How do administrative arrangements affect exit from unemployment payments? The case of the Job Seeker Diary in Australia

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Executive Summary

Objective

This main objective of this study is to examine the effect of the Jobseeker Diary (JSD) on receipt of unemployment payments for NSA/YA(o) recipients aged 18 to 49 years between 1 July 1997 and 30 June 1998.

What is the JSD?

The JSD is a booklet where an unemployment payment recipient must list details of job search episodes for each fortnight over a three months period. The JSD is issued to all new unemployment payment recipients with job search as their main activity type on fortnightly payments. Payment recipients with a JSD are instructed on the minimum number of jobs per fortnight for which they must apply. In most cases this number will be equal to the benchmark set for the region in which they reside that is determined on the basis of local labour market conditions. A payment recipient must return the JSD either when requested, or at the 12 week or 9 month Review. 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.

Descriptive information

The study uses data from the FaCS LDS 10% Unemployment payment sample. From this source data are available on over 57,500 NSA/YA(o) payment spells that commence during 1997/98. It is found that about three-quarters of NSA/YA(o) recipients who begin payment spells during the sample period have at least one fortnight of JSD requirement. Almost all NSA/YA(o) payment spells involve only a single episode of participation in JSD. And for over 95 per cent of those payment recipients, their first spell on JSD begins in the first fortnight of their payment spell. The modal number of required job contacts during the sample period was eight (Figure 1).

How effects of the JSD are measured?

A variety of outcome measures related to receipt of unemployment payments are used to assess the effect of the JSD. One measure is the effect of JSD on the incidence of exit from unemployment payments by 3 months and 6 months after JSD commencement. A second measure is the effect of JSD on whether payment recipients are on unemployment payments at 6 months and 9 months after JSD commencement. The third measure applied is the effect of JSD on the number of fortnights on unemployment payments during the 6 months and 12 months after JSD commencement.

Empirical methodology

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. Estimates of the effect of JSD participation are the 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. (It is shown that very similar results are obtained when the comparison is made with a sub-sample of NSA/YA(o) recipients who never participate in JSD.)

The effect of JSD – Basic results

JSD participation is found to reduce the likelihood of a payment recipient remaining on unemployment payments, and to reduce total time subsequently spent in receipt of unemployment payments. These effects are quite large, and statistically significant. One example is that the proportion of JSD participants who had exited unemployment payments by 3 months after commencement of their payment spells is 5.1 percentage points higher for JSD participants than non-participants (36.6 per cent compared to 31.5 per cent). Another example is that JSD participants spend on average 0.9 fortnight less on unemployment payments in the year after commencing their payment spells than non-participants (13 fortnights compared to 13.9 fortnights).

Outcome measure	**Difference in outcomes: JSD participants minus non-participants**
% Off payments	
By 3 months	+5.1
By 6 months	+4.3
% On payments	
At 6 months	-4.6
At 12 months	-4.3
Time on payments (fortnights)	
First 6 months	-0.409
First 12 months	-0.930

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 (during the period where the JSD 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 of a similar job search requirement program introduced as part of the Maryland experiment. In both cases the duration of time on payments over a 12 month period after program participation was estimated to be reduced (relative to the average outcome for non-participants) by about 6 per cent.

The effect of JSD – Results for sub-groups

Disaggregate analysis reveals 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.

The effect of JSD – Results for later time periods

Estimated effects of JSD participation for 1998-99 and 1999-2000 also show that JSD participation reduces the likelihood of receipt of unemployment payments. However, the magnitude of the effects is declines across time. We suggest the main potential explanation for the declining magnitude of the JSD effect is behavioural changes by JSD participants – for example, reduced effort in meeting JSD requirements due to learning about the probability of being detected to have not met the JSD requirement or of being penalized once detected. But such evidence must be regarded as suggestive rather than conclusive. It also appears that differences in the quality of the matching analysis that can be done in 1999-2000 could partly explain the difference in estimated effects in that time period.

Comparison with international evidence

In most respects the findings from analysis of JSD effects match findings from international studies of job search programs:

- Participation in job search programs appears to improve labour market outcomes for unemployed persons;
- Job search programs seem to primarily improve labour market outcomes by increasing intensity of job search by unemployed persons;
- Job search programs are most effective where they do not distort the ‘type’ of job search activities able to be undertaken;
- It does appear that job search programs that involve only work search requirements (and not also job search training) can improve labour market outcomes.

1. Introduction

The rise of mass unemployment in industrialized economies has brought a substantial concern on the part of governments to develop policies designed to address this problem. Specifically, from the early 1990s onwards, a burgeoning area of interest has been assessing the efficacy of programs and interventions that directly target unemployed job-seekers. Underlying that interest have been the adoption of program evaluation methods from the statistics literature, and an increased availability of administrative data sources; much has already been learned about these targeted interventions.[1]

Some of the general lessons from the program evaluation literature concern the relative efficacy of different types of interventions for unemployed persons – for example, counseling and monitoring of job seekers compared to classroom training compared to public sector job creation. However, another significant lesson is about the heterogeneity of program impacts. There appears to be as much variation in the estimated impact of different examples of the same type of program, as there is variation between types of programs. For that reason there is significant value in continuing research on the impacts of interventions and programs targeted at unemployed persons. Such research can both provide a broader perspective on the efficacy of a program type, but will also contribute to knowledge on the details and environmental factors that determine whether a specific program will improve labour market outcomes for unemployed persons.

This study examines what appears – in the international context – to be a unique intervention designed to affect job search behaviour of unemployed persons in Australia. The intervention – the Jobseeker Diary (JSD) – aims to increase job search activity of unemployment payment recipients by requiring them to complete a fortnightly diary in which details of a specified minimum number of job applications must be recorded.

A quasi-experimental matching method is used to estimate the effects of a JSD requirement on durations of unemployment payment spells. Since a JSD requirement can commence for an unemployment payment recipient at different payment spell durations, and can begin for different unemployment payment recipients at different points in any year, it is necessary for the matching approach to accommodate this complexity. These issues are addressed in this study by estimating a policy effect that is the average effect 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 other critical aspect of methodology is to provide a justification for the validity of a matching estimator as a method to examine the effects of the JSD. It is argued that – first, there is a sufficiently rich set of covariates that can be used for matching; and second, details of the implementation of JSD during the sample period – specifically, geographic distribution of JSD participation – suggest that there is a significant source of randomness in assignment of unemployed persons between participating and not participating in the JSD.

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

2. Literature review

The JSD can be categorized as a type of job search intervention – that is, intended to raise the intensity and/or effectiveness of job search by an unemployed person. Other examples of this type of intervention would be counseling and monitoring about job search; job clubs; job search training; and information provision about job vacancies. Apart from the JSD, other examples under existing arrangements in Australia would be the twelve week and nine month review interviews, and the performance review that occurs on exit from a Mutual Obligation activity.

Only one Australian study on the net impact of a job search-related intervention has been previously undertaken. Breunig et al. (2003) examine the effect of a randomized experiment designed to evaluate the effect of providing counseling to very long-term unemployed (payment spells of more than five years duration). That study found no significant effect on employment or job search, but did find positive effects on time spent in training and social participation activities. It is suggested that the minimal impact of the intervention can be primarily explained by its modest scale relative to the degree of disadvantage of the very long-term unemployed.

There has been a much wider range of evaluations of job search interventions in the international literature. The general finding is that these types of programs can improve labour market outcomes for the unemployed. Martin (1998, p.289-90) concludes from a review of programs implemented in OECD countries that:

> "…investment in active placement and raising the motivation of the unemployed, as well as taking steps to encourage and monitor their job-search behaviour, pay dividends in terms of getting the unemployed back into work faster."

One main source of evidence on job search programs has been from experimental studies undertaken in the United States. Meyer (1995) reviews evidence on four experimental job search programs (for example, Johnson and Klepinger, 1994). These programs provided different mixes of job finding services and increased enforcement of job search requirements. Meyer (p.125) concludes:

> "…various combinations of increased enforcement of work search rules and additional job finding services can reduce UI receipt…The more intensive treatments tend to have bigger effects…"

More recent studies have examined the optimal mix of work search verification and job search training. Ashenfelter et al. (1999) examine an experimental study that provided work search verification but did not incorporate any job search training. They find that (p.1) "…the treatments provided no benefits". Martin (1998, p.290) concludes from these studies that:

> "…While the optimal combination of additional job-placement services and increased monitoring of job-seekers and enforcement of the work test is unclear, the evidence suggests that both are required…"

More recent evidence however suggests that enforcement and verification of work search requirements may be sufficient to reduce time spent on payments. Klepinger et al. (2002) examine effects of an experimental program designed to test the impact of alternative work-search programs: requiring additional job contacts (similar to the JSD); verification of work-search contacts; a requirement to participate in a job search workshop; and elimination of any requirement to report work-search contacts. It is found that the first three types of measures significantly reduce payment receipt (by roughly similar magnitudes), whereas the fourth measure increases payment receipt. [2] It is argued that the decline in payment receipt associated with the first three measures is due to an increase in job search intensity.

More generally, Bloom and Michalopoulos (2001) review experimental evidence on mandatory employment programs that provide different mixes of emphasis on job search and general education/training. This evidence also suggests that programs that provide only job search requirements/training can improve labour market outcomes; but it is also concluded (p.25) that '…the programs that produced the largest effects used a mix of job search and education as initial activities…'.

In Europe, evidence on job search programs is available for the United Kingdom and the Netherlands. These studies also provide general support for the positive effects of job search programs, although with the caveat that any program should not 'distort' optimal job search behaviour.

In the United Kingdom a recent example of a job search program that appears to have had a positive impact on labour market outcomes is the New Deal for Young Unemployed. During the Gateway phase an unemployed person is matched with a personal advisor with whom they meet at least once every two weeks; intensive job search assistance is provided and a small basic skills course. Quasi-experimental matching analysis of the Gateway phase has found that program participation increased the rate of outflow from unemployment for young males by about 20 per cent. There is direct evidence that the program increased job search intensity of unemployed participants. Important factors in explaining the positive program impact have been argued to be the role of personal advisors, and that the program is mandatory and sanction-enforced (Blundell et al., 2001, and Finn, 2002).

The Restart program in the United Kingdom was targeted at unemployment payment claimants with spell durations of six months who were required to attend a mandatory (sanction-enforced) interview with a counselor. In the interview the claimant's job search history would be reviewed, advice on future options offered, and in some cases direct contact with employers initiated. Experimental analysis finds that the program had both short-run (6 months) and long-run (5 years) positive effects for males – causing a reduction in the rate of unemployment by 5 to 10 per cent; and a short-run but no long-run effect for females (Dolton and O'Neill, 1996, 2002). The impact of the program appears to have derived primarily from a compliance effect (suspension of payments for non-attendance at interview), a positive effect on job search intensity, and improved information about job opportunities. Interestingly, for a group of males for whom the Restart interview was delayed from six to twelve months, the program had no significant effect on unemployment.

In the Netherlands, there have been two main studies of the effect of counseling and monitoring. Gorter and Kalb (1996) study an experiment where an increased amount of time was spent in an interview with one group of unemployment benefit claimants. That extra time was used to verify in greater detail job search activity, and to discuss future options. Overall there is no effect of counseling and monitoring on the job finding rate, although a significant positive effect does exist for unemployed who had previously held a permanent job. Any positive program effect derives from an increase in job search intensity (job applications made) rather than an increased rate of job offers. Van den Berg and van der Klaauw (2001) examine an experiment where a group of new unemployment benefit claimants had a monthly interview with a counselor for the first six months of their payment spells. It is found that this program had no effect on the exit rate from payments to work. It is argued that the absence of any effect is most likely due to the relatively low-intensity of the program, and that the program caused substitution of search effort from informal search to formal search (where informal search is the channel more likely to produce job offers).

Several main conclusions emerge from the literature review:

- Participation in job search programs appears to improve labour market outcomes for unemployed persons;
- The scale of program (bigger equals more impact), and timing of intervention (earlier equals more impact) matter;
- There is mixed evidence on the relative efficacy of a job search program that include both work search verification and job search training, compared to a program where only work search verification occurs;
- Job search programs seem to primarily improve labour market outcomes 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.

3. 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 activity test arrangements. It was introduced in July 1996, as part of a set of measures intended to tighten administration of the activity test. The JSD is a booklet where an unemployment payment recipient must list details of job search episodes 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 issued to all new unemployment payment recipients with job search as their main activity type who receive fortnightly payments. It can also be issued to payment recipients with an abridged claim (who have a break in a payment spell that requires a new claim), or at a Review Interview where a judgment is made that a payment recipient has made ‘marginal work efforts’. In the period that will constitute the main focus of this study, 1 July 1997 to 30 June 1998, over 90 per cent of JSDs were issued to NSA/YA(o) recipients at the commencement of a payment spell (Department of Family and Community Services, 2000).

There are a variety of possible reasons why a payment recipient could be exempted from the JSD in 1997-98 – Discretionary (assessed as unable to comply with JSD requirements); 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). For the main time period for this study, 1997-98, the primary reasons for exemption were discretionary and abridged payment spells. Omitting exemptions for abridged spells, in 1997-98 about two-thirds of exemptions were discretionary (Department of Family and Community Services, 2000). (Discretionary is now no longer a possible reason for exempting a payment recipient from the JSD.)

Payment recipients with a JSD are instructed on the minimum number of jobs per fortnight for which they must apply. In most cases this number will be equal to the benchmark set for the region in which they reside that is determined on the basis of local labour market conditions; however Centrelink staff can vary the number downwards to take account of personal characteristics of a payment recipient. At the time of introduction of the JSD, the maximum number of job applications required was 8 jobs per fortnight; this was subsequently increased to 10 jobs per fortnight.

A payment recipient must return the JSD either when requested, or at the 12 week or 9 month Review. 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.[3]

4. 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.[4] 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) (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.[5] 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

In this study the main focus will be on new unemployment payment spells (on NSA or YA(o)) that begin between 1 July 1997 and 30 June 1998. Our reason for choosing this time period is that it 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; however, as an extension to the main analysis we do also consider time periods from 1 July 1998 to 30 June 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. An important consideration is that our rule for determining new spells should be consistent with the approach used by FaCS. This is because it is the FaCS definition that is the basis for implementation of spell duration-determined requirements on payment recipients such as the JSD. In fact, our definition involves a longer break in payments than the FaCS definition. [6] Data limitations mean that it is necessary to having 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. [7]

The sample is restricted to payment recipients aged 18 to 49 years, where payment receipt is subject to the activity test and the payment recipient has job search as the activity type at the start of their payment spell. The age restriction 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 (Department of Family and Community Services, 2000). The activity test and job search restrictions are imposed since these requirements are necessary for an unemployment payment recipient to be eligible for the JSD.

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.

c. Descriptive information

Descriptive information on participation in the JSD is presented in Tables 1 to 4, and in Figure 1. In the sample period (1997-98) there are 57,779 new NSA/YA(o) payment spells. About three-quarters of NSA/YA(o) recipients who begin payment spells have at least one fortnight of JSD requirement (Table 1). 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 2). Almost all NSA/YA(o) payment spells involve only a single episode of participation in JSD (Table 3). And for over 95 per cent of those payment recipients, their first spell on JSD begins in the first fortnight of their payment spell (Table 4). Finally, the modal number of required job contacts during the sample period was eight (Figure 1). Appendix Table 2 provides data on the incidence of JSD-related breaches. Data is available only for a subset of the 1997/98 period; however for the period available it is shown that breaches were imposed for about 0.5 per cent of on-going NSA/YA(o) spells involving JSD participation.

5. Methodology

a. 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. In a search-theoretic labour market model 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).

b. Outcome measures

The study will examine effects of JSD on a variety of outcome measures related to receipt of unemployment payments. 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 9 months after JSD commencement. The second and third 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.

Outcome measures

Effect of JSD on:

- Incidence of exit from unemployment payments at 3 months after JSD commencement;
- Incidence of exit from unemployment payments at 6 months after JSD commencement;
- Incidence of receipt of unemployment payments at 6 months after JSD commencement;
- Incidence of receipt of unemployment payments at 9 months after JSD commencement;
- Total time in receipt of unemployment payments during 6 months after JSD commencement; and
- Total time in receipt of unemployment payments during 6 months after JSD commencement.

c. 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. In this sub-section the exact definition of the treatment and control groups, and the policy effect identified, are described.

Participation in the JSD can begin for an individual payment recipient at many 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. In the absence of JSD participation, NSA/YA(o) recipients would be required 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.

There are two main implications of this approach to definition of treatment and control groups. First, over 95 per cent of NSA/YA(o) recipients who ever participate in JSD are represented in the treatment group, however there is a small proportion of NSA/YA(o) recipients in the potential control group who subsequently participate in JSD. Second, the approach does not address the issue of intensity of participation. All JSD participants in the treatment group are accorded equal weight regardless of their duration of participation. (The maximum duration of JSD participation is 6 fortnights but exit prior to that time might occur.)

The empirical method therefore has direct consequences for the policy effect that is identified in this study. 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.

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.

The matching approach follows Sianesi (2001) 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 treating 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:

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

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:

$$(3) \qquad \tau = \sum_{f=1}^{F} E(Y_{P_f} - Y_{W_f} \mid D^f = 1) \cdot \Pr(D^f = 1 \mid D = P)$$

Although not the primary focus, this exercise is undertaken later in this study for the case of NSA/YA(o) recipients who commence JSD participation in fortnight 2 of their payment spells. (There are not sufficient JSD participants who commence in later fortnights to examine effects beyond the second fortnight.)

d. 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 – for the particular study being undertaken – the CIA is likely to hold. In this study we take two approaches to making that justification. First, 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. In this study of course it is payment history rather than labour market history that is included as a covariate; however, recent work by Moffitt (2001) suggests that total time on welfare payments is strongly (inversely) related to an individual's employment rate.

The second 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. Our argument is that during the initial phase of its operation, there was 'exogenous' assignment of JSD participation between geographic regions that was uncorrelated with local labour market conditions. This pattern of geographic assignment – which is explained by industrial relations disputes in Centrelink over the initial period of operation of the program - effectively constitutes a source of randomness in assignment of NSA/YA(o) payment recipients between treatment and control groups.

First, we demonstrate geographic non-randomness in participation in the JSD. Figure 3 shows the proportion of all news spells and new spells that involve JSD participation by ABS Labour Force Region. With random assignment between regions all observations should lie along the 45 degree line. It is evident that there is some degree of concentration along the 45 degree line, but by no means perfect random assignment. More formally, we apply the 'dartboard' test statistic for geographic randomness devised by Ellison and Glaeser (1997). That 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 (ABS Labour Force) regions.[9] It is evident that there is a significant difference between the actual geographic concentration and predicted random geographic distribution.

The existence of geographic non-randomness appears to be explained by industrial relations disputes within Centrelink during the initial phase of operation of the JSD.[10] The disputation caused significant differences between Centrelink offices in the extent of implementation of the JSD program. These differences 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.[11]

Second, it can be demonstrated that the geographic distribution of JSD participation is not correlated with local labour market conditions. Figure 4 shows the rate of unemployment and incidence of JSD participation by ABS Labour Force Region. Appendix Table 3 reports results of a regression of the rate of unemployment on the proportion of payment recipients participating in JSD by ABS LFR. It is evident that the hypothesis of a significant relation can be rejected at the 10 per cent level.

Evidence of exogenous assignment of JSD participation between regions that is uncorrelated with local labour market conditions means that our empirical approach is therefore to use local labour market conditions (rate of unemployment) as a matching variable, but to still believe that there is randomness in assignment to JSD participation that is not controlled for in the set of matching covariates.

e. 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) have shown that where the set of observable characteristics (X) used for matching satisfies the CIA then matching on the predicted probability of treatment using that same set of observable characteristics will also satisfy CIA.

Exact matching – where treatment and control group observations with the exact same observable characteristics are matched – is not feasible in this study due to the dimensionality of the set of possible combinations of observable characteristics, and the relative number of JSD participants and non-participants.

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 Labour Force Region (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.

An important consideration is the appropriate functional form of the probit model for participation in JSD. To find an appropriate functional form we use the balancing test (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. Our approach to implementing the balancing test involves several steps. First, we divide the data into strata based on predicted probabilities of program participation. Second, within each strata we apply the Hotelling T-test to examine whether there are significant differences between mean values of matching variables for JSD participants and non-participants. Third, where it is found that significant differences exist, we seek to include interactions between or higher order terms of the matching variables in the probit model for participation in JSD. Fourth, steps one to three are reapplied.

Stage two of the PSM is to match treatment and control group observations on the basis of predicted probability of commencing participation in JSD in the first fortnight of payment spell. There are a variety of possible ways to implement this stage of the PSM approach. 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) Caliper method;
(b) Use linear predicted score from PSM;
(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:

(4) $$\tau^1 = (1/n) \sum_{i \in D^1 = 1} [Y_{P_1 i} - \sum_{j \in D^1 = 0} w(i,j) Y_{W_1 j}]$$

where n = number of treatment observations, w(i,j) is the weight placed on the jth potential control group observation in constructing a comparison for the ith treatment group observation, and $Y_{P_1 i}$ and $Y_{W_1 j}$ 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:

(5a) $$w(i,j) = G_{ij} / [\sum_{j \in \{D^1 = 0\}} G_{ij}]; \text{ and}$$

(5b) $$G_{ij} = G[(X_i \hat{\beta} - X_j \hat{\beta}) / a_{5\%}]$$

where G_{ij} is the kernel for ith treatment and jth control observations, $X_i \hat{\beta}$ and $X_j \hat{\beta}$ are linear predicted scores for the respective treatment and control observations, and

$a_{5\%}$ represents the use of a 5% confidence interval bandwidth around $X_i\hat{\beta}$. In this approach the biweight kernel is used.

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.

These alternative approaches can be formally represented as:

(6a) $w(i,j) \in \{0,1\}$

[Weight equals 1 where $\{$i,j such that $\min\left|X_i\hat{\beta} - X_j\hat{\beta}\right|\}$ and zero otherwise];

(6b) See Heckman et al (1997, p.631);[12]

(6c) $w(i,j) = G_{ij} / [\sum_{j\in\{D^1=0\}} G_{ij}]$; and $G_{ij} = G[(\phi(X_i\hat{\beta}) - \phi(X_j\hat{\beta})) / a_{5\%}]$

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

(6d) $w(i,j) = G_{ij} / [\sum_{j\in\{D^1=0\}} G_{ij}]$; and $G_{ij} = G[(X_i\hat{\beta} - X_j\hat{\beta}) / a]$

[Where a is fixed bandwidth equal to 0.1]

(6e) $w(i,j) = 1/n_{5\%}$

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

A final aspect of the methodology for undertaking the quasi-experimental analysis is to define the sample of unemployment payment recipients to be included. The basic definition of the sample has been presented in section 4b. One extra restriction that is imposed for the quasi-experimental analysis is to exclude JSD participants whose activity type was not job search in the JSD commencement fortnight, and to only match with control observations whose activity type was job search at the fortnight being matched.[13]

Another type of modification of the ‘basic’ method is to examine what might be termed ‘quasi-exact’ matching. This approach involves a combination of exact matching and the PSM approach. Use of a quasi-exact matching approach is motivated by the consideration that there are some variables – such as payment history – for which it may be particularly important to achieve an exact match between treatment and control observations (see for example, Kluve et al., 2001). 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.)

f. Matching quality

i. Balancing test

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. The best outcome was achieved by splitting the sample between males and females (or including a set of interaction terms between gender and other matching variables), and including a quadratic term for the rate of unemployment (by ABS Labour Force Region). This functional form minimizes the number of matching variables for which a significant difference (at 5% level of significance using

Hotelling T-test) was found to exist between JSD participants and non-participants. (Only for one matching variable did a significant difference exist. Entering other interaction effects, or higher order terms of the rate of unemployment variable, did not improve the balancing test result.)

On the basis of the findings from the balancing test, in the second stage of application of the PSM, we match treatment and control observations separately for males and females. Therefore, the matching approach can be described as ‘quasi-exact matching’ where there is a first stage of exact matching on the basis of fortnight of payment spell (implicit in choice of treatment and control groups) and gender, and a second stage using the predicted linear score from the PSM probit model 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. The estimated probit model used to predict participation in JSD is presented in Appendix Table 4.

ii. Is the CIA satisfied?

There is no statistical test that can be applied to directly assess the validity of the CIA. But some methods for indirectly approaching this issue do exist. In this study we have used a pre-test method to assess the choice of matching variables (see Heckman and Hotz, 1989, Rosenbaum, 1996, and Bertrand et al., 2001).

In this exercise the findings on the determinants of JSD participation in the sample period are used to assign unemployment payment recipients from a pre-sample period between hypothetical treatment and control groups. Under the hypothesis that CIA is satisfied there should be no significant difference in payment outcomes between these groups – since in the pre-sample period the JSD did not exist.

More 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).[14] Second, we sort observations into 92 strata on the basis of predicted probability of participation in JSD.[15] 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. (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 the 1997/98 period it was found that results were very stable at more than 40% random samples.) 400 repetitions of the third and fourth steps are made. From these repetitions an average JSD effect is calculated for each of the six main outcome measures used in this study. Results are presented in Table 6. For each measure the estimated JSD effect is very close to zero. This provides strong indirect support for validity of the CIA assumption.

iii. Is the common support assumption satisfied?

Figure 5 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 are not able to 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.

iv. Comparison of mean values of matching variables

A further method for assessing the quality of matching is to compare the mean values of characteristics used in matching for treatment and control observations. (This is different to the balancing test. The comparison proposed here is directly between treatment observations and a weighted average of the control observations to which they were matched.) Table 6 shows this information for selected characteristics. Mean values for control observations are derived as follows – For each treatment observation the corresponding control observation characteristic is calculated as the kernel weighted average of that characteristic for all control observations matched to the treatment observation. Some differences between treatment and control groups are apparent by age and indigenous status – but the differences are quantitatively small. Overall, the results in Table 6 suggest that the choice of control observations has created a comparison group that is on average very similar to the set of treatment observations.

g. Methodology – Other issues

There are several limitations with the methodology that should be noted. First, as has already been described, the scope of the LDS means that it is not possible to examine the effects of JSD participation on labour market outcomes such as employment or earnings. Second, the possibility that JSD participants who exit payments (into employment) are substituting for non-participants who would otherwise have made an exit, is not addressed. Third, as well as affecting outcomes for unemployed persons who register for unemployment payments, a further potential effect of the JSD could be to decrease the rate of inflow to unemployment payments. Theoretical search models would predict such a 'compliance' effect where the JSD is seen as an intervention that imposes 'distuility' on an unemployed person, and hence raises the relative utility of employment compared to unemployment. Due to an absence of suitable time-series data on receipt of unemployment payments disaggregated by payment spell duration it has not however been possible to examine this type of effect. Fourth, it does not seem possible to examine the effect of JSD participants being required to make different numbers of job contacts. The main problem is that Centrelink staff assign the number of required job contacts on the basis of the degree of disadvantage of a job seeker and local labour market conditions. Hence it would be difficult to argue that the CIA would hold in making comparisons between groups with different numbers of required job contacts.

6. 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 time on payments – 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 Figures 6a and 6b. These figures show the proportion of payment recipients in treatment and matched control groups who have exited NSA/YA(o) payments in each month after commencing participation in JSD. 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 per cent 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 period the first 3 months after commencement of a payment spell (during the period where the JSD 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 over time.) These findings are intuitively plausible. The nature of the JSD requirement is such that it would mainly be expected to impact on outcomes during the period where it is directly affecting job search behaviour. And the analysis of JSD participation by treatment and control groups (Figure 2) has shown that it is only during the first 4 to 5 fortnights that any significant difference in participation exists.

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 does in fact affect job search behaviour. Some 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).

For one of the outcome measures in this study (time on payments in 12 months after JSD commencement) a comparison can be made with the Maryland experimental analysis of additional required employer contacts. Both programs are quite similar in the increase in job search requirements 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 from four to eight per fornight. For the JSD it is found that participation reduces time on payments by 0.93 fortnights and that the control group spend 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 are having very similar effects.

b. Sensitivity analysis

Results on the estimated effect of the JSD using the alternative matching methods are displayed in Table 8. The main point to note is that the findings are highly robust to choice of matching method. Only for the nearest neighbour method is there any significant difference from the basic method; and for this method it is still found that JSD participation increases exit from unemployment payments and decreases time on payments.

Table 9 shows results from extending the exact matching component of the matching method to include payment history, and from using alternative payment history variables. 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.) In the results reported in Table 9 ex-ante matching is applied using five categories of payment history, and the ex-post matching is applied using 16 categories of payment history. It is evident that the results are highly robust to the use of the alternative matching method. Table 9 also shows estimated JSD effects for the basic matching method for two 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). Introducing the alternative payment history variables is also shown to have only a minimal impact on estimated JSD effects.

The effects of using alternative treatment and control groups, and of an alternative definition of exit from payments, are shown in Table 10. First, we consider the effect of using a control group of payment recipients who never participate in JSD is examined. Similar results are obtained using this 'restricted' control group. 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). 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 unobserved difference in JSD participants and non-participants – for example, that non-participants are more likely to have a condition that allows them to claim disability payments. Table 11 presents the destination payment types for NSA/YA(o) payment recipients who exit that payment. There is not evidence of significant unobservable differences between JSD participants and non-participants. Although an apparent difference is that a larger proportion of non-participants in JSD shift onto DSP, disability payments account for only a small share of total destination payments.

Third, we consider a treatment group who begin their JSD participation in the second fortnight of their payment spell. In this case the estimated policy effect is the average effect of commencing JSD in the second fortnight compared to not having commenced JSD at that stage. The estimated magnitudes of policy effects are quite small, and not statistically significant. It seems likely that this is primarily due to the relatively small number of treatment group observations.

c. 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). This is the same method that was used as the pre-test of the CIA. The output from that exercise consists of a set of estimated policy effects for each outcome measure from each of the 400 repetitions of testing for a treatment 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 each outcome measure for the 'basic' method (Table 7) are reported in Table 12. 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. (We do not undertake the same exercise for other estimation approaches due to the very large amount of computing time required.)

c. Results for disaggregate groups

Analysis of the estimated effects of the JSD for disaggregate groups reveals that the impact of participation has varied between NSA/YA(o) recipients with different payment histories in the twelve months prior to commencement of their payment spell, by gender and age, and across regions of residence.

Results on the JSD impact by payment history are presented in Table 13. These results are derived using ex-ante quasi exact matching. 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 14. 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 therefore seems to be 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 contrasts with the recent study of van den Berg and van der Klaauw (2001). In that study it is argued that job search enforcement programs will have the largest impact on unemployed with the worst job prospects and in times of the worst macroeconomic conditions. This argument is based on a theoretical job search model where unemployed persons can engage in formal and informal search, and where job search enforcement introduces a binding constraint on the amount of formal job search. Unemployed persons with good job search opportunities are assumed to use both types of search, but the introduction of the program causes them to redistribute search effort from informal to formal. Hence there will be little overall effect on their job search (or to the extent that informal job search causes a higher arrival rate of job offers than informal search there may be a decline in the rate of outflow from payments). Unemployed persons with poor job search opportunities are assumed to use only formal search, and hence the program will increase their total job search activity and rate of outflow from payments. It seems possible to reconcile the findings from this study with those of van den Berg and van der Klaauw in several ways. First, the theoretical results rely on a specific form of 'cost of job search' that induces substitution between formal and informal search. For alternative cost of search specifications, such substitution would not occur, and there would be a program impact on all unemployed. Second, the model is a supply-side representation of the labour market. It fails to take account of the possibility that a program that promotes job search will have the largest impact for the least disadvantaged unemployed due to higher relative labour demand for those job seekers.

6. The impact of the JSD in other time periods

Thus far the study has focused on NSA/YA(o) spells that commenced in 1997-98. The choice of a one-year time period was made on the basis of computational considerations, and having a unit of time over which macroeconomic conditions would not vary substantially. The specific choice of 1997-98 was due to its being the period closest to the commencement of the JSD requirement for which data on JSD participation is available, and that we believe the method of implementation of the JSD during this period (for example, geographic non-randomness and use of discretionary exemptions) support the validity of the matching approach.

There is interest, however, in also making some assessment of the effects of the JSD during later time periods. Table 15 and Figure 7 present results from application of the basic matching approach to the periods 1998-99 and 1999-2000 as well as 1997-98. (Descriptive statistics on JSD participation in those time periods are presented in Appendix Tables 5 to 7.) Estimated effects of JSD participation on exit from payments and time on payments for 1998-99 and 1999-2000 are of the same sign as for 1997-98, and statistically significant; as well, the timing of effect of the JSD – concentrated within the second and third months after commencement of the JSD spell – is similar between the different time periods. However, the estimated size of the JSD effect is smaller in the latter time periods – more precisely, the time-series pattern across the three years shows a declining impact of JSD.

What might explain the decline in the estimated impact of JSD between time periods? We suggest that there are four main potential explanations:

(a) Change in the composition of the sample of JSD participants – An increase in the proportion of JSD participants for whom the requirement has smaller effects on payment outcomes (for example, concentration of JSD participation in high unemployment regions);

b) Change in the macro-economy – On the basis that disaggregate results indicate that the JSD impact is likely to be largest where labour demand is relatively stronger, therefore worsening macro-economic conditions might account for a decline in the impact of the JSD;

c) Change in matching quality – Findings from quasi-experimental matching analysis will be sensitive to the validity of applying that method in different time periods; and

d) Change in behaviour by JSD participants – The effect of the JSD on job search intensity may diminish over time as participants learn how to 'game' the system or as administration of the JSD becomes less rigorous (for example, international studies raise the possibility of a 'shock' effect of a program at the time of its introduction that dissipates over time – see Blundell et al., 2001).

Changes in the composition of JSD participants do not appear to explain the time-series variation in the estimated JSD impact. Table 16 presents results from a shift-share analysis of the effect of changes in the composition of JSD participants – by gender; age; and payment history. The overall impact of JSD in period s, Δ_s , can be defined as:

(7a) $$\Delta_s = \sum_{z=1}^{Z} p_{zs}\Delta_{zs}$$

where p_{zs} and Δ_{zs} are respectively the share of JSD participants in group z in period s and the average effect of the JSD for participants in group z in period s. The effect of composition changes in JSD on the change in the estimated impact of the JSD between periods s and s' is then estimated as:

(7b) $$\sum_{z=1}^{Z} p_{zs}\Delta_{zs} - \sum_{z=1}^{Z} p_{zs'}\Delta_{zs} \, .$$

The results in Table 16 show that composition changes can explain at best only a minuscule portion of the change in the estimated impact of the JSD. (Similar results are obtained for a comparison between 1997-98 and 1998-99.)

The evolution of the aggregate rate of unemployment in Australia over the sample period for this study is displayed in Figure 8. During the period the aggregate rate of unemployment was trending slowly downwards. This makes it unlikely that changes to the macro-economic environment can explain the change in the estimated effect of the JSD.

Third, we examine the possibility that changes to matching quality can explain the change in the estimated impact of the JSD. One perspective on this issue is to consider the validity of the matching method. First, the issue of validity of CIA and the geographic distribution of JSD participation can be addressed. One point is that there does appear to be significant geographic clustering in each time period, although the degree of non-randomness does decrease over time, particularly between 1998-99 and 1999-2000 – This is for example evident from Table 15 where the Ellison-Glaeser dartboard test is reported. A second point is that there does not appear to be a highly significant relation between the geographic distribution of JSD participation and local labour market conditions in any time period, although the relation is significant at the 10% level in 1999-2000. A final point is that it is known from FaCS administrative reports that discretionary exemptions accounted for a declining share or JSD exemptions from about March 1999 onwards (see Department of Family and Community Services, 2000, Chart 2.4). Hence, on the basis that the degree of randomness in assignment to the JSD seems to have been less in 1999-2000 than in previous periods, there is reason to believe that the matching method is less likely to be valid for that time period. A second perspective is available from comparing mean values of characteristics used in matching for treatment and control observations (see Appendix Table 8). Generally, this dimension of matching quality appears quite similar between years. However, it does seem that the basic approach does not do as well matching on payment history in 1999/2000 as in the two earlier time periods.

Overall, it seems that the validity of application of matching, and matching quality, is similar between 1997/98 and 1998/99, and hence cannot explain the decrease in estimated JSD effects between those periods. However, matching quality does seem somewhat weaker in 1999/2000 than in the earlier time periods, and this might explain a decline in estimated JSD effects in that period.

The final possible explanation for the time-series pattern of estimated JSD effects is behavioural effects of unemployment payment recipients associated for example with reduced job search effort due to 'learning' about the probability of monitoring, or with program administration changes. Unfortunately, only indirect methods are available for seeking to assess the behavioural explanation. One approach is to estimate JSD effects separately for JSD participants in 1998-99 and 1999-2000 who had and had not previously participated in JSD. The idea underlying this approach is that previous JSD participants may be better able to adapt their behaviour to minimize the impact of the JSD on their job search activities. The findings are presented in Table 17. In 1998-99 it appears that there are slightly stronger JSD effects for participants who had previously participated in the JSD compared to those who had not. However, in 1999-2000 this pattern is reversed for most of the outcome measures. Hence, on the basis of this comparison, there is not support for a role for behavioural effects in accounting for the reduced impact of the JSD between 1997-98 and 1998-99, but behavioural effects may have played a role in the reduced impact of the JSD in the 1999-2000 period. Moreover, it is important to highlight that this approach has not tested for the possibility that all JSD participants – regardless of previous participation experience – are able over time to reduce the extent to which their job search behaviour is affected by the JSD. A second approach is to examine time-series data on JSD-related breaches. Appendix Table 2 shows that the incidence of breaches did increase substantially across time. This increase in breaches would be consistent with a decrease in the proportion of payment recipients meeting the JSD requirement, and hence with a decrease in the average job search intensity effect due to the JSD. (For example, suppose that payment recipients can choose to meet the JSD requirement or to 'shirk' and not meet the requirement. Suppose that there is some probability, p, of being monitored and detected to have 'shirked'. Suppose also that penalties for shirking (breach) are the same, but that gains from 'shirking' are heterogenous between payment recipients. Then it is possible that, if payment recipients initially over-estimate the probability of monitoring, but learn the true probability over time, that the incidence of ‘shirking’ and hence of breaches, will also increase over time.) However, it is also possible that the increase in the incidence of breaches does not reflect a change in behaviour of JSD participants, but instead is due to a higher degree of monitoring by Centrelink.

In summary, there appears to be some evidence for the hypothesis that an explanation for the decrease in the estimated effect of the JSD is behavioural changes by JSD participants – due, for example, to learning about the probability of being detected to have not met the JSD requirement or of being penalized once detected. But such evidence must be regarded as suggestive rather than conclusive. It also appears that matching quality could partly explain the difference in estimated effects in 1999/2000. The decline in the magnitude of the estimated JSD effect cannot be attributed to composition changes in the sample of JSD participants, and most likely not to macro-economic effects.

Endnotes

1. For example, it seems accepted that public sector job creation programs will generally have only a small net impact on labour market outcomes for unemployed persons. (For recent reviews see Heckman et al., 1999; Friedlander et al., 1997; and Robinson, 2000.)

2. The difference in findings between the Ashenfelter et al. (1998) and Klepinger et al. (2002) studies appears to be explained by the longer period post-program time eriod that is examined in the latter study (see Klepinger at al., 2002, pp.18-19.

3. 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) (Department of Family and Community Services, 2000).

4. Appendix Table 1 shows that the proportion of NSA/YA(o) recipients undertaking JSD in each quarter in 1997/98 in the LDS 10% sample is extremely close to the proportion in the overall population of NSA/YA(o) recipients.

5. 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).

6. 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 in which 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.

7. 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.

8. In making this comparison control group observations are weighted using the same kernel weights 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. 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).

10. 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.

11. At the individual-level, for the sample of payment spells examined in 1997/98 it is known that exemptions from the JSD in this period were primarily 'discretionary' rather than being in exemption categories that explicitly related to participants' characteristics likely to be associated with payment outcomes such as disability or literacy. The definition of spell commencements, and the definition of the treatment group as payments recipients whose JSD participation begins in the first fortnight of their payment spell, means that JSD participation and exemptions associated with abridged spells are excluded from the study. Hence it follows that two-thirds of exemptions during 1997/98 are 'discretionary' (Department of Family and Community Services, 2000, Table 2.4).

12. 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'.

13. This explains why – for example - the total number of treatment observations in the quasi-experimental analysis (see Table 7) is less than the number of NSA/YA(o) payment recipients who commence a JSD spell in the first fortnight of their payment spell (see Table 4).

14. JSD commenced in July 1996 – hence the pre-program test must incorporate only payment spells that commence before that date. The LDS data set only provides information on payment receipt from January 1995 onwards. Hence, to be able 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.

15. The 92 groups 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.

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

Not on JSD	On JSD
15263 (26.4)	42516 (73.6)

Table 2: 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 spells 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 3: 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 4: 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

	Number	Percent	Cumulative Percent
Fortnight			
1	40942	96.3	96.3
2	264	0.6	96.9
3	145	0.3	97.2
4	80	0.2	97.4
5	37	0.1	97.5
6	53	0.1	97.6
7	67	0.2	97.8
8	42	0.1	97.9
9	22	0.05	97.95
10	14	0.05	98.0
11	14	0.05	98.05
12	24	0.05	98.1
13	34	0.1	98.2
14-26	479	1.1	99.3
27-52	139	0.3	99.6
52+	160	0.4	100.0

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)	(Diff.)		
67 regions	0.000306977	1.78742E-05	0.000289	3.40619E-06	84.87564983

Table 6: 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

Table 7: 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 8: 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)

Table 9: 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)

Table 10: 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)**	**(4)**
	Basic method	**Control group – Never on JSD**	**Exit off all payments**	**Treatment group – Begin JSD spell in fortnight 2**
% Off payments				
By 3 months	+5.1 (0.000)	+4.5 (0.000)	+7.4 (0.000)	+0.5 (0.869)
By 6 months	+4.3 (0.000)	+4.1 (0.000)	+7.9 (0.000)	+2.3 (0.437)
% On payments				
At 6 months	-4.6 (0.000)	-4.6 (0.064)	-8.4 (0.000)	-1.2 (0.702)
At 12 months	-4.3 (0.000)	-4.5 (0.000)	-9.0 (0.000)	-1.4 (0.637)
Time on payments (Fortnights)				
First 6 months	-0.409 (0.000)	-0.390 (0.000)	-0.701 (0.000)	-0.025 (0.915)
First 12 months	-0.930 (0.000)	-0.917 (0.000)	-1.799 (0.000)	+0.015 (0.976)

Table 11: 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%

Table 12: '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 13: 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 14: 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:					
	% Off payments		**% On payments**		**Time on payments (Fortnights)**	
	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 15: Effects of JSD – NSA/YA(o) recipients aged 18 to 49 years with at least one fortnight on JSD – July 1997 to June 2000

	Difference in outcome:		
	(1)	**(2)**	**(3)**
	1997-98	**1998-99**	**1999-2000**
% Off payments			
By 3 months	+5.1 (0.000)	+2.2 (0.000)	+1.5 (0.000)
By 6 months	+4.3 (0.000)	+2.8 (0.000)	+1.9 (0.000)
% On payments			
At 6 months	-4.6 (0.000)	-2.3 (0.000)	-1.6 (0.000)
At 12 months	-4.3 (0.000)	-2.5 (0.000)	-1.9 (0.000)
Time on payments (Fortnights)			
First 6 months	-0.409 (0.000)	-0.094 (0.000)	-0.039 (0.080)
First 12 months	-0.930 (0.000)	-0.396 (0.000)	-0.266 (0.006)
Ellison-Glaeser dartboard test = (G-E(G))/SD(G)	84.8	74.3	13.4

Table 16: Effects of JSD – NSA/YA(o) recipients aged 18 to 49 years with at least one fortnight on JSD – Effect of change in composition of JSD participants on estimated JSD effect – 1997-98 minus 1999-2000

		Effect of composition effects		
	(1)	**(2)**	**(3)**	**(4)**
	1997-98 minus 1999-2000	**Gender**	**Age**	**Payment history**
% Off payments				
By 3 months	3.6	-0.036	0.013	0.038
By 6 months	2.4	-0.035	0.048	0.007
% On payments				
At 6 months	-3.0	0.032	-0.010	-0.064
At 12 months	-2.4	0.024	-0.013	-0.105
Time on payments (Fortnights)				
First 6 months	-0.370	0.003	0.001	-0.007
First 12 months	-0.664	0.007	-0.005	-0.018

Note: Derived using shift-share analysis with 1997-98 share weights as base.

Table 17: Effects of JSD – NSA/YA(o) recipients aged 18 to 49 years with at least one fortnight on JSD – By whether have previous participation in JSD – July 1998 to June 2000

	1998-99		**1999-2000**	
	(1)	**(2)**	**(3)**	**(4)**
	Previous JSD	**No previous JSD**	**Previous JSD**	**No previous JSD**
% Off payments				
By 3 months	+2.2 (0.000)	+2.1 (0.000)	+3.3 (0.000)	+1.8 (0.000)
By 6 months	+3.6 (0.000)	+2.4 (0.000)	+2.0 (0.000)	+3.0 (0.000)
% On payments				
At 6 months	-3.0 (0.000)	-2.0 (0.000)	-1.5 (0.001)	-3.1 (0.000)
At 12 months	-2.6 (0.000)	-2.2 (0.000)	-1.4 (0.001)	-2.9 (0.000)
Time on payments (Fortnights)				
First 6 months	-0.131 (0.038)	-0.075 (0.006)	-0.192 (0.000)	-0.090 (0.012)
First 12 months	-0.526 (0.000)	-0.335 (0.000)	-0.345 (0.000)	-0.466 (0.000)

Appendix Table 1: Proportion of NSA/YA(o) payment recipients with JSD – 18 to 49 years

	DFaCS administrative data	10% LDS sample
15 August 1997	12.4	12.5
21 November 1997	10.4	10.2
27 February 1998	13.4	13.3
22 May 1998	10.2	10.2

Appendix Table 2: JSD related breaches, 1997/98 to 1999/2000

Year	Average no. of breaches per fortnight	Percentage of breaches to all on going spells per fortnight
1997/98	3.8182	0.0041
1998/99	16.2308	0.0170
1999/2000	35.0385	0.0421

Notes: (a) Data for 1997/98 are for May to September 1998; 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; and (b) Data on JSD-related breaches are only available for May 1998 onwards.

Appendix Table 3: Regression results – Relation between rate of unemployment and JSD participation – By ABS Labour Force Region

Dependent variable: Proportion of payment recipients participating in JSD

	Rate of ue		Constant		Observations
	Coefficient	p-value	Coefficient	p-value	
1997-98	-0.0068	0.182	0.7982	0.000	67
1998-99	-0.0054	0.202	0.7412	0.000	67
1999-2000	-0.0042	0.079	0.7949	0.000	67

Appendix Table 4: 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 5: Payment spells of NSA/YA(o) recipients aged 18 to 49 years with at least one fortnight on JSD, July 1997 to June 2000

	1997-1998	**1998-1999**	**1999-2000**
Not on JSD	15263 (26.4)	15389 (30.5)	11822 (23.5)
On JSD	42516 (73.6)	34988 (69.5)	38439 (76.5)

Appendix Table 6: 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 2000

	1997-98		**1998-99**		**1999-2000**	
	JSD	**non JSD**	**JSD**	**non JSD**	**JSD**	**non JSD**
Gender						
Male	67.4	68.8	68.2	66.2	68.2	61.8
Female	32.6	31.2	31.8	33.8	31.8	38.2
Age						
18-24	47.0	36.3	43.5	40.6	44.2	49.7
25-34	31.9	32.7	34.3	29.4	33.1	23.8
35-49	21.1	31.1	22.2	30.1	22.7	26.5
Unemployment history in previous 4 quarters						
Not on UE payment	65.1	49.7	66.1	56.6	62.6	61.3
Not frequent/Not recent	22.7	24.8	22.0	22.7	23.6	21.1
Not frequent/Recent	9.5	16.4	9.1	12.0	9.9	10.6
Frequent/Not recent	1.1	3.6	1.2	3.7	1.7	3.0
Frequent/Recent	1.6	5.6	1.6	5.0	2.2	4.0
Rate of unemployment – Local labour market						
1st quartile (Lowest rate of ue)	21.9	21.8	22.5	22.3	23.7	22.9
2nd quartile	27.4	25.2	25.9	23.6	25.3	25.3
3rd quartile	23.7	22.5	22.6	26.7	24.4	23.4
4th quartile (Highest rate of ue)	27.0	30.6	29.0	27.4	26.6	28.4

Appendix Table 7: 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 2000

	1997-1998			**1998-1999**			**1999-2000**		
	Number	**Percent**	**Cumulative Percent**	**Number**	**Percent**	**Cumulative Percent**	**Number**	**Percent**	**Cumulative Percent**
Fortnight									
1	40942	96.3	96.3	31877	91.1	91.1	36426	94.8	94.8
2	264	0.6	96.9	1347	3.8	95.0	318	0.8	95.6
3	145	0.3	97.2	220	0.6	95.6	157	0.4	96.0
4	80	0.2	97.4	108	0.3	95.9	113	0.3	96.3
5	37	0.1	97.5	62	0.2	96.1	87	0.2	96.5
6	53	0.1	97.6	63	0.2	96.3	133	0.4	96.9
7	67	0.2	97.8	59	0.2	96.5	127	0.3	97.2
8	42	0.1	97.9	49	0.1	96.6	103	0.3	97.5
9	22	0.05	97.95	31	0.1	96.7	73	0.2	97.7
10	14	0.05	98.0	30	0.1	96.8	50	0.1	97.8
11	14	0.05	98.05	33	0.1	96.9	48	0.1	97.9
12	24	0.05	98.1	38	0.1	97.0	39	0.1	98.0
13	34	0.1	98.2	56	0.2	97.2	34	0.1	98.1
14-26	479	1.1	99.3	603	1.7	98.9	550	1.4	99.5
27-52	139	0.3	99.6	345	1.0	99.9	181	0.5	100.0
52+	160	0.4	100.0	64	0.2	0.1	0	0	100.0

Appendix Table 8: Comparison of means of treatment and control group observations - NSA/YA(o) recipients aged 18 to 49 years, July 1997 to June 1998

	1997-98		**1998-99**		**1999-00**	
	Difference	**p-value**	**Difference**	**p-value**	**Difference**	**p-value**
Age	+0.095	0.013	+0.02	0.645	-0.208	0.000
Immigrant status/Ethnicity						
%ESB	0.000	0.897	0.001	0.692	-0.003	0.443
%NESB	0.000	0.796	0.002	0.219	0.001	0.389
%ATSI	-0.000	0.000	-0.002	0.020	-0.004	0.000
Marital status/Children						
%Married – Partner not on payment	0.000	0.970	0.000	0.812	-0.002	0.008
%Married – Partner on payments	0.002	0.377	-0.004	0.057	-0.002	0.327
Have children	0.002	0.342	-0.002	0.350	-0.001	0.534
Have child under 6	0.001	0.491	-0.001	0.378	0.000	0.840
Have child under 13	0.001	0.478	-0.002	0.281	0.000	0.816
Payment history						
No payment history	0.000	0.971	0.007	0.002	0.017	0.000
Not frequent/ not recent	0.002	0.385	-0.004	0.052	-0.010	0.000
Frequent/not recent	0.000	0.724	0.001	0.392	-0.005	0.001
Not frequent/recent	-0.001	0.107	-0.002	0.001	-0.001	0.331
Frequent/recent	-0.001	0.008	-0.002	0.000	-0.001	0.059
Unemployment rate at spells start	0.018	0.193	-0.002	0.893	-0.022	0.059

Figure 1: Distribution of number of JSD contacts - NSA/YA(o) recipients aged 18 to 49 years -
July 1997 to June 1998 - By fortnight

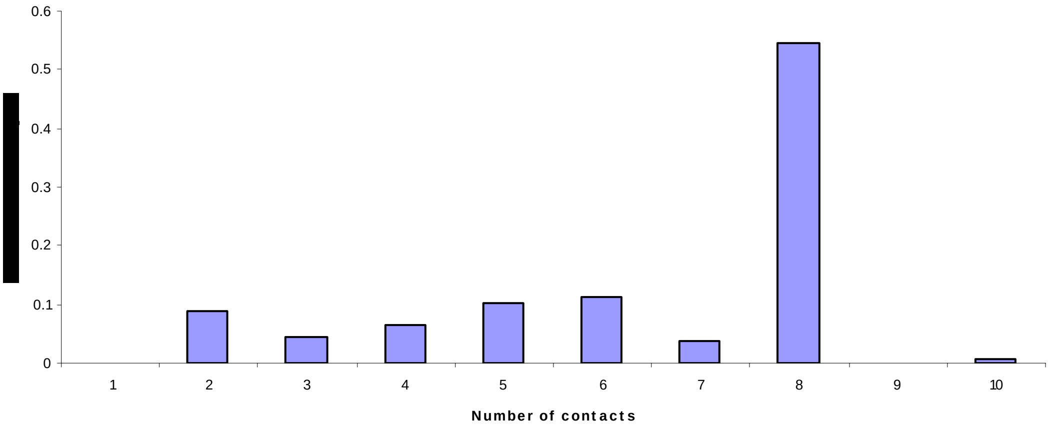


Figure 2: Proportion of treatment and control observations participating in JSD – By payment spell duration (fortnight)

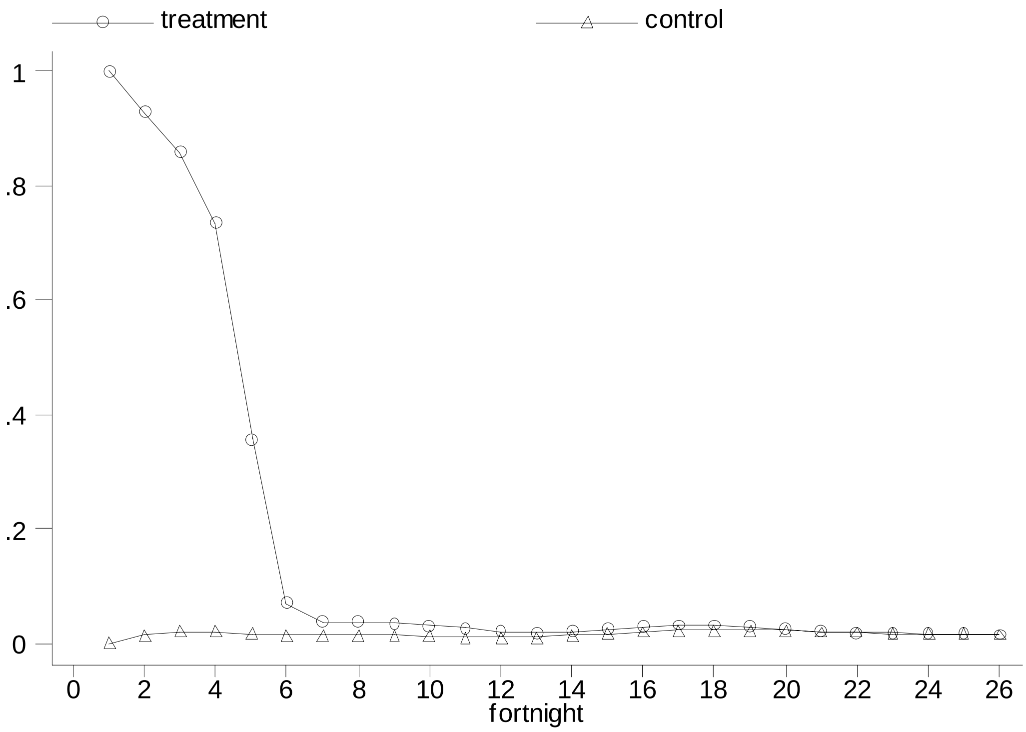


Figure 3: Proportions of all new payment spells and JSD spells by ABS Labour Force Region - NSA/YA(o) recipients aged 18-49 years - July 1997 to June 1998

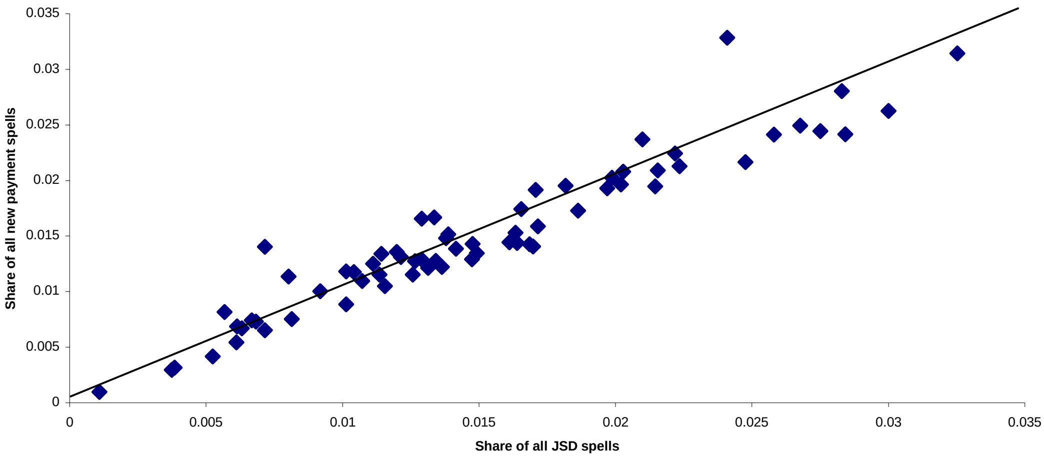


Figure 4: Rate of unemployment and incidence of participation in JSD by ABS Labour Force Region – NSA/YA(o) recipients aged 18 to 49 years – July 1997 to June 1998

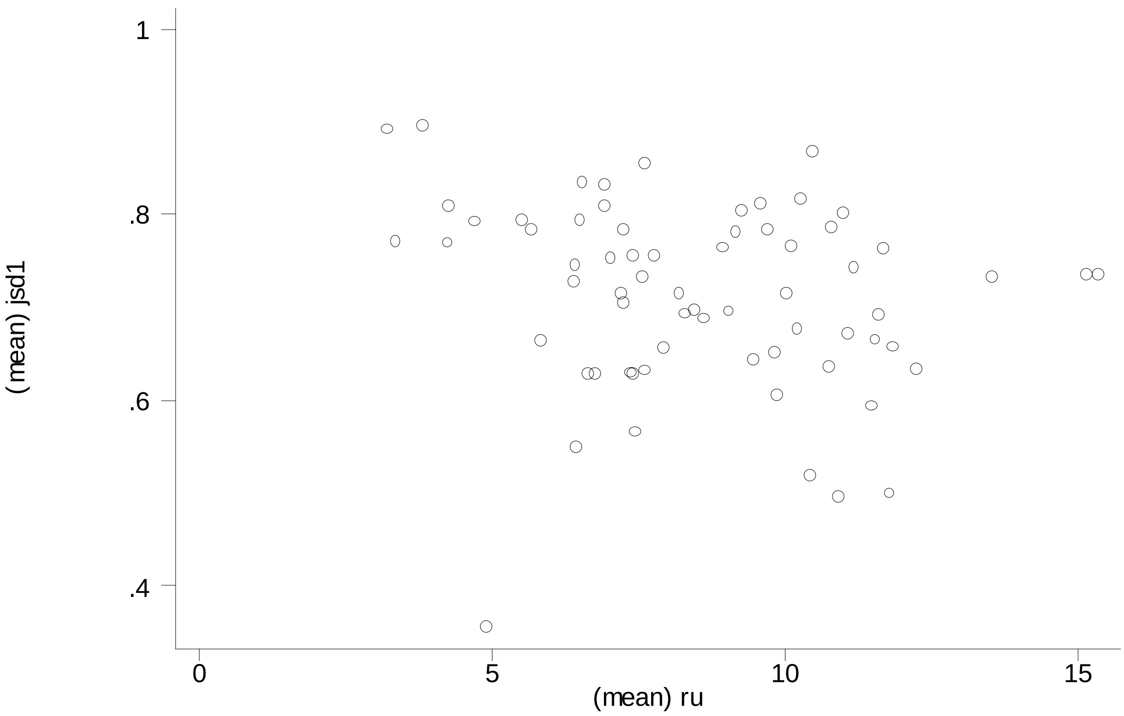


Figure 5: Predicted probability of commencing in JSD in first fortnight of payment NSA/YA(o) spell – July 1997 to June 1998

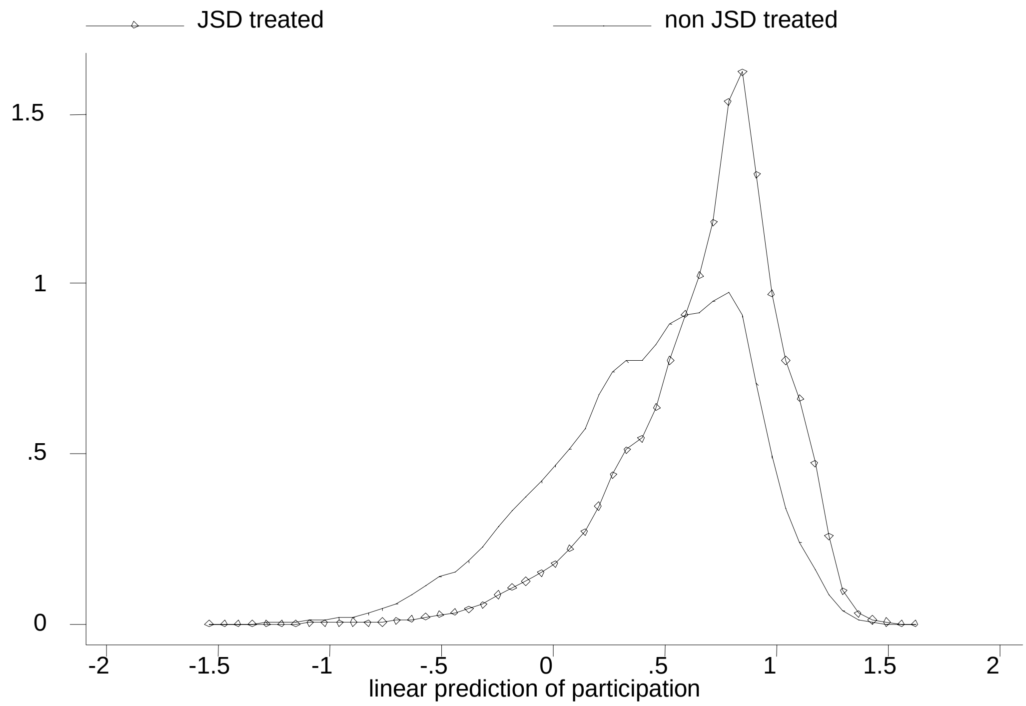


Figure 6a: NSA/YA(o) payment recipients exiting payments - By month after commencement of JSD (New spells commencing July 1997 to June 1998)

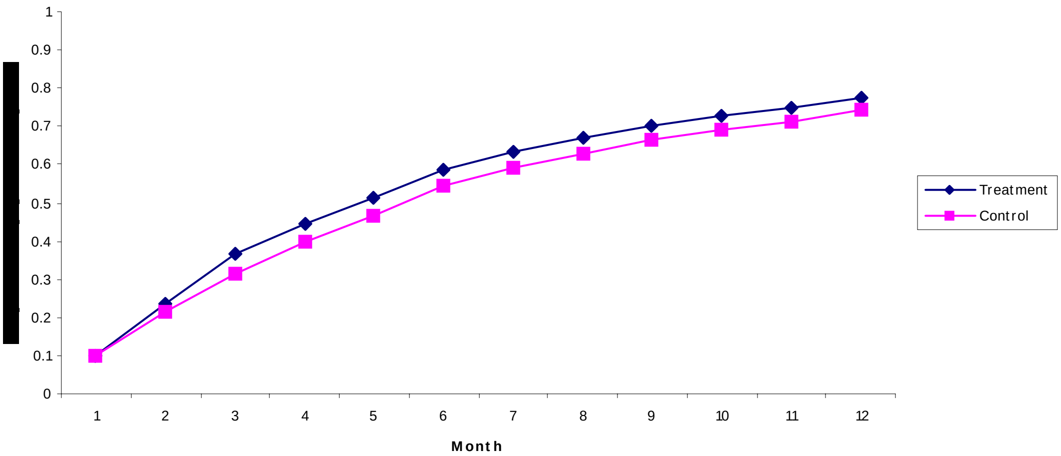


Figure 6b: NSA/YA(o) payment recipients - Difference in proportion of treatment and matched control groups exiting payments by month after JSD commencement (New spells commencing July 1997 to June 1998)

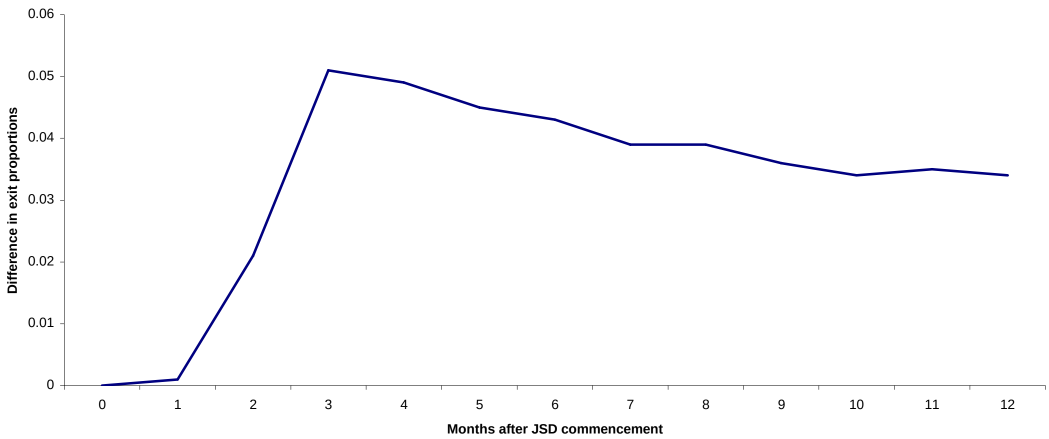


Figure 7: NSA/YA(o) payment recipients - Difference in proportion of treatment and matched control groups exiting payments by month after JSD commencement
(New spells commencing July 1997 to June 2000)

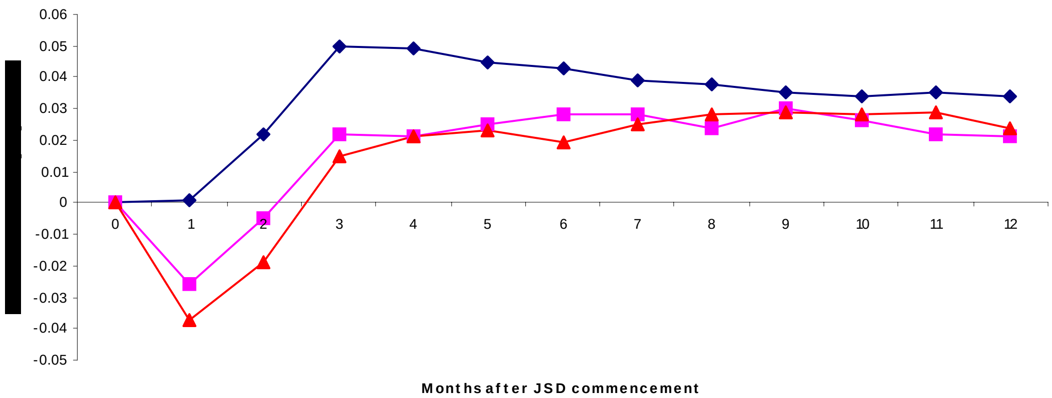
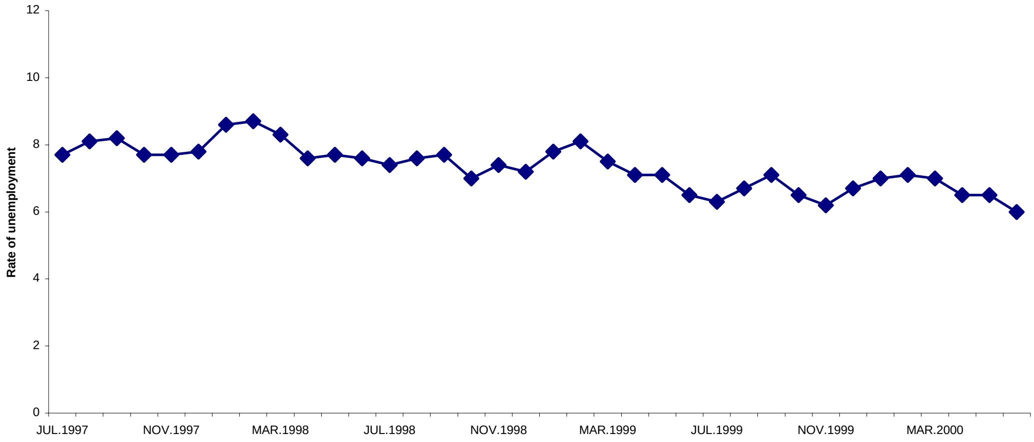


Figure 8: Rate of Unemployment - Persons - Australia - July 1997 to June 2000



Source: ABS, 6291.0.40.001 Labour Force UR Rate, Table 91.