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Does a Minimum Job Search Requirement Reduce Time on Unemployment Payments? Evidence from the Jobseeker Diary in Australia

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Abstract

This study examines the impact of the Jobseeker Diary (JSD), a program designed to increase the job search effort of unemployed persons in Australia. The JSD program is distinguished by combining a focus on work search verification with large scale implementation. Applying a quasi-experimental matching method to data on unemployment spells occurring in 1997-98, the authors find that JSD participation was associated with an increased rate of exit from unemployment payment recipiency and a shorter total time spent on payments. Payment receipt duration is estimated to have fallen for about one-half of JSD participants. The largest effects of the JSD occurred for payment recipients for whom labor demand conditions were the most favorable. Cost-benefit analysis suggests a fairly large net societal gain per program participant.

KEYWORDS: job search, Jobseeker Diary

DOES A MINIMUM JOB SEARCH REQUIREMENT REDUCE TIME ON UNEMPLOYMENT PAYMENTS? EVIDENCE FROM THE JOBSEEKER DIARY IN AUSTRALIA

JEFF BORLAND and YI-PING TSENG*

This study examines the impact of the Jobseeker Diary (JSD), a program designed to increase the job search effort of unemployed persons in Australia. The JSD program is distinguished by combining a focus on work search verification with large scale implementation. Applying a quasi-experimental matching method to data on unemployment spells occurring in 1997–98, the authors find that JSD participation was associated with an increased rate of exit from unemployment payment reciprocity and a shorter total time spent on payments. Payment receipt duration is estimated to have fallen for about one-half of JSD participants. The largest effects of the JSD occurred for payment recipients for whom labor demand conditions were the most favorable. Cost-benefit analysis suggests a fairly large net societal gain per program participant.

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

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With the rise of mass unemployment in industrialized economies since the 1970s, governments have devoted increasing attention to the design and implementation of policies to improve labor market outcomes for unemployed job seekers. One important type of policy—job search intervention—consists of programs that seek to raise the intensity and effectiveness of job search through such means as work search verification and job search assistance.

Existing empirical evidence on the impact of job search programs is primarily from a range of random experiment studies undertaken in the United States and Europe. For the United States, Ashenfelter et al. (2005) analyzed a four-state random experiment

A data appendix with additional results, and copies of the computer programs used to generate the results presented in the paper, are available from the first author at Department of Economics, University of Melbourne, Melbourne VIC 3010, Australia; e-mail jib@unimelb.edu.au.

on effects of stricter enforcement and verification of work-search, and Klepinger et al. (2002) examined a Maryland experiment to test the effect of alternative job search programs. Studies by Meyer (1995) and Bloom and Michalopoulos (2001) reviewed other experimental evidence for the United States. In the United Kingdom, a random experiment study evaluated the impact of the Restart program, which required unemployment payment recipients with spell durations of six months or more to meet with a counselor (Dolton and O'Neill 1996, 2002); and the Gateway phase of the New Deal—in which an unemployed person meets regularly with a personal advisor and is given intensive job search assistance—has been evaluated using quasi-experimental methods (Blundell et al. 2004). Random experiment studies of the effects of increased counseling and monitoring of unemployed job seekers have been undertaken for the Netherlands (Gorter and Kalb 1996; van den Berg and van der Klaauw 2001), and one Australian study has examined effects of a random experiment to provide extra counseling to very long-term unemployed with payment spells lasting more than five years (Breunig et al. 2003).

Several main conclusions emerge from the existing literature:

- participation in job search programs appears to improve labor market outcomes for unemployed persons;
- the scale of the job search program, and the timing of intervention, matter (the impact is more positive where the intervention has higher intensity—for example, a larger amount of contact time between the unemployed person and a case worker—and where intervention occurs at an earlier stage of an unemployment spell);
- there is mixed evidence on the relative efficacy of job search programs that include both work search verification and job search training, compared to those with only work search verification;
- job search programs appear to improve labor market outcomes primarily by increasing intensity of job search by unemployed persons; and
- job search programs are most effective when they do not constrain which "types" of job search activities—formal versus informal search methods, for example—can be pursued.

This study adds to the body of knowledge

on job search programs in several ways. First, most previous studies (except for studies of U.K. programs) have looked at experiments targeting only a small subset of the unemployed population. Hence our study adds considerably to understanding about the effects of large-scale job search interventions. Second, the JSD program's exclusive focus on work search verification enables us to evaluate that strategy's effects in a setting free of possible confounding influences of other strategies. Most previous studies, in contrast, have examined experiments that mix work search verification with job search assistance. (Only the recent U.S. studies by Ashenfelter et al. [2005] and Klepinger et al. [2002] have sought to address the question of the independent effect of work search verification.) Third, even though a major theme of reviews of the impact of labor market programs is the heterogeneity of program effects (for example, Heckman et al. 1999:2053), the range of countries where job search programs have been studied is still fairly narrow. Adding an examination of a job search program in Australia to existing studies of programs in other, primarily northern hemisphere, countries may help us understand how specific program characteristics and environmental factors affect the degree of success in improving labor market outcomes for unemployed persons. Finally, this study is the first of which we are aware that undertakes a cost-benefit analysis of a job search program.

The Job Seeker Diary

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

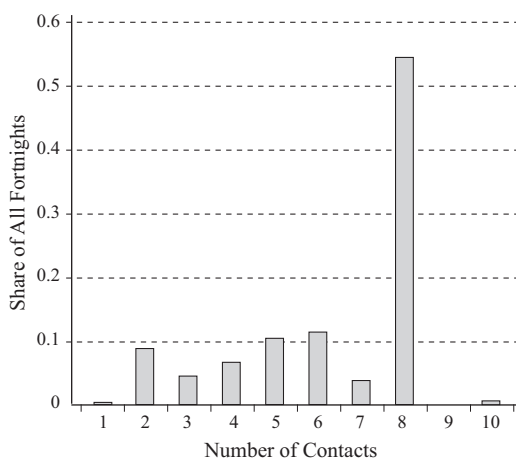
duration for which unemployment payments can be claimed in Australia.

The JSD—introduced in July 1996—constitutes one component of existing activity test arrangements. The JSD is a booklet in which an unemployment payment recipient must list details of job applications for each fortnight over a three-month period. Information required on each job search episode includes the employer name and contact details, the job description, and the job search method used to find the vacancy. The objectives of the JSD are to encourage more active job search and to give payment recipients a record of their job search (Centrelink 1996). The JSD is administered by Centrelink, a Commonwealth government agency with responsibility for service delivery to unemployment payment recipients.

The JSD is issued to all new unemployment payment recipients who receive fortnightly payments and have “job search” as their main activity type, as well as to others under certain circumstances (for example, payment recipients who, at Review Interviews, are judged to have been making only “marginal work efforts”). On the other hand, payment recipients may also be *exempted* from the JSD requirement for a variety of reasons: “Discretionary”; “Exempt from activity test for more than 10 weeks”; “In case management”; “On variable reporting”; “Have significant disability problems”; “Have literacy problems”; “Have psychiatric or substance abuse problems”; or “Have not worked in previous 12 months” (Centrelink 1996).

Payment recipients with a JSD are instructed on the minimum number of jobs per fortnight for which they must apply. This number is supposed to equal a benchmark set for the region in which a payment recipient resides, determined on the basis of local labor market conditions, but Centrelink staff can vary the number downward to take account of a payment recipient’s personal characteristics (Centrelink 1996). Figure 1 shows that the modal minimum number of required contacts per fortnight for JSD participants was eight, with over 50% of payment recipients being subject to that rule. Unemployment payment recipients who were eligible for JSD participation but did not take part in it had

Figure 1. Distribution of the Number of JSD Contacts: NSA/YA(o) Recipients Aged 18 to 49 Years, July 1997 to June 1998, by Fortnight.



to comply with the regular activity test, which involves a requirement to undertake job search and to name two job search contacts made each fortnight.

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

Data and Sample Characteristics

The Database

The database for this study is the Commonwealth Department of Family and Community Services Longitudinal Administrative

¹Activity test breaches result in a reduction of payments by 18% for 26 weeks (first breach within a two-year period); 24% for 26 weeks (second breach within a two-year period); and 100% for 8 weeks (third breach within a two-year period) (Commonwealth Department of Family and Community Services 2000).

Data Set (LDS). More specifically, we use the LDS Unemployment Payment File, a 10% random sample of unemployment payment recipients for the period from January 1995 to June 2000. The LDS is created from administrative records of social security payment receipt in Australia. It includes the date on which any social security payment was made; the type and amount of payment; and payment recipients' assets, income, and demographic characteristics such as date of birth, country of birth, and family characteristics (Commonwealth Department of Family and Community Services 2002). Because the database is tied to payment intervals, it is fortnightly in periodicity.

The LDS has advantages and disadvantages for evaluating the impact of activity test arrangements. Heckman et al. (1998) suggested that the quality of any quasi-experimental evaluation study using a matching method is likely to be appreciably affected by three key features: whether data for treatment and control groups are collected using the same survey instrument; whether it is possible to control at a detailed level for local labor market conditions; and whether it is possible to match treatment and control observations using labor market history. On each of these criteria the LDS appears satisfactory. First, data on JSD participants (the treatment group) and non-participants (the control group) can be drawn from the same database. Second, data on the region of residence are 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 when 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 labor market status or income. Therefore, analysis of effects of activity test arrangements must focus on outcomes that are related to receipt of unemployment payments.

Sample Choice

The sample is unemployment payment spells (on NSA or YA(o)) that began between July 1, 1997, and June 30, 1998. This time period is the earliest phase of operation of the JSD for which it is possible to identify JSD participants. (Although the JSD was introduced in July 1996, no administrative data were collected on JSD participation for its first year of operation.) In recent years the JSD has had almost universal application, so it would not be possible to use a quasi-experimental method for very recent periods.² JSD participation is identified from the variable "number of JSD contacts" in the LDS. NSA/YA(o) payment recipients are assumed to have participated in JSD in any fortnight in which they had a non-zero entry for that variable.

The sample is restricted to payment recipients subject to the activity test and for whom "job search" was the activity test type at the start of a payment spell. The activity test and job search restrictions are imposed because they were prerequisites for JSD participation. They thus serve to exclude from the sample those payment recipients who would have been ineligible for the JSD either because they were exempt from the activity test, had not worked for 12 months, were on variable reporting, or had problems related to disability or illiteracy. Essentially this should restrict the group of JSD non-participants to payment recipients exempted under the "discretionary" category.³ The sample is also restricted to payment recipients aged

²As a possible extension, we did also consider the period between July 1998 and June 2000, during which JSD was not universally applied. However, we judged that for these time periods it could not be assured that the "conditional independence assumption" would hold. Specifically, the basis on which we will argue that there was an important source of randomness in assignment to JSD participation during 1997-98 does not seem to exist in the later time period.

³The LDS does not include a variable for "reason for exemption from JSD." Hence, it is necessary to use an indirect method (based on the "activity test type" variable) to exclude JSD non-participants likely to have been exempted for reasons associated with labor market disadvantage.

Table 1. Distribution of JSD Participants and All Unemployment Payment Recipients Beginning New Spells, by Characteristics: Payment Recipients Aged 18 to 49 Years, July 1997 to June 1998.

	JSD Participants	Non-JSD Participants
<i>Gender</i>		
Male	67.4	68.8
Female	32.6	31.2
<i>Age</i>		
18–24	47.0	36.3
25–34	31.9	32.7
35–49	21.1	31.1
<i>Unemployment History in the Previous Four Quarters^a</i>		
Never	65.1	49.7
Not Frequent/Not Recent	22.7	24.8
Not Frequent/Recent	9.5	16.4
Frequent/Not Recent	1.1	3.6
Frequent/Recent	1.6	5.6
<i>Rate of Unemployment—Local Labor Market</i>		
1 st Quartile (Lowest Rate of UE)	21.9	21.8
2 nd Quartile	27.4	25.2
3 rd Quartile	23.7	22.5
4 th Quartile (Highest Rate of UE)	27.0	30.6

^aUnemployment relief is defined as “Frequent” for those receiving payments in 3–4 quarters in the previous 12 months, and as “Not Frequent” for those receiving payments in only 1–2 quarters. “Recent” unemployment relief is that received in the quarter immediately prior to commencement of the new payment spell.

18–49. This is motivated by the concentration of JSD participation among younger age groups—in 1997–98, less than 10% of JSD participants were aged 50 years or over (Commonwealth Department of Family and Community Services 2000).

For the purposes of this study a new spell on NSA or YA(o) is defined to begin when either of two conditions is observed: the payment recipient had been off all social security payments for at least four consecutive fortnights, where the payment spell duration was less than or equal to 23 fortnights; or the recipient had been off all payments for at least seven consecutive fortnights, where the payment spell duration was more than 23 fortnights. This definition is adopted for consistency with the FaCS definition of a new payment spell.⁴

⁴The Social Security Act 1991 defines a “notional continuous period of receipt of income support payments” as one in which the maximum break from payments in the first 12 months of payment receipt is 6 weeks, and the maximum break in subsequent months is 13 weeks; and

Descriptive Information⁵

In the sample period 57,779 new NSA/YA(o) payment spells commenced. In 73.4%

where a break in payments begins prior to, but within 6 weeks of, 12 months’ duration, the 13-week test applies. Information on payment receipt from the LDS is only available on a fortnightly basis. Since it is possible for a break in payments of 3 fortnights to correspond to a break in payments of exactly 6 weeks, so that according to the FaCS definition a new spell would not have commenced, to define new spells in this study we adopt the rule of requiring a break of 4 fortnights off payments, where spell duration is less than 23 fortnights. For the case where spell duration is more than 23 fortnights, and the FaCS rule for a new spell is a payment break of 13 weeks, it is necessary to use 7 fortnights as the period off payments to define new spells.

⁵Descriptive statistics compare—for payment spells commencing in 1997–98—all payment recipients who ever participated in JSD with payment recipients who never participated in JSD. By contrast, as will be explained later, the quasi-experimental analysis uses a subset of JSD non-participants and some JSD participants in the control group. As well, some observations included in the descriptive statistics are excluded from the quasi-experimental analysis due to missing information on matching covariates.

of these spells, at least one fortnight of JSD participation occurred. For over 95% of payment recipients in the sample who participated in the JSD program, the first spell on JSD began in the first fortnight of the payment spell. Almost all NSA/YA(o) payment spells involved only a single episode of JSD participation. The gender composition and distribution of JSD participants was similar across local labor markets ranked by unemployment rate, but compared to all new payment recipients in the sample period, JSD participants were slightly younger and less likely to have received unemployment payments in the previous 12 months (Table 1).

Effect of JSD—Theory

JSD introduces a minimum requirement for job search intensity (number of required job contacts per fortnight). Unemployment payment recipients are of course likely to differ from one another in their unconstrained choice of optimal job search intensity. Hence, depending on the level of job search required, for some payment recipients JSD may represent a binding constraint requiring an increase in search intensity, but for others it may require no adjustment in search behavior. Those payment recipients who would need to increase their intensity of job search to meet the JSD requirement must choose whether to comply. This will involve a comparison of the expected costs and expected benefits of complying: the higher opportunity costs of search versus the higher expected flow of future unemployment payments due to a reduced probability of “breach.”⁶ Any increase in search intensity during the period of JSD participation may also have spillover effects on search behavior outside that time—for example, payment recipients could reduce the cost of complying with JSD by increasing search intensity during JSD participation but decreasing search intensity after completion of participation.

Where implementation of the JSD does increase search intensity for some payment recipients, three main effects are predicted by search-theoretic labor market models. First, the program will cause an increase in the rate of outflow from unemployment to employment due to an increase in the rate of matching between unemployed workers and job vacancies (an inward shift of the Beveridge curve). Second, it will raise labor market tightness, since increased intensity of job search elevates the productivity of new jobs, which in turn increases the rate of creation of new jobs. There may also be a further effect of the JSD. The requirement to undertake extra job search may increase the “disutility” of unemployment. This would lower the reservation wage of an unemployed job-seeker, and hence increase the rate at which job offers are received and thereby the rate at which exit from unemployment will occur. Each of the possible effects of JSD identified will cause an increase in the rate of outflow from unemployment, and a reduction in the equilibrium rate of unemployment (Pissarides 2000, Chap. 5).

Methodology

Empirical Method—Introduction

The empirical approach used to estimate the effect of the JSD is a quasi-experimental matching method. Fundamentally, this involves comparing payment outcomes for a treatment group of NSA/YA(o) recipients who participated in JSD with payment outcomes for a matched control group of NSA/YA(o) recipients.

Effects of the JSD on a variety of outcome measures are examined. The JSD requirement is for a maximum period of six fortnights. Outcome measures have been chosen to attempt to capture short-run and possible long-run effects of the JSD. One measure will be the effect of JSD on the incidence of exit from payments by three months and six months after JSD commencement. An exit from payments is defined as occurring when a NSA/YA(o) payment recipient ceases receiving the payments for at least three consecutive fortnights. A recipient is defined to be “on

⁶In 1998–2000 the incidence of breaches related to JSD was on average 2–3% of payment spells with JSD participation. This suggests a relatively high degree of enforcement of the program.

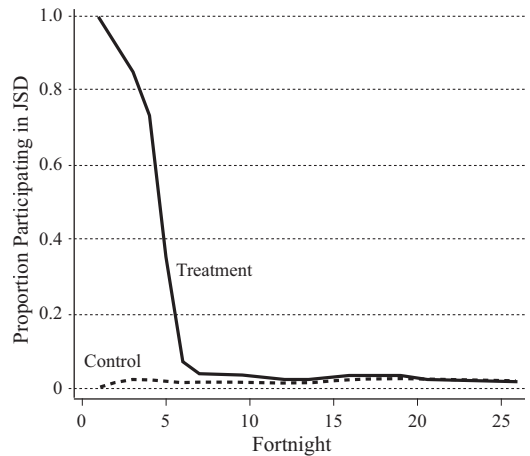
payments” in any fortnight in which he or she lodges a claim form (SU19), regardless of payment entitlement. A second measure is whether payment recipients are on payments at six months and twelve months after JSD commencement. The first and second measures will diverge where payment recipients exit payments, but then begin a new payment spell that is on-going at the specified duration. The third measure applied is the effect of JSD on the number of fortnights of payments during the six months and twelve months after JSD commencement.

JSD participation can begin at any of various times after unemployment payments commence, and for some payment recipients it occurs throughout our sample period. This potentially complicates the classification of payment spells as treatment or control observations. Our basic approach (following Sianesi 2004) is to define (a) a treatment group—NSA/YA(o) recipients who commenced JSD participation in the first fortnight of a payment spell; and (b) a potential control group—NSA/YA(o) recipients who did not commence JSD participation in the first fortnight of a payment spell. The rationale for our treatment group definition is that virtually all JSD participants commence participation in the first fortnight of their payment spell and have only a single spell on JSD. The sample of unemployment payment recipients is also restricted to those with “job search” as their activity type; hence the control group will exclude unemployed persons ineligible for or exempted from JSD participation in the first fortnight of their payment spell for reasons associated with labor market disadvantage.⁷

The empirical method has direct consequences for the policy effect that is identified. Estimates of the effect of JSD participation are the *average effect of commencing participation in*

⁷As well, all payment recipients classified as commencing participation in JSD in the first fortnight of a payment spell will have been assigned into the program from day 1 of their spell, and their JSD participation status on that day is known from the LDS; hence it is not possible for a payment recipient to have been assigned to the JSD and exited payments without the assignment to JSD being observed.

Figure 2. Proportion of Treatment and Control Observations Participating in JSD, by Payment Spell Duration (Fortnight).



JSD in the first fortnight of a payment spell (for the specified group of NSA/YA(o) recipients aged 18 to 49 years) compared to not commencing participation in JSD in the first fortnight of a payment spell. In other words, the policy effect identified is the effect of “treatment on the treated” for payment recipients who commenced a JSD spell in the first fortnight of their spell on unemployment payments.⁸

Figure 2 provides information on the pattern of participation in the JSD for the treatment and control groups.⁹ By definition, in the first fortnight the participation level is

⁸More generally, it is possible to extend the matching approach to estimate the average effect of participation in treatment for individuals who commenced treatment at other specific payment spell durations, or an overall average effect for individuals who commenced between fortnights 1 and F . The number of JSD participants who commenced JSD in later fortnights was too small, however, for us to perform such an exercise.

⁹In making this comparison, we weight control group observations using the same kernel weights subsequently used in the matching analysis. Note that the proportion of the treatment group participating in JSD declines with spell duration for two reasons. First, some payment recipients exit JSD but remain on unemployment payments. Second, other payment recipients may exit unemployment payments.

100% for the treatment group and 0% for the control group. In subsequent fortnights there is convergence, primarily as JSD participants exit payments. For the first four fortnights treatment group participation is above 75% and control group participation is below 10%; in the fifth fortnight the respective rates of participation are about 40% and 5%; and from the sixth fortnight onward, treatment and control group participation is similar. Hence, what is essentially being studied is the effect of a program that on average involves a large difference in participation by treatment and control groups for 4–5 fortnights.

Empirical Method—Motivation

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

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

Almost certainly the most important precondition for performing a matching analysis is to establish a sound basis for the CIA. In this study we approach that task in two ways. First, we suggest a source of randomness in assignment of unemployment payment recipients between participation and non-participation in the JSD. (This is of course conditional on already having restricted the sample to payment recipients eligible for JSD participation.)

During the initial phase of the JSD program's operation, the pattern of assignment to JSD participation was sharply affected by an industrial relations dispute in Centrelink. The existence of this dispute is known from newspaper reports and interviews undertaken by the authors with Centrelink staff.¹⁰ The dispute is difficult to incorporate directly into analysis of effects of the JSD since it was not formally declared an industrial action, was not universal, and did not have definite start and stop dates. Briefly, in 1996–98, staff at some offices, as part of their protest against changes to activity test arrangements introduced by the new federal government, staff cutbacks, and other developments, refused for various periods of time to assign unemployment payment recipients to JSD. Information provided to the authors in interviews with Centrelink staff indicates that variation across Centrelink offices in the proportion of staff participating in the dispute was related to industrial relations issues rather than characteristics of payment recipients; and that within Centrelink offices specific Centrelink staff members imposed a complete ban on the JSD, rather than, for example, *each and every* staff member excluding some chosen sample of unemployment payment recipients from assignment to JSD. Hence the dispute potentially constitutes a source of randomness in the probability of assignment to JSD between geographic regions, and in assignment to JSD within Centrelink offices.

Evidence is available to support the effect of the industrial relations dispute on assignment to JSD between and within geographic regions. First, it is possible to analyze the geographic distribution of assignment. In the absence of the industrial relations dispute, one would expect uniform assignment to JSD across geographic regions. Application of the “dartboard” test statistic (Ellison

¹⁰See, for example, “PS union urges dole diary boycott,” by Innes Wilcox, *The Age*, July 17, 1996, p. A6; and “Public service strikes at cuts,” by Joanne Painter, *The Age*, July 24, 1996, p. A4. The interviews were with Tony Hedditch and other Centrelink officers at the Moorabbin and Glen Waverley Centrelink offices in February 2001.

and Glaeser 1997), however, shows that JSD participation was not uniformly distributed. The Ellison-Glaeser test statistic measures the deviation of actual geographic concentration from the predicted concentration under an assumption of random distribution. Applying the test using 67 local labor market regions (ABS Labor Force Regions, or LFRs), we find a statistically significant difference between the actual and predicted random geographic distribution.¹¹ As well, it can be demonstrated that the geographic distribution of JSD participation was not correlated with local labor market conditions. Various measures of local labor market conditions—rate of unemployment, rate of inflow to unemployment, rate of outflow from unemployment, and first-differences of these measures—were regressed on the rate of participation in JSD in 1997–98 by ABS LFR.¹² This was done using labor market measures from 1997–98 (the period of analysis of effects of the JSD), 1996–97 (the first year of operation of JSD), and 1995–96 (the year prior to operation of JSD). For none of the local labor market measures is there evidence of a consistent statistically significant relation with JSD participation. Furthermore, the geographic variation in JSD participation would not have been related to the incidence of other labor market programs. In the time period analyzed in this study, other major Commonwealth programs, such as Mutual Obligation, were yet to be introduced; and state governments in Australia do not have an important role in provision of labor market assistance.

Second, the degree of randomness in assignment of individual payment recipients to JSD can be examined. One manifestation of the effect of the industrial relations dispute appears to have been the very high propor-

tion of JSD exemptions in the “discretionary” category during this period. For example, in July 1997 this exemption category accounted for about three-quarters of non-participation in JSD, and for the whole period 1997–98 it accounted for about two-thirds of total exemptions. After that time—consistent with resolution of Centrelink industrial relations problems—discretionary exemptions were less common; for example, they accounted for only about one-quarter of total exemptions in mid-1999 (Commonwealth Department of Family and Community Services 2000, Chart 2.4). Where the industrial relations dispute affected the pattern of JSD participation, we would also expect to see some structural change in the determinants of JSD participation between time periods when the dispute did and did not affect assignment. To test this hypothesis, we estimated a set of models for JSD participation by unemployment payment recipients. In these models we treat 1997–98 as the “dispute” period and 1998–99 and 1999–2000 as “non-dispute” periods. Each model includes observations for the dispute and non-dispute periods, and the joint significance of interactions between the set of explanatory variables for JSD participation and non-dispute periods is used to test for structural change in the determinants of participation between dispute and non-dispute periods. In each model (comparing 1997–98 with 1998–99, 1999–2000, and 1998–2000), it is possible to reject the hypothesis of no structural change in the determinants of participation at the 1% level of significance.¹³

¹¹Actual geographic dispersion is measured as $G = \sum_i (s_i - x_i)^2$, where s_i and x_i are, respectively, the share of JSD participants in ABS Labor Force Region (LFR) i and the share of payment recipients in LFR i . The benchmark geographic dispersion for random assignment is $E(G) = (1 - \sum_i (x_i)^2) / H$, where $H = \sum_i (1 / \sum_i (x_i)^2)$. For the variance formula, see Ellison and Glaeser (1997:907).

¹²Details of the results are available from the authors on request.

¹³We are grateful to a referee for suggesting this test. Each model estimated includes as explanatory variables gender, age category, country of birth category, marital status and whether the person's partner is receiving payments, presence of children, indigenous status, housing type, unemployment payment history category, rate of unemployment by ABS LFR, and the calendar month in which the payment spell commenced, as well as interactions of these variables with an indicator for “non-dispute” time period(s). For each model the hypothesis of joint significance of the interaction between the “non-dispute” time period and the explanatory variables cannot be rejected at the 1% level of significance using a χ^2 test. Further details of results are available from the authors on request.

The second justification for the CIA is that treatment and control group observations can be matched using a relatively rich set of covariates. Most important, it is possible to match on the basis of local labor market characteristics and unemployment payment history. These two factors have been identified as of particular importance in evaluations of matching estimators (for example, Card and Sullivan 1988; Heckman et al. 1999; Kluge 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 the ability to control for unemployment payment history. Recent studies for Australia, using other data sources, establish the importance of labor force history in explaining labor market status. Le and Miller (2001) and Knights et al. (2002) have shown that once labor market history is controlled for, other standard covariates have very little explanatory power for whether a labor force participant is unemployed or employed.

Empirical Method—Implementation

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

Stage one of the PSM approach is to estimate a probit model for whether a payment recipient in the sample group commences participation in JSD in the first fortnight of the payment spell. Covariates included in the model are gender; age category; country of birth category; marital status and whether the individual's partner is receiving unemployment payments; presence of children; indigenous status; housing type; unemployment payment history category; rate of unemployment by ABS LFR; and calendar month the payment spell began. The unemployment payment history variable is defined over the twelve months prior to the commencement of the payment spell of each treatment or control group observation. The twelve-month period is divided into four quarters, and each quarter receives a {0,1} clas-

sification for whether the individual was ever observed to receive unemployment payments in that period. Hence there are sixteen possible combinations of payment history—for example, (0,0,0,0) would denote that in no quarter in the previous twelve months was the individual in receipt of unemployment payments, and (1,1,1,1) would denote that the individual was on unemployment payments in at least one fortnight in each of the previous four quarters.

To find an appropriate functional form of the probit model for participation in JSD, we use a balancing test (see Dehejia and Wahba 1999, 2002; Smith and Todd 2005). Rosenbaum and Rubin (1983, theorem 2) showed that the functional form of the PSM model should be chosen such that, after conditioning on the predicted probability of participation from the probit model, there is no further dependence between participation and higher-order terms or interactions of the matching variables. This motivates the “balancing test”—a test of whether, after conditioning on the predicted probability of program participation, there is a statistically significant difference between program participants and non-participants in the value of any matching variable.

Application of the balancing test revealed that the functional form that minimized the number of strata for which a jointly statistically significant difference in the set of matching variables was found to exist between JSD participants and non-participants (at the 5% level of significance using Hotelling T-test) was one that included the interaction between gender and age, the interaction between presence of children and age, and the rate of unemployment at spell commencement, as well as a quadratic term for the rate of unemployment by ABS LFR.¹⁴

¹⁴To apply the balancing test, we divided the observations into 40 strata according to predicted probability of participation in JSD. Because it was found that test results were insensitive to choice of number of strata greater than 40, the analysis was restricted to this level of disaggregation. For the chosen functional form, only for one set of matching variables did a statistically significant difference exist. Entering other interaction effects, or higher-order terms of the rate of unemployment variable, did not improve the result.

Stage two of the PSM is to match treatment and control group observations, using what we describe as “quasi-exact matching.” This procedure involves two steps: exact matching on the basis of fortnight of payment spell (implicit in choice of treatment and control groups) and gender, and then use of the results from the PSM probit models to match treatment and control observations within each gender group. To obtain aggregate estimates of the JSD program effect, we calculate a weighted average of the estimated effects for men and women.¹⁵

Previous studies have suggested that when the “curse of dimensionality” rules out implementing an exact matching approach, exact matching on a subset of covariates that are particularly important determinants of the outcome may still be important to obtain valid estimates of program effects (see, for example, Card and Sullivan 1988; Kluve et al. 2002). Previous researchers have, for that reason, sought to apply exact matching on important covariates (for example, Rubin and Thomas 2000), and the same considerations motivate our exact matching on gender (and subsequent robustness analysis that involves exact matching on payment history). An alternative way to incorporate exact matching by gender would be to also estimate separate PSMs by gender, prior to the step of matching treatment and control observations within gender groups. (Our approach can be referred to as ex-post quasi-exact matching, and the alternative approach as ex-ante quasi-exact matching.) However, since a range of variables interacting gender and the matching covariates is included in the PSM in this study, these approaches should be very similar. Subsequent robustness analysis shows that estimating separate PSMs by gender has no statistically significant effect on the results, either in aggregate or disaggregated by gender.

A formal description of the estimated JSD effect using the “basic” matching estimation method is

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

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

Implementing matching of treatment and control observations (within each gender group) involves two steps. First, each treatment observation is matched to a weighted average of control observations in a 5% confidence interval (the caliper method), and a difference in outcome is calculated. Second, the aggregate effect of JSD participation is then calculated as the average of the difference in outcomes across all treatment observations. Matching is undertaken using the linear predicted score from PSM, which is preferred to the predicted probability as this allows symmetry in selection of control observations using the caliper method. Re-sampling of control observations across different treatment observations is allowed. Weights for control observations are derived using kernel weighting,

$$(2a) \quad w_m(i, j) = G_m^{ij} / [\sum_{j \in \{D_m^1=0\}} G_m^{ij}]$$

and

$$(2b) \quad G_m^{ij} = G[X_i \hat{\beta}_m - X_j \hat{\beta}_m] / a_{5\%},$$

where G_m^{ij} is the kernel for the i^{th} treatment and j^{th} control observations for the male sample, $X_i \hat{\beta}_m$ and $X_j \hat{\beta}_m$ are linear predicted scores for

¹⁵Results of the first stage probit model are available from the authors on request.

Table 2. Effects of JSD: NSA/YA(O) Recipients Aged
18 to 49 Years with at Least One Fortnight on JSD, July 1997 to June 1998.
("Basic" Matching Method)

	<i>Treatment</i>	<i>Control</i>	<i>Difference</i>	<i>p-Value</i>
<i>Percent off Payments</i>				
By 3 Months	36.6	31.5	+5.1	0.000
By 6 Months	58.7	54.4	+4.3	0.000
<i>Percent on Payments</i>				
At 6 Months	49.1	53.7	-4.6	0.000
At 12 Months	35.1	39.4	-4.3	0.000
<i>Time on Payments</i>				
First 6 Months	7.887	8.296	-0.409	0.000
First 12 Months	12.958	13.888	-0.930	0.000
<i>Number of Observations</i>				
Observations Matched	39,280	15,643		
Total Number of Observations	39,287	15,645		

the respective treatment and control observations in the male sample, and $a_{5\%}$ represents the use of a 5% confidence interval bandwidth around $X_i\hat{\beta}_m$. In this approach the biweight kernel is used.¹⁶

Three alternative ways to implement the matching method are also considered. The alternatives involve (a) changing the set of control observations by using nearest neighbor matching, local linear matching, or a common caliper, (b) using the predicted probability of participation, and (c) changing the weights on control observations by restricting to equal weights for each observation.

For the matching estimator to be valid, the CIA and common support assumptions must hold. There is no formal test for the CIA; instead, above we have cited reasons for believing that the assumption is justified. The common support assumption, however, can be assessed empirically. Figure 3 presents the linear predicted score from the PSM for treatment and control observations. It is apparent that the common support assumption is satisfied, there being a high degree of overlap between the distributions—although clearly the treatment observations are more concentrated at higher predicted scores. Using the

basic matching method, only 7 out of 39,287 treatment observations cannot be matched to a control group observation. The average number of times each control observation was used is 2,423, with a minimum of zero and a maximum of 5,652. The average number of control observations matched to each treatment observation is 965, with a minimum of zero and maximum of 10,468. The average proportion of matched control observations that began a JSD spell in the second fortnight or later is 9.5%, with a minimum of zero and maximum of 74.6%. Therefore, on average a relatively small proportion of control observations that are used in the matching ever participated in JSD.¹⁷

Effects of the JSD

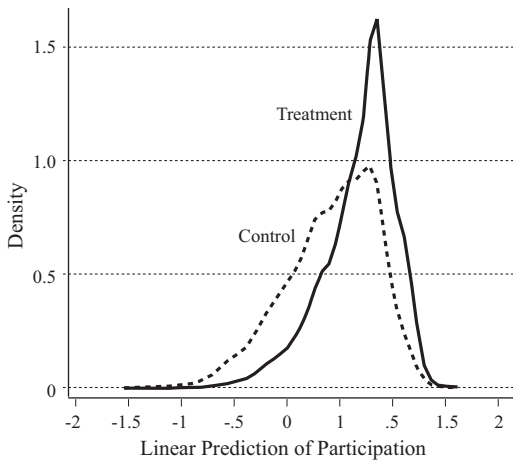
Basic Model Results

Findings from matching method analysis of the effects of the JSD for the basic approach

¹⁶The terms $w_{(i,j)}$ and G_f^{ij} are defined in the same way for the female sample.

¹⁷One method for assessing the quality of matching is to compare the mean values of characteristics used in matching for treatment and control observations. Some differences are apparent by age and indigenous status, but they are quantitatively small. Overall, the results suggest that the choice of control observations has created a comparison group that is on average very similar to the set of treatment observations. Results are available from the authors on request.

Figure 3. Linear Prediction of Commencing in JSD in First Fortnight of Payment NSA/YA(o) Spell, July 1997 to June 1998.



are presented in Table 2. The results demonstrate that JSD participation had a quite large, and statistically significant, negative effect on the duration of unemployment payment spells. For example, the proportion of JSD participants who had exited unemployment payments by three months after the start of their payment spell is 36.6%; by comparison, the weighted average exit rate for control observations is 31.5%. Similarly, over the twelve months after commencement of a payment spell, JSD participants spent on average about 13 fortnights on unemployment payments, whereas the weighted average for the control group observations is about 13.9 fortnights.

Extra information on exit from unemployment payments is presented in Figure 4, which compares the proportion of payment recipients in treatment and matched control groups who exited NSA/YA(o) payments in each month after commencement of their payment spells. Differences in rates of exit between JSD participants and non-participants emerged in the second and third months after JSD commencement; in subsequent months there was a slight convergence in exit rates, but the difference appears to have stabilized at about 3.5 percentage points by 9 months

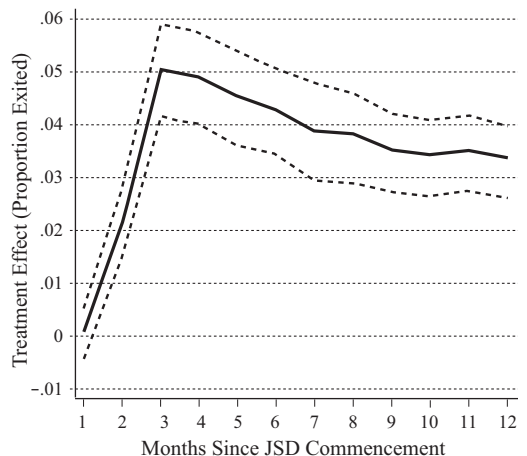
after commencement of JSD participation.

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

Comparison with International Evidence

Using the outcome measure of “time

Figure 4. NSA/YA(o) Payment Recipients: Difference in Proportion of Treatment and Matched Control Groups Exiting Payments, by Month after JSD Commencement (New Spells Commencing July 1997 to June 1998).



Note: The solid line is the estimated average effect of treatment. Dashed lines are 95% confidence intervals estimated using a bootstrap method with 200 replications.

on payments in the twelve months after commencement of JSD,” we can compare our results to those from the Maryland experimental analysis of additional required employer contacts (Klepinger et al. 2002). The two programs are quite similar in the increased job search requirement they imposed: required contacts per fortnight rose from two to eight (for most participants) under the JSD program and from four to eight in the Maryland experiment. JSD participation is found to have reduced time on payments by 0.93 fortnights, representing a reduction of about 6.7% in the average of approximately 13.9 fortnights the control group spent on payments; similarly, in the Maryland study, the 0.36-fortnight reduction in the time on benefits that was associated with the increased number of required job contacts represented a reduction of about 6% in the average of 6 fortnights the control group spent on payments (Klepinger et al. 2002, Table 3). Therefore, it appears that the programs had very similar effects.

Sensitivity Analysis

Results estimated using the “basic” matching method can be compared with results from alternative methods of estimation. One method is a comparison of mean outcomes using raw data. A second is to use a set of single equation models with the same set of explanatory variables used for matching as well as an indicator for JSD participation. Models for the binary outcome variables are estimated using a probit model with a marginal effect of JSD participation reported, and OLS models are estimated for the time on payment outcome variables. The third approach is an IV method. To implement this approach, a model for each outcome is estimated by maximum likelihood with JSD participation treated as an endogenous regressor and the incidence of JSD participation by ABS Labor Force Region included as an “instrument” (see Heckman 1978). Other explanatory variables in the model are the same as the set of covariates for the matching model. For the binary outcomes, the mean marginal effect of JSD participation is calculated as an average of the effect for

each treatment observation—which is the predicted change in the estimated probability of the outcome between JSD participation and non-participation holding other explanatory variables at their values for that observation. For the time on payment outcomes, the estimated effect of JSD participation is simply the coefficient estimate from the model.

The results, shown in Table 3, are qualitatively similar across the three analyses. The magnitude of estimated JSD effects is also similar across the matching, IV, and probit/OLS methods, but it differs from the raw data means. The similarity in results between matching and IV methods suggests that the industrial dispute does provide a source of randomness in assignment to JSD. It also appears that while it is important to control for differences in covariates between treatment and control groups, the results are not strongly sensitive to the exact approach used to control for those differences.

Estimates of the effect of the JSD using alternative matching methods are also highly robust.¹⁸ Only for the nearest neighbor method do the results differ appreciably from the basic method results; and even for the nearest neighbor method it is found that JSD participation was associated with a statistically significant increase in exit from unemployment payments and decreased time on payments. For example, using the basic approach we find that JSD participation reduced time on payments during the first twelve months after commencement by 0.93 fortnight. Using the nearest neighbor method, the estimate is a decrease of 0.69 fortnight, but estimates from the other approaches are from -0.93 to -0.96 fortnight.

At the aggregate level, the results are also unchanged in sign and statistical significance when we use ex-ante quasi-exact matching (that is, estimating separate PSMs by gender rather than a single PSM), as well as when we keep a single PSM but do not apply quasi-exact matching by gender. For example, the three methods estimate the reduction in time on payments in the twelve months after

¹⁸Results for the sensitivity analyses described in this subsection are available from the authors on request.

*Table 3. Effects of JSD: NSA/YA(O) Recipients
Aged 18 to 49 Years with at Least One Fortnight on JSD, July 1997 to June 1998.
(Alternative Estimation Methods)*

	<i>Basic Method</i>	<i>IV</i>	<i>Probit/OLS</i>	<i>Raw Data— Difference in Means</i>
<i>Percent off Payments</i>				
By 3 Months	+5.1	+4.8 (2.0)	+4.3 (0.4)	+6.6 (0.4)
By 6 Months	+4.3	+2.7 (2.1)	+4.0 (0.5)	+6.4 (0.5)
<i>Percent on Payments</i>				
At 6 Months	-4.6	-4.7 (2.1)	-4.5 (0.5)	-7.0 (0.5)
At 12 Months	-4.3	-7.3 (2.0)	-3.9 (0.5)	-6.4 (0.5)
<i>Time on Payments</i>				
First 6 Months	-0.409	-0.415 (0.169)	-0.352 (0.039)	-0.570 (0.035)
First 12 Months	-0.930	-0.901 (0.353)	-0.826 (0.082)	-1.380 (0.074)

Notes: IV = maximum likelihood estimation of the model with JSD participation as an endogenous regressor and “percentage of payment recipients in ABS LFR” as that instrumental variable; Probit/OLS = single equation estimation of the effect of JSD participation.

Standard errors are in parentheses. All standard errors are calculated using a bootstrap method with 400 replications.

commencing JSD to be between 0.91 and 0.93 fortnight. However, results by gender differ depending on whether quasi-exact matching is applied. This suggests that the approach of quasi-exact matching by gender is to be preferred, but that the findings are not sensitive to estimating separate PSMs by gender.

Other robustness checks involve alternative ways of incorporating payment history. One check is to extend the exact matching stage to include payment history. Ex-ante matching is applied using five categories of payment history, and the ex-post matching is applied using 16 categories of payment history. Again, the results are found to be highly robust with respect to the use of the alternative matching method. A second check is to use alternative payment history variables—specifically, for each six-month period over the previous two years, (a) whether the person was on any payment in any fortnight (16 categories), and (b) whether the person was on unemployment payments in any fortnight (16 categories).

These alternative tests are also found to have only a minimal impact on estimated JSD effects. Estimates of the reduced time on payments during the twelve months after JSD commencement are 0.91 and 0.84 fortnight (respectively), compared to the estimate of 0.93 fortnight from the basic approach.¹⁹

Finally, we consider the effects of using alternative treatment and control groups, and of an alternative definition of exit from payments. First, we examine the effect of using a control group of payment recipients

¹⁹Standard errors generated thus far to test differences between treatment and control group outcomes assume only “normal” sampling variation. However, estimation of propensity scores and the process of matching between treatment and control observations are both extra sources of variation that need to be taken into account (Smith 2000:13). Hence we also apply a method for calculating standard errors using the approach of “randomization inference” (see Rosenbaum 1996; Bertrand et al. 2004). Using this approach, we find that all JSD effects reported in Table 2 are significant at the 1% level.

*Table 4. Effects of JSD: NSA/YA(O) Recipients
Aged 18 to 49 Years with at Least One Fortnight on JSD, July 1997 to June 1998.
(Exact Matching on Payment History and Disaggregated by Payment History)*

	<i>No History</i>	<i>Not Frequent</i>	<i>Frequent</i>
<i>Percent off Payments</i>			
By 3 Months	+4.5 (0.000)	+4.0 (0.000)	+5.6 (0.000)
By 6 Months	+3.5 (0.000)	+2.9 (0.000)	+1.4 (0.392)
<i>Percent on Payments</i>			
At 6 Months	-3.6 (0.000)	-3.7 (0.000)	-2.0 (0.211)
At 12 Months	-3.6 (0.000)	-1.5 (0.001)	-2.5 (0.107)
<i>Time on Payments (Fortnights)</i>			
First 6 Months	-0.317 (0.000)	-0.285 (0.000)	-0.221 (0.079)
First 12 Months	-0.777 (0.000)	-0.508 (0.000)	-0.423 (0.105)

Note: No History = unemployment payments not received in any quarter in the previous 12 months; Not Frequent = unemployment payments received in 1 or 2 quarters in the previous 12 months; Frequent = unemployment payments received in 3 or 4 quarters in the previous 12 months.

who never participated in JSD. The results obtained using this “restricted” control group are similar to those from the “basic method.” For example, the estimated reduction in time on payments in the twelve months after program commencement is 0.92 fortnight. The lack of sensitivity is probably not surprising given that those observations, on average, account for less than 10% of the control group. Second, exit from payments is defined to occur only where a NSA/YA(o) recipient exits from all income support payments. This represents a stricter definition of exit—since exit will not now be defined to occur where a NSA/YA(o) recipient exits from the unemployment-related allowance but commences a spell on some other income support payment (such as a Disability Support Pension). With the alternative definition of exit, the estimated effect of JSD on the rate of exit from payments and time on payments is increased. For example, the estimate of the reduced time on payments in the twelve months after program commencement is 1.8 fortnights. This suggests that JSD participants are less likely than non-participants

to move onto other payment types after exiting NSA/YA(o).²⁰

Results for Disaggregate Groups

Estimated effects of the JSD for NSA/YA(o) recipients with different payment histories and in different demographic groups are shown in Tables 4 and 5. The impact of JSD participation tended to be higher for payment recipients with no history of receiving unemployment payments in the previous twelve months than for those who had received payments for 1–2 quarters in the previous twelve months. Also, the estimated JSD effects for those who had been on unemployment payments for 3–4 quarters in the previous twelve

²⁰One concern arising from these findings is that the higher rate of entry to other types of income support payments after exit from NSA/YA(o) might signify some difference between JSD participants and non-participants—for example, a higher likelihood of eligibility for disability payments among the latter. However, further investigation has found that movements to payment types that might signify a difference in degree of labor market disadvantage account for only a small share of total destination payments.

Table 5. Effects of JSD by Characteristics of Payment Recipients:
NSA/YA(O) Recipients Aged 18 to 49 Years with at Least One Fortnight on JSD.

	<i>Difference in Outcome</i>					
	<i>Percent off Payments</i>		<i>Percent on Payments</i>		<i>Time on Payments (Fortnights)</i>	
	<i>By 3 Months</i>	<i>By 6 Months</i>	<i>At 6 Months</i>	<i>At 12 Months</i>	<i>First 6 Months</i>	<i>First 12 Months</i>
<i>Gender</i>						
Male	+5.5 (0.000)	+4.6 (0.000)	-5.2 (0.000)	-5.1 (0.000)	-0.453 (0.000)	-1.032 (0.000)
Female	+4.2 (0.000)	+3.6 (0.000)	-3.6 (0.000)	-2.7 (0.000)	-0.319 (0.000)	-0.719 (0.000)
<i>Age</i>						
18–24 Years	+3.9 (0.000)	+3.0 (0.000)	-2.8 (0.000)	-2.8 (0.000)	-0.298 (0.000)	-0.593 (0.000)
25–34 Years	+6.6 (0.000)	+5.7 (0.000)	-7.0 (0.000)	-5.6 (0.000)	-0.553 (0.000)	-1.352 (0.000)
35–49 Years	+4.7 (0.000)	+5.0 (0.168)	-5.1 (0.000)	-5.2 (0.000)	-0.414 (0.000)	-0.997 (0.000)
<i>Rate of Unemployment—Local Labor Market</i>						
1 st Quartile (Lowest Rate of UE)	+7.0 (0.000)	+5.9 (0.000)	-6.9 (0.000)	-6.6 (0.000)	-0.626 (0.000)	-1.483 (0.000)
2 nd Quartile	+7.2 (0.000)	+5.7 (0.000)	-6.0 (0.000)	-5.2 (0.000)	-0.572 (0.000)	-1.125 (0.000)
3 rd Quartile	+3.3 (0.000)	+3.4 (0.000)	-2.7 (0.000)	-3.1 (0.000)	-0.235 (0.000)	-0.537 (0.000)
4 th Quartile (Highest Rate of UE)	+2.9 (0.000)	+2.5 (0.000)	-3.2 (0.000)	-2.8 (0.000)	-0.231 (0.000)	-0.547 (0.000)

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

months are similar in size to the estimated effects for those who had received payments for only 1–2 quarters, though they are generally not statistically significant. Slightly stronger effects of JSD participation are apparent for men than for women, and for recipients aged 25–34 years than for those aged 18–24 or 35–49 years. There are very large differences in the impact of the JSD between low- and high-unemployment regions. For example, the estimated effect of JSD participation on the rate of exit from NSA/YA(o) payments in the first three months after spell commencement is 7 percentage points in the lowest quartile rate of unemployment LFRs, but is only 2.9 percentage points in the highest quartile of LFRs ranked using rate of unemployment.

The main finding from the disaggregate analysis is that the impact of the JSD was largest in conditions where labor demand for unemployed job seekers was likely to be relatively strong—that is, where payment recipients did not have an extensive history of unemployment payments, and in regions where the rate of unemployment was relatively low. This finding seems plausible if labor market outcomes from JSD participation depend both on its effect on job search behavior and on labor demand conditions. In a search-theoretic model, the effect of the JSD is to introduce a binding constraint that increases job search effort of some payment recipients. In the situation where the marginal effect of increased job search effort on the arrival

Table 6. Effects of JSD by Number of Required Job Contacts: NSA/YA(O) Recipients Aged 18 to 49 Years with at Least One Fortnight on JSD.

	<i>Percent off Payments by 3 Months</i>		<i>Time on Payments—First 12 Months</i>	
	<i>Difference</i>	<i>p-Value</i>	<i>Difference</i>	<i>p-Value</i>
2	-0.008	0.646	0.365	0.260
3	-0.023	0.034	0.950	0.000
4	-0.014	0.151	0.639	0.260
5	0.009	0.197	-0.195	0.127
6	0.026	0.019	-0.254	0.039
7	0.048	0.028	-0.679	0.002
8	0.076	0.000	-1.528	0.000

rate of job offers is increasing with level of labor demand, payment recipients who have more favorable labor demand conditions will receive more job offers. This would tend to increase outflow from unemployment. An offsetting effect, however, will be the tendency of unemployed persons who expect to receive more job offers to increase their reservation wage, reducing outflow from unemployment. Which effect dominates is an empirical question. Pissarides (2000:161) noted that “the usual assumption made ... is that the job-offer effect ... dominates the reservation wage effect” and that “this assumption is plausible and available empirical evidence strongly supports it.” Thus the findings on the relation between the effects of JSD and labor demand conditions appear consistent with predictions and empirical evidence from existing search theory literature.

Intensity of JSD Participation

There is evidence that effects of the JSD vary with intensity of participation—that is, by number of required job contacts. The pattern shown in Table 6 suggests a threshold at six required job applications: below six, JSD participation generally had no statistically significant effect; at six, the impact became statistically significant; and increments in the number of required applications beyond six were associated with further increases in impact. This finding seems consistent with the prediction from search theory that JSD will increase outflow from unemployment where it introduces a binding constraint that increases search intensity, since this is

more likely to occur the larger the number of required job applications.

One potential problem with this analysis stems from administrative features of the JSD program: since Centrelink case workers were supposed to assign the number of required job applications on the basis of the degree of labor market disadvantage of a job seeker and local labor market conditions, the results could be simply proxying for selection effects, or for labor demand conditions. However, OLS regression analysis reveals that payment history (16 categories) and local rate of unemployment can explain only 3.1% of variation in the number of required job contacts among the sample of JSD participants. Hence it appears that labor demand conditions did not strongly influence the choice of the number of required job contacts or, through that variable, differences in effects of the number of job contacts on receipt of unemployment payments.

Distributional Effects

An overall perspective on heterogeneity in the impact of the JSD can be obtained by comparing the distribution of the outcome measure—time on payments in the twelve months after JSD participation—across the treatment and control groups. Following Heckman et al. (1997) and Heckman (2001), we make this comparison for alternative assumptions on the rank correlation between treatment and control groups.

Two main findings are evident from the results, which are reported in Table 7. First, at least half of the JSD participants are estimated to have had lower time on payments

Table 7. Distribution of the JSD Effect on Fortnights on Payments in the First 12 Months after Commencement of JSD, by Percentile, 1997–98.

	<i>Perfect Positive Correlation</i>	<i>Perfect Negative Correlation</i>	<i>Independent (Random)</i>
5 th Percentile	-3	-24	-21
25 th Percentile	-2	-19	-11
50 th Percentile	-2	-2	-1
75 th Percentile	0	16	7
90 th Percentile	0	24	20
Percent with Less Time on Payments	73.4 (1.74)	53.7 (0.46)	51.6 (3.87)
Impact Standard Deviation	1.04 (0.05)	16.99 (0.03)	12.13 (0.61)

Notes: The “Perfect Positive Correlation” case matches the top percentile in the treatment group with the top percentile in the control group, the second top percentiles in the treatment group with second top in the control group, and so on; the “Perfect Negative Correlation” case matches percentiles in reverse order, so that the top percentile in the treatment group is matched with the bottom percentile in the control group, and so on; and the “Independent” case is based on 400 random matches of percentiles. For each case, the difference between each percentile of the treatment and control distributions is the impact for that percentile. The percent positive is the percentage of the percentile impacts greater than zero. These percentile effects constitute the distribution of effects. The impact standard deviation is the standard deviation of the percentile differences.

Bootstrapped standard errors (based on 400 repetitions) are in parentheses.

in the twelve months after commencement of a payment spell than they would have had in the absence of JSD participation. Second, there is evidence of considerable heterogeneity in program effects. For each approach, the impact standard deviation measure is significantly different from zero at the 5% level. It is important in this connection to note that heterogeneity in the JSD program impact does not invalidate the matching method used in this study. If our defense of the CIA is correct (as discussed earlier), JSD assignment does not depend on anticipated benefits from participation; putting this in Heckman’s (2001:F669) terms, the “veil of ignorance” should apply.

Given the heterogeneity in individual effects of JSD participation, it might seem surprising that the estimated effects of the matching and IV approaches (the average effect of treatment on the treated compared to the local average treatment effect) are so similar (Table 3). Certainly this is not a finding that would be expected if there had been self-selection into JSD participation. However, where the instrumental variable is geographic incidence of JSD participation, which it has been argued is uncorrelated with characteristics of local labor markets or individual payment recipients, there is

no basis for expecting the estimated effect of JSD participation on all participants to differ from its effect on participants whose participation status was affected by the geographic pattern of assignment to JSD. (In fact, as noted earlier, the similarity of the matching and IV estimates may be seen as some confirmation of a source of randomness in assignment to participation.)

Cost-Benefit Analysis

Thus far, our analysis of JSD has focused exclusively on how it affected the pattern of payment receipt. Ultimately, however, the social costs and benefits of the program—measured in resource terms—are what should matter to policy-makers. Hence in this subsection we undertake a cost-benefit analysis of the JSD.²¹

The main costs of the JSD program are those associated with diary printing and Centrelink staff time. Using quotes from commercial printers, we estimate the cost of designing and printing to be about \$0.85 per diary. Using information from Centrelink on the average time spent, per payment recipi-

²¹Details are available from the authors on request.

ent, explaining the JSD (to new recipients only), processing information, reviewing diary content and contacting employers if necessary (when the diary is returned), and implementing any breach penalties, together with statistics on the salary and on-costs for a Centrelink case manager, we estimate the cost of administration per participant to be \$13.90.

The main social benefit of the JSD is the gain in resources to society from increased time spent in employment by participants. The average increase in time spent in employment for a JSD participant is calculated using the result on the decrease in time spent on payments in the twelve months after program participation (Table 3), and adjusting for the proportion of individuals exiting unemployment payments who move to employment (using data from the Australian HILDA survey; see Borland and Vu 2005). The resource gain is measured as the average weekly earnings of a person who moves from unemployment payments to employment (again using data from the HILDA survey; see Borland and Vu 2005). Two scenarios for substitution and deadweight loss effects—60% and 90%—are examined, based on Calmfors's (1994) review, which suggested that such effects are likely to be substantial.

The results from the cost-benefit analysis are quite striking. Even where a 90% substitution/deadweight effect is assumed, the estimated net benefit per JSD participant in the twelve months after commencement on the program is \$78.61; when a 60% substitution/deadweight effect is assumed, it is \$358.72. Analysis of the JSD program's costs and benefits for the government's budget also suggests that a net gain exists. In this case, simply taking account of the estimated reduction in unemployment payments, it is found that with a 90% substitution/deadweight effect, there is an improvement in the government budget of \$22.86 per JSD participant in the twelve months after commencing JSD participation. While in this analysis we have not directly incorporated the cost of reduced leisure time for a JSD participant (see Heckman et al. 1999:2044–45), it is evident that such costs

would need to be substantial to offset net benefits of such magnitude.²²

Conclusion

This study has provided quasi-experimental evidence that one job search program—the JSD in Australia—has had a large and sustained effect on the rate of unemployed job-seekers' exit from unemployment payments. While there is already a body of international literature that suggests job search programs can improve labor market outcomes, what is notable about the JSD is its large-scale implementation and its focus on work search verification. The scale of implementation of the JSD, and the broad coverage of the treatment and control groups in this study, mean that there should not be large effects of JSD participation outside those groups. Hence, the findings on JSD can, for example, be interpreted as satisfying the criterion of internal validity, and as providing large-scale evidence in support of findings from the recent Maryland experiment that work search verification does not need to be supplemented by job search assistance in order to increase the rate of exit from payments (Klepinger et al. 2002).

Our analysis suggests lessons for policy-makers both on the circumstances in which a job search program is most likely to be beneficial and on details of implementation. Results for disaggregated groups of program participants show that time on payments fell for a majority of JSD participants, but that the program's impact varied greatly depending on the stringency of its requirements and characteristics of the participants. The largest effects of JSD are found to have occurred for payment recipients for whom labor demand conditions were the most favorable (defined in terms of their own labor market history and local labor market conditions) and who were required to make at least six

²²Note that the measure of benefits for JSD participants is also an under-estimate of the benefit relative to non-participation in JSD, since the control group in this study includes payment recipients who commenced JSD after the first fortnight of their payment spells.

job applications per fortnight.

Our findings on the impact of the JSD are highly robust with respect to application of a wide range of matching methods. The only methodological variable that had a major effect on results was whether a nearest neighbor or alternative kernel-weighted method for choosing control observations was used. This contrasts with early research comparing results from experimental and quasi-experimental studies, which tended to suggest that the results are highly sensitive to the precise matching method used (for example, Heckman et al. 1998). Possibly

the greater resiliency of our findings is owing to the large number of treatment and control observations that were available to us for matching.

A novel aspect of this study is its cost-benefit analysis. This analysis suggests quite large net benefits, both to government and to society, from the JSD. Since the “technology” for implementation of job search programs is not likely to differ much across countries, and the estimated effects of the JSD are similar to those for programs in some other countries, it seems likely that this finding has high generalizability.

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