Does 'Work for the Dole' work?

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Executive Summary

Objective

This project examines the effect of the introduction of the Work for the Dole (WfD) program on exit from payments of Newstart allowance (NSA) recipients. The focus of the analysis will be the 'pilot phase' when WfD operated between November 1997 and June 1998 prior to the introduction of Mutual Obligation (MO).

What is Work for the Dole?

The objectives of the WfD scheme are to provide opportunities for unemployed people to gain work experience; build networks; improve their self-esteem, communication skills, and motivation; and contribute to projects that are of value to the community. Under the pilot phase of WfD eligible unemployment payment recipients were required to participate in specified projects of benefit to the community. The target population for the scheme was recipients of NSA aged 18 to 24 years on full rate of income support who had been in receipt of income support for at least six months. Participation in WfD was required for a maximum of six months; working for six hours per day for two days if aged 18 to 20 years, and working for six hours per day for two and a half days if aged 21 to 24 years.

How are the effects of WfD measured?

Effects of WfD are examined for a variety of outcome measures related to receipt of unemployment payments –
(i) The effect of WfD on the incidence of exit from payments by 6 months and 12 months after WfD commencement;
(ii) The effect of WfD on whether payment recipients are on payments at 9 months and 15 months after WfD commencement; and
(iii) The effect of WfD on the number of fortnights on payments during the 6 months and 12 months after WfD commencement.

What can be learned from this study?

In analysing the effect of WfD on exit from payments this study is obviously focused on just a subset of its objectives; and this must be taken into account in seeking to extrapolate from the findings of this study to an overall assessment of the program. It is also the case that the study focuses on the very earliest stage of the program, so it is necessary to be cautious in generalising the findings to WfD today; on the other hand, we argue that there are several reasons for thinking that there are indeed lessons in the study for WfD today.

Descriptive information

There are 888 payment spells during the period from November 1997 to June 1998 where a NSA recipient is observed to participate in WfD. Commencement of participation in WfD during the pilot phase does not appear to be highly concentrated at any single point in payment spell duration. Over 50 per cent of participants began

participation between 13 and 26 fortnights, about 25 per cent between 27 and 52 fortnights, and about 10 per cent at longer than 52 fortnights. There appears to be quite a high degree of geographic concentration of participation in WfD between ABS Labour Force regions. For example, about 10% of the population of individuals aged 18 to 24 years with payment spells during the sample period are in regions where there is zero participation in WfD; by contrast, 53.2% of participants in WfD are in regions where only 21.2% of those with payment spells reside.

Empirical methodology

The empirical approach used to estimate the effect of the WfD is a quasi-experimental matching method. Fundamentally, this involves comparing payment outcomes for a treatment group of NSA recipients who participate in WfD, and a matched control group who never commence WfD. Hence the policy effect that is identified is the average effect of commencing participation in WfD for payment recipients aged 18 to 24 years who commence participation during the 'pilot' phase compared to matched payment recipients who never commence participation in WfD.

The type of quasi-experimental matching method used in this study is an 'exact matching method'. Treatment group observations (participants in WfD) are matched with a control group observation(s) with the same: Payment spell duration; Quarter spell on WfD commences; Age; Gender; Country of birth; ATSI status; Marital status; Activity type in previous fortnight; Rate of unemployment in ABS Labour Force region; and Payment history over previous 12 months. With this approach each treatment observation can potentially be classified in one of 3,369,000 cells.

The effect of WfD – Basic results

The main conclusion is that there appear to be quite large significant adverse effects of participation in WfD. For example, for the group of matched WfD participants it is found that there is a difference in exit from NSA payments between WfD participants and non-participants equal to minus 12.1 percentage points; the difference in the proportions of WfD participants and non-participants on NSA payments at 9 months (after start of spell on WfD) is 8.5 percentage points; and the difference in fortnights on NSA payments between WfD participants and non-participants (in the first 6 months after start of spell on WfD) is 0.99 fortnights. Each of these differences is statistically significant at the 1% level.

Outcome measure	Difference in outcomes: WfD participants minus non-participants
% Off payments	
By 3 months	-12.1
By 6 months	-10.5
% On payments	
At 6 months	+8.5
At 12 months	+8.9
Time on payments (fortnights)	
First 6 months	+0.99

First 12 months	+2.20

More detailed analysis of exit from payments suggests that there is an adverse effect of WfD on exit from payments associated directly with the period of participation in WfD, but that there is partial 'catch-up' by WfD participants after the conclusion of WfD.

How robust are the findings?

Several methods for assessing the robustness of the 'basic' empirical approach are used. One method addresses the issue that some WfD participants are excluded from the 'basic' analysis because no matching control observations exist. Results from this method (bounds analysis) suggest that the distribution of possible outcomes is heavily towards adverse effects of WfD; although it is not possible to exclude the possibility of a zero or small positive effect on receipt of payments from WfD participation. The second method is to test for whether referral effects of WfD (whereby payment recipients exit payments on referral to WfD) might have biased estimates of the effect of WfD. The answer appears to be negative. The third type of analysis is to examine how estimates of the WfD effect are changed by introducing extra variables to match between WfD participants and control group payment recipients. The findings are found to be not significantly altered by use of education attainment in this way. Hence, we make the general conclusion is that the results are highly robust.

The effect of WfD – Results for sub-groups

Effects of WfD for disaggregated groups of payment recipients can be estimated, and are of potential interest. WfD effects are found to be slightly more adverse for females than males; and more adverse for payment recipients aged 21 to 24 years than for those aged 18 to 20 years. Participation in WfD has large negative effects on labour market outcomes for payment recipients who have received payments in at least three quarters in the previous 12 months compared or who did not receive payments in the previous 12 months, but has no significant effect for payment recipients who received payments in one or two quarters in the previous 12 months. Hence there is not a consistent ordering of WfD effects by payment history. Between geographic regions, WfD participation has a significant adverse effect on labour market outcomes for participants in regions with above-median rates of unemployment, but generally no significant effect in below-median rate of unemployment regions. It is also found that WfD effects are more adverse for NSA recipients who had not participated in JSD in the previous 12 months, whose WfD classification was compulsory (compared to voluntary), and who did not have earnings from labour market activity in the fortnight prior to commencing in WfD.

Why might WfD have adverse effects on exit from unemployment payments?

What might explain negative effects of WFD participation on exit from unemployment payments? There appear to be three main potential explanations: (i) Stigma effects; (ii) Effect on job search activity – Participation in WfD may allow participants to reduce their job search activity, and may adversely affect the type of job search activity undertaken; and (iii) Scale of intervention – The WfD program

represents a relatively 'minimalist' intervention. Of these explanations, the potential 'chilling' effect of WfD on job search activity, seems to be most supported by international evidence. This explanation also seems to be supported by the time-series pattern of WfD effects (increasingly adverse during the six-month phase of participation in WfD, but then reversing to some extent after that time). However, there are some aspects of our findings that are more difficult to reconcile with effect of WfD on job search activity being the main explanation for the negative estimates of the program impact.

1. Introduction

This project examines the effect of the introduction of the Work for the Dole (WfD) program on exit from payments of Newstart allowance (NSA) recipients. The focus of the analysis will be the 'pilot phase' when WfD operated between November 1997 and June 1998 prior to the introduction of Mutual Obligation (MO).

The main motivation for the project is the importance that the WfD program has assumed as part of activity test requirements for unemployment payment recipients. For example, in 2002-03 the federal government provided funding for 55,000 WfD places. WfD is the 'residual' category of activity for payment recipients undertaking a MO requirement; and payment recipients not required to participate in MO may still be directly referred to WfD.

Despite the importance of the WfD program, there has thus far been little analysis of its effects. There has been only two studies of the 'net impact' of the WfD program, and we argue that the findings in those studies are problematic due to methodological issues (see literature review in section 3 for details). More generally, there appear to be no other evaluations of the net impact of work experience programs in Australia. This is perhaps surprising, given that the international literature raises significant doubts about whether such programs will have positive effect on labour market outcomes for participants.

In this study a quasi-experimental matching method is used to estimate the effects of participation in WfD on durations of unemployment payment spells. Since – during the pilot phase of WfD - unemployment payment recipients could commence WfD participation at different payment spell durations, it is necessary for the matching method to accommodate this complexity. These issues are addressed in this study by estimating a policy effect that is the weighted average outcome from commencing participation in WfD in a given fortnight of a payment spell, compared to outcomes for payment recipients who never commence WfD during their payment spells; and then aggregating across all payment spell durations where some payment recipient commences on WfD. 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 WfD. 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 WfD during the 'pilot' phase – specifically, the geographic distribution of WfD participation due to limits on project funding – suggest that there is a significant source of randomness in assignment of unemployed persons between participating and not participating in the JSD.

The WfD program is on-going, yet in this report we choose to focus on the initial 'pilot phase' of the program. There are three main reasons. First, after 1 July 1998 WfD became part of the Mutual Obligation (MO) program. Under MO, unemployment payment recipients aged 18 to 24 with payment spells of at least six months are required to participate in one of a range of possible activities (including WfD). As payment recipients can choose the activity that best suits their interests, under MO it is not likely that participants in WfD constitute a random sample of payment recipients, and it will be more difficult to satisfy the assumption – necessary to implement matching quasi-experimental methods – that there is a group of non-WfD participants who, after controlling for differences in observable characteristics, are 'identical' to WfD participants. Second, on 1 July 1998 Youth Allowance (other) (YA(o)) replaced NSA for payment recipients aged 18 to 20 years. The YA(o) differed from the NSA in that eligibility and payment amount depend on a parental means test; hence it is likely that the eligible population for WfD is different before and after 1 July 1998. A third reason is that the relatively small number of WfD participants in the 'pilot' phase allows an 'exact' method of quasi-experimental matching to be applied.

Because our results are obtained for a 'pilot phase' of the WfD program, it would of course be necessary to exercise caution in using those results to make judgements about outcomes from WfD in more recent time periods. On the other hand, it cannot simply be assumed that the findings in this study are uninformative about WfD outcomes today. First, it is shown that our results are quite similar to findings from international studies on work experience programs. Second, although details of the operation and administration of WfD have evolved since the 'pilot phase', the essential objectives and nature of the program have not changed.

Section 2 describes the WfD program. Section 3 presents a review and evaluation of some relevant literature. Section 4 describes the data source, and sample of payment recipients, used in this study, and in section 5 descriptive information on the sample of 'pilot' phase WfD participants is presented. Section 6 provides a brief overview of predictions about the effect of a program such as WfD on unemployment from standard search theory models of the labour market. The empirical methodology is described in section 7, and results are presented in section 8. An evaluation of the results is undertaken in section 9.

2. The WfD program

The objectives of the WfD scheme were (and continue to be) to provide opportunities for unemployed people to (Department of Family and Community Services, 2002a, section 3.2.8.80):

i)   Gain work experience;

ii)   Build networks;

iii)   Improve their self esteem, communication skills, and motivation; and

iv)   Contribute to projects that are of value to the community.

Under the pilot phase of the scheme eligible unemployment payment recipients were required to participate in specified projects of benefit to the community. The target population for the scheme was recipients of NSA aged 18 to 24 years on full rate of income support who had been in receipt of income support for at least six months. Participation in WfD was required for a maximum of six months; working for six hours per day for two days if aged 18 to 20 years, and working for six hours per day for two and a half days if aged 21 to 24 years. Participants were paid a WfD supplement of $20 per fortnight in recognition of working costs.

The obligation to participate in MOI derives from social security legislation on eligibility conditions for receipt of unemployment benefit payments. The Social Security Act 1991 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 (Section 601). Subject to meeting the activity test requirement, there is no time limit on the duration for which unemployment payments can be claimed in Australia.

As a program with multiple objectives, the assessment in this study of whether WfD achieves a specific objective – improving labour market outcomes for participants - can only constitute a partial test of its performance. Nevertheless, it does seem valid to undertake such an analysis of the effect of WfD. Work experience is stated as an objective of the program, and it would seem difficult to argue that meaningful work experience is being provided in the absence of evidence that the work experience has improved participants' labour market outcomes (over some time horizon) and thereby reduced receipt of welfare payments. (Originally, the work experience objective was expressed as being to 'develop work habits' in young people – DEWR, 1999.) As well, there is more direct evidence of an expectation that the Work for the Dole program would improve labour market outcomes. For example, on its introduction the Prime Minister, John Howard, stated that '…the government will for the first time introduce, as part of the armoury of the policies it is employing to attack the problem of youth unemployment, a work for the dole scheme…' (Commonwealth of Australia, House of Representatives, 10 February 1997; Hansard volume H, R211-212, p.466).

WfD services are managed by Community Work Coordinators, and delivered through community or government organisations or agencies. During the 'pilot' phase 174 projects commenced. Projects in the 'pilot' phase involved environmental work such as tree planting, construction projects such as building cycling and walking tracks, maintenance of community facilities, and work in hospitals and the aged care sector undertaking administrative tasks and roles such as meals on wheels delivery (Department of Employment and Workplace Relations, 1999, pp.2, 33).

3. Literature review

Three studies of the effects of WfD have been undertaken. One study has specifically examined outcomes from the 'pilot' phase of WfD (Department of Employment and

Workplace Relations, 1999). That study was primarily concerned with a description of outcomes for project participants, and analysis of qualitative data on their attitudes to the program. The other studies have sought to examine the 'net impact' of WfD on labour market outcomes – first, for a sample of payment recipients who completed spells on WfD in late 1999 (Department of Employment and Workplace Relations, 2000); and second, for a sample of payment recipients who commenced payment spells between January and June 1998 and commenced Mutual Obligation activities between July and December 1998 (Richardson, 2003).

The 'pilot' phase study involved analysis of employment outcomes for WfD participants, case studies of WfD projects, and in-depth interviews with some participants. The main findings were that the program received a high degree of support from stakeholders such as participants and communities, that WfD was meeting its objectives of developing work habits, and that over 50 per cent of WfD participants had some spell of employment in the three months after exiting from the program.

The DEWR (2000) analysis of the net impact of WfD involved a comparison of labour market outcomes for 2100 WfD participants who left a WfD project in August 1999 with outcomes for 2100 matched payment recipients who had not participated in or been referred to WfD in the previous six months. Matching between participants and non-participants was on the basis of age, gender and duration of payments. The main finding from the study is that over the first five months after exit from WfD the average proportion of WfD participants off payments is 30 per cent whereas for non-participants the proportion is about 17 per cent. In the first month after ending participation in WfD the difference in the proportion off payments is about 20 percentage points; by the fifth month this has declined to 5 percentage points. It is estimated that 80 to 90 per cent of those who move off payments from WfD are shifting to work or education. The other main dimension of this study is analysis of the effect of referral to WfD. It is found that there appears to be a significant referral effect of WfD. For example, it is estimated that about one-third of payment recipients referred to WfD during 1999 did not commence participation; and furthermore, rates of exit from payments are similar for those who are referred to WfD but do not commence, as for those who commence.

Significant criticisms of the methodology applied in the net impact study of WfD have been raised (Productivity Commission, 2002, Appendix E, and OECD, 2001, p.220). One problem derives from the assumption in the net impact methodology that participation in WfD is 'time out of the labour force'. But during the period of WfD participation it is possible that some payment recipient will exit payments. Hence, the DEWR methodology matches a group of program participants, some of whom have already left payments, with a group of non-participants, all of whom are unemployed on payments. Other things equal, this will tend to bias upward estimates of the net impact of WfD. The second problem is that the group of WfD participants is matched to a group of non-participants using duration of spell on unemployment payments prior to WfD participation. Hence, WfD participants will on average have longer payment spells than non-participants. Since the probability of finding employment is likely to decline with duration of payment spell, therefore, the effect of longer average spell duration is that WfD participants will have lower probabilities of exiting payments than non-participants. In other words, this problem imparts a downward bias to estimates of the net impact of WfD.

Analysis of the net impact of WfD by Richardson (2003) is part of a more general study of the effect of participation in Mutual Obligation (MO) activities. The main sample group is payment recipients aged 23-24 years who participate in an MO activity between July and December 1998. A 'matched difference-in-difference' method applied is to compare outcomes for MO participants within each stream of activity to a control group of matched payment recipients aged 25-26 years; and then to difference with the difference in outcomes for matched samples of payment recipients aged 23-24 years and 25-26 years in a pre-MO sample period. The main finding with regard to WfD is that there is no significant effect of participation on financial dependence on income support – either in the 6 or 12 months after participation in WfD.

A difficulty with the Richardson study is that the validity of the matching methodology applied requires that – conditional on the set of matching covariates used – assignment between participation in WfD and the control group should be random (see for example, Gerfin and Lechner, 2002 for discussion of necessary

conditions for identification of program effects where participants in a program may enter different streams of types of programs). This random assignment requirement seems problematic in the case of assignment to WfD. For example, selection between the MO activities of part-time work and WfD is likely to involve those payment recipients who are already in part-time work choosing part-time work as their activity, and individuals without labour market attachment being assigned to WfD. (In general, we would expect MO activity selection to be such that payment recipients with the 'worst' labour market prospects to be assigned to WfD. This is consistent with evidence presented in Richardson's study that shows the likelihood of participating in WfD is positively related to time on payments in previous time periods.)

The key question is whether the set of matching covariates used are sufficient to control for all unobserved differences between WfD participants and the control groups. Certainly, the problem of selection bias is mitigated by comparing outcomes between WfD participants and a control group who do not participate in MO (rather than for example, MO participants whose activity is part-time work), and by using payment history as a matching variable. Nevertheless, in the absence of evidence of a mechanism (or natural experiment) that suggests random assignment between MO activities, and with evidence that WfD participants are particularly disadvantaged in their labour market prospects, there must be concern about the validity of the matching methodology. To the extent that the 'unobserved' characteristics of WfD participants are associated with worse labour market outcomes, it suggests that Richardson's study will under-estimate the effect of WfD.

More generally, findings from studies of other types of work experience programs are relevant background for the WfD. For Australia the main type of work experience programs related to WfD have been public sector job creation schemes such as New Work Opportunities in the mid-1990s. Studies of these programs have focused on describing outcomes for participants. For example, Webster (1998, p.198) offers the following review of these studies:

"…existing studies [of public sector job creation schemes] have placed little emphasis on estimating whether the subsidised participant would have gained a job in the

absence of the program. Most evaluations have concentrated on the post-program effects, which generally are assessed to be marginally positive or negligible."

Recent reviews of studies of work experience programs in Europe and the United States are fairly dismal in their assessment of the likely effects of those programs. From reviews of European policies Kluve and Schmidt (2002, p.439) conclude that '…both direct job creation and employment subsidies in the public sector almost always seem to fail'; and Robinson (2000, p.24) suggests that '…there is no convincing evidence that work programs improve the employment chances of participants'. Heckman et al. (1999, p.2053) summarise evidence on government employment programs for the United States. The main findings are that: (i) On average these programs appear to have 'at best a modest positive impact on adult earnings' but 'no impact on youth's earnings'; (ii) There is a high level of heterogeneity in estimated programme impacts; and (iii) There is some evidence that the largest program effect is for low-skill labour force participants.

4. Data source and sample

a. Data source

The database for this study is the Department of Family and Community Services Longitudinal Administrative Data Set (LDS). 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. In this study a special-purpose data set from the LDS is used - a 20 per cent sample of unemployment payment recipients who had payment spells commencing between 1 January 1997 and 30 June 1999.

The LDS has advantages and disadvantages for evaluating the impact of WfD. 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.[1] On each of these criteria the LDS performs well. First, data on WfD participants (treatment group) and WfD 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

In this study the potential sample of payment recipients is any person receiving NSA at some time between 1 October 1997 and 30 June 1998. WfD participation is identified from the 'WfD – Compulsory' and 'WfD – Voluntary' activity type variables in the LDS. A payment recipient is classified as being a participant in the 'pilot' phase of WfD if their activity type in some fortnight during the period between 1 October 1997 and 30 June 1998 is one of the WfD categories.[2]

To measure payment spell duration, it is necessary to have a procedure for defining the starting date for a payment spell. A new spell on NSA 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.[3] Exit from a spell is defined to

occur where a payment recipient is off unemployment-related payments (NSA) for at least three consecutive fortnights. A payment recipient is defined to be 'on payments' in any fortnight in which they lodge a claim form (SU19) regardless of payment entitlement.

5. Descriptive information on WfD participation

Results from descriptive analysis of participation in WfD during the 'pilot' phase are presented in Tables 1 to 4, and in Figures 1 and 2. There are 888 payment spells during the period from November 1997 to June 1998 where the payment recipient is observed to participate in WfD. These are concentrated mainly amongst persons aged 19 to 22 years. The size of the potential control group (payment recipients aged 18 to 24 years who do not participate in WfD during the sample period) is very large – about 65,000. Commencement of participation in WfD does not appear to be highly concentrated at any single point in payment spell duration. Over 50 per cent of participants began participation between 13 and 26 fortnights, about 25 per cent between 27 and 52 fortnights, and about 10 per cent at longer than 52 fortnights. Commencement of participation in WfD during the period of this study appears to have been concentrated between December 1997 and April 1998.

There appears to be quite a high degree of geographic concentration of participation in WfD between ABS Labour Force regions. For example, about 10% of the population of individuals aged 18 to 24 years with payment spells during the sample period are in regions where there is zero participation in WfD; by contrast, 53.2% of participants in WfD are in regions where only 21.2% of those with payment spells reside.

6. Effects of WfD – Search theory

Participation in WfD may potentially have two types of effects on exit from payments (and time on payments) – first, a referral effect that causes an increase in the rate of exit from payments at the time at which participation in WfD would be required to begin; and second, an effect due to participation in WfD. In this study the focus is on

estimating the effect of participation in WfD. The standard approach to predicting the effect of participation in WfD on exit from unemployment would be to use a search theoretic labour market model (Pissarides, 2000). An objective of WfD is to 'improve work habits' of the unemployed. In the search theoretic framework this can be represented as an increase in a job-seeker's skills that should increase the 'arrival rate' of job offers, and hence increase the rate of outflow from unemployment.

7. Effects of WfD – Empirical method

a. Outcome measures

The study will examine effects of WfD on a variety of outcome measures related to receipt of unemployment payments. The WfD requirement is for a maximum six months period. Outcome measures have been chosen to attempt to capture short-run effects at the end of participation in WfD, and longer-run post-participation effects.

One measure will be the effect of WfD on the incidence of exit from payments by 6 months and 12 months after WfD commencement. A second measure will be the effect of WfD on whether payment recipients are on payments at 9 months and 15 months after WfD commencement. The first and second measures will diverge where payment recipients exit payments, but then begin a new payment spell that is on-going at the specified duration. The third measure applied is the effect of WfD on the number of fortnights on payments during the 6 months and 12 months after WfD commencement.

Outcome measures
Effect of WfD 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.

b. Introduction

The empirical approach used to estimate the effect of the WfD is a quasi-experimental matching method. Fundamentally, this involves comparing payment outcomes for a treatment group of NSA recipients who participate in WfD, and a matched control group. In this sub-section the exact definition of the treatment and control groups, and the policy effect identified, are described.

Participation in WfD can begin for an individual payment recipient at many different payment spell durations; and occurs for different payment recipients throughout the sample period. This potentially complicates the classification of payment spells as treatment or control observations. Our basic approach is to define: (a) Treatment group – NSA recipients who commence WfD participation during the sample period; and (b) Potential control group - NSA recipients who never commence WfD. Control group payment 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.

Using this approach estimates of the effect of WfD participation are the <u>average effect of commencing participation in WfD for payment recipients aged 18 to 24 years who commence participation during the 'pilot' phase compared to matched payment recipients who do not commence participation in WfD</u>. Therefore, the estimated effect of WfD participation is the average effect of 'treatment on the treated'.

c. 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 WfD and outcomes in the absence of the WfD (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 conditions, and unemployment payment history of each payment recipient. 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. Studies by 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 rationing of projects due to the size of the program budget - effectively constitutes a source of randomness in assignment of NSA/YA(o) payment recipients between treatment and control groups.

Figure 2 shows the distribution by ABS Labour Force region of all NSA payment spells that are on-going during some part of the sample period, and payment spells that involve WfD participation. With random assignment all observations should lie along the 45 degree line. It is evident that there is a only a low degree of concentration along the 45 degree line, and a relatively large number of observations that lie some distance from the line. 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. 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. The calculation of predicted concentration under the assumption of random distribution incorporates information on (in this case) the size distribution of WfD projects. Table 5 reports findings from the test. For two alternative geographic classifications – into 35 or 67 regions – the difference between the actual and predicted random degree of concentration appears highly significant. [4]

The existence of geographic non-randomness appears to be explained by limited funding initially allocated to the scheme so that only a small subset of applications for

projects were funded (see for example, Ewin Hannan, 'Plan to expand work for the dole', The Age, January 14, 1998, p.1).

Second, it can be demonstrated that the geographic distribution of JSD participation is not correlated with local labour market conditions. Figure 3 shows the rate of unemployment and incidence of WfD participation by ABS Labour Force Region. Appendix Table 1 (Panel A) reports results of a regression of the rate of unemployment on the proportion of payment recipients participating in WfD by ABS LFR. It is evident that the hypothesis of a significant relation can be rejected at the 5 per cent level.

This evidence is consistent with exogenous assignment of WfD participation between geographic regions that is uncorrelated with local labour market conditions. On the basis of this evidence 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 geographic randomness in assignment to WfD participation that is not controlled for in the set of matching covariates.

d. Implementation

The type of quasi-experimental matching method used in this study is an 'exact matching method'. Treatment group observations (participants in WfD) are matched with a control group observation(s) with the same:

1. Payment spell duration (for example, a payment recipient who begins participation on WfD in the jth fortnight is matched with payment recipients who have on-going spells in the jth fortnight, whose activity type in that fortnight is job search, and who never participate in WfD) (65);
2. Quarter spell on WfD commences (3);
3. Age (18-20 or 21-24 years) (2);
4. Gender (2);
5. Country of birth (Australian-born; ESB immigrant; NESB immigrant) (3);
6. ATSI status (2);
7. Marital status (Single, Partner - not on payments; Partner - on payments) (3);
8. Activity type in previous fortnight (6);

9. Rate of unemployment in ABS Labour Force region (4); and

10. Unemployment payment history over previous 12 months (5).

The five categories of payment history are never on unemployment payments; frequent/recent on payment; frequent/not recent on payments; not frequent/recent on payments; and not frequent/not recent on payments. Frequent (not frequent) is defined as being on payments in at least one fortnight in 3-4 (1-2) quarters in the previous 12 months. Recent (not recent) is defined as being on payments in at least one fortnight in the quarter immediately prior to commencement of the new payment spell (not on payments in quarter immediately prior to commencement of new payment spell).

As well, payment recipients are only used as a matching control group observation if they have zero earnings from labour market activity in the fortnight of payment spell duration at which their treatment observation commences participation in WfD. This restriction is imposed since payment recipients were exempt from WfD if they had positive earnings from labour market activity.[5]

With this approach each treatment observation can potentially be classified in one of 3,369,000 cells. For each cell in which there is a treatment group observation and at least one control group observation, differences in average outcomes are calculated for control and treatment observations. A weighted average of the cell differences (taking the proportion of treatment group observations in each cell as weights) is then taken as the overall average effect of WfD participation. Payment recipients are only used as a matching control group observation if they have zero earnings from labour market activity in the fortnight

This exact matching estimator can be expressed formally as (Smith, 2002):

$$\Delta = \sum_{k} [n_{1k}/\sum_{k} n_{1k}][\sum_{i \in k \cap \{D_i=1\}} (Y_{1i}/n_{1k}) - \sum_{i \in k \cap \{D_j=1\}} (Y_{0j}/n_{0k})]$$

where: n_{1k} and n_{0k} are respectively the number of treatment and control observations in cell k, Y_{1i} and Y_{0j} are respectively outcomes for treatment group observation i, and

control group observation j, and D is an indicator variable for participation in treatment.

e. Standard errors

Using the type of standard errors commonly generated in statistical packages to test for differences between treatment and control group outcomes involves an assumption that only 'normal' sampling variation exists. 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). Therefore, in this study boostrapped standard errors are reported. The bootstrap procedure involves several stages – first, a sample with replacement is drawn from the set of treatment and control observations used in the basic model equal to the total number of treatment and control observations used; second, the basic model is implemented to obtain an estimate of the WfD effect; and third, stages one and two are repeated 1,000 times. The output is a distribution of estimated WfD effects for each outcome measure. Five per cent confidence intervals from the boostrapped estimates of WfD effects are reported. (These are generated as the 2.5 and 97.5 percentiles of the distribution of estimated WfD effects for each outcome measure.)

8. Effects of WfD

a. Preliminary information on matching

Using the exact matching method it is possible to match 802 (of the 888) WfD participants to control group observations. The WfD participants and control observations are matched into 727 cells. The median number of WfD participants in each cell is one, and the median number of control observations in each cell is 21. (Further information on the distribution of treatment and control observations by cell is in Appendix Table 2.) Table 6 presents information on average characteristics of the full and matched treatment groups. Some differences are apparent – primarily with regard to country of birth, indigenous status, and marital status. Since not all WfD participants can be matched to control group observations, therefore the

estimated effects of WfD reported below are for a subset of WfD participants. In this circumstance, where there is heterogeneity in the impact of WfD between participants, the estimated effect can no longer be interpreted as the average effect of treatment on WfD participants. Instead, it represents the average effect for WfD participants with the same characteristics as those participants who can be matched with control group observations. It would be a complex exercise to characterise this type of treatment effect. Rather, the approach that will be applied to deal with this problem is to use information on outcomes for matched and unmatched treatment group observations to estimate upper and lower bounds on estimated program effects (Lechner, 2000).

b. Basic results

Table 7 reports findings from the exact matching analysis. The main conclusion is that there appear to be quite large significant adverse effects of participation in WfD. For example, for the group of matched WfD participants it is found that there is a difference in exit from NSA payments between WfD participants and non-participants at 6 months after JSD commencement is equal to minus 12.1 percentage points (28.4 per cent for WfD participants compared to 40.6 per cent for non-participants). And the difference in fortnights on NSA payments between WfD participants and non-participants (in the first 12 months after start of spell on WfD) is 2.2 fortnights (11.13 fortnights on average for WfD participants compared to 10.14 fortnights on average for non-participants). From the confidence intervals reported each of these differences is statistically significantly different from zero at the 5% level.

Figures 4a and 4b show the proportions of WfD participants and the matched control group who exit NSA payments in each month after the commencement of WfD participation. Over the first 6 months after commencement there is a steadily widening gap between outcomes for WfD participants and the matched control group; after that time there is some reversal over the next 12 months. The difference after 6 months is 12.1 percentage points, and after 18 months is 8.9 percentage points – and over the final few months the gap appears largely to have stabilized. It is notable that the 'break-point' in the evolution of the difference in outcomes for WfD participants and the matched control group is at 6 months, which is the duration of participation in

the WfD program. It suggests that there is an adverse effect of WfD on exit from payments associated directly with the period of participation in WfD, but that there is partial 'catch-up' by WfD participants after the conclusion of WfD.

b. Bounds analysis

In the basic model 802 out of 888 WfD participants can be matched with control observations. It has been noted earlier that – where there is heterogeneity in WfD effects - this may bias the estimate of the aggregate WfD effect.

As one way to address this potential problem we estimate upper and lower bounds for the aggregate WfD effects (Lechner, 2000):

$$UB = \beta(\Delta_{TT}) + (1-\beta)(\overline{\Delta}) \text{ ; and}$$
$$LB = \beta(\Delta_{TT}) + (1-\beta)(\underline{\Delta})$$

where β = proportion of WfD participants matched with control observations; Δ_{TT} = estimated average treatment on treated WfD effect from basic model; and $\overline{\Delta}$ and $\underline{\Delta}$ are respectively the maximum and minimum possible values of the WfD effect – for example, for the outcome 'exit from payments by 3 months after WfD commencement' the maximum possible WfD effect is +1, and minimum possible effect is –1. This method calculates lower and upper bound WfD estimates by assuming that the estimated WfD effect applies to WfD participants who can be matched to control observations, and that the effect for the non-matched WfD participants is (respectively) as adverse and as positive as is possible.

Results from application of bounds analysis are reported in Table 8. It shows that for each outcome measure the distribution of possible outcomes is heavily towards adverse effects of WfD; although from the upper bound estimates (apart from the outcome measure of exit from NSA by 6 months after WfD commencement) it is not possible to exclude the possibility of a zero or small positive effect on receipt of payments from WfD participation.

An alternative approach to estimating the aggregate effect of WfD is to impute WfD effects for WfD participants who it was not possible to match with control observations. This is done in two stages – first, an OLS regression model is estimated with the difference in outcome between WfD participants and matched control observations as the dependent variable, and the matching covariates (payment spell fortnight commence WfD spell is aggregated to 13 categories) as explanatory variables; and second, results from the model are used to predict WfD effects for WfD participants not matched with control observations. (This approach assumes that effects of characteristics of payment recipients on WfD effects are homogenous across cells.) The aggregate WfD effect is then calculated as a weighted average of the estimated WfD effects for participants who could be matched, and imputed WfD effects for participants who could not be matched. Estimated WfD effects using the regression imputation approach are reported in Table 8. The effects are slightly lower than the basic model, but are still statistically significant at the 5% level.

c. Results for disaggregated groups

Effects of WfD for disaggregated groups of payment recipients can be estimated, and are of potential interest. Table 9 reports estimated WfD effects for groups of NSA recipients disaggregated on the basis of characteristics used as covariates in the matching analysis. There are several findings. First, WfD effects appear to be slightly more adverse for females than males. Second, WfD effects are more adverse for payment recipients aged 21 to 24 years than for those aged 18 to 20 years. Third, participation in WfD has large negative effects on labour market outcomes for payment recipients who have received payments in at least three quarters in the previous 12 months compared or who did not receive payments in the previous 12 months, but has no significant effect for payment recipients who received payments in one or two quarters in the previous 12 months. Hence there is not a consistent ordering of WfD effects by payment history. Fourth, between geographic regions, WfD participation has a significant adverse effect on labour market outcomes for participants in regions with above-median rates of unemployment, but generally no significant effect in below-median rate of unemployment regions. Finally, the adverse effect of WfD participation is significant for payment recipients who commence participation in fortnights 14-52 of their payment spells, but for other

durations does not have a significant effect (although this is probably mainly relate to small sample sizes in other payment spell categories).

Table 10 reports estimated WfD effects for groups of NSA recipients disaggregated on the basis of characteristics not used as matching covariates. These estimates are obtained by classifying treatment observations on the basis of the extra characteristic (for example, participate/not participate in JSD) as well as the matching covariates, and then calculating a weighted average of the estimated WfD effects across those categories. (Since we are classifying using a characteristic not included in the set of matching covariates, it is not possible to calculate standard errors.) The findings suggest that WfD effects are more adverse for NSA recipients who had not participated in JSD in the previous 12 months, whose WfD classification was compulsory, and who did not have earnings from labour market activity in the fortnight prior to commencing in WfD. The findings appear plausible in that it is likely that WfD participants without a history of labour market activity and who are made to compulsorily to participate in WfD would have worse employment prospects, and hence be less likely to exit payments. Nevertheless, even for the classifications whose WfD outcomes are relatively better (such as payment recipients with earnings from labour market activity, and who participate voluntarily in WfD), there are still quite substantial adverse estimated effects of WfD participation.

d. Robustness checks

In this sub-section three types of robustness checks on the findings on the effects of WfD are presented. One type of check is motivated by a concern that a referral effect of WfD may cause bias in estimates of the effect of participation in WfD. The second type of check is motivated by wanting to control for potential differences between WfD participants and control group observations that have not been taken into account in the analysis above. The third check is to examine the effect of an alternative definition of exit from payments.

d.i. Referral effects

From the group of unemployment payment recipients referred to WfD some may exit payments prior to participation, and others will begin participation. Where the number of 'drop-outs' is large this has two potential implications for quasi-experimental matching analysis:

(i) Individual payment recipients in the control group may exit payments due to the 'threat' of participation in WfD; and

(ii) Individual payment recipients in the treatment group – a group of those referred to WfD who have decided to participate rather than exit payments - may differ in their 'unobserved characteristics' from other payment recipients.

Of these two effects, the first would tend to cause a downward bias in the estimated program effect since part of the 'referral effect' of treatment is being manifested in a higher rate of exit from payments for the control group; and it is probably most reasonable to characterize the second as having an ambiguous impact on the estimated program effect (on the one hand, participation in WfD may reveal higher motivation; on the other hand, participation may reveal an absence of work opportunities).

In order to investigate the potential effect of a 'referral' effect on our findings, we undertake a difference-in-difference comparison of rates of outflows from payments between labour force regions (for 18-24 year old payment recipients) with different levels of participation in WfD and between the periods prior to the beginning of the WfD 'pilot' phase and during the 'pilot' phase. Evidence of a significant 'referral effect' of participation in WfD would be a larger increase (or smaller decrease) in the rate of outflow from unemployment payments in regions with higher rates of participation in WfD. (Individual-level data on referrals to WfD are not available for the pilot phase of WfD. This is why it is necessary to adopt an aggregate-level approach to testing for referral effects.)

Figure 5 presents data on a difference-in-difference measure of the rate of outflow from payments, and the rate of participation in WfD by unemployment payment recipients aged 18 to 24 years, by ABS Labour Force region. It is evident that there does not appear to be a significant relation between the series. This is confirmed by regression analysis that finds no relation between the series at the 10% level of significance (see Panel B in Appendix Table 1).

Another possibility is that estimates of the effect of WfD may be biased by exit from payments due to cancellation of payments for WfD related breaches. Payment recipients whose payments are cancelled could potentially appear as control group observations who exit payments. This would increase the rate of exit from payments of the control group; but since the explanation for their exit is failure to participate in WfD, this should be considered as a source of downward bias in the estimated effect of WfD. To investigate this possible source of bias we excluded control group observations with WfD-related breaches. The results are reported in Table 8, and it is apparent that the estimated effects are almost identical to the basic model.

d.ii. Extra matching variables

For the matching estimator to provide valid estimates of the effect of the WfD program, it must be that any treatment and control group observations matched on the basis of the observable characteristics used in the exact matching, are otherwise identical. To investigate the robustness of the basic model to introduction of another matching variable – that is likely to be an important predictor of labour market outcomes and possibly WfD participation - education attainment (four categories – Not completed high school; completed high school; trade qualification/diploma; and degree and above) is introduced as an extra matching characteristic. (This is not included as a matching variable in the basic model as missing data for some treatment and control observations would cause a significant reduction in sample size.) Findings from this exercise are reported in Table 8. The estimated adverse effects of WfD are found to be very similar to the basic model. This result adds to the degree of confidence that can be attached to our belief that the matching covariates in the basic model control in a satisfactory manner for differences in labour market outcomes and likelihood of WfD participation between WfD participants and the control group.

d.iii. Alternative exit definition

In this exercise, 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)). Results in Table 8 show that using this alternative definition does reduce somewhat the estimated adverse effect of WfD participation; however, there is still an adverse effect on receipt of payments that is statistically significant. The findings suggest that WfD participants who exit NSA are slightly less likely to exit to other payment types than the matched control group.

9. How to explain the findings?

What might explain negative effects of WFD participation on exit from payments? There appear to be three main potential explanations:
(i) Stigma effects – Participation in WfD may act as a negative signal to potential employers, and hence decrease the rate of job offers for WfD participants relative to non-participants;
(ii) Effect on job search activity – Participation in WfD may allow participants to reduce their job search activity, and may adversely affect the type of job search activity undertaken; and
(iii) Scale of intervention – The WfD program represents a relatively 'minimalist' intervention. It is designed only to provide work experience – not training – and for that reason its effect the human capital of payment recipients is likely to be relatively small. Therefore it should not be expected to have a significant impact on outcomes for payment recipients (Curtain, 2001, pp.16-18).

Of these explanations, the potential effect of WfD on job search activity, seems of most interest. There is a growing international literature that suggests that a 'lock-in' or 'attachment' factor may be an important dimension of understanding the effects of programs for unemployed. For example, a recent evaluation of the Community Work Program in New Zealand found that many participants viewed their work experience placements as 'work' and therefore did not engage in job search activity (de Boer, 2000, p.6; see also Carling et al., 1996; and Van den Berg and van der Klaauw, 2001)

Some evidence from this study does appear consistent with the existence of a 'lock-in' effect of WfD participation due to a 'chilling' effect on job search activity.

Detailed time-series analysis (Figure 4b) showed that the WfD effect on exit from unemployment payments becomes progressively more negative throughout the duration of the WfD program (from one to six months after commencement of WfD spell); but that after the six-month duration point there is a partial reversal of the negative effect of WfD. This suggests that WfD participants may reduce job search activity relative to non-participants during the period of WfD participation – hence they are less likely than non-participants to exit payments. After WfD participation concludes, those WfD participants still on payments may increase job search activity – and hence their rate of exit from payments is more rapid in the post-WfD phase than for non-participants. But there is not complete 'catch-up'. The difference in the proportions of WfD participants and the matched group of non-participants who have exited payments after 6 months is minus 12.1 percentage points, and after 18 months is still minus 8.9 percentage points. This suggests that there may be some permanent 'scarring' effect on WfD participants. Such an effect could arise due to behavioural changes in payment recipients as a result of WfD participation, or to employers stigmatizing WfD participants. [6]

However, there are also some aspects of our findings that appear inconsistent with an effect of WfD on job search. For example, if it is correct that WfD disrupts job search, it would be expected to have the largest negative effect on exit from payments for unemployment payment recipients whose job search is otherwise most likely to result in exit to a job. Since the effect of job search on exit to employment is likely to be negatively correlated the rate of unemployment in the local labour market, therefore on this argument, the largest negative effect of WfD would be expected for those who reside in regions with the lowest rates of unemployment. By contrast, in the results in Table 9, the impact of WfD appears to be most adverse in ABS LFRs with highest rates of unemployment. Of course this argument assumes that WfD participation has the same 'chilling' effect on job search of all unemployment payment recipients. Where the 'chilling' effect of WfD on job search is largest for unemployment payment recipients with lowest job finding probabilities – and this might occur due to psychological factors – then this could explain the pattern of WfD effects. This is an issue that it seems would require further investigation – using alternative empirical approaches – to resolve in a satisfactory manner.

Endnotes

1. 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).

2. Advice from FaCS and DEWR is that both categories should be treated as identifying compulsory participants.

3. Our definition of new payment spells is slightly stricter than the FaCS definition. 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.

4. Formally, for each case the difference between actual and predicted random concentration is at least four times greater than the standard deviation of the mean of concentration under the null hypothesis of randomness. 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 WfD 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 $H = \sum_j (z_j)^2$, and z_j = the proportion of WfD participants in the jth project. Data on the number of participants in the 'pilot' phase WfD projects was obtained from Centrelink. For the variance formula see Ellison and Glaeser (1997, p.907).

5. Information provided by Robert Lipp of DEWR.

6. Some evidence of employer stigma effects is the unwillingness of unemployed persons to request employers at jobs they would genuinely like to be offered to fill out Employer Contact Certificates – see Tann and Sawyers (2000, p.16).

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Table 1: Payment spells of NSA recipients aged 18 to 24 years with at least one fortnight participation in WfD, November 1997 to June 1998

	Number of observations	Frequency
Age		
18	58	6.53
19	206	23.2
20	141	15.88
21	149	16.78
22	131	14.75
23	107	12.05
24	96	10.81
Total	888	100

Table 2: Distribution of starting date on WfD by duration of payment spell - NSA recipients aged 18 to 24 years with at least one fortnight participation in WfD, November 1997 to June 1998

	Number of observations	Cumulative frequency
Duration - fortnights		
1	4	0.45
2	1	0.56
3	7	1.35
4	8	2.25
5	5	2.81
6	7	3.6
7	4	4.05
8	8	4.95
9	7	5.74
10	8	6.64
11	14	8.22
12	11	9.46
13	22	11.94
14	24	14.64
15	35	18.58
16	49	24.1
17	48	29.51
18	45	34.58
19	29	37.85
20	42	42.58
21	38	46.86
22	38	51.14
23	42	55.87
24	41	60.49
25	37	64.66
26	25	67.48
27-39	162	85.72
40-52	58	92.28
53+	69	100

Table 3: Distribution of starting date on WfD by month - NSA recipients aged 18 to 24 years with at least one fortnight participation in WfD, November 1997 to June 1998

	Number of observations	Frequency
Month		
1997 – November	6	0.68
1997 - December	119	13.4
1998 – January	164	18.47
1998 – February	206	23.2
1998 – March	164	18.47
1998 – April	111	12.5
1998 – May	62	6.98
1998 – June	56	6.31

Table 4: Participation in WfD by ABS Labour Force region - NSA recipients aged 18 to 24 years with at least one fortnight on payments, November 1997 to June 1998

Labour Force Region	Participate in WfD	Percent	All Payment Spells	Percent
2	0	0	1253	1.78
4	0	0	624	0.89
5	22	2.48	917	1.30
6	14	1.58	916	1.30
7	45	5.07	1436	2.04
8	11	1.24	892	1.27
9	3	0.34	813	1.15
10	4	0.45	879	1.25
11	0	0	356	0.51
12	1	0.11	224	0.32
13	0	0	340	0.48
14	24	2.7	1046	1.48
15	0	0	178	0.25
17	33	3.72	1049	1.49
18	2	0.23	496	0.70
19	31	3.49	2025	2.87
20	13	1.46	624	0.89
21	31	3.49	421	0.60
22	0	0	1173	1.66
23	61	6.87	2237	3.17
24	25	2.82	1784	2.53
25	14	1.58	959	1.36
28	2	0.23	1237	1.75
29	9	1.01	2526	3.58
30	0	0	1209	1.72
31	0	0	1566	2.22
32	6	0.68	1347	1.91
33	17	1.91	917	1.30
34	11	1.24	1195	1.70
35	18	2.03	1461	2.07
37	4	0.45	1317	1.87
38	31	3.49	830	1.18
39	22	2.48	834	1.18
40	18	2.03	985	1.40
41	2	0.23	972	1.38
42	63	7.09	869	1.23
44	15	1.69	1805	2.56
45	16	1.80	1939	2.75
46	7	0.79	1399	1.98
47	34	3.83	1585	2.25
48	0	0	70	0.10
49	9	1.01	1350	1.92
50	41	4.62	928	1.32

51	21	2.36	1414	2.01
52	8	0.90	779	1.11
53	13	1.46	1039	1.47
54	7	0.79	1110	1.57
55	11	1.24	2051	2.91
56	7	0.79	252	0.36
58	13	1.46	1540	2.18
59	1	0.11	953	1.35
60	4	0.45	899	1.28
61	6	0.68	1228	1.74
62	23	2.59	768	1.09
63	5	0.56	523	0.74
66	2	0.23	452	0.64
67	2	0.23	730	1.04
68	9	1.01	1623	2.30
69	18	2.03	1033	1.47
70	19	2.14	1165	1.65
71	4	0.45	796	1.13
72	14	1.58	907	1.29
75	24	2.70	1060	1.50
76	5	0.56	590	0.84
77	8	0.90	474	0.67
79	3	0.34	932	1.32
80	2	0.23	1185	1.68

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

	Index	E(G)	Index-E(G)	SD(G)
	(Actual)	(Random)	(Diff.)	
35 regions	0.01377	0.00701	0.00676	0.00177
67 regions	0.01483	0.00710	0.00773	0.00123

Table 6: Characteristics of WfD participants and all payment recipients - NSA recipients aged 18 to 24 years with at least one fortnight on payments, November 1997 to June 1998

	Treatment	Matched Treatment	Control (Characteristic at 1 Oct)
No. of Observations	888	802	65481
Mean(age)	21.41271	21.28998	21.38112
Female	307	277	27127
	34.57	34.54	41.43
Male	581	525	38354
	65.43	65.46	58.57
Age			
18-20	405	375	29284
	45.61	46.76	44.72
21-24	483	427	36197
	54.39	53.24	55.28
Country of Birth			
Australia	802	741	56096
	90.32	92.39	85.67
ESB Immigrant	28	17	3367
	3.15	2.12	5.14
NESB Immigrant	58	44	6018
	6.53	5.49	9.19
Payment History			
No UB history	498	466	39814
	56.08	58.1	60.8
Not frequent / not recent	251	221	15863
	28.27	27.56	24.23
Frequent / not recent	110	90	7054
	12.39	11.22	10.77
Not frequent / recent	7	6	1220
	0.79	0.75	1.86
Frequent / recent	22	19	1530
	2.48	2.37	2.34
ATSI status			
Non ATSI	784	790	57760
	88.29	98.5	88.21
ATSI	104	12	7721
	11.71	1.5	11.79
Marital Status			
Single	798	750	60055
	89.86	93.52	91.71
Married (partner on IS)	11	6	1679
	1.24	0.75	2.56
Married (partner not on IS)	79	46	3747
	8.9	5.74	5.72

Children

Have no child	848	772	63727
	95.5	96.26	97.32
Have child	40	30	1754
	4.5	3.74	2.68

State of Residence

ACT	2	2	1144
	0.23	0.25	1.75
NSW	334	301	18936
	37.61	37.53	28.92
VIC	203	188	16160
	22.86	23.44	24.68
QLD	189	165	14615
	21.28	20.57	22.32
SA	52	49	5492
	5.86	6.11	8.39
WA	68	62	6277
	7.66	7.73	9.59
TAS	37	32	1969
	4.17	3.99	3.01
NT	3	3	888
	0.34	0.37	1.36

Table 7: Effect of WfD on exit from payments and time on payments – Exact matching method - NSA recipients aged 18 to 24 years, November 1997 to June 1998

	Treatment	Control	Difference	5% confidence interval
% Off payments				
By 3 months	28.4	40.5	-12.1	(-16.2 ~ -8.5)
By 6 months	50.0	60.5	-10.5	(-14.9 ~ -6.7)
% On payments				
At 6 months	67.7	59.2	8.5	(1.9 ~ 12.5)
At 12 months	58.7	49.8	8.9	(4.8 ~ 12.8)
Time on payments				
First 6 months	11.13	10.14	0.99	(0.37 ~ 1.25)
First 12 months	19.79	17.59	2.20	(1.60 ~ 2.87)
number of observations				
Observations matched	802			
Total no. of observations	888			

Table 8: Effect of WfD on exit from payments and time on payments – Exact matching method – Robustness analysis - NSA recipients aged 18 to 24 years, November 1997 to June 1998

	Off payment		On payments		Time on payments (fortnights)		no. of obs.	
	6 months	12 months	9 months	15 months	6 months	12 months	total	matched
Basic model	-12.1 (-16.2 ~ -8.5)	-10.5 (-14.9 ~ -6.7)	8.5 (1.9 ~ 12.5)	8.9 (4.8 ~ 12.8)	0.99 (0.74 ~ 1.25)	2.20 (1.60 ~ 2.87)	888	802
Bounds analysis								
Lower bound	-20.7	-19.1	17.4	17.7	2.05	4.41		
Upper bound	-1.3	0.2	-2.0	-1.7	-0.27	-0.44		
Regression adjusted average effects for all treatment observations	-11.2 (-15.7 ~ -7.0)	-10.0 (-15.1 ~ -5.1)	7.9 (3.4 ~ 12.1)	8.7 (3.9 ~ 13.4)	0.91 (0.60 ~ 1.21)	2.03 (1.33 ~ 2.74)	888	
Basic model – Excluding individuals with WFD breaches	-11.9 (-15.8 ~ -8.2)	-10.1 (-14.7 ~ -6.2)	8.5 (4.6 ~ 12.5)	8.5 (4.3 ~ 12.8)	0.99 (0.73 ~ 1.27)	2.20 (1.60 ~ 2.88)	829	747
Basic model - Plus education attainment	-11.7 (-16.2 ~ -8.0)	-10.0 (-15.6 ~ -5.9)	8.3 (4.3 ~ 13.1)	6.9 (2.9 ~ 12.4)	1.00 (0.69 ~ 1.31)	2.19 (1.57 ~ 3.00)	888	698
Basic model – Exit all payment	-10.2 (-14.4 ~ -6.7)	-9.0 (-13.6 ~ -5.3)	6.3 (2.8 ~ 10.3)	7.0 (3.2 ~ 11.1)	0.86 (0.62 ~ 1.14)	1.81 (1.23 ~ 2.49)	888	802

Table 9: Effect of WfD on exit from payments and time on payments – Exact matching method - NSA recipients aged 18 to 24 years, November 1997 to June 1998

	Off payment		On payments		Time on payments (fortnights)		no. of obs.	
	6 months	12 months	9 months	15 months	6 months	12 months	total	matched
Basic model	-12.1 (-16.2 ~ -8.5)	-10.5 (-14.9 ~ -6.7)	8.5 (1.9 ~ 12.5)	8.9 (4.8 ~ 12.8)	0.99 (0.74 ~ 1.25)	2.20 (1.60 ~ 2.87)	888	802
Disaggregated:								
Gender								
Female	-14.5 (-20.4 ~ -8.0)	-10.0 (-17.2 ~ -3.2)	11.3 (4.2 ~ 17.9)	8.1 (0.9 ~ 15.4)	1.17 (0.67 ~ 1.66)	2.58 (1.40 ~ 3.63)	307	277
Male	-10.9 (-16.0 ~ -6.8)	-10.7 (-15.9 ~ -6.3)	7.1 (2.8 ~ 11.9)	9.3 (4.4 ~ 13.9)	0.89 (0.61 ~ 1.25)	2.00 (1.31 ~ 2.85)	581	525
Age								
Less than 21 years	-10.6 (-16.1 ~ -5.3)	-8.5 (-15.4 ~ -3.1)	6.2 (0.6 ~ 12.0)	5.5 (-0.5 ~ 11.3)	0.85 (0.49 ~ 1.24)	1.67 (0.76 ~ 2.64)	405	375
21 years and above	-13.5 (-18.6 ~ -8.8)	-12.2 (-18.0 ~ -6.9)	10.6 (5.6 ~ 15.9)	11.9 (6.3 ~ 17.4)	1.10 (0.74 ~ 1.47)	2.66 (1.89 ~ 3.45)	483	427
Payment history								
No UB history	-13.0 (-17.7 ~ -8.4)	-12.2 (-17.0 ~ -6.9)	9.0 (4.0 ~ 13.8)	10.6 (5.0 ~ 15.3)	0.99 (0.64 ~ 1.34)	2.27 (1.45 ~ 3.02)	498	466
Not frequent	-8.8 (-16.6 ~ -1.7)	-4.7 (-14.0 ~ 3.2)	2.6 (-4.9 ~ 10.7)	4.3 (-3.4 ~ 13.1)	0.90 (0.39 ~ 1.40)	1.57 (0.47 ~ 2.83)	258	227
Frequent	-15.7 (-27.7 ~ -6.2)	-15.2 (-28.7 ~ -3.9)	18.6 (8.6 ~ 28.7)	11.0 (0.4 ~ 23.6)	1.16 (0.44 ~ 1.92)	3.20 (1.64 ~ 4.97)	132	109

Region UE								
1st quartile	-15.4	-8.4	4.6	5.7	0.96	1.80		
	(-24.3 ~ -7.1)	(-17.6 ~ -0.3)	(-4.2 ~ 12.8)	(-3.5 ~ 14.5)	(0.27 ~ 1.63)	(0.41 ~ 3.25)	172	158
2nd quartile	-8.3	-1.4	-2.1	4.0	0.89	0.95		
	(-17.7 ~ 1.7)	(-11.2 ~ 9.7)	(-13.9 ~ 7.3)	(-7.3 ~ 14.6)	(0.17 ~ 1.62)	(-1.03 ~ 2.53)	135	126
3rd quartile	-7.9	-7.9	9.6	8.1	0.82	2.10		
	(-15.0 ~ -0.6)	(-15.6 ~ -0.5)	(2.4 ~ 16.2)	(-0.4 ~ 15.7)	(0.31 ~ 1.31)	(0.93 ~ 3.20)	240	211
4th quartile	-15.0	-17.0	14.3	13.1	1.15	2.99		
	(-20.9 ~ -9.8)	(-24.2 ~ 10.6)	(8.9 ~ 20.5)	(6.6 ~ 20.0)	(0.77 ~ 1.57)	(2.08 ~ 4.04)	341	307
Fortnight commence WfD								
1-13	-5.0	-8.9	1.1	3.2	0.49	0.82		
	(-16.3 ~ 5.1)	(-20.2 ~ 1.2)	(-9.6 ~ 12.5)	(-7.9 ~ 13.8)	(-0.31 ~ 1.26)	(-0.78 ~ 2.63)	106	98
14-20	-13.0	-7.3	6.2	3.8	1.09	2.08		
	(-20.5 ~ -6.4)	(-14.7 ~ -0.9)	(-0.1 ~ 13.1)	(-4.1 ~ 10.7)	(0.59 ~ 1.61)	(1.03 ~ 3.31)	272	247
21-26	-12.8	-9.3	12.3	7.5	1.07	2.49		
	(-20.2 ~ -6.1)	(-17.8 ~ -1.3)	(4.6 ~ 19.4)	(0.0 ~ 16.0)	(0.57 ~ 1.57)	(1.32 ~ 3.74)	221	204
27-52	-15.7	-16.8	12.0	19.3	1.14	2.99		
	(-23.7 ~ -9.1)	(-25.3 ~ -8.5)	(4.3 ~ 19.3)	(11.3 ~ 26.9)	(0.66 ~ 1.72)	(1.81 ~ 4.21)	220	194
53+	-6.6	-9.8	6.1	9.9	0.56	1.37		
	(-18.5 ~ 4.3)	(-26.2 ~ 3.7)	(-4.2 ~ 19.4)	(-6.3 ~ 27.6)	(-0.22 ~ 1.31)	(-0.39 ~ 3.47)	69	59

Note: p-values in parentheses.

Table 10: Effect of WfD on exit from payments and time on payments – Exact matching method – NSA recipients aged 18 to 24 years, November 1997 to June 1998

	Off payment		On payments		Time on payments (fortnights)		no. of obs.	
	6 months	12 months	9 months	15 months	6 months	12 months	total	matched
Basic model	-12.1 (-16.2 ~ -8.5)	-10.5 (-14.9 ~ -6.7)	8.5 (1.9 ~ 12.5)	8.9 (4.8 ~ 12.8)	0.99 (0.74 ~ 1.25)	2.20 (1.60 ~ 2.87)	888	802
JSD participation in previous 12 months								
Participate	-8.1	-6.6	4.1	7.1	0.76	1.45		
Not participate	-15.2	-13.4	11.9	10.3	1.16	2.77		
WfD category								
Compulsory	-14.1	-11.4	10.5	9.8	1.08	2.54		
Voluntary	-9.6	-9.3	6.1	7.8	0.86	1.77		
WfD participants – Whether earnings in fortnight prior to JSD commencement								
Have earnings	-8.3	-4.3	4.0	2.6	0.59	0.91		
Do not have earnings	-12.4	-10.9	8.8	9.3	1.01	2.29		

Figure 1: NSA spells with WfD participation - By starting point in payment spell (Fortnights) - November 1997 to June 1998
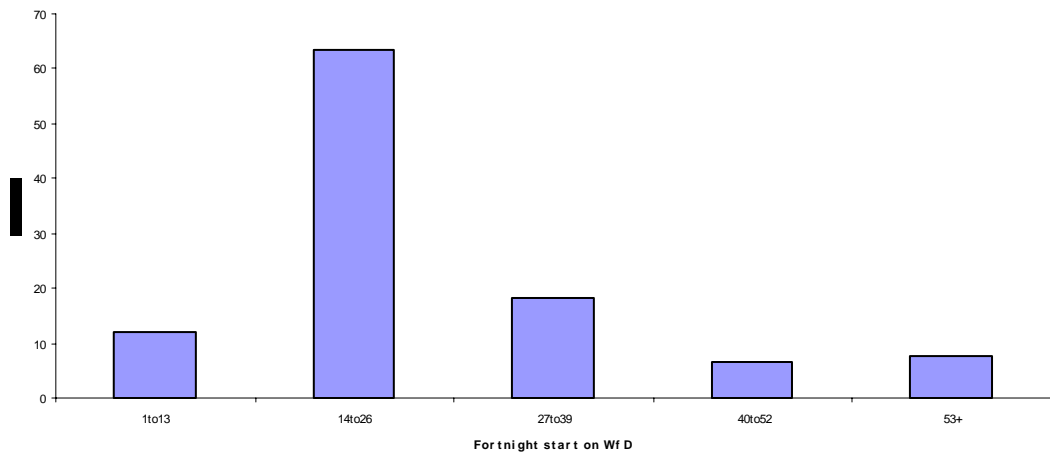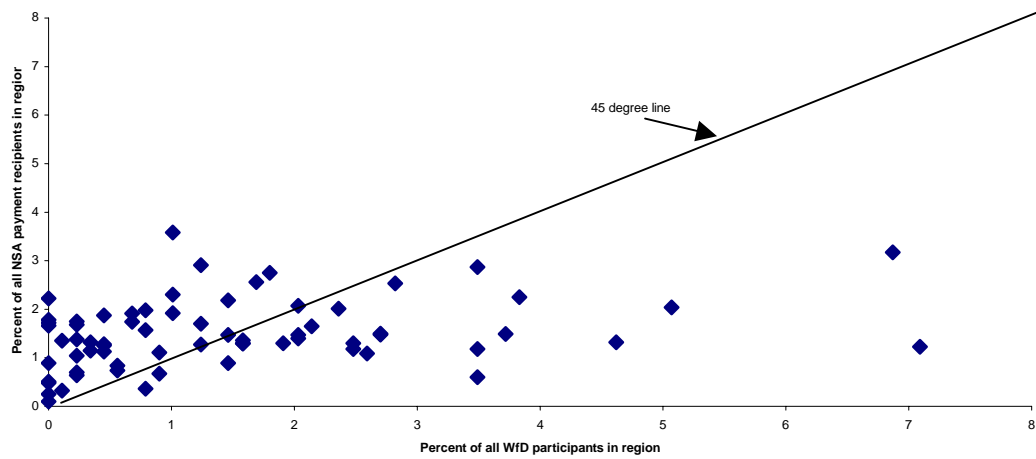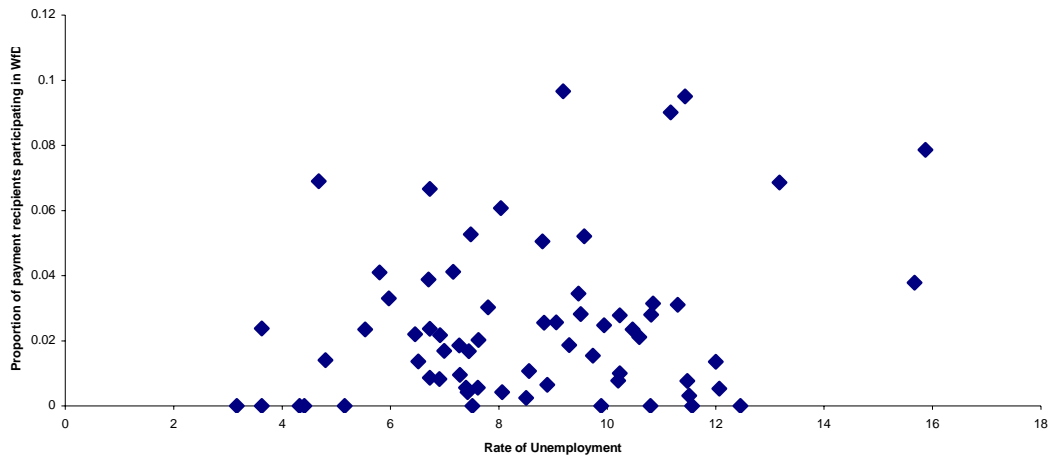

Figure 2: Participation in WfD and all NSA payment spells - Recipients aged 18 to 24 years - Geographic distribution by ABS labour force region

**Figure 3: WfD participation (NSA recipients aged 18-24 years) and rate of unemployment -
By ABS Labour Force Region - October 1997 to June 1998**

**Figure 4a: Proportion of NSA recipients exiting NSA payments - Aged 18 to 24 years ·
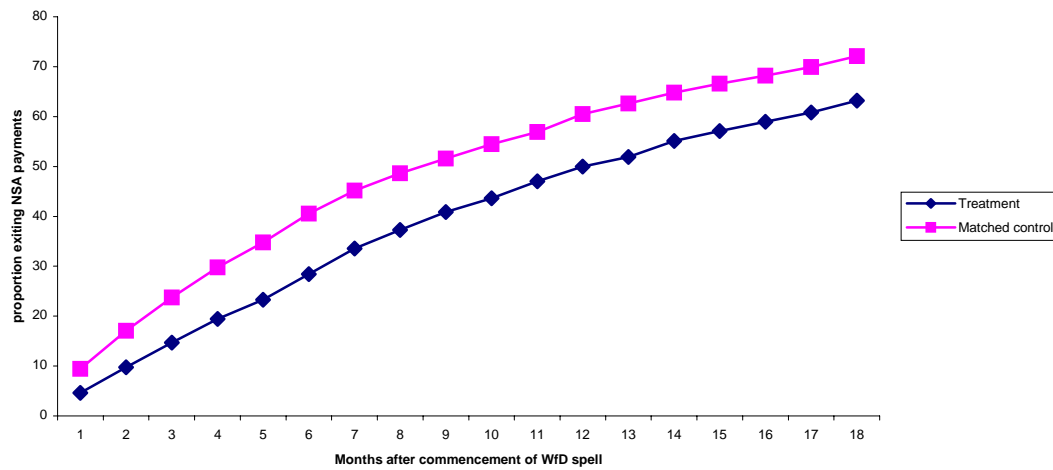By month after commencement of WfD spell**

Figure 4b: Proportion of NSA payment recipients exiting NSA payments - Difference between WfD participants and matched control group - Aged 18-24 years - By month after commencement of WfD spell
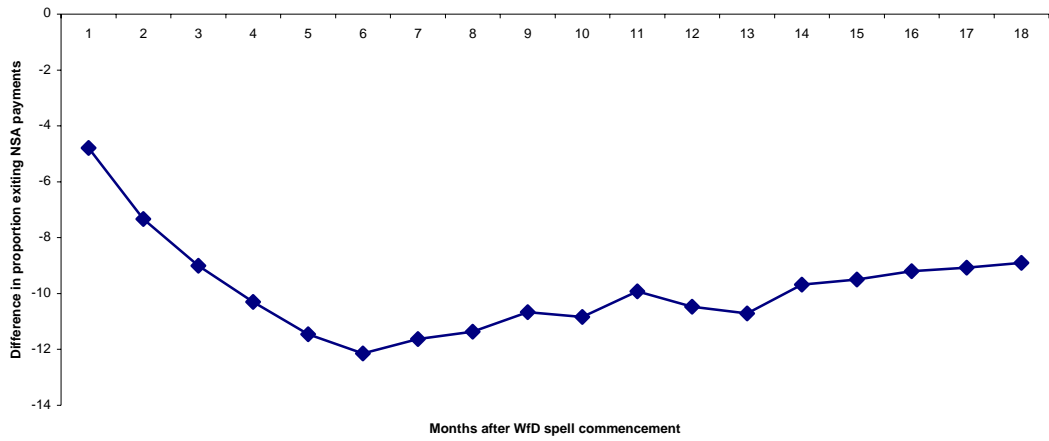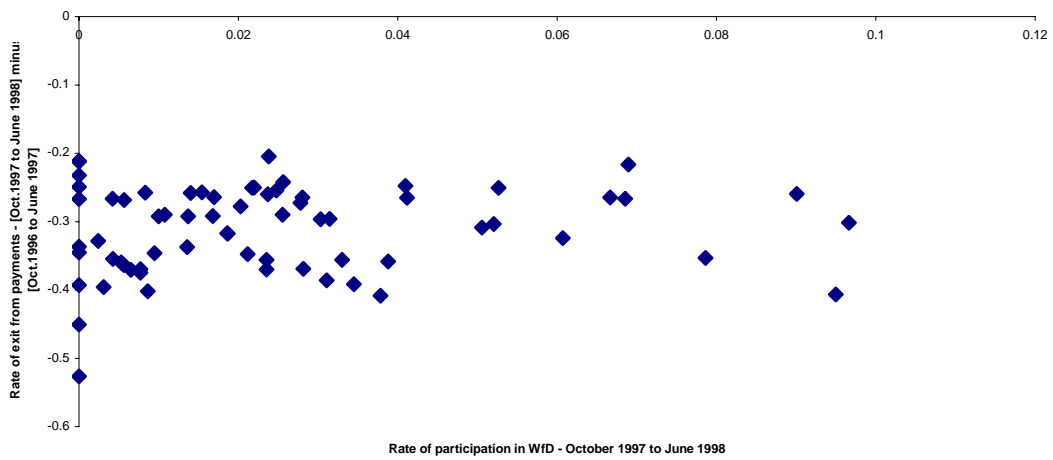

Figure 5: Rate of exit from payments and incidence of participation in WfD - NSA recipients aged 18-24 years - By ABS Labour Force Region

Appendix Table 1: Regression results – Relation between rate of unemployment and JSD participation – By ABS Labour Force Region (OLS – Robust standard errors)

Panel A: Dependent variable: Proportion of payment recipients participating in JSD (Figure 3)

Rate of ue		Constant		Observations
Coefficient	p-value	Coefficient	p-value	
0.0022	0.063	0.0058	0.546	67

Panel B: Dependent variable: Rate of exit from NSA payments (Figure 5)

Proportion of payment recipients in JSD		Constant		Observations
Coefficient	p-value	Coefficient	p-value	
0.2841	0.419	-0.3185	0.000	67

Appendix Table 2: Number of treatment and control observations by cell

Percentile	Treatment observations	Control observations
1	1	1
5	1	1
10	1	2
25	1	5
50	1	21
75	1	49
90	1	76
95	1	93
99	2	244