Does a minimum job search requirement for unemployment payment recipients reduce their time on payments? Evidence from the Job Seeker Diary in Australia

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June 2004

Abstract

This study examines the impact of the Jobseeker Diary (JSD), a large-scale program designed to increase job search effort of unemployed persons in Australia. The scale and its focus on work-search verification make the JSD program relatively unique in the international context. A quasi-experimental matching method is applied to examine the effect of JSD participation. Justification for use of the matching method is the existence of a 'natural experiment' – an industrial relations dispute amongst case workers implementing the program - that is argued introduced a significant source of randomness into assignment to the JSD. Participation in the JSD is found to significantly increase the likelihood of an unemployment payment recipient exiting payments, and to reduce total time spent on payments. About one-half of JSD participants are estimated to have reduced time on payments. Largest effects of the JSD occur for payment recipients for whom labour demand conditions are the most 'favourable'.

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1. Introduction

This study examines a large-scale intervention intended to increase job search effort of unemployed persons in Australia. The intervention – the Jobseeker Diary (JSD) – is a work search verification program that requires unemployment payment recipients to complete a fortnightly diary in which details of a specified minimum number of job applications must be recorded. The scale, and its focus on work-search verification, make the JSD program relatively unique in the international context.

With the rise of mass unemployment in industrialized economies, governments have devoted increasing attention to the design and implementation of policies to improve labour market outcomes for unemployed job seekers. One important type of policy can be categorized as job search intervention – that is, programs that seek to raise the intensity and/or effectiveness of job search. The main types of job search programs that have been implemented are work search verification ,and job search assistance.

Existing empirical evidence on the impact of job search programs is primarily from a range of random experiment studies that have been undertaken in the United States and Europe. For the United States, Ashenfelter et al. (1999) analyse a four-state random experiment on effects of stricter enforcement and verification of work-search; and Klepinger et al. (2002) examine a Maryland experiment to test the effect of alternative job search programs. Studies by Meyer (1995) and Bloom and Michalopoulos (2001) review other experimental evidence for the United States. In the United Kingdom, a random experiment study has been undertaken to examine the impact of the Restart program that required unemployment payment recipients with spell durations of at least six months to attend a mandatory interview with a counselor (Dolton and O'Neill, 1996, 2002); and the Gateway phase of the New Deal – where an unemployed person meets regularly with a personal advisor and is given intensive job search assistance – has been evaluated using quasi-experimental methods (Blundell et al., 2002, and Finn, 2002). Random experiment studies of effects of increased counseling and monitoring of unemployed job seekers have been undertaken for the Netherlands (Gorter and Kalb, 1996, and Van den Berg and van der Klaauw, 2001), and one Australian study has examined effects of a random experiment to provide extra counseling to very long-term unemployed with payment spells of more than five years duration (Breunig et al., 2003).

Several main conclusions emerge from the existing literature:

• Participation in job search programs appears to improve labour market outcomes for unemployed persons;

• The scale of job search program, and timing of intervention, matter. (The impact is more positive where the intervention has higher intensity – for example, a larger amount of contact time between the unemployed person and a case worker; and where intervention occurs at an earlier stage of an unemployment spell);

• There is mixed evidence on the relative efficacy of job search programs that include both work search verification and job search training, compared to where only work search verification occurs;

• Job search programs seem to improve labour market outcomes primarily by increasing intensity of job search by unemployed persons; and

• Job search programs are most effective where they do not distort the 'type' of job search activities able to be undertaken – for example, between formal and informal search methods.

This study adds to the body of knowledge on job search programs in several ways. First, most previous studies (except for the United Kingdom programs) have been of experiments that have targeted only a small subset of the unemployed population. Hence it adds considerably to understanding about the effects of large-scale job search interventions. Second, the precise nature of the intervention – exclusively work search verification – means that the study can provide a more exact perspective on the impact of this specific type of job search program than most previous studies which have examined experiments that confound both work search verification and job search assistance. (Only the recent United States studies by Ashenfelter et al., 1999, and Klepinger et al., 2002, seek to address the question of the independent effect of work search verification.) Third, the range of countries where job search programs have been studied is still fairly narrow. And given that a major theme of reviews of the impact of labour market programs is the heterogeneity of program impacts (for example, Heckman et al., 1999, p.2053), this study therefore provides a valuable opportunity to assess the impact of job search programs through the perspective of an alternative labour market in Australia. Such research can contribute to an

understanding of how specific details and environmental factors determine whether a program improves labour market outcomes for unemployed persons.

A quasi-experimental matching method is used to assess the effect of the JSD requirement on durations of unemployment payment spells. The specific policy effect estimated is the average effect for the treatment group of commencing participation in JSD in the first fortnight of a payment spell compared to not commencing participation in JSD in the first fortnight of a payment spell. The nature of the JSD – whereby most participants commence JSD participation in the first fortnight of a payment spell, but commencements occur at different points in any time period – makes this the most appropriate approach for estimating the policy effect (see for example, Sianesi, 2004).

The critical methodological issue is to justify the validity of the matching estimator. The existence of a 'natural experiment' in assignment of unemployed persons to the JSD program provides the main rationale for use of the matching method. An industrial relations dispute at the government agency responsible for implementing the JSD is argued to have introduced a significant source of randomness in assignment into the program. A second justification is that the data source allows for a relatively rich set of covariates to be used for matching, and the sample of JSD participants and non-participants used in the study to be restricted to the population eligible for participation.

Participation in the JSD program is found to increase the rate of exit from unemployment payments, and to reduce total time subsequently spent in receipt of unemployment payments. The effects are quite large, and statistically significant. The findings suggest that the effect of JSD participation on exit from payments for the treatment group occurs entirely during the period the first 3 months after commencement of a payment spell – exactly the period where the JSD participation requirement exists. The finding that JSD participation affects the rate of outflow from unemployment payments and total time on payments is supported by qualitative evidence that JSD participants believe their job search levels would decline without the JSD; and that JSD participants self-reported a significantly higher number of job applications than non-participants. The JSD effect on time on payments that is estimated in this study is shown to be very similar to the estimated effect in the Maryland experiment (Klepinger et al., 2002); and thereby provides

corroborating evidence that a job search program that involves only work search verification can significantly improve job search outcomes.

Section 2 describes the details of the Jobseeker Diary intervention. Section 3 provides information on the data source, and descriptive statistics on the sample of unemployed payment recipients, used in the study. Section 4 presents a detailed description of the quasi-experimental matching methodology. Results are presented in Section 5. Concluding remarks are in section 6.

2. The Job Seeker Diary

Government income support payments available to unemployed persons in Australia are Newstart Allowance (NSA) (persons aged 21 and over), and Youth Allowance (YA(o)) (persons aged 16 to 20 years). Social Security legislation in Australia requires that (unless exempted) unemployment payment recipients must meet an 'activity test' – to be actively looking for work, or undertaking activities to improve their employment prospects, and be willing to accept offers of suitable employment (Social Security Act 1991, Section 601). There is no time limit on the duration for which unemployment payments can be claimed in Australia.

The JSD constitutes one component of existing activity test arrangements. It was introduced in July 1996. The JSD is a booklet where an unemployment payment recipient must list details of job applications for each fortnight over a three months period. Information required on each job search episode includes: employer name, address and telephone; job description; and the job search method used to find the vacancy. The objectives of the JSD are to encourage more active job search, and to give payment recipients a record of their job search (Centrelink, 1996). The JSD is administered by Centrelink, a Commonwealth government agency with responsibility for service delivery to unemployment payment recipients (as well as other social security payment recipients).

The JSD is issued to all new unemployment payment recipients with job search as their main activity type who receive fortnightly payments, or in other circumstances such as at a Review Interview where a judgment is made that a payment recipient has made 'marginal work efforts'. There are a variety of possible reasons why a payment recipient could be

exempted from the JSD – Discretionary; Exempt from activity test for more than 10 weeks; In case management; On variable reporting; Have significant disability problems; Have literacy problems; Have psychiatric or substance abuse problems; or Have not worked in previous 12 months (Centrelink, 1996).

Payment recipients with a JSD are instructed on the minimum number of jobs per fortnight for which they must apply. It is intended that this number should equal a benchmark set for the region in which a payment recipient resides that is determined on the basis of local labour market conditions; but that Centrelink staff can also vary the number downwards to take account of personal characteristics of a payment recipient (Centrelink, 1996). At the time of introduction of the JSD in 1996, the maximum number of job applications required was 8 jobs per fortnight; this was subsequently increased to 10 jobs per fortnight. NSA/YA(o) recipients eligible for JSD participation, but who do not participate, would have to comply with the regular activity test that involves a requirement to undertake job search and to nominate two job search contacts made each fortnight.

A payment recipient must return the JSD either when requested, or at a review meeting with a case worker that can occur at either the 12 week or 9 month point in payment spell duration. Failure to return the JSD can result in imposition of an administrative breach penalty. Lodgement of a JSD that shows unsatisfactory work efforts can be the basis for imposition of an activity test breach.[1]

3. Data and sample characteristics

a. The database

The database for this study is the Department of Family and Community Services Longitudinal Administrative Data Set (LDS). More specifically, the LDS Unemployment Payment File, a 10 per cent random sample of unemployment payment recipients for the period from January 1995 to June 2000, is used. The LDS is created from administrative records of social security payment receipt in Australia. It includes information on the date on which any social security payment was made; type and amount of payment; assets, income, and demographic characteristics of payment recipients (for example, date of birth,

country of birth, and family characteristics) (Commonwealth Department of Family and Community Services, 2002). Payments are made at fortnightly intervals, and hence that is the periodicity of the database.

The LDS has advantages and disadvantages for evaluating the impact of activity test arrangements. Heckman et al. (1998) suggest that the quality of any quasi-experimental evaluation study using a matching method is likely to be significantly affected by three key features – whether data for treatment and control groups is collected using the same survey instrument; whether it is possible to control at a detailed level for local labour market conditions; and whether it is possible to match treatment and control observations using labour market history.[2] On each of these criteria the LDS performs well. First, data on JSD participants (treatment group) and JSD non-participants (control group) can be drawn from the same database. Second, data on the region of residence is available in the LDS at a highly disaggregated (postcode) level. Third, the LDS allows variables to be constructed that provide a detailed representation of unemployment payment history.

The main disadvantage of the LDS is that it does not provide information on payment recipients for time periods where they are not receiving social security payments. This has the important implication that, for unemployment payment recipients observed to exit payments, it is not possible to determine labour market status or income. Therefore, analysis of effects of activity test arrangements must focus on outcomes that are related to receipt of unemployment payments.

b. Sample choice

The sample in this study is unemployment payment spells (on NSA or YA(o)) that begin between 1 July 1997 and 30 June 1998. This time period is the earliest phase of operation of the JSD for which it is possible to identify JSD participants. (Although the JSD was introduced in July 1996 no administrative data were collected on JSD participation for its first year of operation.) At present the JSD has almost universal application so it would not be possible to use a matching method for those recent periods.[3] JSD participation is identified from a variable 'Number of JSD contacts' in the LDS. NSA/YA(o) payment

recipients are assumed to participate in JSD in any fortnight in which they have a non-zero entry for that variable.

The sample is restricted to payment recipients subject to the activity test and with job search as their 'activity test type' at the start of a payment spell. The activity test and job search restrictions are imposed since these requirements are necessary for an unemployment payment recipient to be eligible for participation in the JSD. That is, by making this restriction, payment recipients who would have been ineligible for the JSD due to being exempt from the activity test; not having worked for 12 months; being on variable reporting; or having disability/literacy problems, will be excluded from the sample. Essentially this should restrict the group of JSD non-participants to payment recipients exempted under the 'discretionary' category.[4] The sample is also restricted to payment recipients aged 18 to 49 years. This is motivated by the concentration of JSD participation amongst younger age groups – In 1997-98 less than 10 per cent of JSDs were assigned to unemployment payment recipients aged 50 years or over (Commonwealth Department of Family and Community Services, 2000).

For the purposes of this study a new spell on NSA or YA(o) is defined to begin if a payment recipient has been off any social security payment for at least four consecutive fortnights where that payment spell duration is less than or equal to 23 fortnights; or off all payments for at least seven consecutive fortnights where that payment spell duration is more than 23 fortnights. This definition is adopted to be consistent with the FaCS definition of a new payment spell. In fact, our definition involves a longer break in payments than the FaCS definition, as data limitations mean that it is necessary to have a longer break in payments, to ensure that our sample is restricted to spells that would be classified as new spells under the FaCS definition.[5]

c. Descriptive information

Descriptive information on participation in the JSD is presented in Tables 1 to 4, and in Figure 1.[6] In the sample period 57,779 new NSA/YA(o) payment spells commenced. From these spells 73.4 per cent have at least one fortnight of JSD requirement. For over 95 per cent of those payment recipients, their first spell on JSD begins in the first fortnight of

their payment spell (Table 1). And almost all NSA/YA(o) payment spells involve only a single episode of participation in JSD (Table 2). JSD participants have a similar gender composition and a similar distribution across local labour markets ranked by unemployment rate, but are slightly younger and more likely not to have received unemployment payments in the previous 12 months, compared to all new payment spells in the sample period (Table 3). The modal number of required job contacts during the sample period was eight (Figure 1). Data on the incidence of JSD-related breaches is not available for the sample period 1997/98. However, it is available for the two years immediately after, during which time the incidence of breaches was on average 2-3 per cent of payment spells with JSD participation (Table 4). This suggests a high degree of enforcement of the program.

d. Effect of JSD - Theory

The objective of the JSD is to increase job search intensity of unemployment payment recipients. Search-theoretic labour market models predict that an increase in search intensity will have three main effects. First, it will cause an increase in the rate of outflow from unemployment to employment due to an increase in the rate of matching between unemployed and job vacancies (inward shift of the Beveridge curve). Second, it will raise labour market tightness due to an increase in the rate of creation of new jobs that occurs because the productivity of a new job is positively related to intensity of job search. There may also be a further effect of the JSD. The requirement to undertake extra job search may increase 'disutility' of unemployment. This would lower the reservation wage of an unemployed job-seeker, and hence increase the rate at which job offers are received and thereby the rate at which exit from unemployment will occur. Each of the possible effects of JSD identified will cause an increase in the rate of outflow from unemployment, and a reduction in the equilibrium rate of unemployment (Pissarides, 2000, chapter 5).

4. Methodology

a. Empirical method – Introduction

The empirical approach used to estimate the effect of the JSD is a quasi-experimental matching method. Fundamentally, this involves comparing payment outcomes for a

treatment group of NSA/YA(o) recipients who participate in JSD, and a matched control group of NSA/YA(o) recipients.

Effects of the JSD on a variety of outcome measures are examined. The JSD requirement is for a maximum six fortnights period. Outcome measures have been chosen to attempt to capture short-run (impact) effects of the JSD, and possible long run effects. One measure will be the effect of JSD on the incidence of exit from payments by 3 months and 6 months after JSD commencement. Exit from payments is defined to occur where a NSA/YA(o) payment recipient has three consecutive fortnights off that payment. A payment recipient is defined to be 'on payments' in any fortnight in which they lodge a claim form (SU19), regardless of payment entitlement. A second measure will be the effect of JSD on whether payment recipients are on payments at 6 months and 12 months after JSD commencement. The first and second measures will diverge where payment recipients exit payments, but then begin a new payment spell that is on-going at the specified duration. The third measure applied is the effect of JSD on the number of fortnights on payments during the 6 months and 12 months after JSD commencement.

Participation in the JSD can begin for an individual payment recipient at different payment spell durations; and occurs throughout the sample period for different payment recipients. This potentially complicates the classification of payment spells as treatment or control observations. Our basic approach is to define: (a) Treatment group – NSA/YA(o) recipients who commence JSD participation in first fortnight of a payment spell; and (b) Potential control group - NSA/YA(o) recipients who do not commence JSD participation in first fortnight of a payment spell. As noted above, the sample of unemployment payment recipients is also restricted to those with 'job search' as their activity type; hence the control group will exclude unemployed persons ineligible for or exempted from JSD participation in the first fortnight of their payment spell for reasons associated with labour market disadvantage.

The empirical method has direct consequences for the policy effect that is identified. Estimates of the effect of JSD participation are the <u>average effect of commencing participation in JSD in the first fortnight of a payment spell (for the specified group of NSA/YA(o) recipients aged 18 to 49 years) compared to not commencing participation in JSD in the first fortnight of a payment spell</u>. In other words, the policy effect identified is

the effect of 'treatment on the treated' for payment recipients who commence a JSD spell in the first fortnight of their spell on unemployment payments.

The matching approach follows Sianesi (2004) and may be described formally in the same way. Suppose $D \in \{P,W\}$ is a treatment indicator where P denotes 'commence participation' and W denotes 'not commence participation'. Let Y(i,D) represent an outcome indicator for individual i who has been exposed to treatment D. Denote as τ^f the effect of commencing to 'treat' a payment recipient in the fth fortnight compared to not treating that individual until at least the (f+1)st fortnight. Then:

(1) $\qquad \tau^f = E(Y_{P_f} - Y_{W_f} \mid D^f = 1)$

where P_f and W_f represent respectively commencing participation and not commencing participation in fortnight f, and $D^f = 1$ denotes that D = P and T = f where T represents elapsed payment spell duration.

In this study the main focus is to identify the effect of JSD participation that commences in the first fortnight of a payment spell:[7]

(2) $\qquad \tau^1 = E(Y_{P_1} - Y_{W_1} \mid D^1 = 1)$.

The motivation for selection of the treatment group is that virtually all JSD participants commence in the first fortnight of their payment spell, and have only a single spell on JSD. Moreover, there are not a sufficient number of payment recipients who commence on JSD in any fortnight after the first fortnight to enable implementation of the matching estimator for those JSD participants. The choice of treatment group does mean that there is a small proportion of NSA/YA(o) recipients in the potential control group who subsequently participate in JSD. This is seen as a conservative approach to estimating the impact of the JSD; and sensitivity analysis of the effect of excluding from the control group those payment recipients who subsequently participate in JSD is also undertaken.

Figure 2 provides information on the pattern of participation in the JSD for the treatment and control groups. [8] By definition, in the first fortnight participation by the treatment group is 100 per cent, and by the control group is zero per cent. In subsequent fortnights there is convergence. For the first four fortnights treatment group participation is above 75 per cent and control group participation is below 10 per cent, in the fifth fortnight the respective rates of participation are about 40 per cent and 5 per cent; and from the sixth fortnight onwards treatment group participation ranges from about 5 to 10 per cent while control group participation ranges from 3 to 5 per cent. Hence, what is essentially being studied is the effect of a program that on average involves a large difference in participation by treatment and control groups for between four to five fortnights.

b. Empirical method – Motivation

For the quasi-experimental matching method to be a valid estimator of the JSD treatment effect, it is sufficient that (Rubin, 1979):
(a) Conditional Independence Assumption (CIA) - Conditional on a set of observable variables (X), participation in treatment is unrelated to outcomes in the absence of treatment; and
(b) Common support assumption - For each possible combination of observable variables there is a non-zero probability of non-participation.

Part (a) effectively requires that matching between treatment and control group observations should be conditional on all variables that affect both participation in the JSD and outcomes in the absence of the JSD (Augurzky and Schmidt, 2001). Or, alternatively, after conditioning on the set of X variables, assignment between the treatment and control groups is random. Part (b) is necessary to ensure that, for any treatment group observation, there will be a control group observation with the combination of observable characteristics to which the treatment observation can be matched.

Almost certainly the most important issue in undertaking a matching analysis is to justify why the CIA is likely to hold. In this study we take two approaches to making that justification. The first justification for validity of the CIA is to suggest a likely source of randomness in assignment of unemployment payment recipients between participation and

non-participation in the JSD. (This is of course conditional on already having restricted the sample to payment recipients eligible for JSD participation.) During the initial phase of its operation a critical determinant of assignment of JSD participation was an industrial relations dispute in Centrelink.[9] The dispute caused significant differences in the extent of implementation of the JSD program between Centrelink offices in different geographic regions. Application of the 'dartboard' test statistic (Ellison and Glaeser, 1997) shows that JSD participation was not uniformly distributed across geographic regions. The Ellison-Glaeser test statistic measures the deviation of actual geographic concentration from predicted concentration under an assumption of random distribution. Table 5 reports findings from the test using 67 local labour market regions (ABS Labour Force Regions (LFRs)).[10] It shows that there is a significant difference between the actual geographic concentration and predicted random geographic distribution.

The role of industrial relations problems in assignment to the JSD is also manifested in the very high proportion of JSD exemptions in the 'discretionary' category. For example, in July 1997 this exemption category accounted for about three-quarters of non-participation in JSD, and for the whole period of 1997/98 about two-thirds of total exemptions. After that time – consistent with resolution of Centrelink industrial relations problems - discretionary exemptions were less common; for example, accounting for only about one-quarter of total exemptions in mid-1999 (Commonwealth Department of Family and Community Services, 2000, Chart 2.4).

The geographic distribution of JSD participation, and use of the discretionary exemptions category, suggest a significant impact of the industrial relations dispute on assignment to the JSD program. Importantly, the impact of the dispute on patterns of assignment to JSD participation, would seem to be explained by the attitude of Centrelink staff towards the program and other issues such as staff cut-backs, rather than by their beliefs about the likely effect of the program on outcomes for individual payment recipients. Hence, the industrial relations dispute may be considered to have introduced a source of randomness into assignment to JSD participation. As support, it can be demonstrated that the geographic distribution of JSD participation is not correlated with local labour market conditions. Figure 3 shows the rate of unemployment and incidence of JSD participation by ABS Labour Force Region. It is evident that there is not a strong relation. More

generally, a range of measures of local labour market conditions – rate of unemployment, rate of inflow to unemployment, rate of outflow from unemployment, and first-differences of these measures - were regressed on the rate of participation in JSD in 1997-98 by ABS LFR.[11]   This was done using labour market measures from 1997-98 (contemporaneous with study of effects of the JSD), 1996-97 (first year of operation of JSD), and 1995-96 (year prior to operation of JSD). For none of the measures of local labour market measures is there evidence of a consistent significant relation with JSD participation.

The second justification for the CIA is that treatment and control group observations can be matched using a relatively rich set of covariates. Most significantly, it is possible to match on the basis of local labour market characteristics, and unemployment payment history. These two factors have been identified as of particular importance in evaluations of matching estimators (for example, Card and Sullivan, 1988, Heckman et al., 1999, and Kluve et al., 2001). Although the LDS does not allow matching on some potentially important covariates such as education attainment, in the Australian context this is likely to be compensated for by being able to control for unemployment payment history. Recent studies for Australia, using other data sources, establish the importance of labour force history in explaining labour market status. Le and Miller (2001) and Knights et al. (2002) have shown that once labour market history is controlled for, other standard covariates have very little explanatory power for whether a labour force participant is unemployed or employed. And while in this study it is payment history rather than labour market history that is included as a covariate, support for the approach is provided in recent work by Moffitt (2001) that suggests total time on welfare payments is strongly (inversely) related to an individual's employment rate.

c. Empirical method – Implementation

To implement the matching method we use a Propensity Score Model (PSM) approach. Essentially this involves matching treatment and control group observations on the basis of their predicted probability of participation in JSD (Rosenbaum and Rubin, 1983).[12]

Stage one of the PSM approach is to estimate a probit model for whether a payment recipient in the sample group commences participation in JSD in the first fortnight of the

payment spell. Covariates included in the model are – gender; age category; country of birth category; marital status and whether partner on payments; whether have children; indigenous status; housing type; unemployment payment history category; and rate of unemployment by ABS LFR; and calendar month commenced payment spell.

The unemployment payment history variable is defined over the twelve months prior to the commencement of the payment spell of each treatment or control group observation. The twelve month period is divided into four quarters, and for each quarter a {0,1} classification is made according to whether the individual was ever observed to be on unemployment payments in that period. Hence there are sixteen possible combinations of payment history – for example, (0,0,0,0) would denote no quarter in the previous 12 months during which the individual was on unemployment payments, and (1,1,1,1) would denote that the individual was on unemployment payments in at least one fortnight in each of the previous four quarters.

To find an appropriate functional form of the probit model for participation in JSD a balancing test is used (see Dehejia and Wahba, 1999, 2002, and Smith and Todd, 2003). Rosenbaum and Rubin (1983, theorem 2) show that the functional form of the PSM model should be chosen such that - after conditioning on the predicted probability of participation from the probit model, there should be no further dependence between participation and higher-order terms or interactions of the matching variables. This motivates the 'balancing test' – a test of whether, after conditioning on the predicted probability of program participation, there is a significant difference between the value of any matching variable for program participants and non-participants.

Application of the balancing test revealed that the functional form that minimized the number of strata for which a jointly significant difference in the set of matching variables was found to exist between JSD participants and non-participants (at 5% level of significance using Hotelling T-test), was to split the sample between males and females, and include a quadratic term for the rate of unemployment by ABS LFR.[13]

Stage two of the PSM is to match treatment and control group observations. On the basis of the findings from the balancing test, treatment and control observations are matched

separately for males and females. Therefore, the matching approach can be described as 'quasi-exact matching' where there is a first step of exact matching on the basis of fortnight of payment spell (implicit in choice of treatment and control groups) and gender, and a second step using results from the PSM probit models to match treatment and control observations from within each gender group. To obtain aggregate estimates of the JSD program effect, a weighted average of the estimated effects for males and females is calculated.[14]

To undertake matching we adopt a 'basic' method, and then consider the sensitivity of results to changes in that method. The main components of the basic method are:

  (a) Use linear predicted score from PSM;
  (b) Caliper method;
  (c) Match each treatment observation with control observations in a 5 per cent confidence interval;
  (d) Kernel weighting of control observations; and
  (e) Re-sampling of control observations for different treatment observations.

(The linear predicted score is preferred to the predicted probability as this allows symmetry in selection of control observations using the caliper method.)

A formal description of the matching estimation method is:

$$(3)\ \tau^1 = (n_m/(n_m+n_f))[(1/n_m) \sum_{i \in D_m^1=1} [Y_{P_i} - \sum_{j \in D_m^1=0} w_m(i,j)Y_{W_{1j}}]] +$$

$$(n_f/(n_m+n_f))[(1/n_f) \sum_{i \in D_f^1=1} [Y_{P_i} - \sum_{j \in D_f^1=0} w_f(i,j)Y_{W_{1j}}]]$$

where n_m and n_f are the number of male and female treatment observations, D_m^1 and D_f^1 are indicators for participation in JSD in the first fortnight of payment spell for males and females, $w_m(i,j)$ and $w_f(i,j)$ are the weights placed on the jth potential control group observation in constructing a comparison for the ith treatment group observation for males and females, and Y_{P_i} and $Y_{W_{1j}}$ are respectively outcomes for the ith treatment observation who commences JSD in the first fortnight and the jth control observation who does not commence JSD in the first fortnight.

In the 'basic' approach:

(4a) $w_m(i,j) = G_m^{ij} / [\sum_{j \in \{D_m^l = 0\}} G_m^{ij}]$; and

(4b) $G_m^{ij} = G[(X_i \widehat{\beta}_m - X_j \widehat{\beta}_m) / a_{5\%}]$

where G_m^{ij} is the kernel for ith treatment and jth control observations for the male sample, $X_i \widehat{\beta}_m$ and $X_j \widehat{\beta}_m$ are linear predicted scores for the respective treatment and control observations in the male sample, and $a_{5\%}$ represents the use of a 5% confidence interval bandwidth around $X_i \widehat{\beta}_m$. In this approach the biweight kernel is used. (And $w_f(i,j)$ and G_f^{ij} are defined in the same way for the female sample.)

A range of alternative ways of implementing the matching method is also considered. The alternatives involve: (a) Nearest neighbour matching; (b) Local linear matching; (c) Use of predicted probability of participation; (d) Common caliper; and (e) Equal weights on control observations.[15]

One method for assessing the quality of matching is to compare the mean values of characteristics used in matching for treatment and control observations.[16] (This is different to the balancing test. The comparison proposed here is directly between treatment observations and a kernel weighted average of the control observations to which they were matched.) Some differences between treatment and control groups are apparent by age and indigenous status – but the differences are quantitatively small. Overall, the results suggest that the choice of control observations has created a comparison group that is on average very similar to the set of treatment observations.

Validity of the matching estimator requires that the CIA and common support assumptions should hold. There is no formal test for the CIA; instead above we have provided justification for why we believe the assumption is satisfied. The common support assumption however can be assessed empirically. Figure 4 presents the linear predicted score from the PSM for treatment and control observations. It is apparent that the common support assumption is satisfied, there being a high degree of overlap between the

distributions – although clearly the treatment observations are more concentrated at higher predicted scores. Using the basic matching method only 7 out of 39,287 treatment observations cannot be matched to a control group observation. The average number of times each control observation was used is 2,423, with a minimum of zero and maximum of 5,652. The average number of control observations matched to each treatment observation is 965, with a minimum of zero and maximum of 10,468. The average proportion of matched control observations that began a JSD spell in the second fortnight or later is 9.5 per cent, with a minimum of zero and maximum of 74.6 per cent. Therefore, on average a relatively small proportion of control observations that are used in the matching will ever participate in JSD.

5. Effects of the JSD

a. Basic model results

Findings from matching method analysis of the effects of the JSD for the basic approach are presented in Table 7. The results demonstrate that JSD participation has a quite large, and statistically significant, negative effect on the duration of unemployment payment spells. One example is that the proportion of JSD participants who had exited unemployment payments by 3 months after the start of their payment spell is 36.6 per cent; by comparison, the weighted average exit rate for control observations is 31.5 per cent. Another example is that over the 12 months after commencement of a payment spell JSD participants spent on average about 13 fortnights on unemployment payments, whereas the weighted average for the control group observations is about 13.9 fortnights.

Extra information on exit from unemployment payments is presented in Figure 5. This shows the difference in the proportion of payment recipients in treatment and matched control groups who have exited NSA/YA(o) payments in each month after commencement of their payment spells. Differences in rates of exit between JSD participants and non-participants emerge in the second and third months after JSD commencement; in subsequent months there is a slight convergence in exit rates but the difference appears to stabilize at about 3.5 percentage points by 9 months after commencement of JSD participation.

The findings suggest several conclusions on the timing of the effect of JSD participation. First, it appears that the effect of JSD participation on exit from payments for the treatment group occurs entirely during the first 3 months after commencement of a payment spell during the period where the JSD participation requirement exists. Second, there is only minimal catch-up of the control group to the treatment group in the rate of exit from unemployment payments in the post-JSD participation period. This explains why the gap in time on payments continues to increase with time since spell commencement. These findings on the JSD impact are intuitively plausible. The nature of JSD participation is such that it would mainly be expected to impact on outcomes during the period where it is directly affecting job search behaviour.

A finding that JSD participation affects the rate of outflow from unemployment payments and total time on payments will be most credible if it can be established that the JSD has an effect on job search behaviour. Qualitative evidence to support such a behavioural effect does exist. A survey of job seekers in May 2000 found that about one-quarter of JSD participants believed their job search levels would decline without the JSD (Tann and Sawyers, 2000); and JSD participants self-reported a significantly higher number of job applications than non-participants (Wallis Group, 2000).

b. Comparison with international evidence

For the outcome measure of 'time on payments in the 12 months after commencement of JSD' it is possible to make a comparison with the Maryland experimental analysis of additional required employer contacts (Klepinger et al., 2002). Both programs are quite similar in the increase in job search requirement imposed. The JSD required an increase in contacts per fortnight from two to eight (for most participants), and the Maryland experiment involved an increase in required contacts per fortnight from four to eight. For the JSD it is found that participation reduces time on payments by 0.93 fortnights and that the control group spends on average about 13.9 fortnights on payments – hence this is a reduction of about 6.7 per cent. In the Maryland study it was found that requiring additional job contacts reduced time on benefits by 0.36 fortnights and that the control group spent on average about 6 fortnights on payments (Klepinger et al., 2002, Table 3) – this is a reduction of about 6 per cent. Therefore, it appears that the programs have had very similar effects.

c. Sensitivity analysis[17]

Estimates of the effect of the JSD using the alternative matching methods are highly robust to choice of matching method. Only for the nearest neighbour method is there any large difference from the basic method; and for this method it is still found that JSD participation significantly increases exit from unemployment payments and decreases time on payments.

Another sensitivity check is to extend the exact matching component of the matching method to include payment history. Extending the exact matching stage to include payment history is motivated by the consideration that it may be particularly important to achieve an exact match in that variable between treatment and control observations (see for example, Card and Sullivan, 1988 and Kluve et al., 2001). Two approaches to exact matching on payment history are applied. With 'ex-ante' quasi-exact matching the sample of treatment and control observations is divided on the basis of some observable characteristic and the PSM approach is then applied within each of those sub-samples. Another possible approach is 'ex-post' quasi-exact matching where a PSM is estimated on the whole sample, the sample is then divided on the basis of some observable characteristic, and matching using the PSM approach is applied within each sub-sample. (Generally it seems that the former approach would be preferred. But where there are a large number of categories of the observable characteristic used to divide the sample, it may not be feasible to estimate a PSM for each sub-sample.) Ex-ante matching is applied using five categories of payment history, and the ex-post matching is applied using 16 categories of payment history. Again, results are found to be highly robust to the use of the alternative matching method.

Using alternative payment history variables – whether on any payment in any fortnight in each six-month period over the previous 2 years (16 categories); and whether on unemployment payments in any fortnight in each six-month period over the previous 2 years (16 categories) – is also found to have only a minimal impact on estimated JSD effects.

Finally, effects of using alternative treatment and control groups, and of an alternative definition of exit from payments, are considered. First, we examine the effect of using a control group of payment recipients who never participate in JSD. Similar results are obtained using this 'restricted' control group as for the 'basic method'. It suggests that the results from the 'basic' method are not sensitive to inclusion in the control group of payment recipients who commence JSD spells after the first fortnight of their payment spells. This is probably not surprising given that on average those observations account for less than 10 per cent of the control group. Second, exit from payments is defined to occur only where a NSA/YA(o) recipient exits from all income support payments. This represents a stricter definition of exit – since exit will not now be defined to occur where a NSA/YA(o) recipient exits from the unemployment-related allowance but commences a spell on some other income support payment (such as Disability Support Pension (DSP)). With the alternative definition of exit the estimated effect of JSD on the rate of exit from payments and time on payments is increased. This suggests that JSD participants are relatively less likely than non-participants to move onto other payment types after exiting NSA/YA(o).[18]

d. Standard errors

Standard errors generated thus far to test differences between treatment and control group outcomes assume only 'normal' sampling variation. However, estimation of propensity scores and the process of matching between treatment and control observations are both extra sources of variation that need to be taken into account (Smith, 2000, p.13). Our approach to testing whether this matters for the results in this study is based on the idea of 'randomization inference' (see Rosenbaum, 1996, and Bertrand et al., 2001).

Formally, our approach involves several steps. First, we apply results from the estimated probit model for JSD participation in 1997/98 to predict hypothetical probabilities of JSD participation for NSA/YA(o) recipients with payment spells that begin in January to July 1996 (pre-program).[19] Second, we sort observations into strata on the basis of predicted probability of participation in JSD.[20] Third, observations within each strata are randomly assigned as treatment or control observations to match the proportion of treatment and control observations within the corresponding strata in 1997/98. For example, suppose that

within a strata in 1997/98 there are 20% of observations in the treatment group and 80% in the control group; then the random assignment in the pre-program period is done to assign 20% and 80% of observations in the equivalent strata respectively as 'treatment' and 'control' observations. Fourth, we apply the basic matching method to a 40% random sample of treatment observations from the pre-program period.[21] 400 repetitions of the third and fourth steps are made.

The output is a set of estimated policy effects for each outcome measure from the 400 repetitions of testing for a 'JSD' effect in the pre-JSD time period. In other words, for each outcome measure we have a distribution of estimated policy effects from a time period where the policy did not exist. These distributions are used to test the hypothesis that the estimated JSD effects in the post-JSD period are significantly different from zero. For example, to test significance at the x% level, the (x/2)% and (100-(x/2))% values in the distribution of pre-JSD policy effects are used as cutoff values. Cutoff values for 1%, 5% and 10% for the 'basic' method are reported in Table 8. For each outcome measure the estimated JSD effect lies outside the 1% confidence interval. Hence the results appear robust to taking account of alternative sources of variation.[22]

e. Results for disaggregate groups

Results on the JSD impact by payment history are derived by estimating a separate PSM for each sub-group. Table 9 shows that there is evidence of ordering of effects by payment history. The impact of JSD participation tends to be higher for payment recipients with no history of receiving unemployment payments in the previous twelve months than for those who had received payments for 1-2 quarters in the previous 12 months. And the size of estimated JSD effects for those who had been unemployed for 3-4 quarters in the previous 12 months are similar to those who had received payments for only 1-2 quarters, but are generally not significant.

Estimated effects of the JSD for NSA/YA(o) recipients in different demographic groups are shown in Table 10. The results are derived using ex-post quasi-exact matching. Slightly stronger effects of JSD participation are apparent for males than females, and for recipients aged 25-34 years than 18-24 or 35-49 years. There are very large differences in the impact

of the JSD between low and high unemployment regions. For example, the estimated effect of JSD participation on the rate of exit from NSA/YA(o) payments in the first 3 months after spell commencement is +7 percentage points in the lowest quartile rate of unemployment LFRs, but is only +2.9 percentage points in highest quartile of LFRs ranked using rate of unemployment.

The main finding from the disaggregate analysis is that the impact of the JSD is largest in conditions where labour demand for unemployed job seekers is likely to be relatively strong – where payment recipients do not have an extensive history of unemployment payments; and in regions where the rate of unemployment is relatively low. This finding seems plausible where labour market outcomes from JSD participation depend both on its effect on job search behaviour and on labour demand conditions. In a search-theoretic model, the effect of the JSD is to introduce a binding constraint that increases job search effort of some payment recipients. In the situation where the marginal effect of increased job search effort on the arrival rate of job offers is increasing with level of labour demand, therefore payment recipients who have more favourable labour demand conditions will receive more job offers. This would tend to increase outflow from unemployment. An offsetting effect however is that unemployed persons who expect to receive more job offers will increase their reservation wage, which reduces outflow from unemployment. Which effect dominates is an empirical question. Pissarides (2000, p.161) notes that "…the usual assumption made…is that the job-offer effect…dominates the reservation wage effect"; and that "this assumption is plausible and available empirical evidence strongly supports it". Thus the findings on the relation between the effects of JSD and labour demand conditions appear consistent with predictions and empirical evidence from existing search theory literature.

f. Intensity of JSD participation

There is evidence that effects of the JSD vary with intensity of participation; that is, by number of required job contacts. Table 11 shows that there is a generally insignificant effect of JSD participation where the required number of job applications was 5 or less, but that there is a significant impact that increases with the number of applications for those with 6 or more required job applications. [23] This finding would seem to be consistent with

effects of the JSD predicted by search theory – that JSD will increase outflow from unemployment where it introduces a binding constraint that increases search intensity, and since this is more likely to occur the larger number of required job applications.

One problem that might be thought to exist with empirical analysis of effects of intensity of JSD participation is that since Centrelink case workers are supposed to assign the number of required job applications on the basis of the degree of labour market disadvantage of a job seeker and local labour market conditions, therefore the results could be simply proxying for selection effects, or for labour demand conditions. However, OLS regression analysis reveals that payment history (16 categories) and local rate of unemployment can only explain 3.1 per cent of variation in number of required job contacts amongst the sample of JSD participants. Hence it appears that assignment of number of required job contacts – and thus differences in effects of the number of job contacts on receipt of unemployment payments – are not reflecting labour demand conditions to a significant degree.

g. Distributional effects

An overall perspective on heterogeneity in the impact of the JSD can be obtained by comparing the distribution of the outcome measure - time on payments in 12 months after JSD participation - for treatment and control groups. Following Heckman et al. (1997) and Heckman (2001) we make this comparison for alternative assumptions on the rank correlation between treatment and control groups. Two main findings are evident from the results reported in Table 12. First, at least one-half of the JSD participants are estimated to have had lower time on payments in the 12 months after commencement of a payment spell. Second, there is evidence of a significant degree of heterogeneity in program impacts. For each approach the impact standard deviation measure is significantly different from zero (at 5% level). It is important in this context to note that heterogeneity in the JSD program impact does not invalidate the matching method used in this study. It has already been discussed why the CIA should hold; so that JSD assignment does not depend on anticipated benefits from participation (or using terminology from Heckman (2001, p.F669), the 'veil of ignorance' should apply).

6. Conclusion

This study has provided quasi-experimental evidence of a job search program – the JSD - that has had a large and sustained effect on the rate of exit from payments of unemployed job-seekers. While there is already a body of international literature that suggests job search programs can improve labour market outcomes, what is particularly significant about the JSD is its large-scale implementation and focus on work search verification. (Hence, for example, providing large-scale evidence to support findings from the recent Maryland experiment that work search verification does not need to be supplemented by job search assistance to increase the rate of exit from payments – Klepinger et al., 2002.) Disaggregated analysis of the effects of the JSD reveals that at least one-half of participants had reduced time on payments due to JSD. There is a significant degree of heterogeneity in the program impact. Largest effects of the JSD are found to occur for payment recipients for whom labour demand conditions are the most favourable, and who are required to make at least 6 job applications per fortnight. The findings on the impact of the JSD are highly robust to a wide range of sensitivity checks.

Endnotes

1. Administrative and activity test breaches are the two types of sanctions that can be imposed on unemployment payment recipients. Administrative test breaches cause a reduction in payments of 16% for 13 weeks. Activity test breaches result in a reduction of payment by 18% for 26 weeks (1st breach within 2 year period); 24% for 26 weeks (2nd breach within 2 year period); and 100% for 8 weeks (3rd breach within 2 year period) (Commonwealth Department of Family and Community Services, 2000).

2. It is suggested "…access to a geographically-matched comparison group administered the same questionnaire as program participants and access to detailed information on recent labor force status histories and recent earnings are essential in constructing comparison groups that have outcomes close to those of an experimental control group" (Heckman et al., 1999, p.1021).

3. As a possible extension we did also consider the period between July 1998 and June 2000 during which time there was not universal application of JSD. However, it was judged that for these time periods it could not be assured that the 'conditional independence assumption' would hold. Specifically, the basis on which we will argue that there was a significant source of randomness in assignment to JSD participation between 1997-98, does not seem to exist in the later time period.

4. The LDS does not include a variable for 'reason for exemption from JSD'. Hence, it is necessary to use an indirect method (based on the 'activity test type' variable) to exclude JSD non-participants likely to have been exempted for reasons associated with labour market disadvantage.

5. The Social Security Act 1991 defines a 'notional continuous period of receipt of income support payments' as one in which the maximum break from payments in the first 12 months of payment receipt is 6 weeks, and the maximum break in subsequent months is 13 weeks; and where a break in payments begins prior to, but within 6 weeks of, 12 months duration, the 13-week test applies. Information on payment receipt from the LDS is only available on a fortnightly basis. Since it is possible for a break in payments of 3 fortnights to correspond to a break in payments of exactly 6 weeks, so that according to the FaCS definition a new spell would not have commenced, therefore to define new spells in this study the rule of requiring a break of 4 fortnights off payments where spell duration is less than 23 fortnights is adopted. For the case where spell duration is more than 23 fortnights, and the FaCS rule for a new spell is a payment break of 13 weeks, it is necessary to use 7 fortnights as the period off payments to define new spells.

6. Descriptive statistics compare – for payment spells commencing in 1997/98 - all payment recipients who ever participate in JSD with payment recipients who never participate in JSD. By contrast, as will be explained later, the quasi-experimental analysis uses a subset of JSD non-participants and some JSD participants in the control group. As

well, some observations included in the descriptive statistics are excluded from the quasi-experimental analysis due to missing information on matching covariates.

7. More generally, it is possible to extend the matching approach to estimate the average effect of participation in treatment for individuals who commence treatment at other specific payment spell durations, or an overall average effect for individuals who commence between fortnights 1 and F:

$$\tau = \sum_{f=1}^{F} E(Y_{P_f} - Y_{W_f} \mid D^f = 1) \cdot \Pr(D^f = 1 \mid D = P)$$

Due to there not being a sufficient number of JSD participants who commence on JSD in later fortnights, such an exercise is not undertaken in this study.

8. In making this comparison, control group observations are weighted using the same kernel weights subsequently used in the matching analysis. Note that the proportion of the treatment group participating in JSD declines with spell duration for two reasons. First, some payment recipients exit JSD but remain on unemployment payments. Second, other payment recipients may exit unemployment payments.

9. See for example 'PS union urges dole diary boycott' by Innes Wilcox, The Age, 17/7/1996, p.A6; and 'Public service strikes at cuts' by Joanne Painter, The Age, 24/7/1996, p.A4.

10. Actual geographic dispersion is measured as $G = \sum_i (s_i - x_i)^2$ where s_i and x_i are respectively the share of JSD participants in ABS Labour Force Region (LFR) i and the share of payment recipients in LFR i. The benchmark geographic dispersion for random assignment is $E(G) = (1 - \sum_i (x_i)^2)H$ where $H = \sum_i (1/\sum_i (x_i)^2)$. For the variance formula see Ellison and Glaeser (1997, p.907).

11. Details of results are available on request from the authors (Appendix Table 1).

12. Exact matching is not feasible due to the dimensionality of possible combinations of observable characteristics, and the relative number of JSD participants and non-participants.

13. To apply the balancing test observations were divided into 40 strata according to predicted probability of participation in JSD. It was found that test results were insensitive to choice of number of strata, hence the analysis was restricted to this level of disaggregation. For the chosen functional form, only for one set of matching variables did a significant difference exist. Entering other interaction effects, or higher order terms of the rate of unemployment variable, did not improve the result.

14. Results of the first stage probit model are available on request from the authors (Appendix Table 2).

15. These alternative approaches can be formally represented as:
(a) $w(i,j) \in \{0,1\}$
[Weight equals 1 where $\{i,j$ such that $\min |X_i \widehat{\beta} - X_j \widehat{\beta}|\}$ and zero otherwise];
(b) See Heckman et al (1997, p.631) - The Heckman et al. approach to local linear matching involves several stages: (i) Regress Y on P (where Y = outcome and P =

predicted propensity score) for the control group in the caliper for treatment observation j; (ii) Use the regression result and value of P (treatment observation j) to predict Y (denote Yp) ; (iii) Use Y-Yp as the treatment effect for observation j in the treatment group. In this study we use LP (linear prediction from probit model) instead of P. For the outcome measures of incidence of exit/on payments a probit model is used due to the binomial outcome; and for the outcome measure of time on payments a negative binomial model is used due to the outcome being 'count data';

(c) $w(i,j) = G_{ij} / [\sum_{j \in \{D^1 = 0\}} G_{ij}]$; and $G_{ij} = G[(\phi(X_i \widehat{\beta}) - \phi(X_j \widehat{\beta})) / a_{5\%}]$

[Where $\phi(X_i \beta)$ is predicted probability of commencing JSD participation in first fortnight of payment spell];

(d) $w(i,j) = G_{ij} / [\sum_{j \in \{D^1 = 0\}} G_{ij}]$; and $G_{ij} = G[(X_i \widehat{\beta} - X_j \widehat{\beta}) / a]$

[Where a is fixed bandwidth equal to 0.1]

(e) $w(i,j) = 1/n_{5\%}$

[Where $n_{5\%}$ is number of control observations in 5% confidence interval]

16. Results available on request from the authors (Appendix Table 3).

17. Results for this sensitivity analyses described in this sub-section are available on request from the authors (Appendix Tables 4-7).

18.One concern that could arise from these findings is that the higher rate of entry to other types of income support payments after exit from NSA/YA(o) might signify some difference between JSD participants and non-participants – for example, that non-participants are more likely to have a condition that allows them to claim disability payments. However, further investigation has found that movements to payment types that might signify a difference in degree of labour market disadvantage account for only a small share of total destination payments (see Appendix Table 6).

19. Since JSD commenced in July 1996, and the LDS data set provides information on payment receipt from January 1995 onwards, therefore to include payment history over the previous 12 months as a matching variable, it is necessary to restrict attention to new spells that commence between January and July 1996.

20. The 92 strata into which 'predicted probability' is classified are 0-0.1, 0.1-0.11, 0.11-0.12,…,0.89-0.9, 0.9-1. Observations in the bottom and top deciles are aggregated due to small sample size in those ranges.

21. Due to computational time required, it was necessary to restrict the proportion of the pre-program sample used. From analysis of the JSD effect in 1997/98 it was found that results were very stable at more than 40% random samples.

22. The amount of computing time required has meant that we have restricted this exercise to the 'basic case' methodology.

23. It is not possible to match with the control group using the number of required job applications. Hence the results are obtained using the 'basic' matching method, and then disaggregating the treatment group by number of required job contacts.

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Table 1: Number of spells on JSD by NSA/YA(o) payment spell with at least one fortnight on JSD – Payment recipients aged 18 to 49 years, July 1997 to June 1998

	Number	Percent
1	39857	93.74
2	2449	5.76
3	191	0.45
4	16	0.04
5	3	0.01
Total	42516	100.0

Table 2: Start date for first JSD spell - NSA/YA(o) recipients aged 18 to 49 years with at least one fortnight on JSD, July 1997 to June 1998

	All payment spells	
	Number	Cumulative Percent
Fortnight		
1	40942	96.3
2	264	96.9
3	145	97.2
4	80	97.4
5	37	97.5
6	53	97.6
7	67	97.8
8	42	97.9
9	22	98.0
10	14	98.0
11	14	98.1
12	24	98.1
13	34	98.2
14-26	479	99.3
27-52	139	99.6
52+	160	100.0

Table 3: Distribution of JSD participants and all unemployment payment recipients beginning new spells by characteristics - Payment recipients aged 18 to 49 years, July 1997 to June 1998

	JSD participants	Non JSD participants
Gender		
Male	67.4	68.8
Female	32.6	31.2
Age		
18-24	47.0	36.3
25-34	31.9	32.7
35-49	21.1	31.1
Unemployment history in previous 4 quarters		
Never	65.1	49.7
Not frequent/Not recent	22.7	24.8
Not frequent/Recent	9.5	16.4
Frequent/Not recent	1.1	3.6
Frequent/Recent	1.6	5.6
Rate of unemployment – Local labour market		
1st quartile (Lowest rate of ue)	21.9	21.8
2nd quartile	27.4	25.2
3rd quartile	23.7	22.5
4th quartile (Highest rate of ue)	27.0	30.6

Note: Frequent (not frequent) = On payments in 3-4 (1-2) quarters in previous 12 months. Recent (not recent) = On payments in quarter immediately prior to commencement of new payment spell (not on payments in quarter immediately prior to commencement of new payment spell).

Table 4: JSD related breaches, 1998/99 to 1999/2000

Year	Average no. of breaches per fortnight	Percentage of breaches to all on going spells per fortnight
1998/1999	16.230	0.017
1999/2000	35.038	0.042

Notes: Data for 1998/99 are for October 1998 to September 1999; and for 1999/2000 are for October 1999 to September 2000. Since breaches generally occur at the end of JSD participation, therefore we examine data on breaches for time periods that finish in the 3rd quarter, to match with the data on JSD participation that examines payment spells that begin before the end of the 2nd quarter.

Table 5: Dartboard test for geographic randomness in distribution of JSD participants, July 1997 to June 1998

	Index	E(G)	Index-E(G)	SD(G)	(Index-E(G))/SD(G)
	(Actual)	(Random)	(Difference)		
67 regions	0.000306977	1.78742E-05	0.000289	3.40619E-06	84.87

Table 6: Effects of JSD – NSA/YA(o) recipients aged 18 to 49 years with at least one fortnight on JSD – 'Basic' matching method – July 1997 to June 1998

	Treatment	Control	Difference	p-value
% Off payments				
By 3 months	36.6	31.5	+5.1	0.000
By 6 months	58.7	54.4	+4.3	0.000
% On payments				
At 6 months	49.1	53.7	-4.6	0.000
At 12 months	35.1	39.4	-4.3	0.000
Time on payments				
First 6 months	7.887	8.296	-0.409	0.000
First 12 months	12.958	13.888	-0.930	0.000
number of observations				
Observations matched	39280	15643		
Total no. of observations	39287	15645		

Table 7: 'Randomization inference' two-sided confidence intervals for zero effect hypothesis

	Confidence interval			Basic model – estimated effects
	10%	**5%**	**1%**	
Outcome measure				
Exit 3 months	-1.6, 1.7	-2.1, 1.9	-2.6, 2.7	+5.1
Exit 6 months	-1.7, 1.9	-2.1, 2.0	-2.6, 2.7	+4.3
On 6 months	-1.8, 1.8	-2.0, 2.1	-2.9, 2.6	-4.6
On 12 months	-1.8, 1.6	-2.2, 2.1	-2.9, 2.8	-4.3
Time 6 months	-0.15, 0.14	-0.18, 0.17	-0.23, 0.23	-0.409
Time 12 months	-0.31, 0.29	-0.38, 0.36	-0.48, 0.52	-0.930

Table 8: Effects of JSD by payment history – NSA/YA(o) recipients aged 18 to 49 years with at least one fortnight on JSD – July 1997 to June 1998

	No history	**Not frequent**	**Frequent**
% Off payments			
By 3 months	+4.5 (0.000)	+4.0 (0.000)	+5.6 (0.000)
By 6 months	+3.5 (0.000)	+2.9 (0.000)	+1.4 (0.392)
% On payments			
At 6 months	-3.6 (0.000)	-3.7 (0.000)	-2.0 (0.211)
At 12 months	-3.6 (0.000)	-1.5 (0.001)	-2.5 (0.107)
Time on payments (Fortnights)			
First 6 months	-0.317 (0.000)	-0.285 (0.000)	-0.221 (0.079)
First 12 months	-0.777 (0.000)	-0.508 (0.000)	-0.423 (0.105)

Note: No history = No quarter in which have received unemployment payments in previous 12 months; Not frequent = Received unemployment payments in 1 or 2 quarters in previous 12 months; and Frequent = Received unemployment payments in 3 or 4 quarters in previous 12 months.

Table 9: Effects of JSD by characteristics of payment recipients – NSA/YA(o) recipients aged 18 to 49 years with at least one fortnight on JSD

	Difference in outcome:				Time on payments (Fortnights)	
	% Off payments		% On payments			
	By 3 months	By 6 months	At 6 months	At 12 months	First 6 months	First 12 months
Gender						
Male	+5.5 (0.000)	+4.6 (0.000)	-5.2 (0.000)	-5.1 (0.000)	-0.453 (0.000)	-1.032 (0.000)
Female	+4.2 (0.000)	+3.6 (0.000)	-3.6 (0.000)	-2.7 (0.000)	-0.319 (0.000)	-0.719 (0.000)
Age						
18-24 years	+3.9 (0.000)	+3.0 (0.000)	-2.8 (0.000)	-2.8 (0.000)	-0.298 (0.000)	-0.593 (0.000)
25-34 years	+6.6 (0.000)	+5.7 (0.000)	-7.0 (0.000)	-5.6 (0.000)	-0.553 (0.000)	-1.352 (0.000)
35-49 years	+4.7 (0.000)	+5.0 (0.168)	-5.1 (0.000)	-5.2 (0.000)	-0.414 (0.000)	-0.997 (0.000)

Rate of unemployment – Local labour market						
1st quartile (Lowest rate of ue)	+7.0 (0.000)	+5.9 (0.000)	-6.9 (0.000)	-6.6 (0.000)	-0.626 (0.000)	-1.483 (0.000)
2nd quartile	+7.2 (0.000)	+5.7 (0.000)	-6.0 (0.000)	-5.2 (0.000)	-0.572 (0.000)	-1.125 (0.000)
3rd quartile	+3.3 (0.000)	+3.4 (0.000)	-2.7 (0.000)	-3.1 (0.000)	-0.235 (0.000)	-0.537 (0.000)
4th quartile (Highest rate of ue)	+2.9 (0.000)	+2.5 (0.000)	-3.2 (0.000)	-2.8 (0.000)	-0.231 (0.000)	-0.547 (0.000)

Note: For the Local labour market classification each ABS LFR is ordered on the basis of its average rate of unemployment over the sample period (quarterly data). Regions are then classified between quartiles according to average rate of unemployment on a population weighted basis – so that 25 per cent of the population is in regions classified in each quartile range.

Table 10: Effects of JSD by number of required job contacts – NSA/YA(o) recipients aged 18 to 49 years with at least one fortnight on JSD

| | % off payments by 3 months | | Time on payments – First 12 months | |
	Difference	p-value	Difference	p-value
2	-0.008	0.646	0.365	0.260
3	-0.023	0.034	0.950	0.000
4	-0.014	0.151	0.639	0.260
5	0.009	0.197	-0.195	0.127
6	0.026	0.019	-0.254	0.039
7	0.048	0.028	-0.679	0.002
8	0.076	0.000	-1.528	0.000

Table 11: Distribution of JSD effect on fortnights on payments in first 12 months after commence JSD - By percentile, 1997-98

	Perfect positive correlation	Perfect negative correlation	Independent (Random)
5th percentile	-3	-24	-21
25th percentile	-2	-19	-11
50th percentile	-2	-2	-1
75th percentile	0	16	7
90th percentile	0	24	20
Percent with less time on payments	73.4 (1.74)	53.7 (0.46)	51.6 (3.87)
Impact standard deviation	1.04 (0.05)	16.99 (0.03)	12.13 (0.61)

Notes: (a) The perfect positive correlation case matches the top percentile in the treatment group with the top percentile in the control group, the second top percentiles in the treatment group with second top in the control group, and so on. The perfect negative correlation case matches percentiles in reverse order, so that the top percentile in the treatment group is matched with the bottom percentile in the control group, and so on. The independent case is based on 400 random matches of percentiles; (b) For each case the difference between each percentile of the treatment and control distributions is the impact for that percentile. The percent positive is the percent of the percentile impacts greater than zero. These percentile impact constitute the distribution of impacts. The impact standard deviation is the standard deviation of the percentile differences; (c) Bootstrapped standard errors (based on 400 repetitions) are in parentheses.

Figure 1: Distribution of number of JSD contacts - NSA/YA(o) recipients aged 18 to 49 years - July 1997 to June 1998 - By fortnight
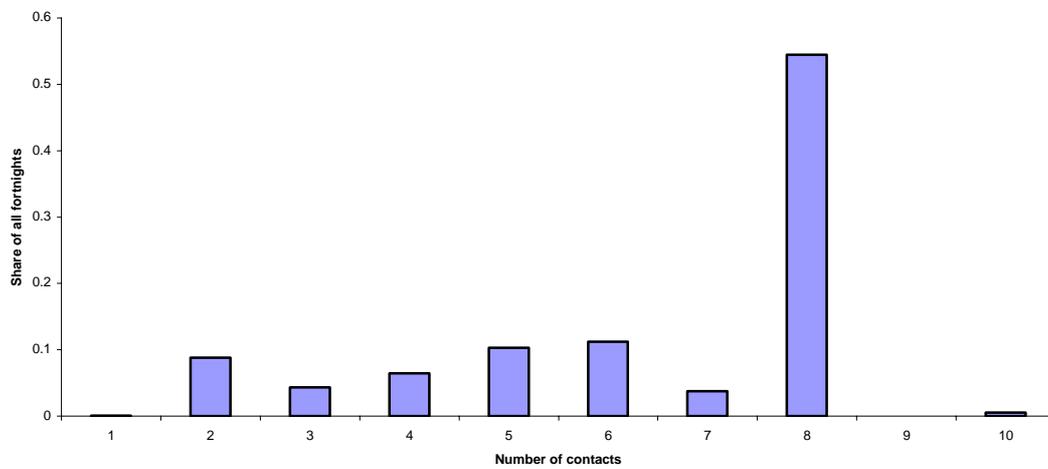
Figure 2: Proportion of treatment and control observations participating in JSD – By payment spell duration (fortnight)
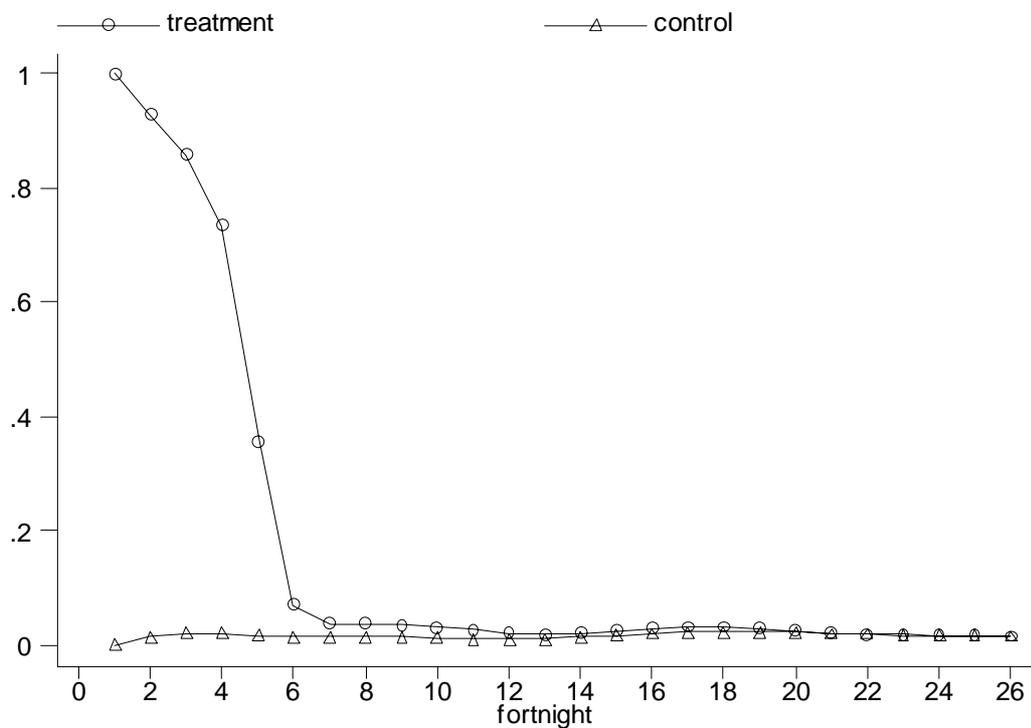
Figure 3: Rate of unemployment and incidence of participation in JSD by ABS Labour Force Region – NSA/YA(o) recipients aged 18 to 49 years – July 1997 to June 1998
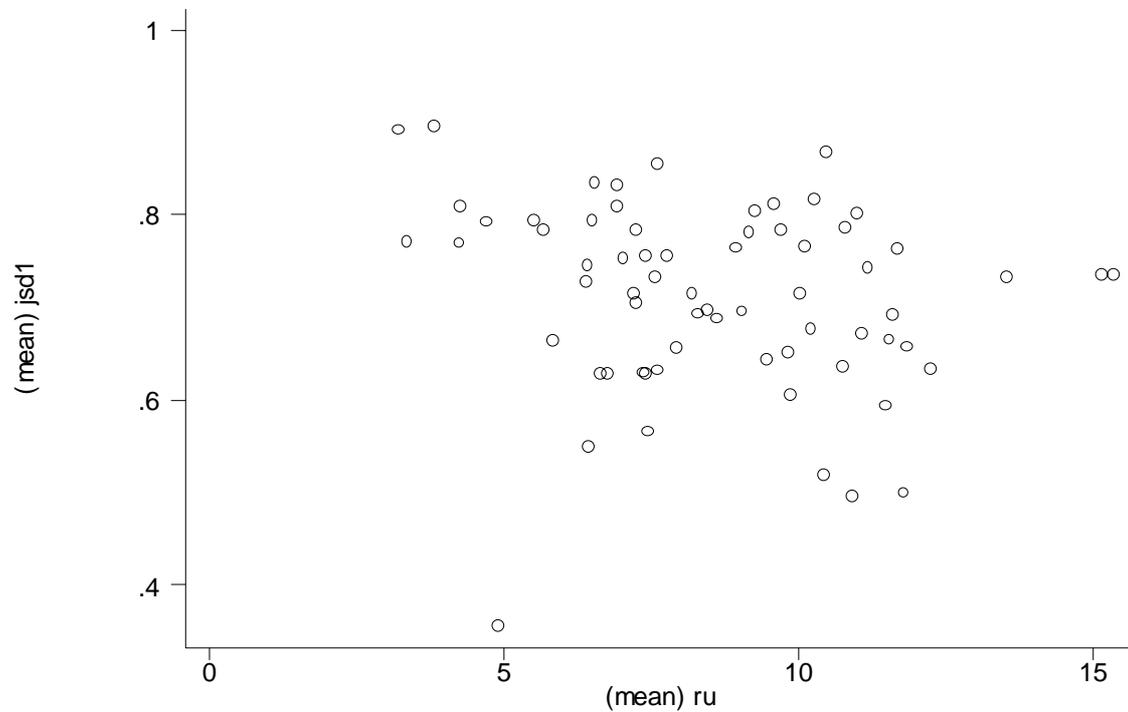
Figure 4: Predicted probability of commencing in JSD in first fortnight of payment NSA/YA(o) spell – July 1997 to June 1998
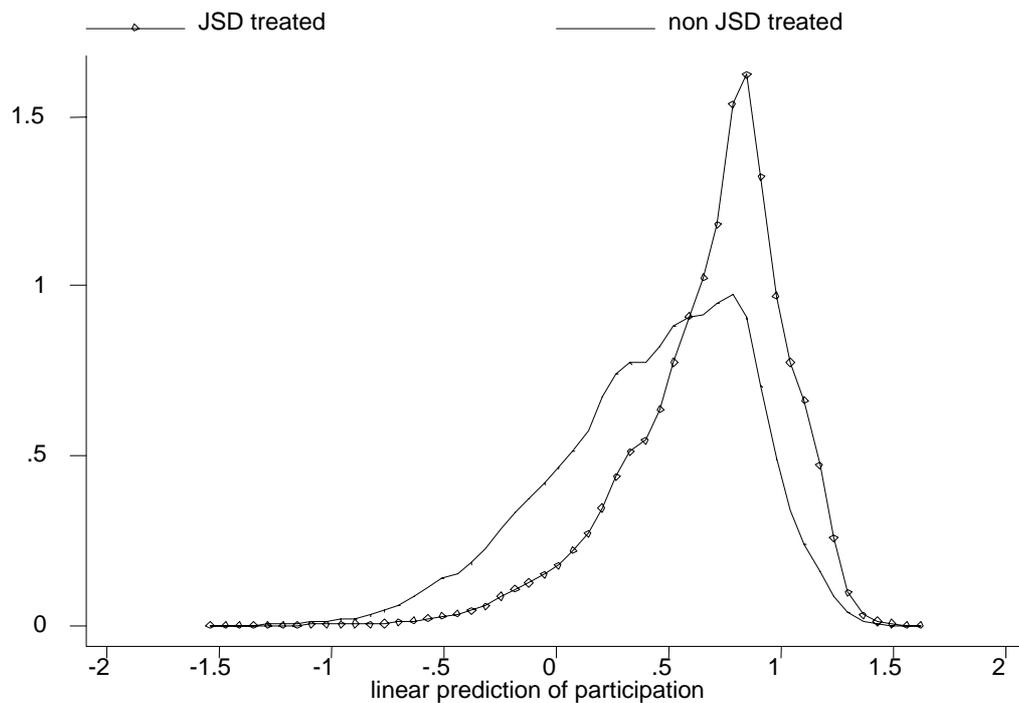
Figure 5: NSA/YA(o) payment recipients - Difference in proportion of treatment and matched control groups exiting payments by month after JSD commencement (New spells commencing July 1997 to June 1998)
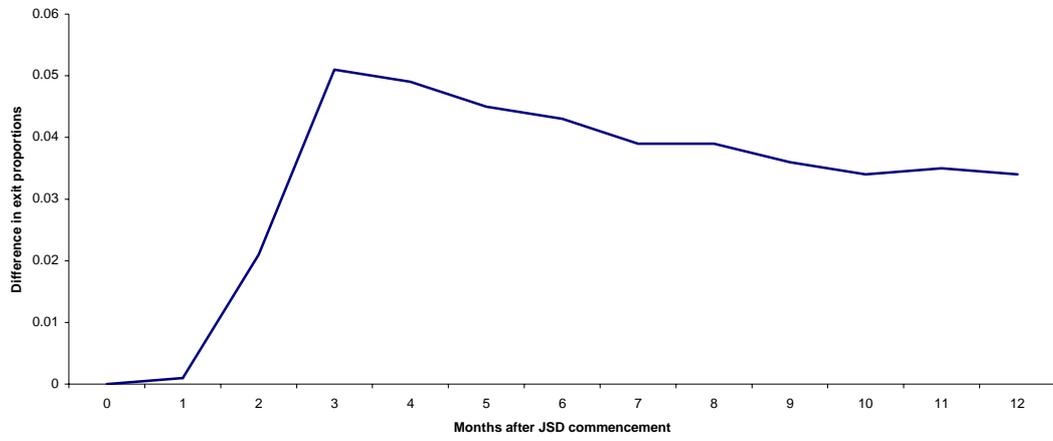
Appendix Table 1: Regression analysis – Correlation between rate of participation in JSD and local labour market – By ABS Labour Force Region

Dependent variable: Proportion of payment recipients participating in JSD, 1997-98

Labour market measure	Coefficient
Rue(t)	-0.0058 (0.0051)
Rue(t-1)	-0.0041 (0.0050)
Rue(t)-Rue(t-1)	-0.0155 (0.0120)
Rue(t-1)-Rue(t-2)	0.0148 (0.0098)
Inflow(t) - Outflow (t)	-0.0002 (0.0003)
Inflow(t-1) – Outflow(t-1)	-0.0001 (0.0003)
Inflow(t-2) – Outflow(t-2)	-0.0002 (0.0003)
Rinflow(t) – Rinflow(t-1)	10.601 (5.791)
Rinflow(t-1) – Rinflow(t-2)	-9.002* (3.062)
Routflow(t) – Routflow(t-1)	5.625 (4.704)
Routflow(t-1) – Routflow(t-2)	-3.124 (3.501)

Notes: a) Rue(t) = Rate of unemployment in year t; Inflow(t) = Inflows to unemployment payments in year t; Outflow(t) = Exit from unemployment payments in year t; Rinflow(t) = Inflow to unemployment payments/Stock of unemployment payment recipients; and Routflow(t) = Exit from unemployment payments/Stock of unemployment payment recipients. (Rate of inflow measure is calculated by: 1. Calculating number of inflows to unemployment payments in a month; 2. Calculating stock of unemployment payment recipients at end of preceding month; 3. Take ratio of 1 and 2; and 4. Repeat steps 1 to 3 across all months in the year, and then calculate annual average. Rate of outflow is calculated in similar way.); b) Year t = 1997-98; Year(t-1) = 1996-97; and Year(t-2) = 1995-96; c) Number of observations = 67; and d) Standard errors are in parentheses.

Appendix Table 2: PSM results – Probit model

	coefficient	Std. Err.	P value
UE history D2	-0.050	0.023	0.03
UE history D3	-0.189	0.053	0.00
UE history D4	-0.176	0.022	0.00
UE history D5	-0.619	0.036	0.00
UE history D6	-0.636	0.068	0.00
UE history D7	-0.572	0.032	0.00
UE history D8	-0.567	0.018	0.00
UE history D9	-0.991	0.105	0.00
UE history D10	-1.009	0.249	0.00
UE history D11	-0.647	0.448	0.15
UE history D12	-0.816	0.250	0.00
UE history D13	-1.013	0.045	0.00
UE history D14	-1.029	0.089	0.00
UE history D15	-1.054	0.058	0.00
UE history D16	-1.097	0.050	0.00
Male	-0.233	0.107	0.03
age 21-24	0.047	0.028	0.10
age 25-29	0.028	0.032	0.39
age 30-34	-0.167	0.043	0.00
age 35-39	-0.316	0.048	0.00
age 40-44	-0.434	0.047	0.00
age 45-49	-0.514	0.044	0.00
male*age 21-24	0.006	0.037	0.88
male*age 25-29	-0.075	0.040	0.06
male*age 30-34	0.057	0.050	0.25
male*age 35-39	0.129	0.055	0.02
male*age 40-44	0.195	0.055	0.00
male*age 45-49	0.210	0.054	0.00
ESC	0.035	0.022	0.12
NESC	-0.190	0.017	0.00
ATSI	-0.783	0.034	0.00
married, partner not on IS	-0.213	0.026	0.00
married, partner on IS	-0.120	0.023	0.00
have child	-0.336	0.107	0.00
have child under 13	0.221	0.145	0.13
have child uner 6	-0.298	0.129	0.02
male*have child	0.346	0.125	0.01
male*have child under 13	-0.200	0.160	0.21
male*have child uner 6	0.338	0.134	0.01
housing: government rent	-0.121	0.039	0.00
housing: other rent	-0.075	0.014	0.00
housing: home owner	-0.050	0.020	0.01
housing: unknown	-0.165	0.025	0.00
UE rate at spell start	-0.128	0.018	0.00
UE rate at spell start sq	0.006	0.001	0.00
male*UE rate at spell start	0.053	0.022	0.02
male*UE rate at spell start sq	-0.003	0.001	0.01
starting months dummies	yes		
Number of observations	54932		
LR chi2 (df=58)	4675.95		

Appendix Table 3: Comparison of means of treatment and control group observations - NSA/YA(o) recipients aged 18 to 49 years, July 1997 to June 1998

	Treatment	**Control**	**Difference**	**p-value**
Age	28.100	28.005	0.095	0.013
Immigrant status/Ethnicity				
%ESB	0.081	0.081	0.000	0.897
%NESB	0.119	0.119	0.000	0.796
%ATSI	0.017	0.019	-0.002	0.000
Marital status/Children				
%Married – Partner not on payment	0.056	0.056	0.000	0.970
%Married – Partner on payments	0.161	0.159	0.002	0.377
Have children	0.128	0.126	0.002	0.342
Have child under 6	0.085	0.084	0.001	0.491
Have child under 13	0.119	0.118	0.001	0.478
Payment history				
No payment history	0.656	0.656	0.000	0.971
Not frequent/ not recent	0.226	0.225	0.002	0.385
Frequent/not recent	0.093	0.093	0.000	0.724
Not frequent/recent	0.011	0.012	-0.001	0.107
Frequent/recent	0.014	0.016	-0.001	0.059
Unemployment rate at spells start	8.807	0.789	0.018	0.193

Appendix Table 4: Effects of JSD – NSA/YA(o) recipients aged 18 to 49 years with at least one fortnight on JSD – Alternative matching methods I

	Difference in outcome:					
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
	Basic method	**Local linear matching**	**Nearest neighbour**	**Propensity score**	**Common caliper**	**Equal weights**
% Off payments						
By 3 months	+5.1 (0.000)	+5.1 (0.000)	+4.4 (0.000)	+5.0 (0.000)	+5.0 (0.000)	+5.0 (0.000)
By 6 months	+4.3 (0.000)	+4.3 (0.000)	+3.1 (0.000)	+4.3 (0.000)	+4.3 (0.000)	+4.3 (0.000)
% On payments						
At 6 months	-4.6 (0.000)	-4.7 (0.000)	-3.6 (0.000)	-4.7 (0.000)	-4.6 (0.000)	-4.6 (0.000)
At 12 months	-4.3 (0.000)	-4.3 (0.000)	-3.3 (0.000)	-4.3 (0.000)	-4.3 (0.000)	-4.3 (0.000)
Time on payments (Fortnights)						
First 6 months	-0.409 (0.000)	-0.411 (0.000)	-0.329 (0.000)	-0.409 (0.000)	-0.408 (0.000)	-0.408 (0.000)
First 12 months	-0.930 (0.000)	-0.960 (0.000)	-0.690 (0.000)	-0.929 (0.000)	-0.928 (0.000)	-0.927 (0.000)

Appendix Table 5: Effects of JSD – NSA/YA(o) recipients aged 18 to 49 years with at least one fortnight on JSD – Alternative matching methods II

	Difference in outcome:				
	(1)	(2)	(3)	(4)	(5)
	Basic method	Exact matching on payment history (Ex-ante)	Exact matching on payment history (Ex-post)	PSM – 2 year history – all payments	PSM – 2 year history – ue payments
% Off payments					
By 3 months	+5.1 (0.000)	+4.9 (0.000)	+5.0 (0.000)	+4.7 (0.000)	+5.0 (0.000)
By 6 months	+4.3 (0.000)	+4.1 (0.000)	+4.3 (0.000)	+3.9 (0.000)	+4.2 (0.000)
% On payments					
At 6 months	-4.6 (0.000)	-4.6 (0.000)	-4.7 (0.000)	-4.6 (0.000)	-4.3 (0.000)
At 12 months	-4.3 (0.000)	-3.9 (0.000)	-4.4 (0.000)	-4.2 (0.000)	-3.8 (0.000)
Time on payments (Fortnights)					
First 6 months	-0.409 (0.000)	-0.401 (0.000)	-0.412 (0.000)	-0.376 (0.000)	-0.402 (0.000)
First 12 months	-0.930 (0.000)	-0.895 (0.000)	-0.942 (0.000)	-0.914 (0.000)	-0.844 (0.000)

Appendix Table 6: Effects of JSD – NSA/YA(o) recipients aged 18 to 49 years with at least one fortnight on JSD – Alternative treatment and control groups

	Difference in outcome:		
	(1)	(2)	(3)
	Basic method	Control group – Never on JSD	Exit off all payments
% Off payments			
By 3 months	+5.1 (0.000)	+4.5 (0.000)	+7.4 (0.000)
By 6 months	+4.3 (0.000)	+4.1 (0.000)	+7.9 (0.000)
% On payments			
At 6 months	-4.6 (0.000)	-4.6 (0.064)	-8.4 (0.000)
At 12 months	-4.3 (0.000)	-4.5 (0.000)	-9.0 (0.000)
Time on payments (Fortnights)			
First 6 months	-0.409 (0.000)	-0.390 (0.000)	-0.701 (0.000)
First 12 months	-0.930 (0.000)	-0.917 (0.000)	-1.799 (0.000)

Appendix Table 7: Destination of NSA/YA(o) payment recipients who take up other payment types

Payment type	non-JSD participants	JSD participants
Sickness	2 (0.66)	4 (1.41)
Disability	59 (19.47)	33 (11.62)
Sole parent	117 (38.61)	115 (40.49)
Partner	107 (35.31)	97 (34.15)
Widow	4 (1.32)	3 (1.06)
Other allowance	14 (4.62)	32 (11.27)
Total	303 (100.0)	284 (100.0)
Proportion of sample on other payment but not UE at 6th month	1.94%	0.72%