, region.

SM5. Liquidity constraints and JSA effects

This section presents evidence on the impact of liquidity constraints on the claimant response to JSA. Defining binary indicators for liquidity constraints, a Diff-in-diff-in-diff technique is used to estimate the JSA Average Treatment Effects (ATE) for constrained claimants relative to unconstrained claimants. The Diff-in-diff-in-diff is employed by augmenting eq. (2) with a full set of interaction terms as follows:

$$\beta' Z = \beta_0 + \beta_1\, d96 + \beta_2\, dq3q4 + \beta_3\, d96 \cdot dq3q4 + \beta_4\, L + \beta_5\, d96 \cdot L + \beta_6\, dq3q4 \cdot L + \beta_7\, d96 \cdot dq3q4 \cdot L + \delta' X, \quad (4)$$

where L indicates liquidity constrained ($L = 1$) and unconstrained ($L = 0$) claimants. The linear index specified in eq. (4) is used to estimate a multinomial logit with J possible outcomes as in eq. (1). ATE estimates are constructed using the following $J \cdot 8$ probabilities:

$$P^j_{t,q,l}(X^\circ) = P(y = j | d96 = t, dq3q4 = q, L = l, X = X^\circ), \quad (5)$$

where $t \in \{0, 1\}$, $q \in \{0, 1\}$, and $l \in \{0, 1\}$ determine the eight possible combinations of time, treatment, and liquidity status in the Diff-in-diff-in-diff, $j \in \{1, \ldots, J\}$ represent possible outcomes, and appropriate values are used for X°. J Diff-in-diff-in-diff coefficients are identified as $DiDiD^j(X) = [(P^j_{1,1,1}(X) - P^j_{1,0,1}(X)) - (P^j_{0,1,1}(X) - P^j_{0,0,1}(X))] - [(P^j_{1,1,0}(X) - P^j_{1,0,0}(X)) - (P^j_{0,1,0}(X) - P^j_{0,0,0}(X))]$, where $DiDiD^j(X^\circ)$ is the JSA effect on the probability of experiencing transition j for liquidity constrained claimants relative to unconstrained claimants.

Following Chetty (2008), liquidity constrains are proxied by two indicators for having to make mortgage payments, and for single- versus dual-earner status. Chetty (2008) considers also asset holding but unfortunately this information is not available in the present data. Sample statistics for the two indicators are reported in Table SM3, similarly to Table 2. Note that the single- versus dual-earner indicator is equal to 1 for married people whose spouse is non-employed as well as for unmarried people. The Table reports also statistics for a third indicator defining liquidity constraints by the co-occurrence of the two conditions, against the alternative of fulfilling only one or none of them. This indicator takes into account that unemployed with mortgage obligations may not be liquidity constrained if they have a working spouse, as well as unemployed without working spouse in the absence of mortgage. Statistics show that among claimants in 1996q3, 31.6% hold a mortgage, 88.8% do not have a working spouse, and 25.2% hold a mortgager and are single earner at the same time. Table SM3 shows that there are no significant deviations across treatment and time status for any of these variables suggesting good balancing. Since nearly all claimants (95%) have at least one constraint, and a relevant share is subject to even tighter constraints, it is possible that a substantial portion of the claimant population is liquidity constrained. One should bear in mind that such constraints may be lessened by availability of liquid assets, and hence one cannot draw definitive conclusions in the absence of such information. However, evidence typically shows that many unemployed individuals have limited or no liquid wealth (Chetty, 2008). Moreover, in the sample used in the present analysis claimants are relatively young (see Table 2), and hence they may have accumulated limited liquid assets. Therefore it is likely that a relevant share of claimants are unable to smooth transitory income losses, and hence may experience a drop in consumptions should they leave benefit rolls.

Table SM4–SM6 report ATE estimates for outcome variables specified like in Table 3, considering as indicators for liquidity constraints, respectively, holding a mortgage, single earner status, or fulfilling both conditions at the same time. Reported ATEs capture the difference in the JSA impact on the specified probability between constrained and unconstrained claimants.

Table SM3: Sample statistics for claimants in 1996q3 and Difference-in-differences in variable means calculated comparing between q3 and q2 and between 1996 and control years. *** significant 0.1%, ** significant 1%, * significant 5%. Standard errors in parenthesis.

	96		94-95-97-98		95-97-98		94-95-97		95-97	
	mean	se	DiD	se	DiD	se	DiD	se	DiD	se
Mortgager	0.316	(0.465)	0.010	(0.015)	0.007	(0.016)	0.010	(0.015)	0.006	(0.017)
Single earner	0.888	(0.316)	0.006	(0.010)	0.010	(0.011)	0.006	(0.011)	0.010	(0.012)
Both constraints	0.252	(0.434)	0.017	(0.014)	0.016	(0.015)	0.016	(0.014)	0.013	(0.015)
Nobs	2376		17652		11401		14953		8702	

Table SM4: JSA ATE on claimant behaviour with alternative control groups. ATE refers to liquidity constrained vs unconstrained claimants. Constraints are defined by having a mortgage

	Treatment = 96			
	94-95-97-98	95-97-98	94-95-97	95-97
(a) C				
$P(C=1)$	-0.026	-0.021	-0.033	-0.034
	(0.031)	(0.034)	(0.032)	(0.035)
$P(C=2)$	-0.031	-0.034	-0.027	-0.025
	(0.023)	(0.025)	(0.024)	(0.027)
$P(C=3)$	0.057*	0.055*	0.060*	0.059*
	(0.024)	(0.026)	(0.025)	(0.027)
(b) Δs^*				
$P^\star(\Delta s^* < 0)$	-0.087**	-0.092**	-0.082**	-0.086**
	(0.029)	(0.031)	(0.029)	(0.032)
$P^\star(\Delta s^* = 0)$	-0.025	-0.025	-0.022	-0.021
	(0.036)	(0.038)	(0.036)	(0.039)
$P^\star(\Delta s^* > 0)$	0.053	0.060	0.043	0.045
	(0.029)	(0.031)	(0.030)	(0.033)
$P(C=3)$	0.059*	0.057*	0.061*	0.061*
	(0.024)	(0.026)	(0.025)	(0.027)
(c) $(C, \Delta s^*)$				
$P(C=1, \Delta s^* < 0)$	-0.058*	-0.062*	-0.054*	-0.058*
	(0.027)	(0.028)	(0.027)	(0.029)
$P(C=1, \Delta s^* = 0)$	0.005	0.010	0.004	0.008
	(0.035)	(0.037)	(0.036)	(0.039)
$P(C=1, \Delta s^* > 0)$	0.025	0.030	0.015	0.014
	(0.029)	(0.030)	(0.029)	(0.031)
$P(C=2, \Delta s^* < 0)$	-0.034*	-0.035*	-0.032*	-0.032*
	(0.014)	(0.016)	(0.014)	(0.017)
$P(C=2, \Delta s^* = 0)$	-0.024	-0.029	-0.022	-0.025
	(0.017)	(0.019)	(0.018)	(0.020)
$P(C=2, \Delta s^* > 0)$	0.028*	0.031*	0.029*	0.034*
	(0.012)	(0.013)	(0.013)	(0.015)
$P(C=3)$	0.058*	0.055*	0.060*	0.059*
	(0.024)	(0.026)	(0.025)	(0.027)
Nobs	22364	16113	19665	13414

Notes: *** significant 0.1%, ** significant 1%, * significant 5%. Robust standard errors are reported in parenthesis. $P^\star(\cdot) := P(\cdot|C \in \{1,2\})$. The outcome variables (a), (b) and (c) make use of the variables C and Δs^*. C identifies transitions from claimant status in wave 1 as follows: $C = 1$ if still non-employed and claimant in wave 2, $C = 2$ if still non-employed but no longer claimant, $C = 3$ if employed. Δs^* is an indicator of change in job search intensity ($\Delta s^* \gtreqless 0$) between wave 1 and wave 2. Reported ATEs are computed from multinomial logit models with Diff-in-diff-diff specification and correspond to changes in the probability of performing the specified transition for constrained vs unconstrained claimants. The following control variables are used: gender, number of kids (< 19), age, age^2, education, duration since last job, marriage status, region.

Table SM5: JSA ATE on claimant behaviour with alternative control groups. ATE refers to liquidity constrained vs unconstrained claimants. Constraints are defined by not having a working spouse

	Treatment = 96			
	94-95-97-98	95-97-98	94-95-97	95-97
(a) C				
$P(C=1)$	-0.084^{*}	-0.098^{*}	-0.071	-0.079
	(0.042)	(0.046)	(0.043)	(0.048)
$P(C=2)$	0.025	0.032	0.020	0.028
	(0.030)	(0.033)	(0.032)	(0.036)
$P(C=3)$	0.059	0.065	0.050	0.051
	(0.032)	(0.035)	(0.033)	(0.036)
(b) Δs^{*}				
$P^{\star}(\Delta s^{*}<0)$	-0.060	-0.082	-0.056	-0.080
	(0.040)	(0.042)	(0.041)	(0.044)
$P^{\star}(\Delta s^{*}=0)$	-0.056	-0.050	-0.055	-0.045
	(0.051)	(0.054)	(0.052)	(0.056)
$P^{\star}(\Delta s^{*}>0)$	0.056	0.067	0.060	0.074
	(0.042)	(0.044)	(0.043)	(0.046)
$P(C=3)$	0.060	0.065	0.051	0.051
	(0.032)	(0.035)	(0.032)	(0.036)
(c) $(C,\Delta s^{*})$				
$P(C=1,\Delta s^{*}<0)$	-0.026	-0.043	-0.025	-0.044
	(0.037)	(0.039)	(0.038)	(0.041)
$P(C=1,\Delta s^{*}=0)$	-0.081	-0.093	-0.072	-0.081
	(0.052)	(0.055)	(0.053)	(0.057)
$P(C=1,\Delta s^{*}>0)$	0.018	0.032	0.022	0.039
	(0.041)	(0.043)	(0.041)	(0.044)
$P(C=2,\Delta s^{*}<0)$	-0.025	-0.030	-0.023	-0.028
	(0.019)	(0.021)	(0.019)	(0.022)
$P(C=2,\Delta s^{*}=0)$	0.018	0.033	0.011	0.029
	(0.022)	(0.024)	(0.024)	(0.027)
$P(C=2,\Delta s^{*}>0)$	0.035^{*}	0.035^{*}	0.035^{*}	0.035^{*}
	(0.014)	(0.015)	(0.015)	(0.017)
$P(C=3)$	0.060	0.066	0.051	0.051
	(0.032)	(0.035)	(0.033)	(0.036)
Nobs	22364	16113	19665	13414

Notes: *** significant 0.1%, ** significant 1%, * significant 5%. Robust standard errors are reported in parenthesis. $P^{\star}(\cdot) := P(\cdot|C \in \{1,2\})$. The outcome variables (a), (b) and (c) make use of the variables C and Δs^{*}. C identifies transitions from claimant status in wave 1 as follows: $C=1$ if still non-employed and claimant in wave 2, $C=2$ if still non-employed but no longer claimant, $C=3$ if employed. Δs^{*} is an indicator of change in job search intensity ($\Delta s^{*} \gtreqless 0$) between wave 1 and wave 2. Reported ATEs are computed from multinomial logit models with Diff-in-diff-diff specification and correspond to changes in the probability of performing the specified transition for constrained vs unconstrained claimants. The following control variables are used: gender, number of kids (<19), age, age^2, education, duration since last job, marriage status, region.

Table SM6: JSA ATE on claimant behaviour with alternative control groups. ATE refers to liquidity constrained vs unconstrained claimants. Constraints are defined by having a mortgage and not having a working spouse

	Treatment = 96			
	94-95-97-98	95-97-98	94-95-97	95-97
(a) C				
$P(C=1)$	−0.061	−0.063	−0.061	−0.065
	(0.034)	(0.037)	(0.035)	(0.039)
$P(C=2)$	−0.032	−0.030	−0.027	−0.021
	(0.026)	(0.028)	(0.027)	(0.030)
$P(C=3)$	0.093***	0.093**	0.088**	0.085**
	(0.026)	(0.028)	(0.027)	(0.029)
(b) Δs^*				
$P^\star(\Delta s^* < 0)$	−0.095**	−0.107**	−0.086**	−0.097**
	(0.032)	(0.033)	(0.032)	(0.035)
$P^\star(\Delta s^* = 0)$	−0.058	−0.060	−0.050	−0.048
	(0.039)	(0.041)	(0.040)	(0.043)
$P^\star(\Delta s^* > 0)$	0.057	0.071*	0.046	0.057
	(0.032)	(0.034)	(0.033)	(0.035)
$P(C=3)$	0.096***	0.096***	0.090***	0.088**
	(0.026)	(0.028)	(0.027)	(0.029)
(c) $(C, \Delta s^*)$				
$P(C=1, \Delta s^* < 0)$	−0.059*	−0.069*	−0.053	−0.062*
	(0.029)	(0.030)	(0.030)	(0.031)
$P(C=1, \Delta s^* = 0)$	−0.025	−0.030	−0.020	−0.024
	(0.039)	(0.041)	(0.039)	(0.043)
$P(C=1, \Delta s^* > 0)$	0.026	0.039	0.015	0.024
	(0.031)	(0.032)	(0.032)	(0.034)
$P(C=2, \Delta s^* < 0)$	−0.039*	−0.042*	−0.037*	−0.040*
	(0.016)	(0.017)	(0.016)	(0.018)
$P(C=2, \Delta s^* = 0)$	−0.025	−0.022	−0.023	−0.017
	(0.020)	(0.021)	(0.021)	(0.023)
$P(C=2, \Delta s^* > 0)$	0.028*	0.030*	0.029*	0.033*
	(0.013)	(0.015)	(0.014)	(0.016)
$P(C=3)$	0.094***	0.095***	0.089***	0.086**
	(0.026)	(0.028)	(0.027)	(0.030)
Nobs	22364	16113	19665	13414

Notes: *** significant 0.1%, ** significant 1%, * significant 5%. Robust standard errors are reported in parenthesis. $P^\star(\cdot) := P(\cdot|C \in \{1,2\})$. The outcome variables (a), (b) and (c) make use of the variables C and Δs^*. C identifies transitions from claimant status in wave 1 as follows: $C = 1$ if still non-employed and claimant in wave 2, $C = 2$ if still non-employed but no longer claimant, $C = 3$ if employed. Δs^* is an indicator of change in job search intensity ($\Delta s^* \gtreqless 0$) between wave 1 and wave 2. Reported ATEs are computed from multinomial logit models with Diff-in-diff-diff specification and correspond to changes in the probability of performing the specified transition for constrained vs unconstrained claimants. The following control variables are used: gender, number of kids (< 19), age, age^2, education, duration since last job, marriage status, region.

SM6. Multinomial logit estimates

Table SM7: Multinomial logit estimates for the effect of JSA on claimant behaviour. See Table 3, column 1, for corresponding ATEs.

	$C = 1, \Delta s^* < 0$		$C = 1, \Delta s^* = 0$		$C = 1, \Delta s^* > 0$		$C = 2, \Delta s^* = 0$		$C = 2, \Delta s^* > 0$		C=3	
d95	-0.068	(0.104)	-0.102	(0.098)	-0.120	(0.102)	0.008	(0.118)	0.056	(0.162)	-0.092	(0.105)
d96	-0.101	(0.142)	-0.084	(0.134)	-0.192	(0.140)	0.055	(0.160)	-0.297	(0.234)	-0.047	(0.143)
d97	-0.458***	(0.115)	-0.500***	(0.108)	-0.491***	(0.112)	-0.589***	(0.139)	-0.186	(0.182)	-0.045	(0.113)
d98	-0.263*	(0.128)	-0.170	(0.120)	-0.318*	(0.126)	-0.360*	(0.152)	-0.302	(0.213)	0.011	(0.127)
dq3q4	-0.074	(0.082)	-0.080	(0.077)	-0.089	(0.080)	-0.028	(0.095)	-0.117	(0.130)	0.022	(0.082)
d96_dq3q4	-0.549**	(0.175)	-0.474**	(0.163)	-0.281	(0.170)	-0.004	(0.194)	0.694*	(0.271)	-0.205	(0.173)
Female	-0.651***	(0.081)	-0.564***	(0.075)	-0.734***	(0.079)	0.612***	(0.088)	-0.188	(0.124)	-0.399***	(0.079)
Age	-0.002	(0.004)	-0.005	(0.004)	-0.005	(0.004)	0.012**	(0.004)	-0.009	(0.006)	-0.024***	(0.004)
Married	-0.256**	(0.092)	-0.515***	(0.087)	-0.295**	(0.090)	-0.253*	(0.105)	-0.245	(0.149)	0.073	(0.092)
Nr. kids 0—18 yrs	-0.030	(0.036)	0.035	(0.034)	-0.022	(0.035)	0.204***	(0.038)	0.121*	(0.050)	-0.010	(0.036)
Higher education	-0.287	(0.215)	-0.422*	(0.206)	-0.363	(0.212)	-0.245	(0.271)	-0.368	(0.357)	-0.611**	(0.208)
GCE, A or eq.	-0.352*	(0.166)	-0.148	(0.159)	-0.328*	(0.163)	0.135	(0.206)	-0.035	(0.257)	-0.624***	(0.160)
GCSE A*–C or eq.	-0.250	(0.166)	-0.213	(0.159)	-0.352*	(0.164)	0.200	(0.203)	-0.068	(0.257)	-0.815***	(0.161)
Other qualifications	-0.152	(0.169)	0.085	(0.161)	-0.058	(0.166)	0.347	(0.206)	-0.024	(0.264)	-0.660***	(0.164)
No qualifications	-0.237	(0.161)	0.158	(0.153)	-0.169	(0.158)	0.397*	(0.197)	-0.077	(0.252)	-0.979***	(0.157)
Last job 3—6 mts	0.071	(0.168)	-0.046	(0.159)	-0.251	(0.163)	-0.068	(0.206)	-0.245	(0.279)	-0.398*	(0.157)
Last job 6—12 mts	-0.141	(0.155)	-0.163	(0.146)	-0.443**	(0.150)	0.049	(0.187)	-0.255	(0.254)	-0.835***	(0.146)
Last job 1—2 yrs	-0.327*	(0.150)	-0.450**	(0.141)	-0.636***	(0.145)	-0.185	(0.182)	-0.124	(0.238)	-1.452***	(0.143)
Last job 2—3 yrs	0.091	(0.178)	0.121	(0.168)	-0.131	(0.172)	0.486*	(0.208)	0.448	(0.267)	-1.425***	(0.177)
Last job 3—4 yrs	0.196	(0.201)	0.173	(0.191)	-0.122	(0.196)	0.601**	(0.231)	0.683*	(0.287)	-1.251***	(0.202)
Last job 4—5 yrs	-0.022	(0.204)	-0.053	(0.193)	-0.234	(0.198)	0.302	(0.237)	0.079	(0.318)	-1.733***	(0.213)
Last job > 5 yrs	-0.075	(0.160)	0.050	(0.149)	-0.472**	(0.155)	0.593**	(0.185)	-0.095	(0.256)	-2.157***	(0.165)
No job before	-0.489**	(0.183)	-0.516**	(0.171)	-0.788***	(0.177)	0.212	(0.213)	-0.014	(0.277)	-1.518***	(0.174)
North West	0.020	(0.174)	0.033	(0.162)	0.018	(0.169)	0.149	(0.203)	-0.218	(0.272)	0.075	(0.174)
York. & Humber.	0.080	(0.185)	0.006	(0.173)	0.003	(0.180)	0.101	(0.217)	0.054	(0.279)	0.106	(0.184)
East Midlands	-0.072	(0.202)	0.021	(0.187)	-0.241	(0.197)	0.510*	(0.225)	0.024	(0.302)	0.214	(0.199)
West Midlands	0.053	(0.180)	-0.062	(0.168)	-0.021	(0.175)	-0.098	(0.213)	-0.293	(0.285)	0.017	(0.181)
Eastern	0.034	(0.189)	-0.133	(0.178)	-0.073	(0.185)	0.011	(0.223)	-0.265	(0.300)	0.139	(0.189)
London	0.173	(0.171)	0.183	(0.160)	0.057	(0.166)	0.168	(0.199)	0.027	(0.259)	-0.020	(0.172)
South East	0.149	(0.187)	0.029	(0.175)	0.164	(0.181)	0.166	(0.219)	-0.049	(0.290)	0.427*	(0.185)
South West	0.217	(0.202)	0.148	(0.190)	0.155	(0.197)	0.433	(0.229)	0.175	(0.303)	0.384	(0.200)
Wales	-0.048	(0.206)	-0.138	(0.193)	-0.152	(0.201)	0.114	(0.239)	0.022	(0.312)	0.064	(0.204)
Scotland	0.019	(0.178)	-0.015	(0.167)	-0.070	(0.174)	0.274	(0.207)	0.103	(0.271)	0.057	(0.178)
Observations	22399											

Notes: *** significant 0.1%, ** significant 1%, * significant 5%. Robust standard errors are reported in parenthesis.

SM7. Tests for anticipatory effects

Table SM8 and Table SM9 report tests for the presence of JSA anticipatory effects in ATE estimates reported in Table 3 and Table 4, respectively. Claimant flows between $q2$ and $q3$ are considered as a treatment group in these tests, and they are compared to transitions between $q1$ and $q2$ and across years to implement a Diff-in-diff strategy.

In interpreting results one should bear in mind that estimated ATEs must sum up to zero, and hence each of them is compensated by other effects reflecting the same behavioural change. The following outcomes are considered counterparts since they do not reflect behavioural change per se: $P(C = 1)$ in panel (a); $P(\Delta s^* = 0)$ in panel (b); $P(C = \cdot, \Delta s^* = 0)$ in panel (c). According to this definition, there are 32 relevant ATEs in Table SM8, of which none is significant. Following a similar argument, the following outcomes are considered counterparts in Table SM9: $P(M_{PEC} = 0)$ in panel (d); $P(C = \cdot, M_{PEC} = 0)$ in panel (e). Hence, there are 24 relevant coefficients in Table SM9. No evidence of anticipatory effects is found in these cases either.

Table SM8: JSA ATE on claimant behaviour with alternative control years. Tests for anticipatory effects. The treatment group comprises flows from q2 to q3, and the control group flows from q1 to q2.

	Treatment = 96			
	94-95-97-98	95-97-98	94-95-97	95-97
(a) C				
$P(C=1)$	−0.003	−0.016	0.013	0.006
	(0.014)	(0.015)	(0.014)	(0.016)
$P(C=2)$	0.013	0.020	0.003	0.006
	(0.010)	(0.011)	(0.010)	(0.011)
$P(C=3)$	−0.010	−0.004	−0.016	−0.012
	(0.012)	(0.013)	(0.012)	(0.013)
(b) Δs^*				
$P^\star(\Delta s^* < 0)$	0.019	0.007	0.018	0.003
	(0.013)	(0.014)	(0.013)	(0.014)
$P^\star(\Delta s^* = 0)$	0.012	0.018	0.022	0.037*
	(0.015)	(0.016)	(0.016)	(0.017)
$P^\star(\Delta s^* > 0)$	−0.021	−0.020	−0.025	−0.027
	(0.013)	(0.014)	(0.014)	(0.015)
$P(C=3)$	−0.010	−0.005	−0.016	−0.012
	(0.012)	(0.013)	(0.012)	(0.013)
(c) $(C, \Delta s^*)$				
$P(C=1, \Delta s^* < 0)$	0.010	−0.002	0.012	−0.002
	(0.012)	(0.012)	(0.012)	(0.013)
$P(C=1, \Delta s^* = 0)$	0.009	0.008	0.025	0.034*
	(0.015)	(0.016)	(0.015)	(0.017)
$P(C=1, \Delta s^* > 0)$	−0.022	−0.022	−0.024	−0.026
	(0.013)	(0.013)	(0.013)	(0.014)
$P(C=2, \Delta s^* < 0)$	0.009	0.009	0.006	0.004
	(0.007)	(0.007)	(0.007)	(0.007)
$P(C=2, \Delta s^* = 0)$	0.003	0.010	−0.003	0.003
	(0.007)	(0.008)	(0.007)	(0.008)
$P(C=2, \Delta s^* > 0)$	0.001	0.002	−0.000	−0.000
	(0.005)	(0.005)	(0.005)	(0.005)
$P(C=3)$	−0.010	−0.005	−0.016	−0.012
	(0.012)	(0.013)	(0.012)	(0.013)
Nobs	23530	16966	20846	14282

Notes: *** significant 0.1%, ** significant 1%, * significant 5%. Robust standard errors are reported in parenthesis. See Table 3.

Table SM9: JSA ATE on use of PEC with alternative control years. Tests for anticipatory effects. The treatment group comprises flows from q2 to q3, and the control group flows from q1 to q2.

	Treatment = 96			
	94-95-97-98	95-97-98	94-95-97	95-97
(d) M_{PEC}				
$P^{\star}(M_{PEC} = -1)$	0.010	0.000	0.013	0.002
	(0.008)	(0.008)	(0.008)	(0.008)
$P^{\star}(M_{PEC} = 1)$	−0.007	−0.005	−0.007	−0.006
	(0.006)	(0.006)	(0.007)	(0.007)
$P^{\star}(M_{PEC} = 0)$	0.007	0.011	0.009	0.015
	(0.014)	(0.015)	(0.014)	(0.015)
$P(C = 3)$	−0.011	−0.006	−0.015	−0.011
	(0.012)	(0.013)	(0.012)	(0.013)
(e) (M_{PEC}, C)				
$P(C = 1, M_{PEC} = -1)$	0.002	−0.004	0.006	0.001
	(0.006)	(0.006)	(0.006)	(0.007)
$P(C = 1, M_{PEC} = 1)$	−0.005	−0.003	−0.004	−0.002
	(0.006)	(0.006)	(0.006)	(0.007)
$P(C = 1, M_{PEC} = 0)$	0.009	0.000	0.025	0.024
	(0.015)	(0.016)	(0.016)	(0.017)
$P(C = 2, M_{PEC} = -1)$	0.009	0.006	0.008	0.001
	(0.005)	(0.005)	(0.005)	(0.005)
$P(C = 2, M_{PEC} = 1)$	−0.003	−0.003	−0.003	−0.004
	(0.002)	(0.003)	(0.002)	(0.003)
$P(C = 2, M_{PEC} = 0)$	−0.001	0.011	−0.016	−0.010
	(0.009)	(0.010)	(0.009)	(0.010)
$P(C = 3)$	−0.011	−0.006	−0.016	−0.011
	(0.012)	(0.013)	(0.012)	(0.013)
Nobs	23467	16930	20727	14190

Notes: *** significant 0.1%, ** significant 1%, * significant 5%. Robust standard errors are reported in parenthesis. See Table 4.

SM8. Placebo tests

Tables SM10–SM11 report falsification tests on "placebo" JSA reforms. The outcomes in these Tables correspond to the ones in Table 3 and Table 4 in the main text, respectively. The tests are based on the same sample used in estimation with the exclusion of 1996, the year JSA was introduced. Years used to form the control group, namely 1994, 1995, 1997 and 1998, are considered in turn as the treatment group in the placebo tests, using the remaining years as control group. Each column of Tables SM10–SM11 hence report tests on the equality of seasonal effects between the focal year and the remaining years.

In Table SM10, significant seasonal effects are found for $P(C = 1, \Delta s^* < 0)$ (panel (c)) in 1994 and 1995, with positive and negative sign respectively. These effects are similarly reflected in coefficients for $P^*(\Delta s^* < 0)$ in panel (b) and for $P(C = 1)$ in panel (a). However, results reported in Table 3 show that treatment effects estimated for these outcomes are somewhat stable across alternative combinations of comparison years in terms of sign, magnitude and significance. Therefore, results are only marginally influenced by such yearly fluctuations, and main conclusions are not affected qualitatively. Moreover remark that no significant effects are found for any outcome of leavers ($P(C = 2)$).

In Table SM11, significant effects are found only for $P(C = 1, M_{PEC} = 0)$ (panel (e)) in 1994, and for $P(C = 3)$ in 1995. In the first case, since the outcome $P(C = 1, M_{PEC} = 0)$ is not associated to a behavioural change, and no other significant effects are found in the model, there does not appear to be any relevant change in behaviour in this year (see also the discussion in Section SM7). In the second case, a positive effect on $P(C = 3)$ for 1995 suggests an unusual employment inflow associated to transitions between $q3$ and $q4$ relatively to transitions between $q2$ and $q3$. However, evidence for this seasonal effect appears quite weak since the coefficient is only marginally significant, and the effect on $P(C = 3)$ turns out to be insignificant in panel (a), (b), and (c) in Table SM10. Moreover the estimated JSA impacts on $P(C = 3)$ reported in Tables 3 and 4 are never close to significance, even when 1995 and 1997 are used alone as comparison group, suggesting that treatment effect estimation is not influenced significantly. Therefore, this yearly fluctuation does not appear to challenge the well established conclusion of a null employment impact of JSA. Remark that no significant effects are found for any outcome of leavers even in this Table.

Table SM10: JSA ATE on claimant behaviour with alternative control years. Placebo tests.

Treatment =	94	95	97	98
	95-97-98	94-97-98	94-95-98	94-95-97
(a) C				
$P(C=1)$	0.030*	−0.017	−0.013	−0.011
	(0.014)	(0.015)	(0.017)	(0.019)
$P(C=2)$	−0.008	−0.007	0.014	0.010
	(0.010)	(0.010)	(0.013)	(0.014)
$P(C=3)$	−0.021	0.023	−0.001	0.001
	(0.012)	(0.012)	(0.014)	(0.015)
(b) Δs^*				
$P^\star(\Delta s^* < 0)$	0.031*	−0.032*	0.022	−0.028
	(0.013)	(0.013)	(0.016)	(0.017)
$P^\star(\Delta s^* = 0)$	0.004	0.006	−0.009	−0.008
	(0.015)	(0.016)	(0.019)	(0.020)
$P^\star(\Delta s^* > 0)$	−0.014	0.003	−0.012	0.034
	(0.013)	(0.013)	(0.016)	(0.018)
$P(C=3)$	−0.021	0.023	−0.001	0.001
	(0.012)	(0.012)	(0.014)	(0.015)
(c) $(C, \Delta s^*)$				
$P(C=1, \Delta s^* < 0)$	0.029*	−0.025*	0.017	−0.029
	(0.012)	(0.012)	(0.015)	(0.016)
$P(C=1, \Delta s^* = 0)$	0.015	−0.001	−0.009	−0.015
	(0.015)	(0.015)	(0.019)	(0.020)
$P(C=1, \Delta s^* > 0)$	−0.015	0.010	−0.020	0.033
	(0.012)	(0.013)	(0.016)	(0.017)
$P(C=2, \Delta s^* < 0)$	0.002	−0.006	0.005	0.000
	(0.007)	(0.007)	(0.007)	(0.008)
$P(C=2, \Delta s^* = 0)$	−0.011	0.007	0.001	0.008
	(0.007)	(0.007)	(0.010)	(0.010)
$P(C=2, \Delta s^* > 0)$	0.001	−0.008	0.007	0.001
	(0.004)	(0.005)	(0.005)	(0.006)
$P(C=3)$	−0.022	0.023	−0.000	0.001
	(0.012)	(0.012)	(0.014)	(0.015)
Nobs	17670	17670	17670	17670

Notes: *** significant 0.1%, ** significant 1%, * significant 5%. Robust standard errors are reported in parenthesis. See Table 3 for estimates with the actual treatment group.

Table SM11: JSA ATE on use of PEC with alternative control years. Placebo tests.

Treatment =	94	95	97	98
	95-97-98	94-97-98	94-95-98	94-95-97
(d) M_{PEC}				
$P^{\star}(M_{PEC} = -1)$	0.006	−0.012	0.018	−0.014
	(0.008)	(0.008)	(0.010)	(0.011)
$P^{\star}(M_{PEC} = 1)$	−0.006	0.000	0.003	0.009
	(0.006)	(0.006)	(0.008)	(0.010)
$P^{\star}(M_{PEC} = 0)$	0.020	−0.012	−0.020	0.007
	(0.014)	(0.015)	(0.017)	(0.019)
$P(C = 3)$	−0.021	0.024*	−0.001	−0.002
	(0.012)	(0.012)	(0.014)	(0.015)
(e) (M_{PEC}, C)				
$P(C = 1, M_{PEC} = -1)$	0.007	−0.007	0.011	−0.014
	(0.006)	(0.006)	(0.008)	(0.009)
$P(C = 1, M_{PEC} = 1)$	−0.009	0.003	0.000	0.013
	(0.006)	(0.006)	(0.008)	(0.009)
$P(C = 1, M_{PEC} = 0)$	0.031*	−0.014	−0.022	−0.005
	(0.015)	(0.016)	(0.019)	(0.020)
$P(C = 2, M_{PEC} = -1)$	0.000	−0.005	0.005	−0.001
	(0.006)	(0.006)	(0.006)	(0.007)
$P(C = 2, M_{PEC} = 1)$	0.003	−0.003	0.002	−0.007
	(0.002)	(0.002)	(0.003)	(0.005)
$P(C = 2, M_{PEC} = 0)$	−0.012	0.002	0.003	0.015
	(0.009)	(0.009)	(0.012)	(0.012)
$P(C = 3)$	−0.021	0.024*	−0.000	−0.001
	(0.012)	(0.012)	(0.014)	(0.015)
Nobs	17598	17598	17598	17598

Notes: *** significant 0.1%, ** significant 1%, * significant 5%. Robust standard errors are reported in parenthesis. See Table 4 for estimates with the actual treatment group.

SM9. Estimates without control variables

Table SM12: JSA ATE on claimant behaviour with alternative control years. No control variables

	Treatment = 96			
	94-95-97-98	95-97-98	94-95-97	95-97
(a) C				
$P(C=1)$	-0.072^{***}	-0.063^{***}	-0.075^{***}	-0.064^{***}
	(0.015)	(0.016)	(0.015)	(0.017)
$P(C=2)$	0.055^{***}	0.053^{***}	0.057^{***}	0.056^{***}
	(0.011)	(0.012)	(0.011)	(0.012)
$P(C=3)$	0.018	0.010	0.018	0.008
	(0.013)	(0.014)	(0.013)	(0.014)
(b) Δs^*				
$P^\star(\Delta s^* < 0)$	-0.018	-0.008	-0.023	-0.012
	(0.013)	(0.014)	(0.014)	(0.015)
$P^\star(\Delta s^* = 0)$	-0.028	-0.026	-0.030	-0.028
	(0.016)	(0.017)	(0.016)	(0.018)
$P^\star(\Delta s^* > 0)$	0.031^{*}	0.026	0.037^{**}	0.034^{*}
	(0.014)	(0.014)	(0.014)	(0.015)
$P(C=3)$	0.015	0.008	0.016	0.006
	(0.013)	(0.014)	(0.013)	(0.014)
(c) $(C, \Delta s^*)$				
$P(C=1, \Delta s^* < 0)$	-0.033^{**}	-0.023	-0.038^{**}	-0.028^{*}
	(0.012)	(0.013)	(0.013)	(0.013)
$P(C=1, \Delta s^* = 0)$	-0.049^{**}	-0.043^{**}	-0.052^{**}	-0.046^{**}
	(0.016)	(0.017)	(0.016)	(0.017)
$P(C=1, \Delta s^* > 0)$	0.009	0.003	0.014	0.009
	(0.013)	(0.014)	(0.013)	(0.014)
$P(C=2, \Delta s^* < 0)$	0.014^{*}	0.015^{*}	0.014^{*}	0.015^{*}
	(0.007)	(0.007)	(0.007)	(0.008)
$P(C=2, \Delta s^* = 0)$	0.019^{*}	0.015	0.021^{**}	0.016
	(0.008)	(0.008)	(0.008)	(0.009)
$P(C=2, \Delta s^* > 0)$	0.022^{***}	0.023^{***}	0.023^{***}	0.025^{***}
	(0.005)	(0.005)	(0.005)	(0.006)
$P(C=3)$	0.018	0.010	0.018	0.008
	(0.013)	(0.014)	(0.013)	(0.014)
Nobs	22399	16148	19697	13446

Notes: *** significant 0.1%, ** significant 1%, * significant 5%. Robust standard errors are reported in parenthesis. See Table 3.

Table SM13: JSA ATE on use of PEC with alternative control years. No control variables

	Treatment = 96			
	94-95-97-98	95-97-98	94-95-97	95-97
(d) M_{PEC}				
$P^\star(M_{PEC} = -1)$	-0.007	-0.005	-0.009	-0.008
	(0.008)	(0.009)	(0.009)	(0.009)
$P^\star(M_{PEC} = 1)$	0.012	0.009	0.013	0.011
	(0.006)	(0.007)	(0.007)	(0.007)
$P^\star(M_{PEC} = 0)$	-0.021	-0.014	-0.020	-0.010
	(0.015)	(0.016)	(0.015)	(0.017)
$P(C = 3)$	0.016	0.009	0.016	0.007
	(0.013)	(0.013)	(0.013)	(0.014)
(e) (M_{PEC}, C)				
$P(C = 1, M_{PEC} = -1)$	-0.011	-0.008	-0.013	-0.011
	(0.007)	(0.007)	(0.007)	(0.007)
$P(C = 1, M_{PEC} = 1)$	0.003	-0.000	0.005	0.002
	(0.006)	(0.006)	(0.006)	(0.007)
$P(C = 1, M_{PEC} = 0)$	-0.066^{***}	-0.056^{***}	-0.068^{***}	-0.056^{**}
	(0.016)	(0.017)	(0.016)	(0.018)
$P(C = 2, M_{PEC} = -1)$	0.004	0.004	0.004	0.004
	(0.006)	(0.006)	(0.006)	(0.006)
$P(C = 2, M_{PEC} = 1)$	0.008^{**}	0.009^{**}	0.008^{**}	0.010^{**}
	(0.003)	(0.003)	(0.003)	(0.004)
$P(C = 2, M_{PEC} = 0)$	0.043^{***}	0.039^{***}	0.046^{***}	0.043^{***}
	(0.009)	(0.010)	(0.010)	(0.011)
$P(C = 3)$	0.018	0.011	0.018	0.008
	(0.013)	(0.013)	(0.013)	(0.014)
Nobs	22310	16092	19617	13399

Notes: *** significant 0.1%, ** significant 1%, * significant 5%. Robust standard errors are reported in parenthesis. See Table 4

SM10. Estimates after deleting claimants on New Deal in 1998

Table SM14: JSA ATE on claimant behaviour with alternative control years. A small number of individuals on New Deal in 1998 have been dropped.

	Treatment = 96			
	94-95-97-98	95-97-98	94-95-97	95-97
(a) C				
$P(C=1)$	-0.075^{***}	-0.066^{***}	-0.076^{***}	-0.065^{***}
	(0.015)	(0.016)	(0.015)	(0.016)
$P(C=2)$	0.055^{***}	0.053^{***}	0.058^{***}	0.056^{***}
	(0.010)	(0.011)	(0.011)	(0.012)
$P(C=3)$	0.020	0.013	0.019	0.009
	(0.012)	(0.013)	(0.012)	(0.013)
(b) Δs^*				
$P^\star(\Delta s^* < 0)$	−0.019	−0.008	−0.024	−0.013
	(0.013)	(0.014)	(0.014)	(0.015)
$P^\star(\Delta s^* = 0)$	−0.029	−0.027	−0.029	−0.028
	(0.016)	(0.017)	(0.016)	(0.017)
$P^\star(\Delta s^* > 0)$	0.031^{*}	0.025	0.037^{**}	0.034^{*}
	(0.014)	(0.014)	(0.014)	(0.015)
$P(C=3)$	0.017	0.011	0.017	0.007
	(0.012)	(0.013)	(0.012)	(0.013)
(c) $(C, \Delta s^*)$				
$P(C=1, \Delta s^* < 0)$	-0.034^{**}	−0.024	-0.039^{**}	-0.029^{*}
	(0.012)	(0.013)	(0.012)	(0.013)
$P(C=1, \Delta s^* = 0)$	-0.050^{**}	-0.045^{**}	-0.052^{**}	-0.046^{**}
	(0.016)	(0.016)	(0.016)	(0.017)
$P(C=1, \Delta s^* > 0)$	0.008	0.002	0.014	0.009
	(0.013)	(0.014)	(0.013)	(0.014)
$P(C=2, \Delta s^* < 0)$	0.014^{*}	0.016^{*}	0.014^{*}	0.016^{*}
	(0.007)	(0.007)	(0.007)	(0.008)
$P(C=2, \Delta s^* = 0)$	0.020^{**}	0.015	0.022^{**}	0.017^{*}
	(0.007)	(0.008)	(0.008)	(0.009)
$P(C=2, \Delta s^* > 0)$	0.022^{***}	0.023^{***}	0.022^{***}	0.025^{***}
	(0.005)	(0.005)	(0.005)	(0.006)
$P(C=3)$	0.020	0.013	0.019	0.009
	(0.012)	(0.013)	(0.012)	(0.013)
Nobs	22310	16059	19697	13446

Notes: *** significant 0.1%, ** significant 1%, * significant 5%. Robust standard errors are reported in parenthesis. See Table 3.

Table SM15: JSA ATE on use of PEC with alternative control years. A small number of individuals on New Deal in 1998 have been dropped.

	Treatment = 96			
	94-95-97-98	95-97-98	94-95-97	95-97
(d) M_{PEC}				
$P^\star(M_{PEC}=-1)$	−0.006	−0.003	−0.009	−0.008
	(0.008)	(0.009)	(0.009)	(0.009)
$P^\star(M_{PEC}=1)$	0.012	0.010	0.013	0.011
	(0.006)	(0.007)	(0.007)	(0.007)
$P^\star(M_{PEC}=0)$	−0.024	−0.018	−0.021	−0.012
	(0.014)	(0.015)	(0.015)	(0.016)
$P(C=3)$	0.018	0.012	0.017	0.008
	(0.012)	(0.013)	(0.012)	(0.013)
(e) (M_{PEC}, C)				
$P(C=1, M_{PEC}=-1)$	−0.010	−0.007	−0.013	−0.011
	(0.007)	(0.007)	(0.007)	(0.007)
$P(C=1, M_{PEC}=1)$	0.003	0.000	0.005	0.002
	(0.006)	(0.006)	(0.006)	(0.007)
$P(C=1, M_{PEC}=0)$	−0.069***	−0.061***	−0.069***	−0.058***
	(0.016)	(0.017)	(0.016)	(0.017)
$P(C=2, M_{PEC}=-1)$	0.004	0.004	0.004	0.004
	(0.006)	(0.006)	(0.006)	(0.006)
$P(C=2, M_{PEC}=1)$	0.008**	0.009**	0.008**	0.010**
	(0.003)	(0.003)	(0.003)	(0.004)
$P(C=2, M_{PEC}=0)$	0.044***	0.039***	0.047***	0.043***
	(0.009)	(0.010)	(0.009)	(0.011)
$P(C=3)$	0.021	0.014	0.019	0.009
	(0.012)	(0.013)	(0.012)	(0.013)
Nobs	22221	16003	19617	13399

Notes: *** significant 0.1%, ** significant 1%, * significant 5%. Robust standard errors are reported in parenthesis. See Table 4

SM11. JSA ATE on the use of PEC as main method

This Section reports complementary evidence with respect to estimates in Table 4, considering a more restrictive definition of use of PEC; namely whether PEC is used as main method of search. In this case an inflow (respectively outflow from) to PEC corresponds to a transition from non-use or from use as non-main method (respectively use as main method) in wave 1 to use as main method (respectively non-use or use as non-main method) in wave 2. Table SM16 reports estimates of the JSA ATEs based on this definition, showing very similar results to the ones reported in Table 4.

Table SM17 and Table SM18 report tests for anticipatory and placebo effects respectively. Following the discussion in Section SM7, there is no evidence of JSA anticipatory effects in Table SM17. Placebo tests in Table SM18 show a limited set of significant coefficients. Beyond $P(C = 3)$ and counterpart effects already discussed in Section SM8, two significant effects in Table SM18 require discussion. First, a negative effect for $P(C = 1, M_{PEC} = 1)$ in 1994 indicates that in this year PEC inflows of stayers have been unusually low between $q3$ and $q4$ relatively to between $q2$ and $q3$. Results in Table SM16 show that the estimated JSA effect on this outcome is not significant for any of the alternative control groups, not even when 1994 is omitted, suggesting that this result appears robust to the anomalous flows in 1994.

Second, a negative coefficient of -1.1% is found for $P(C = 2, M_{PEC} = 1)$ in 1995, indicating that relative PEC inflows for leavers were unusually low in this year. The corresponding JSA ATE reported in Table SM16 is positive and significant in the range of 0.8–1.1%. Since the two effects are similar in magnitude and all control groups in Table SM16 include 1995, it is possible that the estimated positive ATE is driven by the anomalous fluctuation in 1995 and hence it does not capture a policy effect. In fact, by re-estimating the models without the 1995 cohort, it turns out that the ATE shrinks and loses significance with all comparison groups. Therefore, the most robust available evidence seems to indicate that the JSA impact on $P(C = 2, M_{PEC} = 1)$ was null when this specific definition of PEC use is considered. Remark that this seasonal effect is not observed for PEC transitions based on the more general definition (see Table SM11), therefore the JSA impact on $P(C = 2, M_{PEC} = 1)$ is robust in this case.

Table SM16: JSA ATE on use of PEC (as main method) with alternative control years.

	Treatment = 96			
	94-95-97-98	95-97-98	94-95-97	95-97
(d) M_{PEC}				
$P^{\star}(M_{PEC} = -1)$	0.005	0.003	0.004	0.001
	(0.009)	(0.009)	(0.009)	(0.010)
$P^{\star}(M_{PEC} = 1)$	0.004	−0.001	0.006	0.001
	(0.009)	(0.009)	(0.009)	(0.009)
$P^{\star}(M_{PEC} = 0)$	−0.027	−0.013	−0.027	−0.010
	(0.015)	(0.016)	(0.015)	(0.017)
$P(C = 3)$	0.017	0.011	0.017	0.008
	(0.012)	(0.013)	(0.012)	(0.013)
(e) (M_{PEC}, C)				
$P(C = 1, M_{PEC} = -1)$	0.005	0.004	0.003	0.001
	(0.008)	(0.008)	(0.008)	(0.009)
$P(C = 1, M_{PEC} = 1)$	−0.005	−0.011	−0.003	−0.009
	(0.008)	(0.009)	(0.008)	(0.009)
$P(C = 1, M_{PEC} = 0)$	−0.075***	−0.060***	−0.077***	−0.057**
	(0.016)	(0.017)	(0.016)	(0.018)
$P(C = 2, M_{PEC} = -1)$	0.001	−0.000	0.002	0.001
	(0.004)	(0.005)	(0.004)	(0.005)
$P(C = 2, M_{PEC} = 1)$	0.008**	0.010**	0.008**	0.011**
	(0.003)	(0.003)	(0.003)	(0.004)
$P(C = 2, M_{PEC} = 0)$	0.046***	0.043***	0.048***	0.045***
	(0.009)	(0.010)	(0.010)	(0.011)
$P(C = 3)$	0.020	0.013	0.019	0.009
	(0.012)	(0.013)	(0.012)	(0.013)
Nobs	22310	16092	19617	13399

Notes: *** significant 0.1%, ** significant 1%, * significant 5%. Robust standard errors are reported in parenthesis. $P^{\star}(\cdot) := P(\cdot|C \in \{1, 2\})$. The outcome variables (d) and (e) make use of the variables C and M_{PEC}. C identifies transitions from claimant status in wave 1 as follows: $C = 1$ if still non-employed and claimant in wave 2, $C = 2$ if still non-employed but no longer claimant, $C = 3$ if employed. M_{PEC} identifies transitions involving the search method PEC as main method for claimants in wave 1, such as abandoning PEC ($M_{PEC} = -1$) as main method in wave 2, adopting PEC ($M_{PEC} = 1$) as main method in wave 2, or consistently using or not PEC as main method in both waves ($M_{PEC} = 0$). Reported ATEs are computed from multinomial logit models with Diff-in-diff specification and correspond to changes in the probability of performing the specified transition. The following control variables are used in the multinomial logit specification: gender, number of kids (< 19), age, age^2, education, duration since last job, marriage status, region. See Table 2 for descriptive statistics.

Table SM17: JSA ATE on use of PEC (as main method) with alternative control years. Tests for anticipatory effects. The treatment group comprises flows from q2 to q3, and the control group flows from q1 to q2.

	Treatment = 96			
	94-95-97-98	95-97-98	94-95-97	95-97
(d) M_{PEC}				
$P^{\star}(M_{PEC} = -1)$	−0.001	−0.001	−0.000	0.001
	(0.009)	(0.009)	(0.009)	(0.009)
$P^{\star}(M_{PEC} = 1)$	−0.013	−0.011	−0.014	−0.013
	(0.008)	(0.009)	(0.008)	(0.009)
$P^{\star}(M_{PEC} = 0)$	0.025	0.017	0.029*	0.023
	(0.015)	(0.016)	(0.015)	(0.016)
$P(C = 3)$	−0.011	−0.006	−0.015	−0.012
	(0.012)	(0.013)	(0.012)	(0.013)
(e) (M_{PEC}, C)				
$P(C = 1, M_{PEC} = -1)$	−0.004	−0.003	−0.001	0.001
	(0.008)	(0.008)	(0.008)	(0.009)
$P(C = 1, M_{PEC} = 1)$	−0.011	−0.007	−0.012	−0.008
	(0.008)	(0.008)	(0.008)	(0.008)
$P(C = 1, M_{PEC} = 0)$	0.022	0.004	0.041**	0.032
	(0.016)	(0.017)	(0.016)	(0.017)
$P(C = 2, M_{PEC} = -1)$	0.003	0.002	0.001	−0.000
	(0.004)	(0.004)	(0.004)	(0.004)
$P(C = 2, M_{PEC} = 1)$	−0.002	−0.004	−0.003	−0.005
	(0.003)	(0.003)	(0.003)	(0.003)
$P(C = 2, M_{PEC} = 0)$	0.003	0.014	−0.011	−0.008
	(0.009)	(0.010)	(0.009)	(0.010)
$P(C = 3)$	−0.011	−0.006	−0.015	−0.011
	(0.012)	(0.013)	(0.012)	(0.013)
Nobs	23467	16930	20727	14190

Notes: *** significant 0.1%, ** significant 1%, * significant 5%. Robust standard errors are reported in parenthesis. See Table SM16.

Table SM18: JSA ATE on use of PEC (as main method) with alternative control years. Placebo tests.

Treatment =	94	95	97	98
	95-97-98	94-97-98	94-95-98	94-95-97
(d) M_{PEC}				
$P^{\star}(M_{PEC} = -1)$	−0.005	0.001	0.011	−0.005
	(0.009)	(0.009)	(0.011)	(0.011)
$P^{\star}(M_{PEC} = 1)$	−0.013	−0.000	0.010	0.012
	(0.008)	(0.008)	(0.010)	(0.010)
$P^{\star}(M_{PEC} = 0)$	0.038**	−0.024	−0.019	−0.005
	(0.015)	(0.015)	(0.018)	(0.019)
$P(C = 3)$	−0.020	0.024*	−0.001	−0.002
	(0.012)	(0.012)	(0.014)	(0.015)
(e) (M_{PEC}, C)				
$P(C = 1, M_{PEC} = -1)$	0.001	0.002	0.007	−0.012
	(0.008)	(0.008)	(0.010)	(0.010)
$P(C = 1, M_{PEC} = 1)$	−0.017*	0.008	0.004	0.011
	(0.008)	(0.008)	(0.010)	(0.010)
$P(C = 1, M_{PEC} = 0)$	0.045**	−0.027	−0.023	−0.006
	(0.015)	(0.016)	(0.019)	(0.021)
$P(C = 2, M_{PEC} = -1)$	−0.005	−0.002	0.003	0.006
	(0.004)	(0.004)	(0.005)	(0.005)
$P(C = 2, M_{PEC} = 1)$	0.004	−0.010**	0.005	0.001
	(0.003)	(0.003)	(0.003)	(0.003)
$P(C = 2, M_{PEC} = 0)$	−0.007	0.004	0.005	0.001
	(0.009)	(0.010)	(0.012)	(0.013)
$P(C = 3)$	−0.021	0.024*	−0.000	−0.001
	(0.012)	(0.012)	(0.014)	(0.015)
Nobs	17598	17598	17598	17598

Notes: *** significant 0.1%, ** significant 1%, * significant 5%. Robust standard errors are reported in parenthesis. See Table SM16 for estimates with the actual treatment group.

SM12. Attrition

Table SM19 reports attrition rates and average characteristics of attriters and non-attriters for 1996 and alternative control years. All years include treated and untreated claimants together ($q3$ and $q2$). The Table shows that 29.1% of claimants in 1996$q3$ and 1996$q2$ are not observed in the subsequent quarters. $\sim 20\%$ of this group are individuals at the last interview, while the remaining share reflects non-responses. Non-responses comprise individuals not present at the previous wave address because of move or temporary absence. Attrition may introduce bias if differences between attriters and non-attriters are systematically related to the treatment status. This issue is investigated in Table SM20. The Table reports in the first column the Diff-in-diff in variable means calculated between attriters and non-attriters and between treated and untreated unemployed claimants for 1996. This statistic is not significant for any of the variables hence showing no evidence of attrition bias for 1996. The following columns report the Diff-in-diff-in-diff statistics computed comparing the Diff-in-diff between 1996 and alternative control years. The statistics are never significantly different from zero for any of the control group and for any of the variables. This evidence strongly suggests that differences between attriters and non-attriters are not systematically related to the treatment status.

Table SM19: Attrition statistics by treatment and control years. All unemployed claimants in q2 and q3.

	96		94-95-97-98		95-97-98		94-95-97		95-97	
Attrition	0.291		0.293		0.295		0.295		0.298	
	(0.454)		(0.455)		(0.456)		(0.456)		(0.457)	
	yes	no	yes	no	yes	no	yes	no	yes	no
Female	0.276	0.262	0.270	0.261	0.262	0.253	0.277	0.265	0.273	0.258
	(0.447)	(0.440)	(0.444)	(0.439)	(0.440)	(0.434)	(0.448)	(0.442)	(0.445)	(0.438)
Age	33.664	35.391	33.569	35.611	33.696	35.794	33.287	35.499	33.260	35.663
	(12.357)	(12.375)	(12.562)	(12.722)	(12.588)	(12.780)	(12.364)	(12.621)	(12.266)	(12.629)
Married	0.285	0.314	0.282	0.333	0.267	0.319	0.287	0.343	0.271	0.331
	(0.452)	(0.464)	(0.450)	(0.471)	(0.443)	(0.466)	(0.453)	(0.475)	(0.445)	(0.471)
Nr. kids 0—18 yrs	0.682	0.695	0.654	0.697	0.641	0.682	0.660	0.714	0.647	0.707
	(1.156)	(1.131)	(1.127)	(1.149)	(1.110)	(1.150)	(1.128)	(1.154)	(1.107)	(1.160)
s^*	4.320	4.355	4.328	4.319	4.436	4.447	4.306	4.299	4.431	4.453
	(2.475)	(2.458)	(2.483)	(2.424)	(2.498)	(2.416)	(2.486)	(2.429)	(2.508)	(2.422)
$meth_{PEC}$	0.766	0.776	0.773	0.786	0.790	0.802	0.768	0.780	0.785	0.797
	(0.423)	(0.417)	(0.419)	(0.410)	(0.407)	(0.398)	(0.422)	(0.414)	(0.411)	(0.402)
Degree or equiv.	0.091	0.085	0.082	0.074	0.085	0.078	0.082	0.072	0.088	0.076
	(0.288)	(0.279)	(0.274)	(0.262)	(0.280)	(0.268)	(0.275)	(0.259)	(0.283)	(0.265)
Higher education	0.043	0.047	0.042	0.042	0.046	0.044	0.041	0.042	0.046	0.045
	(0.204)	(0.213)	(0.200)	(0.201)	(0.209)	(0.205)	(0.199)	(0.201)	(0.209)	(0.206)
GCE, A or eq.	0.194	0.203	0.200	0.205	0.193	0.195	0.202	0.210	0.194	0.201
	(0.395)	(0.402)	(0.400)	(0.403)	(0.395)	(0.396)	(0.402)	(0.407)	(0.396)	(0.401)
GCSE A*–C or eq.	0.211	0.218	0.201	0.190	0.204	0.194	0.198	0.185	0.199	0.188
	(0.408)	(0.413)	(0.401)	(0.392)	(0.403)	(0.396)	(0.399)	(0.388)	(0.399)	(0.391)
Other qualifications	0.183	0.175	0.192	0.196	0.190	0.200	0.194	0.196	0.192	0.202

	(0.387)	(0.380)	(0.394)	(0.397)	(0.392)	(0.400)	(0.395)	(0.397)	(0.394)	(0.402)
No qualifications	0.279	0.272	0.283	0.294	0.282	0.289	0.282	0.295	0.281	0.288
	(0.448)	(0.445)	(0.450)	(0.456)	(0.450)	(0.453)	(0.450)	(0.456)	(0.449)	(0.453)
Last job < 3 mts	0.101	0.110	0.101	0.114	0.105	0.119	0.100	0.112	0.104	0.117
	(0.302)	(0.312)	(0.301)	(0.318)	(0.307)	(0.324)	(0.300)	(0.316)	(0.306)	(0.321)
Last job 3—6 mts	0.118	0.120	0.118	0.118	0.122	0.124	0.115	0.117	0.120	0.123
	(0.322)	(0.325)	(0.322)	(0.323)	(0.328)	(0.329)	(0.319)	(0.321)	(0.324)	(0.329)
Last job 6—12 mts	0.180	0.157	0.154	0.160	0.159	0.156	0.153	0.159	0.158	0.153
	(0.385)	(0.364)	(0.361)	(0.366)	(0.366)	(0.363)	(0.360)	(0.365)	(0.365)	(0.360)
Last job 1—2 yrs	0.166	0.155	0.175	0.162	0.161	0.149	0.181	0.168	0.168	0.154
	(0.372)	(0.362)	(0.380)	(0.369)	(0.368)	(0.356)	(0.385)	(0.374)	(0.374)	(0.361)
Last job 2—3 yrs	0.077	0.088	0.098	0.097	0.086	0.088	0.103	0.103	0.091	0.094
	(0.267)	(0.283)	(0.297)	(0.296)	(0.281)	(0.283)	(0.304)	(0.303)	(0.287)	(0.292)
Last job 3—4 yrs	0.076	0.069	0.072	0.065	0.067	0.058	0.074	0.068	0.070	0.061
	(0.265)	(0.253)	(0.258)	(0.247)	(0.250)	(0.235)	(0.262)	(0.252)	(0.255)	(0.240)
Last job 4—5 yrs	0.045	0.058	0.044	0.048	0.045	0.051	0.047	0.050	0.049	0.055
	(0.207)	(0.234)	(0.205)	(0.214)	(0.207)	(0.220)	(0.211)	(0.217)	(0.217)	(0.228)
Last job > 5 yrs	0.143	0.160	0.137	0.153	0.152	0.171	0.128	0.143	0.140	0.159
	(0.351)	(0.367)	(0.344)	(0.360)	(0.359)	(0.376)	(0.334)	(0.350)	(0.347)	(0.366)
No job before	0.093	0.083	0.102	0.082	0.102	0.085	0.101	0.081	0.100	0.084
	(0.291)	(0.276)	(0.303)	(0.274)	(0.303)	(0.279)	(0.301)	(0.272)	(0.300)	(0.277)
North East	0.052	0.055	0.060	0.062	0.061	0.062	0.061	0.061	0.062	0.060
	(0.221)	(0.227)	(0.238)	(0.242)	(0.240)	(0.241)	(0.239)	(0.240)	(0.242)	(0.237)
North West	0.115	0.115	0.122	0.128	0.123	0.129	0.119	0.125	0.118	0.123
	(0.319)	(0.319)	(0.327)	(0.335)	(0.328)	(0.335)	(0.323)	(0.331)	(0.323)	(0.329)
York. & Humber.	0.091	0.086	0.095	0.092	0.098	0.095	0.091	0.090	0.092	0.092
	(0.287)	(0.280)	(0.294)	(0.290)	(0.298)	(0.293)	(0.288)	(0.286)	(0.290)	(0.288)
East Midlands	0.073	0.067	0.069	0.063	0.066	0.061	0.070	0.063	0.067	0.061
	(0.260)	(0.250)	(0.253)	(0.243)	(0.248)	(0.239)	(0.255)	(0.244)	(0.250)	(0.240)
West Midlands	0.111	0.100	0.097	0.096	0.096	0.095	0.096	0.095	0.096	0.093
	(0.314)	(0.300)	(0.295)	(0.294)	(0.295)	(0.293)	(0.295)	(0.293)	(0.295)	(0.291)
Eastern	0.069	0.079	0.076	0.076	0.076	0.077	0.079	0.079	0.080	0.082
	(0.253)	(0.270)	(0.266)	(0.265)	(0.265)	(0.266)	(0.269)	(0.270)	(0.271)	(0.274)
London	0.178	0.180	0.176	0.162	0.174	0.164	0.178	0.164	0.177	0.168
	(0.383)	(0.385)	(0.381)	(0.368)	(0.379)	(0.370)	(0.382)	(0.370)	(0.382)	(0.374)
South East	0.092	0.096	0.094	0.091	0.084	0.088	0.097	0.094	0.087	0.092
	(0.289)	(0.295)	(0.292)	(0.287)	(0.278)	(0.283)	(0.297)	(0.291)	(0.282)	(0.289)
South West	0.075	0.069	0.067	0.073	0.070	0.069	0.069	0.075	0.074	0.073
	(0.264)	(0.254)	(0.250)	(0.259)	(0.256)	(0.254)	(0.253)	(0.263)	(0.263)	(0.259)
Wales	0.048	0.053	0.049	0.052	0.052	0.054	0.047	0.050	0.051	0.050
	(0.214)	(0.225)	(0.215)	(0.222)	(0.223)	(0.225)	(0.212)	(0.217)	(0.220)	(0.218)
Scotland	0.097	0.099	0.096	0.105	0.098	0.107	0.094	0.104	0.095	0.107
	(0.296)	(0.299)	(0.295)	(0.307)	(0.297)	(0.310)	(0.291)	(0.306)	(0.293)	(0.309)
Nobs	2024	4970	7553	18393	4913	11865	6472	15638	3832	9110

Table SM20: Attrition statistics. Difference-in-differences (DiD) in variable means between attriters and non-attriters and between treated and untreated unemployed claimants are reported for 1996. Differences in DiD between 1996 and control years are reported in following columns (DiDiD). *** significant 0.1%, ** significant 1%, * significant 5%. Standard errors in parenthesis.

	96		94-95-97-98		95-97-98		94-95-97		95-97	
	DiD	se	DiDiD	se	DiDiD	se	DiDiD	se	DiDiD	se
s^*	-0.064	(0.111)	0.042	(0.148)	0.138	(0.156)	-0.01	(0.15)	0.078	(0.163)
$meth_{PEC}$	-0.001	(0.019)	0.016	(0.025)	0.019	(0.026)	0.005	(0.026)	0.002	(0.027)
Control variables										
Female	0.023	(0.02)	0.006	(0.026)	0.001	(0.028)	0.006	(0.027)	-0.001	(0.029)
Age	-0.449	(0.551)	-0.106	(0.74)	0.087	(0.783)	0.164	(0.751)	0.603	(0.81)
Married	-0.024	(0.02)	-0.006	(0.027)	-0.008	(0.028)	-0.003	(0.028)	-0.003	(0.03)
Nr. kids 0—18 y	-0.018	(0.052)	0.027	(0.069)	0.025	(0.072)	0.014	(0.07)	0.003	(0.075)
Degree or equiv.	0.028	(0.013)	-0.012	(0.017)	-0.01	(0.018)	-0.015	(0.018)	-0.014	(0.019)
Higher education	0.010	(0.01)	0.005	(0.012)	-0.002	(0.013)	0.009	(0.012)	0.001	(0.014)
GCE, A or eq.	-0.014	(0.018)	0.001	(0.024)	0.01	(0.025)	-0.003	(0.024)	0.004	(0.026)
GCSE A*–C or eq.	-0.012	(0.018)	-0.004	(0.024)	-0.009	(0.026)	-0.007	(0.025)	-0.015	(0.027)
Other qual.	0.012	(0.017)	0.014	(0.023)	0.026	(0.025)	0.015	(0.024)	0.029	(0.026)
No qual.	-0.027	(0.02)	-0.005	(0.027)	-0.014	(0.028)	0.002	(0.027)	-0.005	(0.029)
Last job < 3 m	0.012	(0.014)	0.027	(0.018)	0.019	(0.019)	0.022	(0.019)	0.01	(0.02)
Last job 3—6 m	0.001	(0.014)	0.011	(0.019)	0.005	(0.021)	0.015	(0.02)	0.011	(0.021)
Last job 6—12 m	-0.025	(0.017)	-0.018	(0.022)	-0.004	(0.024)	-0.025	(0.023)	-0.011	(0.025)
Last job 1—2 y	-0.003	(0.017)	0.023	(0.022)	0.017	(0.023)	0.032	(0.023)	0.03	(0.024)
Last job 2—3 y	0.012	(0.012)	0.000	(0.017)	0.001	(0.017)	-0.002	(0.017)	-0.001	(0.018)
Last job 3—4 y	-0.020	(0.012)	-0.024	(0.016)	-0.009	(0.016)	-0.027	(0.016)	-0.009	(0.017)
Last job 4—5 y	-0.003	(0.009)	-0.011	(0.013)	-0.013	(0.014)	-0.011	(0.013)	-0.013	(0.014)
Last job > 5 y	0.003	(0.016)	-0.003	(0.021)	-0.017	(0.023)	0.005	(0.021)	-0.007	(0.023)
No job before	0.023	(0.013)	-0.004	(0.018)	0.000	(0.019)	-0.01	(0.018)	-0.009	(0.019)
North East	-0.012	(0.01)	-0.001	(0.013)	0.001	(0.014)	-0.001	(0.013)	0.002	(0.015)
North West	-0.008	(0.014)	-0.006	(0.019)	-0.004	(0.02)	-0.004	(0.019)	0.001	(0.021)
York. & Humber.	0.010	(0.013)	0.016	(0.017)	0.023	(0.018)	0.012	(0.017)	0.017	(0.019)
East Midlands	-0.006	(0.012)	-0.006	(0.015)	-0.008	(0.016)	-0.008	(0.016)	-0.012	(0.017)
West Midlands	0.007	(0.014)	0.017	(0.018)	0.012	(0.019)	0.021	(0.018)	0.017	(0.02)
Eastern	-0.001	(0.011)	-0.012	(0.015)	-0.01	(0.016)	-0.013	(0.016)	-0.012	(0.017)
London	-0.008	(0.018)	-0.005	(0.024)	-0.015	(0.025)	0.004	(0.024)	-0.003	(0.026)
South East	0.009	(0.013)	0.008	(0.017)	0.012	(0.018)	0.001	(0.018)	0.000	(0.019)
South West	0.014	(0.012)	0.001	(0.016)	-0.005	(0.017)	-0.002	(0.016)	-0.011	(0.017)
Wales	-0.004	(0.01)	0.004	(0.013)	0.007	(0.014)	0.006	(0.013)	0.01	(0.014)
Scotland	-0.002	(0.013)	-0.018	(0.018)	-0.012	(0.019)	-0.017	(0.018)	-0.01	(0.019)
Nobs	6994		25946		16778		22110		12942	