

Does ‘Work for the Dole’ Work?*

Jeff Borland

**Department of Economics and Melbourne Institute of Applied Economic and Social
Research, University of Melbourne**

and

Yi-Ping Tseng

**Melbourne Institute of Applied Economic and Social Research,
University of Melbourne**

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Melbourne Institute of Applied Economic and Social Research

The University of Melbourne

Victoria 3010 Australia

***Telephone* (03) 8344 2100**

***Fax* (03) 8344 2111**

***Email* melb-inst@unimelb.edu.au**

***WWW Address* <http://www.melbourneinstitute.com>**

Abstract

This study examines the effect of a community-based work experience program - Work for the Dole (WfD) - on transitions out of unemployment in Australia. To evaluate the WfD program a quasi-experimental exact matching approach is applied. Justification for the matching approach is a 'natural experiment' - limits on WfD project funding - that it is argued constituted a source of random assignment to the program. Participation in the WfD program is found to be associated with a large and significant adverse effect on the likelihood of exiting unemployment payments. The main potential explanation is existence of a 'lock-in' effect whereby program participants reduce job search activity.

1. Introduction

This study examines the effect of the Australian ‘Work for the Dole’ (WfD) program on transitions out of unemployment. WfD is a community-based work experience program initially designed for young unemployed persons. The focus of this evaluation is on a pilot phase of operation of the WfD scheme between November 1997 and June 1998.

High youth unemployment has been a feature of labour markets in industrialized economies since the mid-1970s. The social and economic consequences of that unemployment have brought a substantial concern on the part of governments to develop policies 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. One of the main findings from existing studies of program impacts is the extent of heterogeneity (for example, Heckman et al., 1999, p.2053). There is 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 it is of significant value to continue research on the impacts of interventions and programs targeted at unemployed persons. Such research can both provide a broader perspective on the efficacy of specific types of programs, 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.

Hence the major contribution of evaluation of the WfD program in this study is to add to international knowledge on the effects of work experience programs for young unemployed. The analysis that is undertaken adopts a quasi-experimental methodology using an administrative database on unemployment payment recipients. It has several notable features. First, the database that is used includes a sufficiently large number of observations to enable an exact matching method to be applied. Second, fortnightly data allow a detailed analysis of the evolution of program effects across time; for example, whether program ‘lock-in’ effects exist. Third, aspects of the implementation of the WfD pilot phase appear to have provided a ‘natural experiment’ so that there is a significant source of random assignment into WfD.

Section 2 describes the WfD program. Section 3 presents the data source and information on the sample of unemployment payment recipients used in the study, and discusses predicted effects of the WfD program. Section 4 describes the empirical methodology is described. Results and analysis are presented in section 5. Concluding remarks are in section 6.

2. The WfD program

2.1 Introduction to program

The objectives of the WfD scheme are 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 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 Newstart Allowance (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 WfD derives from social security legislation on eligibility conditions for receipt of unemployment benefit payments in Australia. 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.

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).

The WfD program is on-going. However in this study the focus is on the initial pilot phase of the program. There are two 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). Payment recipients can choose the activity that best suits their interests, and hence has been described above, under MO it is not likely that participants in WfD would constitute a random sample of payment recipients. Second, the sample of unemployment payment recipients for this study is Newstart Allowance recipients aged 18 to 24 years. 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.

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 rate at which new job offers are received, and hence increase the rate of outflow from unemployment.

2.2 Previous studies

There have been two main previous studies of the effects of participation in WfD on exit from unemployment payments. The Commonwealth Department of Employment, Workplace Relations and Small Business (DEWRSB) (2000) used a matching method to compare outcomes for WfD participants who exited from WfD in August 1999 with a control group of unemployment payment recipients who had not participated in WfD in the previous six months. That study found that in the five months after August 1999 30 per cent of WfD participants had exited payments, whereas for the control group only 17 per cent exited payments. Richardson (2003) examines the effects of WfD participation as part of a more general evaluation of the Mutual Obligation (MO) program. (In July 1998 participation in WfD became one activity that would satisfy the new MO requirement for unemployment payment recipients.) For a sample of payment recipients aged 23-24 years who participated in WfD in July to December 1998 it is found that – compared to a matched control group – there is no significant difference in financial dependence on income support during a twelve month period after participation.¹

The findings from these previous studies therefore suggest a positive, or at worst insignificant, effect of WfD participation on receipt of unemployment payments. However, both studies have methodological problems that are likely to have caused selection bias in estimates of the effect of the WfD program. Significant criticisms of the methodology applied in the DEWRSB study have been raised (Productivity Commission, 2002, Appendix E, and OECD, 2001, p.220). One major problem is that a group of program participants, some of whom have already left unemployment payments, are being matched with a control group of non-participants, all of whom are on unemployment payments in August 1999. Hence there is a bias towards finding a positive effect of WfD participation. A second problem is that duration of spell on unemployment payments is used as a matching variable, but time spent in the WfD program is excluded from that duration measure; therefore WfD participants will on average have longer unemployment payment spells than non-participants. Where the probability of exit from unemployment varies with spell duration, this will be a source of selection bias. 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.² This random assignment requirement seems problematic in the time period in which WfD is a component of the MO program. This is because the type of activity to which a payment recipient is assigned is likely to be related to their characteristics and labour market history. In general, we would expect MO activity selection to have been such that payment recipients with the ‘worst’ labour market prospects are 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. Hence, there is a significant possibility of a selection bias in findings on the effect of WfD participation during the MO period.

The methodology in this study is designed to overcome these problems. First, a standard quasi-experimental matching approach that is based on a sample of entrants to the WfD program is applied. 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. Second, by choosing to examine the pilot phase of WfD between November 1997 and June 1998, during which time the MO program did not operate, and where it will be argued that a ‘natural experiment’ for assignment into the WfD program existed, problems of selection bias should be less significant than at later times.

3. Data source and sample

3.1 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 LDS 20% sample of unemployment payment recipients is used.

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.³ 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.

3.2 Sample

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.⁴

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.⁵ 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.

3.3 Descriptive information on WfD participation

Descriptive information on participation in WfD during the ‘pilot’ phase is presented in Tables 1 to 3, 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. (For details see Appendix Table 1.)

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

| Month | Number of observations | Frequency |
|-----------------|------------------------|-----------|
| 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 |

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

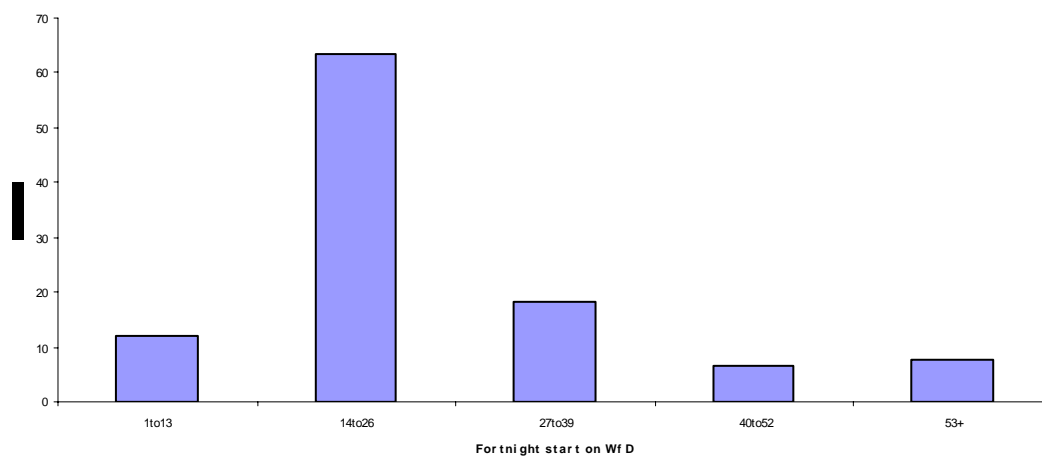
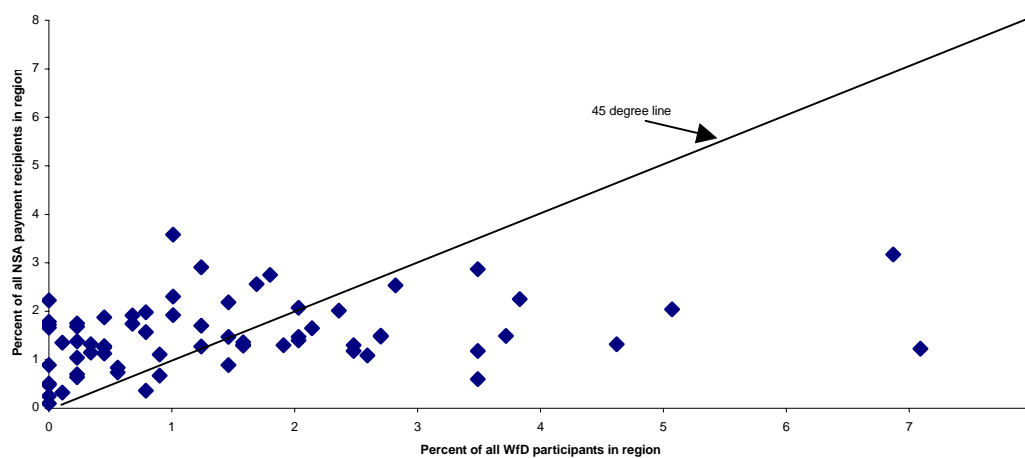


Figure 2: Participation in WfD and all NSA payment spells - Recipients aged 18 to 24 years - Geographic distribution by ABS labour force region



4. Methodology

This section describes the empirical methodology. A general characterization of the methodology is presented in 4.1. Validity of the methodology is discussed in 4.2. Details of implementation of how the methodology is implemented are described in 4.3, and the method of calculating standard errors is presented in 4.4.

4.1 Empirical method - 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. 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 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. Therefore, the estimated effect of WfD participation is the average effect of ‘treatment on the treated’.

Effects of WfD on a variety of outcome measures related to receipt of unemployment payments are examined.⁶ 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.

4.2 Motivation

For the quasi-experimental matching method to be a valid estimator of the WfD 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. One justification is to describe a natural experiment that is a likely source of randomness in assignment of eligible unemployment payment recipients to WfD participation. The second justification is that WfD participants and non-participants can be matched using a relatively rich set of covariates.

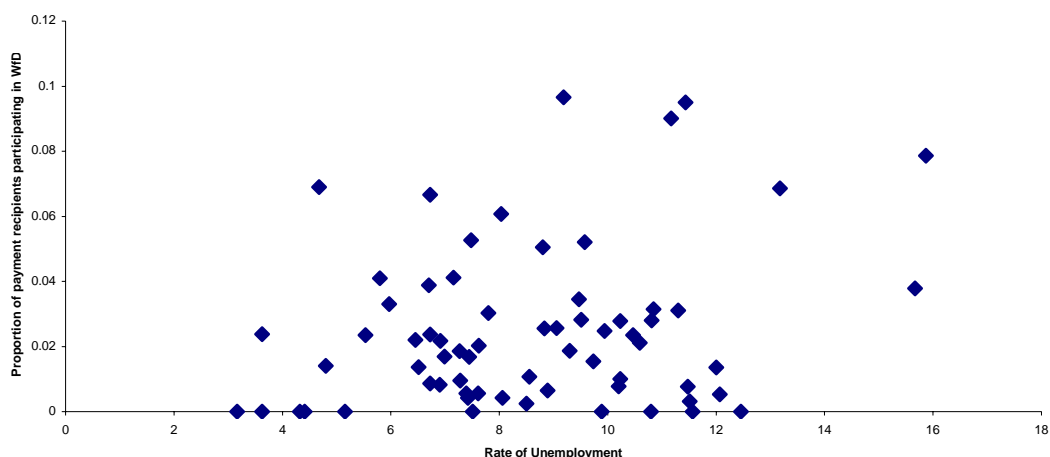
During the pilot phase of WfD, limits to total funding for the scheme meant that only a small subset of applications for projects were funded (see for example, Ewin Hannan, ‘Plan to expand work for the dole’, *The Age*, January 14, 1998, p.1). The limits to funding appear to have influenced assignment to the WfD program. This is evident from the geographic distribution of WfD projects. 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 a uniform distribution across regions all observations should lie along the 45-degree line. However, there is 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. The calculation of predicted concentration under the assumption of random distribution incorporates information on (in this case) the size distribution of WfD projects. Table 4 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.⁷

Table 4: 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 |

Importantly, the effect of restrictions on the set of projects funded on patterns of assignment to WfD participation, is not related to local labour market conditions. Figure 3 shows the rate of unemployment and incidence of WfD participation by ABS Labour Force Region (LFR). Appendix Table 2 (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.

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



The geographic pattern of assignment – explained by rationing of projects due to limits on funding for the WfD program – therefore effectively constitutes a natural experiment; that is, a source of randomness in assignment of NSA payment recipients between treatment and control groups. As a source of a natural experiment, limits on funding of WfD is not dissimilar to the ‘tie-breaking’ experiment in the study of reemployment services in Kentucky in Black et al. (2003). That experiment uses the institutional feature that reemployment services in Kentucky are provided up to a specified number of funded places. Hence, payment claimants with a ‘profiling score’ (that is, expected duration of unemployment) equal to the marginal score for receiving reemployment services may be randomly assigned into or out of that program.

The second justification for why the CIA should hold is that treatment and control group observations can be matched using a relatively rich set of covariates. Most significantly, it is possible to match on the basis of local labour market 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). Recent studies for Australia have also established 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.

4.3 Implementation

The type of quasi-experimental matching method used in this study is exact matching. Participants in WfD are matched with a control group observation(s) with the same: Payment spell duration (for example, a payment recipient who begins WfD participation in the j th fortnight is matched with payment recipients who have on-going spells in the j th fortnight, whose activity type in that fortnight is job search, and who never participate in WfD) (65 categories); Quarter WfD spell commences (3); Age (18-20 or 21-24 years) (2); Gender (2); Country of birth (Australian-born; ESB immigrant; NESB immigrant) (3); Indigenous status (2); Marital status (Single, Partner - not on payments; Partner - on payments) (3); Activity type in previous fortnight (6); Rate of unemployment in ABS LFR (4); and Unemployment payment history over the previous 12 months (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 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.⁸ The five categories of unemployment 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).

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

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

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.

4.4 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, the process of matching between treatment and control observations is an extra source of variation that needs to be taken into account (Smith, 2000, p.13). Therefore, in this study bootstrapped 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. Ninety-five per cent confidence intervals from the bootstrapped 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.)

5. Effects of WfD

This section reports results of the empirical analysis. Preliminary information on matching quality is presented in 5.1. Basic results, and findings from sensitivity analysis using a bounds method, are in 5.2-5.3. Results for disaggregated groups are reported in 5.4. Different approaches to testing for robustness are presented in section 5.5. In section 5.6 possible explanations for the main findings are discussed.

5.1 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 3.) Table 5 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).

Table 5: 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 |

5.2 Basic results

Table 6 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 WfD 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.

Table 6: 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 | 95% 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 | 813 | | | |
| Total no. of observations | 888 | | | |

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.

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

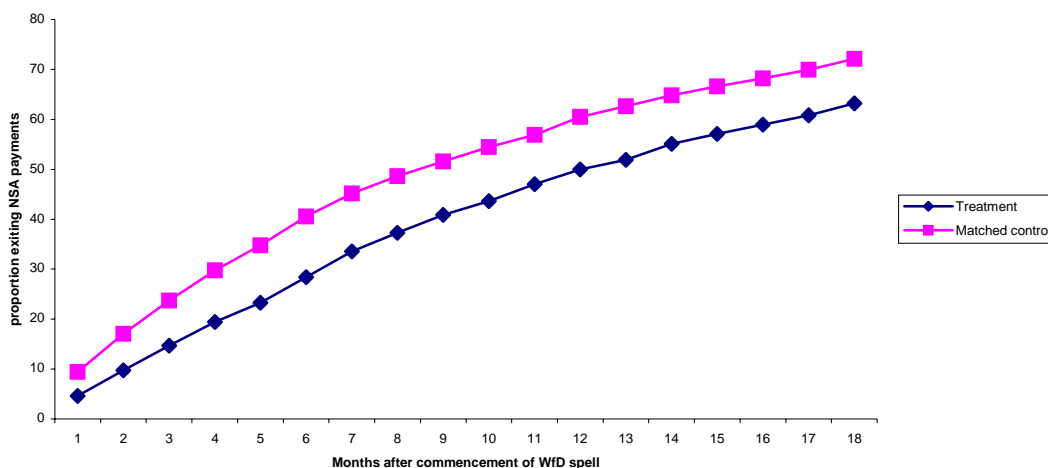
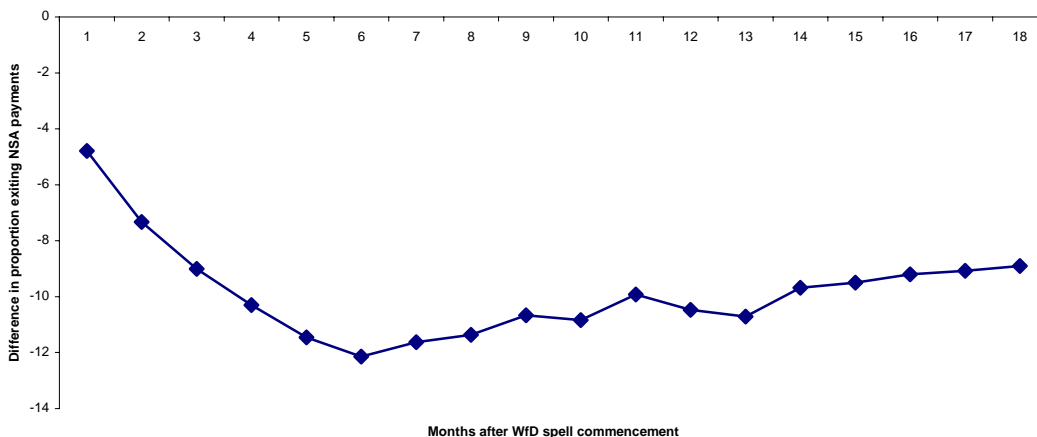


Figure 4b: Proportion of NSA payment recipients exiting NSA payments - Difference between WfD participants and matched control group - Aged 18-24 years - By month after commencement of WfD spell



Findings on the adverse effects of the WfD program are quite consistent with evidence from international literature. For North America, Heckman et al. (1999, p.2053) summarise evidence on government employment programs. The main conclusions 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’; and (ii) There is some evidence that the largest program effect is for low-skill labour force participants. 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’.

5.3 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):

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

$$LB = \beta(\Delta_{TT}) + (1-\beta)(\underline{\Delta})$$

where β = proportion of WfD participants matched with control observations;

Δ_{TT} = estimated average treatment on treated WfD effect from basic model; and $\bar{\Delta}$ 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 7. 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 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 7. The effects are slightly lower than the basic model, but are still statistically significant at the 5% level.

5.4 Results for disaggregated groups

Estimated effects of WfD participation for disaggregated groups of payment recipients are reported in Table 8. There are several findings. First, 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. Second, the adverse effect of WfD participation is significant for payment recipients who commence participation in fortnights 14-52 of their payment spells, and tends to become larger for later commencement durations. Third, WfD effects appear to be slightly more adverse for females than males, and are more adverse for payment recipients aged 21 to 24 years than for those aged 18 to 20 years. Finally, 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.

Table 7: 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 |
| Basic model – Control group participate in WfD | -8.5 (-12.3 ~ -5.1) | -5.3 (-9.8 ~ -2.0) | 5.1 (1.8 ~ 8.9) | 4.9 (1.0 ~ 8.8) | 0.74 (0.48 ~ 1.01) | 1.49 (0.94 ~ 2.13) | 888 | 809 |

Note: 95% confidence intervals are in parentheses.

Table 8: 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 |

Table 8 continued:

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

Note: 95% confidence intervals are in parentheses.

5.5 Robustness checks

In this sub-section four 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 basic model. The third check is to examine the effect of an alternative definition of exit from payments. A final check is to examine sensitivity to changing the control group to include unemployment payment recipients who participate in WfD. This modification makes the approach identical to that in Sianesi (2004) where the control group for a group of treatment observations who commence treatment in a specific fortnight, may include individuals who will commence treatment in a subsequent fortnight.

5.5.a 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) 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 2).

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 due to a WfD breach 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 7, and it is apparent that the estimated effects are almost identical to the basic model.

5.5.b 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 another matching variable – that is likely to be an important predictor of labour market outcomes and possibly WfD participation -

education attainment is introduced. Education attainment is included in four categories – Not completed high school; completed high school; trade qualification/diploma; and degree and above. (It 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 7. 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.

5.5.c Alternative exit definition

In this exercise, exit from payments is defined to occur only where a NSA 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 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 7 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.

5.5.d Control group includes WfD participants

Table 7 shows that the effect of including WfD participants as potential control group is to cause a small increase the number of matched treatment observations, and to slightly decrease the negative estimated effect of WfD. This result is consistent with those new extra control observations – WfD participants - having a lower incidence of exit from payments than the original control group. Nevertheless, even with the alternative definition of the control group, participation in WfD is found to have a significant negative effect on exit from payments, and to increase time on payments.

5.6 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) Effect on job search activity – Participation in WfD may allow participants to reduce their job search activity, or may adversely affect the type of job search activity undertaken;
- (ii) 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; 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 (Heckman et al., 1999, p.2053, and 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). Evidence of ‘lock-in’ effects has also been found for a job search counselling program in the Netherlands, wage subsidy schemes and public job creation in the Slovak Republic and Germany, and for training programs in Denmark (see Van den Berg and van der Klaauw, 2001, van Ours, 2002, Hujer et al., 2003, and Bolvig et al., 2003).

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. 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).

6. Conclusion

This study has examined the effect of the introduction of the Work for the Dole (WfD) program on exit from and time spent on unemployment payments by young unemployed in Australia. The focus of the analysis is on a pilot phase of the WfD scheme between November 1997 and June 1998. During 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 unemployment payment recipients 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 or two and a half days per week.

The empirical approach used to estimate the effect of the WfD is a quasi-experimental matching method. 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. It was argued that limits on funding of WfD during its pilot phase constitute a ‘natural experiment’ that has been a source of randomness in assignment to WfD participation is random. Evidence on the geographic pattern of WfD

participation, and on the absence of correlation with local labour market conditions, is presented to support this argument.

The main conclusion from the study 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 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. 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.

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, and to be consistent with the time-series pattern of WfD effects (that is, increasingly adverse during the six-month phase of participation in WfD, but then reversing to some extent after that time). However, stigma effects may also have played a role; and the minimal scale of intervention through the WfD is a reason why positive effects from the program would be unlikely.

Endnotes

1. Other studies of WfD that do not use quasi-experimental methods are a study of the pilot phase of WfD that examined attitudes of participants (DEWR, 2000), and a study of post-program employment outcomes for WfD participants (DEWR, 2004). 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."
2. 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.
3. 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).
4. Advice from FaCS and DEWR is that both categories should be treated as identifying compulsory participants.
5. 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.
6. 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 and thereby reduced receipt of welfare payments. 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).

7. 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 j th 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).

8. Information provided by Robert Lipp of DEWR.

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Appendix Table 1: 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 |

Appendix Table 2: Regression results – Relation between rate of unemployment and WfD participation – By ABS Labour Force Region (OLS – Robust standard errors)

Panel A: Dependent variable: Proportion of payment recipients participating in WfD (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: Difference in rate of exit from NSA payments 1997/98 minus 1996/97 (Figure 5)

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

Appendix Table 3: 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 |



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