Effects of activity test arrangements on exit from payments: Mutual Obligation*

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Abstract

This paper examines how the MOI program has affected exit from, and time on, welfare payments for unemployment payment recipients. The focus is the initial phase of the MOI; analysis of the effects of the program for 18 to 24 year old unemployment payment recipients during the first 12 months of operation of the program between 1 July 1998 and 30 June 1999. The initial implementation of the MOI program appears for males to have significantly increased the rate of exit from payments prior to the time at which participation in MOI would have commenced; however, a similar effect is not observed for females. For both groups there is little evidence of this type of threat effect of MOI from 18 months after commencement of the MOI program. The analysis of the effect of MOI participation suggests a strong attachment or lock-in effect of the program. MOI participants are significantly less likely to exit payments than matched non-participants during the period of participation; but once MOI participants exit the program, the effect of participation on exit from payments is fairly quickly undone – MOI participants and matched non-participants have similar rates of receipt of payments twelve months after participation commences.

1. Introduction

Over the past decade the principle of mutual obligation has been one of the main forces driving reform of eligibility requirements for unemployment payments in Australia (Reference Group on Welfare Reform, 2000). Mutual obligation is a requirement that unemployment payment recipients should engage in opportunities provided by the government "… to improve employability and make a contribution to the community in return for payments of unemployment benefits" (Howard, 1999). The formal embodiment of the policy has been the Mutual Obligation Initiative (MOI) under which benefit recipients, whose duration of payment spell reaches a threshold level, must participate in an approved work experience, training or job search activity. In the initial phase of the MOI program, that commenced in July 1998, unemployment payment recipients aged 18 to 24 years who had continuous payment spells of six months, were required to participate in MOI. Subsequently the program has been extended to unemployment payment recipients aged 25 to 49 years.

This paper examines how the MOI program has affected exit from, and time on, welfare payments for unemployment payment recipients. The focus of the analysis is the initial phase of MOI – to examine the effects of the program for 18 to 24 year old unemployment payment recipients during the first 12 months of operation of the program between 1 July 1998 and 30 June 1999. Two main questions are addressed. First, did the introduction of MOI cause a 'threat' or 'compliance' effect whereby payment recipients increase their rate of exit from payments at the date at which they would be required to commence participation? Second, does the experience of participation in the MOI cause a change in the rate of exit from welfare payments or time on payments?

Whether participation in labour market programs improves labour market outcomes for young unemployed persons is an issue of considerable policy significance. Not only does the population aged 18 to 24 years in Australia have the highest rates of unemployment, but as well, potential lifetime costs to society of skill loss and adverse health consequences from unemployment will be higher than for other population groups (Borland and Kennedy, 1998). Hence it is a critical policy issue to have effective ways of providing skills and finding jobs for young unemployed.

Most international literature on youth labour market programs suggests an absence of positive effects on labour market outcomes. Heckman et al. (1999, p.2053) state that: "In the US, studies consistently report that these programs have no impact (or sometimes even a negative

impact) on youth's earnings". For Europe, Kluve and Schmidt (2002, p.440) argue "…youth programmes have usually displayed negative effects. Recent evaluation studies indeed conclude that in Europe, like in the US, it is also true that youths are especially difficult to assist". Nevertheless, it is also the case that review articles always note that there is a high degree of heterogeneity in estimates of the impact of any type of labour market program (for example, Heckman et al., 1999, p.2053). So it is not surprising that there are also recent studies that have found positive effects on youth labour market outcomes from interventions. One example (similar to MOI) from the United Kingdom is the 'New Deal for the Young Unemployed'. Under this program job-seekers aged 18 to 24 years who have been in receipt of unemployment payments for six months enter a two-stage program: first, a four month period of intensive job search; and second, assignment to one of four options that involve training or work experience (Blundell at el., 2001). Initial evaluations of the New Deal program that have focused on the impact of the Gateway job search phase have found that program participation increased the rate of outflow from unemployment for young males by about 20 per cent (Blundell et al., 2001, and Finn, 2002). Another example is the Youth Unemployment Programme (YUP) in Denmark implemented in 1996 to lower youth unemployment. This program – targeted at persons under 25 years unemployed for at least 6 months – involved a requirement to undertake 18 months of vocational training during which unemployment benefits were reduced by 50 per cent. It is found (Jensen et al., 1999) that the program increased the transition from unemployment to formal schooling, and to some extent to employment. The increased outflow from unemployment is attributed partly to a threat or sanction effect on unemployed persons about to become eligible for participation in vocational training, and partly due to program participation.

The existence of heterogeneity in program impacts indicates that it is of considerable importance to evaluate program impacts on a case-by-case basis. The absence of rigorous empirical analysis of previous labour market programs for young unemployed in Australia, and differences in the labour market environment between Australia and other countries where program evaluation has been undertaken, suggests in particular that it is valuable to seek to provide such analysis for the MOI program.

The evaluation of the MOI program that is reported in this paper has several notable features. First, it presents analysis of both 'threat' effects, and effects of participation in MOI, on exit from payments and time on payments. Second, the availability of data over several years allows a longer-term perspective on effects of the MOI program. 'Threat' effects can be

estimated for several time periods after introduction of MOI, and effects of participation in MOI are examined for up to two years after commencement of participation. This seems important given recent evidence on changes in program impacts across time and by length of time after commencement of participation (see for example, Hotz et al., 2000, Blundell et al., 2001, and Lechner et al., 2005). Third, the method of implementation of the MOI, for a restricted age group, allows the choice of a control group that is composed of individuals 'just outside' that age group (similar to a regression discontinuity approach), and at the same time provides a strong motivation for exogenous (age-based) assignment between treatment and control groups.

To address the question of whether the introduction of MOI caused a 'threat' or 'compliance' effect the main approach used is a 'difference-in-difference' methodology; comparing the difference in rates of exit from payments from time periods before and after the introduction of the MOI program between a 'treatment group' from the 18 to 24 year age group and a 'control group' from the 25 to 31 year age group. The robustness of results from the difference-in-difference method is assessed by also examining the threat effect using quasi-experimental matching and difference-in-difference matching methods.

The effect of participation in MOI on the rate of exit from welfare payments or time on payments is examined using a quasi-experimental matching approach that compares the rate of exit from payments, and time on payments, for a 'treatment group' of 18 to 24 year old unemployed who participate in MOI with the rate of exit for a comparable 'control group' of 18 to 24 year old unemployed who do not participate in MOI. To examine the robustness of the findings, a difference-in-difference matching approach is also applied.

Section 2 describes the MOI program. In section 3 a description of the main data sources used in the study is presented. Section 4 discusses possible effects of the MOI on unemployment payment recipients. Sections 5 and 6 present findings from analysis of the 'threat' effect of MOI, and section 7 presents findings from analysis of the MOI program participation effect. Concluding comments, including a discussion of implications for policy, are in section 8.

The main finding is that the MOI program – during its initial phase of operation – appears to have had a limited effect. During the period immediately after introduction of the MOI program, it had some threat effect for males, by significantly increasing their rate of exit from payment prior to the time at which participation in MOI would have commenced. However

there is no apparent effect for females. And there is little evidence of a threat effect of the MOI program from 12 to 18 months after its introduction. The analysis of the effect of MOI participation suggests a strong attachment or lock-in effect of the MOI program. MOI participants are significantly less likely to exit payments than matched non-participants during the period of participation; and therefore MOI participants spend a longer average time on payments. But once MOI participants exit the program, the effect of participation on exit from payments is fairly quickly undone – MOI participants and matched non-participants have similar rates of receipt of payments twelve months after participation commences.

Research on the effects of the MOI has previously been undertaken by Richardson (2001, 2003). One study (Richardson, 2002) examines whether the 'threat' of participation in MOI induces exit from payments around the payment duration at which participation would commence. This study uses the same 'difference-in-difference' approach, comparing outcomes for payment recipients who were required (aged 23 and 24 years) and were not required (aged 25 and 26 years) to participate in MOI, from time periods before and after the beginning of the program. Similar to this paper, it is found that in its initial period of operation the MOI program had a small and marginally significant positive effect on the probability of exit from payments for males, but that there is no significant effect for females. This paper extends that research by examining the threat effect over a longer time period after introduction of the MOI program and seeking to identify sources of heterogeneity in the estimated threat effect for different treatment groups and time periods, and by using a variety of empirical approaches. The second study (Richardson, 2003) uses a matching approach to examine the effect of participation in MOI. It is found that (p.vi) '…although the MOI did impact on participants in specific activities, it had very little impact on the income support outcomes of the target population as a whole". This is again similar to our main finding, but in this paper we examine the evolution of participation effects of MOI participation over time and hence can assess whether, for example, 'lock-in' effects exist. As well, we undertake a range of extra robustness checks.

2. The Mutual Obligation Initiative

The Mutual Obligation Initiative was introduced on 1 July 1998. Participation in MOI was required for Newstart (NSA) and Youth allowance (other) (YA(o)) recipients aged 18 to 24 years who had been in receipt of unemployment benefit payments for 6 months and whose activity type was job search.

The obligation to participate in MOI derives from social security legislation on eligibility conditions for receipt of unemployment benefit payments. The Social Security Act 1991 requires that (unless exempted) unemployment payment recipients must meet an 'activity test' – to be actively looking for work, or undertaking activities to improve their employment prospects, and be willing to accept offers of suitable employment (Section 601). Subject to meeting the activity test requirement, there is no time limit on the duration for which unemployment payments can be claimed in Australia.

Primarily the MOI involves a requirement to undertake an approved MOI activity as well as continuing to look for work. Possible MO activities include work experience activities, training and education, and job search. A detailed description of possible activities is shown in Table 1.

Table 1: Approved MOI activities, July 1998

Activity	*Participation requirement*	*Job contacts*
Part-time work	Minimum of 6 hours per week for at least 14 out of 26 weeks	2-8
Voluntary work	Minimum of 6 hours per week for at least 14 out of 26 weeks	2-8
Education or training	Approved course. Short course (less than 14 weeks): 6 hours per week + another activity to make up to 14 weeks. Long course (more than 14 weeks): 6 hours per week.	
Literacy and numeracy training	6 to 10 hours per week for up to 2 semesters	2
Work for the dole	For length of the project (generally 6 months) - 24 hours per fortnight for job seekers aged 18 to 20; or 30 hours per fortnight for job seekers aged 21 and over	2
Job search training	Length of course (usually three weeks) followed by 14 weeks of intensive job search.	8-16
Intensive Assistance	Up to two years of individualised job preparation and support	2
Job placement, employment and training program	Minimum of 26 weeks (or must complete another activity to make up 26 weeks). Time per week depends on individual need.	2
Green Corps	Usually 5 days per week for 6-12 months	Not applicable
Relocation	Movement to an area with higher demand for the person's skills followed by 14 weeks of intensive job search.	8-16

Source: Centrelink (1998).

Payment recipients are advised of their potential requirement to participate in MOI at the new claim stage, the 3 months interview (if selected), and at any other interview during the first 6

months of payment receipt. During the 10th or 11th fortnights of a payment spell, payment recipients receive a letter requesting them to attend for an interview at Centrelink to discuss MOI. At that interview the payment recipient is advised of MOI requirements, and asked to negotiate an 'Activity agreement' (The only exception is that in the initial six months of operation of the MOI it is likely that the first notification to a payment recipient of the MOI requirement was the letter requesting attendance at a Centrelink interview.). Between July 1998 and June 1999, the allowed time to commence an MOI activity was 3 months, and the allowed time after commencement to complete the MOI activity was 6 months.

Exemptions from the requirement to participate in MOI could be granted where a payment recipient was exempt from job search activity requirements; was living in an isolated area; had part-time caring responsibilities; or was assessed as eligible for Community Support Program.

3. Data source and sample

3.1. Data sources

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.

Two special-purpose data sets from the LDS are used in this study. One is the LDS Unemployment Payment File, a 10 per cent random sample of unemployment payment recipients for the period from January 1995 to June 2000. The second is a 20 per cent sample of unemployment payment recipients who had payment spells commencing between 1 January 1997 and 30 June 1999. In the first stage of the empirical analysis, where the existence of a threat effect of MOI is examined, it is necessary to have data for 1996/97 in order to undertake a pre-program test of the validity of the difference-in-difference approach. Hence for this stage the 10% data sample is used. In the second stage, where the effect of MOI participation is examined, the time period for analysis is more focused around the time

of program commencement, and a larger sample size is needed, so that the 20% data sample is used.

The LDS has advantages and disadvantages for evaluating the impact of MOI. Heckman et al. (1998) suggest that the quality of any quasi-experimental evaluation study using a matching method is likely to be significantly affected by three key features – whether data for treatment and control groups is collected using the same survey instrument; whether it is possible to control at a detailed level for local labour market conditions; and whether it is possible to match treatment and control observations using labour market history.[1] On each of these criteria the LDS performs well. First, data on MOI participants (treatment group) and MOI 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.[2]

3.2. Sample choice

In this study the main focus will be on unemployment payment spells on NSA or YA(o) that commence during the 1997 and 1998 calendar years. Payment spells that commence during 1997 constitute a pre-MOI period. This because any continuous payment spell that begins in 1997 would reach 6 months duration prior to 1 July 1998, and hence the payment recipient would not have been required to undertake MOI. Payment spells that commence during 1998

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

[2] Recent work by Borland and Vu (2005) using an alternative data set (HILDA) does suggest that most unemployment payment recipients who move off payments shift to employment. For example, of unemployment payment recipients who move off payments, it is found that 88.5 per cent of males, and 74.5 per cent of females, are in employment at any time when not in receipt of payments.

constitute a post-MOI period. This is because all continuous payment spells that begin in 1998 would reach 6 months duration on or after 1 July 1998, and hence unemployment payment recipients aged 18 to 24 years would have been required to undertake MOI. Equivalent time periods are chosen for the pre-MOI and past-MOI periods in order to ensure comparability. Spells commencing in 1996 and 1999 are also used in some analysis that is undertaken; the former to allow a pre-MOI test of the validity of the empirical approach, and the latter to allow some time-series comparison of the effect of MOI across time.

A new spell on NSA or YA(o) is defined to begin if a payment recipient has been off any social security payment for at least four consecutive fortnights where that payment spell duration is less than or equal to 23 fortnights; or off all payments for at least seven consecutive fortnights where that payment spell duration is more than 23 fortnights. Exit from a spell is defined to occur where a payment recipient is off unemployment-related payments (NSA or YA(o)) 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.

An important consideration is that our rules for determining new spells and continuous spell duration should be consistent with the approaches used by FaCS. This is because it is the FaCS definition that is the basis for determining eligibility for MOI – based on whether there is a continuous payment spell of at least 6 months. First, with regard to the definition of new payment spells, our definition is slightly stricter than the FaCS definition.[3] Data limitations mean that it is necessary to have a stricter definition, to ensure that our sample is restricted to spells that would be classified as new spells under the FaCS definition. Second, our definition of continuous payment spells is essentially equivalent to the FaCS definition.[4] Hence, our approach should provide a sample of spells where any payment recipient whose spell

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[3] The Social Security Act 1991 defines a 'notional continuous period of receipt of income support payments' as one in which the maximum break from payments in the first 12 months of payment receipt is 6 weeks, and in which the maximum break in subsequent months is 13 weeks; and where a break in payments begins prior to, but within 6 weeks of, 12 months duration, the 13-week test applies.

[4] Information on payment receipt from the LDS is only available on a fortnightly basis. Since it is possible for a break in payments of 3 fortnights to correspond to a break in payments of exactly 6 weeks so that according to the FaCS definition a new spell would not have commenced, therefore to define new spells in this study the rule of requiring a break of 4 fortnights off payments where spell duration is less than 23 fortnights is adopted. For the case where spell duration is more than 23 fortnights, and the FaCS rule for a new spell is a payment break of 13 weeks, it is necessary to use 7 fortnights as the period off payments to define new spells.

duration reaches six months with our method of calculation would also have a spell duration of six months according to FaCS.[5]

MOI participation is identified from the activity type variable in the LDS. A payment recipient is classified as being a participant in MOI in any fortnight in which the variable 'activity type' is coded as an approved MO activity.[6]

4. Effect of MOI on exit from payments - Theory

Participation in MOI may potentially have two types of effects on exit from payments and time on payments – first, a threat or compliance effect that causes an increase in the rate of exit from payments at the time at which participation in MOI would be required to begin; and second, an effect due to participation in MOI.

The threat or compliance effect can be understood in a search model of the labour market (Pissarides, 2000). In a search model one factor that affects an unemployed job-seeker's reservation wage is 'utility' when unemployed; other things equal, the reservation wage will be lower, and hence rate of exit from unemployment will be higher, where utility in the unemployment state is lower. Participation in MOI may for some unemployed persons lower utility in the unemployment state, and therefore induce an increase in the rate of exit from payments prior to the date at which MOI participation would commence. Lower utility in the unemployment state may also induce more intense job search which would cause a higher rate of outflow from unemployment.

Participation in MOI could give rise to a range of possible effects. First, one objective of the MOI is to 'improve employability' of the unemployed. This might occur through an increase

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[5] There is a slight potential inconsistency. FaCS do not classify a new payment spell to occur where a break is six weeks or less. Our approach classifies exit from payments to occur where a break in payments is for at least six weeks. Hence the potential inconsistency arises where the break in payments is for exactly six weeks.

[6] MO activity types (see Department of Family and Community Services, 2002) are classified as: 27 (Work for the dole – Compulsory); 28 (Work for the dole – Voluntary); 57 (MO – Combined part-time work, voluntary work, and education and training); 58 (MO – Education and training); 59 (Approved full-time voluntary work); 60 (Job placement, education and training); 61 (Literacy/numeracy); 62 (MO – Job search training); 65 (MO – Part-time work); 66 (MO – Combined part-time and voluntary work); 67 (MO – Relocation); 68 (MO – Voluntary work); 82 (Work for the dole – Compulsory); 83 (Work for the dole – Voluntary); 97 (Work for the dole – Voluntary).

in skills or improved job search. In a search model of the labour market both effects would be predicted to increase the rate of outflow from unemployment. An increase in a job-seeker's skills should increase the 'arrival rate' of job offers. Improved job search, such as an increase in the time spent on job search, should induce more rapid 'matching' between job-seekers and job vacancies. Second, it is possible to identify some factors that might cause participation in MOI to reduce the rate of exit from payments. One factor would be where there is a 'stigma' effect associated with MOI participation. A second factor would be the possibility that MOI participation reduces or distorts job search activity, a phenomenon referred to as a 'program attachment' or 'locking-in' effect. There is a growing international literature that suggests this factor may be an important dimension of understanding the effects of programs for unemployed persons (for example, Van den Berg and van der Klaauw, 2001; van Ours (2002); Bolvig et al., 2003; and Larsson, 2003)

5. A threat effect of MOI? Analysis using difference-in-difference method

5.1. Method

A difference-in-difference method is applied to estimate the effect of the introduction of the MOI on the rate of exit from payments. The difference-in-difference approach identifies the threat effect of MOI as the difference in the rate of exit from payments for a treatment group between post-MOI and pre-MOI time periods, minus the difference in the rate of exit from payments for a control group between post-MOI and pre-MOI time periods. Let λ_s^t denote the rate of exit from payments of group s (where $s \in \{\text{treatment; control}\}$) in time period t (where $t \in \{\text{pre-MOI; post-MOI}\}$). Then the difference-in-difference estimate can be formally expressed as:

(1) $$(\lambda_{\text{treatment}}^{\text{post-MOI}} - \lambda_{\text{treatment}}^{\text{pre-MOI}}) - (\lambda_{\text{control}}^{\text{post-MOI}} - \lambda_{\text{control}}^{\text{pre-MOI}})$$

To identify the threat effect of MOI we examine on exit from payments at the time at which participation in MOI would have been required to commence at the 12^{th} to 14^{th} fortnights.

In this study the pre-MOI sample is payment spells that commence in calendar year 1997; and the post-MOI sample is payment spells that commence in calendar year 1998. The potential 'treatment group' is defined as NSA/YA(o) recipients who were aged 18 to 24 years during the respective sample periods. The potential 'control group' is defined as NSA/YA(o) recipients who were aged 25 to 31 years during the respective sample periods. In much of the

analysis attention is restricted to a treatment group of payment recipients aged 24 and 25 years, and a control group of payment recipients aged 25 and 26 years.

The difference-in-difference method will identify the effect of MOI under two assumptions (Blundell and Costa Dias, 2000; Blundell et al., 1998). The first assumption is that any difference in exit rates between the treatment and control groups - that is not due to the policy effect of MOI – should be fixed across time. Where any difference in exit rates is time-invariant then all differences between the treatment and control groups – apart from the MOI effect – will be controlled for by the difference-in-difference method. This is because the method compares the difference in exit rates between treatment and control groups between pre-MOI and post-MOI periods. The time-invariant difference in exit rates will exist in both periods, and by differencing hence it is excluded from the estimated MOI effect. The second assumption is that time effects such as evolution of the state of the macro-economy should have an equivalent effect on exit rates for both the treatment and control groups. Where time effects are common to both treatment and control groups, then those time differences will not be reflected in the estimated MOI effect using the difference-in-difference method. This is because the method compares the change in exit rates between pre-MOI and post-MOI time periods for both the treatment and control groups. Hence common time effects are differenced out.

A hazard model approach is used to estimate the effect of MOI on exit from payments. A hazard function is a representation – at each payment spell duration - of the rate of exit from payments from the set of payment spells that reach that duration (see Kiefer, 1988).

The basic estimation approach used in this study is the proportional hazard model whereby the hazard rate is specified as a function of a 'baseline' hazard and of a set of other explanatory variables:

$$(2) \qquad \lambda(t|x) = \theta(t)\cdot\phi(x) .$$

The baseline hazard $\theta(t)$ shows the rate of exit at each point during a payment spell for a payment recipient with a specified 'base' set of characteristics. Variation in the other explanatory variables $\phi(x)$ will shift the baseline hazard proportionally up or down.

Our estimation approach is to model the baseline hazard as a piecewise constant function (for example, Meyer, 1990, and Barrett, 2000). To implement this approach payment spell duration is divided into a finite set of time intervals. Each time interval is defined by lower

and upper cut-offs. Hence, where there are J intervals, the cut-off points can be designated as $c_1,..., c_{J-1}$ with $c_0 = 0$ and $c_J = \infty$. The baseline hazard rate is assumed to be constant within each interval, but can vary between intervals. In this study the time interval is a fortnight. The effect of the other explanatory variables within each time interval is modelled as an exponential function of observable characteristics (x) and a parameter vector (β). Hence the hazard rate for payment recipient i in time interval j is expressed as:

$$(3) \qquad \lambda_i(t|x) = \lambda_j \exp(x_i(t)'\beta) \quad \text{where } c_{j-1} \le t < c_j .$$

The log-likelihood for the model with a sample of N payment spells is (Richardson, 2002):

$$(4) \qquad L(\gamma,\beta) = \sum_{i=1}^{N} [\delta_i \log(1-\exp(-\exp\gamma(k_i) + z_i(k_i)'\beta)) - \sum_{t=1}^{k_i-1} \exp(\gamma(k_i) + z_i(k_i)'\beta)]$$

where k_i is the observed length of the ith payment spell, δ_i equals one if the payment spell ends before being right-censored and zero if the spell is censored, and $\gamma(t) = \ln \int_t^{t+1} \lambda_0(u)du$ is the set of baseline parameters to be estimated.

The model for the rate of exit from payments includes interactions between fortnight of payment spell and a dummy variable for the post-MOI period; between fortnight of payment spell and a dummy variable for observations in the treatment group (23-24 years); and between fortnight of payment receipt and dummy variables for both the post-MOI period and for observations in the treatment group. With these interaction effects included in the model, the estimated baseline hazard is interpreted as the conditional rate of exit from payments in each time period for the control group (25-26 years) in the pre-MOI period. The first interaction effect controls for deviations from the baseline hazard in the post-MOI period that are common to both treatment and control groups. The second interaction effect controls for differences in the rate of exit between treatment and control groups that are common to both time periods. The third interaction effect – between fortnight and dummy variables for the treatment group and post-MOI period - identifies a difference-in-difference estimate of the conditional rate of exit from payments due to the threat effect of the MOI program for the treatment group in the post-MOI period.

Other explanatory variables are also included in the proportional hazard model: gender; country of birth (3 categories); ATSI status; whether had partner on income support payments; whether had partner on non-income support payments; quarter in which payment

spell commenced; unemployment payment history (16 categories); whether had part-time job at spell commencement; whether had part-time job during payment spell; ABS Labour Force region (35 categories); marital status; whether have children; and housing status (7 categories). The set of unemployment payment history variables are derived from a set of dummy variables for whether an individual was ever on unemployment-related payments in each quarter in the four quarters prior to commencement of their current payment spell; there are sixteen possible combinations of {0,1} across the four quarters.

5.2. Descriptive statistics

Table 2 presents information on the number of new payment spells commencing in each calendar year from the 10% LDS. Information is presented both for the 'full' treatment and control groups of payment recipients aged 18 to 24 years and 25 to 31 years, and for the 'basic' case treatment and control groups of payment recipients aged 23 to 24 years and 25 to 26 years.

Table 2: Numbers of spells commenced each year

age group	1996	1997	1998	1999
18-24	27,851	24,143	24,668	25,155
23-24	7,742	6,469	6,195	5,817
25-26	6,521	5,767	5,411	4,892
25-31	17,902	15,923	15,510	14,707

In order to present descriptive information on the rate of exit from payments in the pre-MOI and post-MOI periods for the treatment and control groups we use a non-parametric or Kaplan-Meier estimator. The Kaplan-Meier estimator of the hazard rate at time k is:

(5) $\lambda(t) = h_t/n_t$

where h_t and n_t are respectively the number of observed payment spells completed at time k and the number of payment spells that are 'at risk' of ending at time t (or on-going to the instant before time t). In a discrete time model, the risk set is the number of spells that are on-going at time t-1 and that are not censored at that time.

Figures 1a and 1c show the empirical hazard rates for treatment and control groups in pre-MOI and post-MOI periods for males and females respectively. Figures 1b and 1d summarise the same information by showing the difference-in-difference of the exit rates calculated from

the empirical hazards. These empirical hazards do not appear to reveal strong evidence of a threat effect within the window between the 12th and 14th fortnights of payment spell during which participation in MOI would be required to commence. For males the difference between post-MOI and pre-MOI rates of exit from payments in the 14th fortnight of payment spells is noticeably higher for the treatment than control group; and large effects are not apparent at other fortnights. For females there is no evidence of a significant departure in the 12th to 14th fortnights from the pattern at other fortnights.

Figure 1a: Male Empirical hazard rates (age 23-24/25-26)

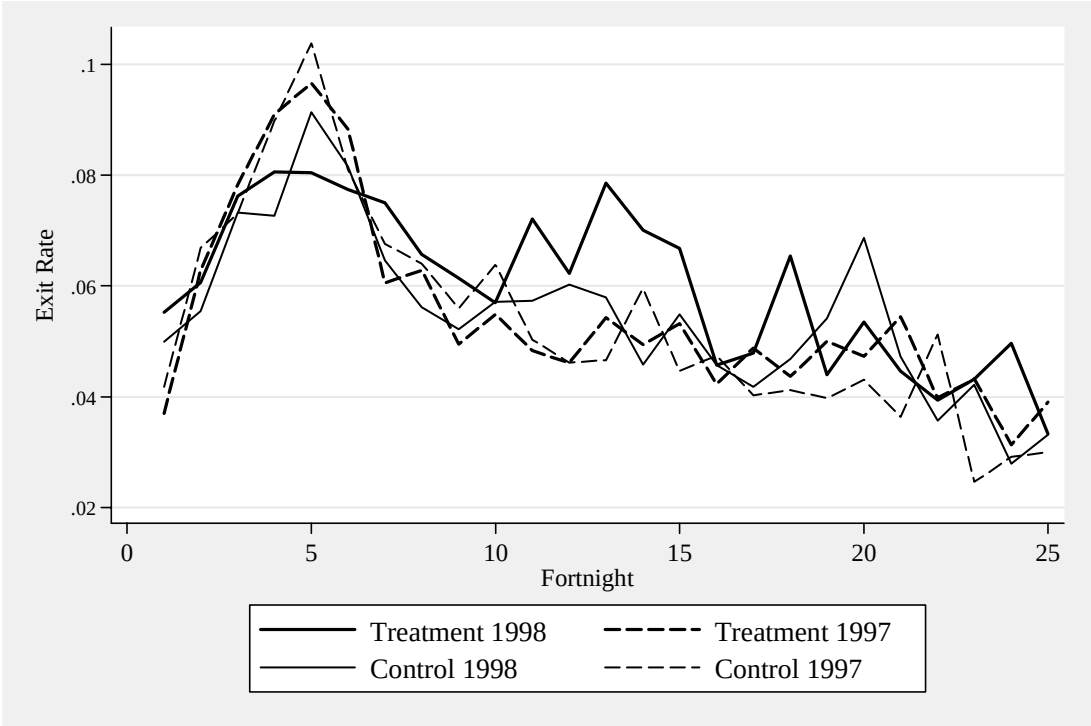


Figure 1b: Male Difference-in-difference Hazard Rate

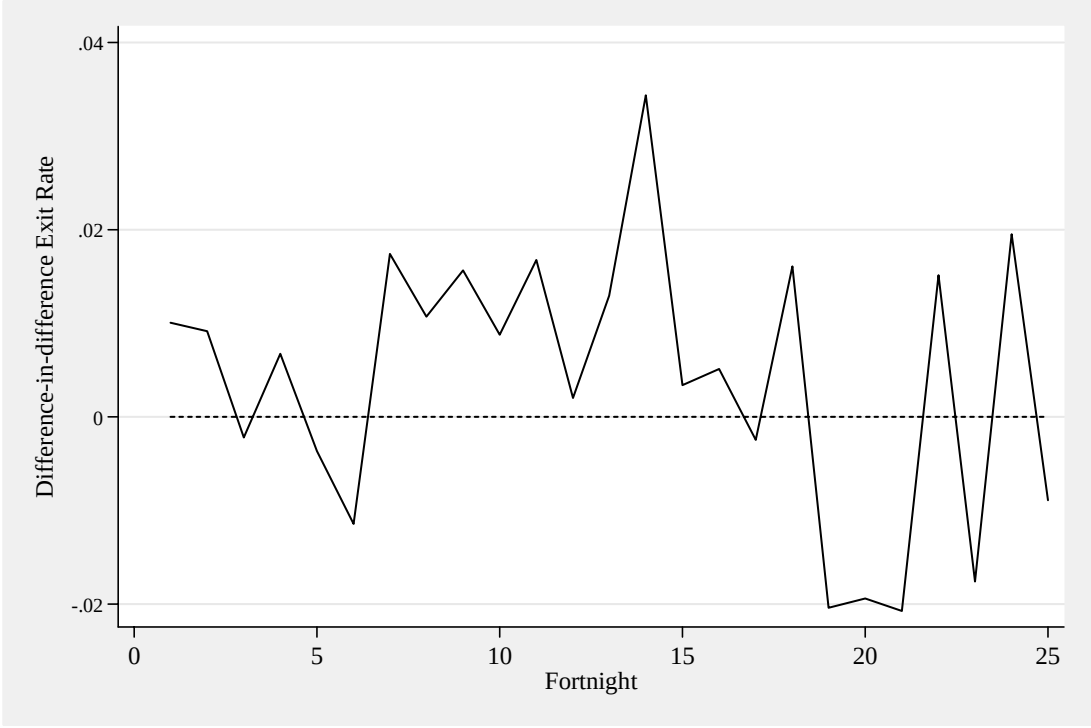


Figure 1c: Female Empirical hazard rates (age 23-24/25-26)

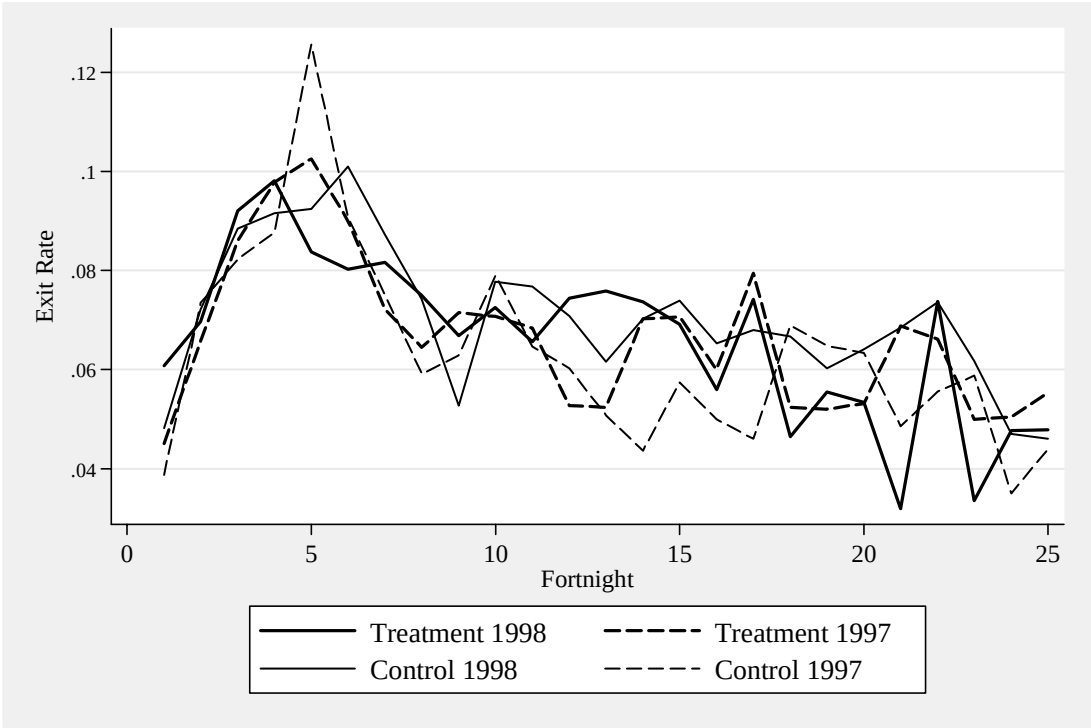
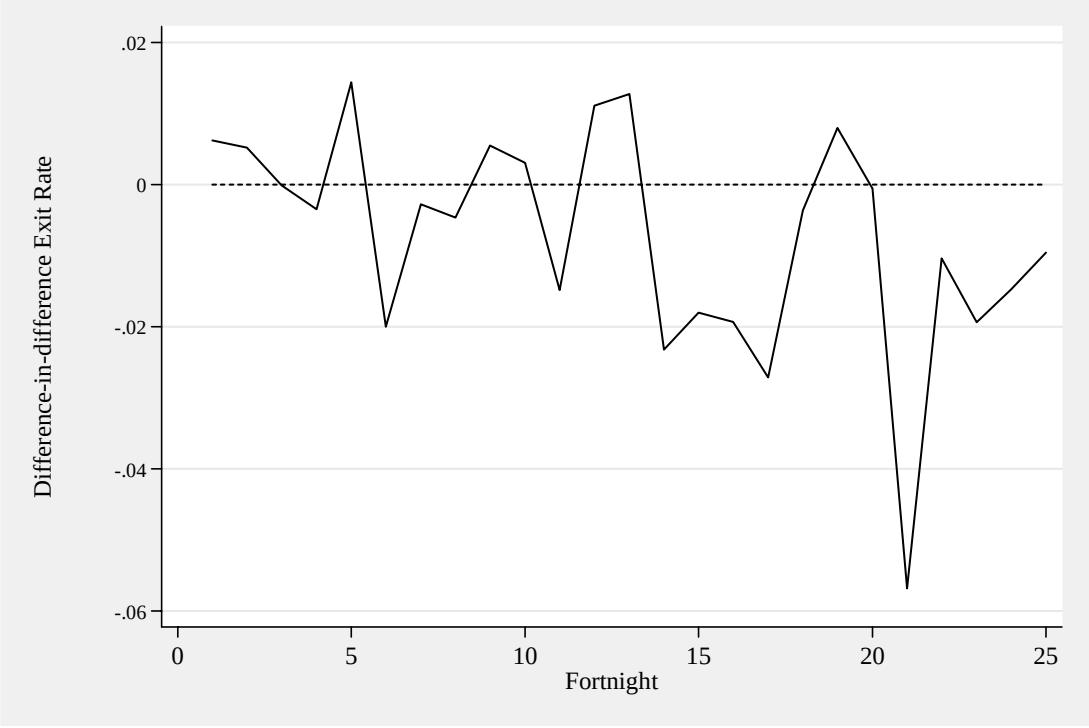


Figure 1d: Female Difference-in-difference Hazard Rate



5.3. Hazard model results

Table 3 presents estimated difference-in-difference effects on the hazard rate in the 12th to 14th fortnights for the treatment group in post-MOI period. This analysis has been undertaken separately for males and females, using different age groups as the treatment and control groups, and for different time periods. (Full results from the basic model are presented in Appendix Table 2.)

The motivation for using alternative age groups as treatment and control groups reflects the absence of any unique 'correct' definition of these groups. The advantage of a narrower definition such as 24 years compared to 25 years (row 1 in Table 3) is that these groups are likely to be relatively similar in their characteristics; whereas the disadvantage is that those aged 24 years may not be representative of the whole treatment group that includes payment recipients aged 18 to 24 years. By contrast, the choice of treatment and control groups as payment recipients aged 18 to 24 years and 25 to 31 years (row 7 in Table 3) involves a comparison that is representative of the whole treatment group, but the treatment and control groups are likely to be less similar in their characteristics.

Table 3: Threat effect estimates: difference-in-difference of hazard rate in fortnights 12-14 estimated using duration analysis by year, age and gender

	Pre-program test		Post-program evaluation			
	1996 v. 1997		1997 v. 1998		1997 v. 1999	
age group	Jan-Jun	Jul-Dec	Jan-Jun	Jul-Dec	Jan-Jun	Jul-Dec
Males						
24 v. 25	-0.007	-0.015	0.017	0.069**	0.046	-0.009
	(0.022)	(0.021)	(0.025)	(0.026)	(0.033)	(0.027)
23-24 v. 25-26	0.000	0.017	0.023	0.049**	0.032	0.006
	(0.014)	(0.014)	(0.017)	(0.016)	(0.021)	(0.024)
22-24 v. 25-27	-0.002	0.011	0.027**	0.043**	0.025	0.018
	(0.014)	(0.010)	(0.014)	(0.011)	(0.015)	(0.018)
21-24 v. 25-28	0.001	0.004	0.025*	0.048**	0.027	0.019
	(0.012)	(0.009)	(0.012)	(0.011)	(0.013)	(0.014)
20-24 v. 25-29	-0.001	0.003	0.032**	0.053**	0.030*	0.018
	(0.010)	(0.009)	(0.011)	(0.011)	(0.011)	(0.013)
19-24 v. 25-30	-0.002	-0.002	0.044**	0.047**	0.033**	0.024
	(0.009)	(0.008)	(0.010)	(0.010)	(0.009)	(0.011)
18-24 v. 25-31	-0.002	-0.005	0.043**	0.042**	0.032**	0.020
	(0.009)	(0.007)	(0.009)	(0.010)	(0.009)	(0.010)
Females						
24 v. 25	0.011	-0.024	0.032	0.059	-0.091**	-0.025
	(0.032)	(0.054)	(0.030)	(0.045)	(0.048)	(0.081)
23-24 v. 25-26	0.038*	0.014	0.004	0.021	-0.063**	-0.021
	(0.024)	(0.029)	(0.023)	(0.031)	(0.028)	(0.046)
22-24 v. 25-27	0.031*	0.008	0.020	0.030	-0.020	0.007
	(0.016)	(0.021)	(0.019)	(0.021)	(0.018)	(0.030)
21-24 v. 25-28	0.028*	-0.002	0.027	0.025	-0.003	0.013
	(0.016)	(0.019)	(0.017)	(0.020)	(0.017)	(0.026)
20-24 v. 25-29	0.026*	-0.002	0.028*	0.018	0.008	0.026
	(0.014)	(0.017)	(0.015)	(0.018)	(0.016)	(0.023)
19-24 v. 25-30	0.015	0.007	0.036**	0.009	0.008	0.034*
	(0.014)	(0.015)	(0.014)	(0.017)	(0.016)	(0.020)
18-24 v. 25-31	0.010	0.003	0.045**	0.008	0.014	0.028
	(0.012)	(0.014)	(0.013)	(0.016)	(0.016)	(0.019)

Note: * significant at 10% level ** significant at 5% level.

Undertaking the analysis for different time periods has two main motivations. First, the pre-program periods involve comparisons between exit rates for spells commencing in 1996 and 1997. For example, the column 'Jan-Jun 1996 v 1997' presents difference-in-difference estimates of the MOI effect using spells commencing in January to June 1996 and January to June 1997. Payment recipients commencing spells in both these years were not required to

undertake MOI. Hence there should be no 'policy effect' that causes a difference in exit rates between treatment and control groups during the period from the 12th to 14th fortnights between 1996 and 1997. This analysis therefore constitutes a pre-program test of the validity of the difference-in-difference estimator (see Heckman and Hotz, 1989). A finding of a significant effect on the rate of exit in the 12th to 14th fortnights at a time when a MOI requirement did not exist would raise the concern that any policy effect estimated in the time period with the MOI requirement was simply reflecting other differences between the treatment and control groups. Second, the post-program results involve comparisons between spells commencing in a year where no MOI requirement would subsequently exist (1997) with spells commencing in years in which that obligation did exist (1998 or 1999). Analysing multiple post-program periods allows analysis of whether there is any change over time in the threat effect of MOI.

The pre-program test shows an absence of significant MOI effects for male payment recipients but some significant effects for females between January to June 1996 and January to June 1997. Hence the test provides quite strong support for the validity of the difference-in-difference estimator for males, and some, but certainly not conclusive, support for females.

In the first year of operation (comparison between 1997 and 1998) the MOI appears to have significantly increased the rate of exit from payment for males prior to the time at which participation in MOI would have commenced. The timing of the effect, and uniformity of the finding across sub-periods and for virtually all choices of treatment and control groups, is consistent with an effect of the MOI. However there is much less evidence of an effect in the second year of the MOI requirement. For females the evidence of an effect for any sub-period is weaker. There are several comparisons where a significant effect on the rate of exit from payments is found, but there is no consistent pattern across time or by choice of treatment and control groups. Given the findings from the pre-program test it is difficult to rule out that any significant effects for females in the post-program period may be due to factors apart from operation of MOI.

5.4. Determinants of variation in estimates of the MOI effect

Why do the results show variation in the estimated difference-in-difference effects between the alternative models? There is a range of possible explanations:

a) Policy effects – Different estimates of the MOI effects between the different models in each pre-MOI/post-MOI comparison could be due to heterogeneity in the treatment effect by

age. For example, if the effect of MOI participation decreased with age, then we would expect to find a stronger effect for the treatment group aged 18 to 24 years than for the 24 year group. Alternatively, differences across pre and post-MOI sample comparison periods within each model could represent time-series variation in the effect of MOI. For example, payment recipients might learn that undertaking MOI is not a requirement that imposes sufficient costs to warrant exiting payments, or may develop strategies to avoid the MOI obligation without exiting payments. In both these cases the estimated threat effect of MOI would decline over time.

b) Cyclical effects – Changes in macro-economic conditions between pre and post-MOI periods differ across the sample periods. Where the treatment and control groups have different cyclical sensitivities to changes in macroeconomic conditions, this could cause different estimates of the rate of exit from payments between those groups across time that are not due to effects of MOI. For example, suppose the rate of exit from payments is more sensitive to the business cycle for younger than older payment recipients, and also that the macro-economy improves by a larger magnitude, the longer is the post-policy comparison period after the pre-policy period. Then with the younger age group being in the treatment group, it would be expected that the rate of exit of the treatment group would increase over time, independently of any MOI effect.

c) Substitution effects – Such effects, whereby the effect of a policy is to increase employment of one group at the expense of another substitute group, are likely to differ depending on the exact composition of treatment and control groups. On average those effects are likely to be larger for a comparison between 24 and 25 year olds than between 18 to 24 and 25 to 31 year olds since there is greater similarity between the former groups than the latter groups. This could explain differences in estimates of the rate of exit from payments for the treatment and control groups between model specifications that again would not represent the effect of MOI.

d) Selection bias – Estimates of exit rates between the 12^{th} and 14^{th} fortnights for treatment and control groups are conditional on rates of exit for those groups in earlier fortnights. Where there is a higher rate of exit in previous fortnights for one group than the other, this may impart selection bias to the comparison of exit rates in the 12^{th} to 14^{th} fortnights (for example, Ham and Lalonde, 1996).

To seek to determine the influence of these factors we estimate the following model:

$$(6) \qquad \hat{\theta}_{jk} = \alpha + \beta \cdot MO_k + \chi \cdot MO_k \cdot k + \delta \cdot agedif_j + \phi \cdot agedif_j \cdot ruedif_k + \varepsilon_{jk}$$

where $\hat{\theta}_{jk}$ is the estimated exit rate due to the MOI effect for the jth choice of treatment and control groups (j = 1,…,7) and kth comparison between time periods (k = 1,…,6) (in other words, the set of 42 estimates of exit due to MOI in the 12th to 14th fortnights reported in Table 3); MO_k is a dummy variable for comparisons where the MOI requirement exists in the 'post' period (equals 1 for k = 3,…,6); k is an index for each of the comparisons between different time periods (for example, 1 = Jan-Jun 1996/1997, 2 = Jul-Dec 1996/97,…); $agedif_j$ is the average difference in ages between treatment and control groups for the jth comparison; and $ruedif_k$ is the difference in the average rate of unemployment in Australia between the pre and post comparison periods for the kth comparison. The measure of the rate of unemployment is the average rate of unemployment (Australia, original series) over the respective 6 month period.

The 'MO' dummy variable is intended to capture any time-invariant effect of the MOI requirement on the rate of exit from payments due to a threat effect. The interaction between the 'MO' dummy variable and the index for the alternative pre and post comparisons is intended to capture any change in the effect of the MOI requirement across time. Effects of the 'agedif' variable on the rate of exit may represent either heterogeneity between age groups in the impact of the MOI requirement, or substitution effects that vary depending on the similarity of the treatment and control groups. Unfortunately it does not seem possible to separately identify the influence of these factors. The interaction between the 'agedif' and 'ruedif' variables is intended to represent what has been described as the cyclical effect – whereby the impact of the business cycle on rates of exit might vary by age. On the assumption that selection effects do not vary between the different comparisons of pre and post comparison periods or with the different choices of treatment and control groups, those effects would be captured in the constant term.

Table 4 reports the regression results. For both males and females the MOI effect is highly significant in the initial period after introduction of the MOI program. There is however a significant decline in the effect across time. Age effects are not significant, but indicate that the effect of MOI is weaker for older age groups. The interaction between the age difference and rate of unemployment difference is marginally significant for females, and insignificant for males. The estimate for females indicates that as the business cycle worsens, for

comparisons involving broader treatment and control groups, the exit rate for the control group increases relative to the treatment group. This would be consistent with a decreasing cyclical sensitivity of the rate of exit from unemployment as age increases.

Table 4: Determinants of alternative estimates of MOI effects, 12th to 14th fortnights

	Males		***Females***	
	Coef.	*Std. Err.*	*Coef.*	*Std. Err.*
MO	0.048**	0.007	0.024**	0.009
MO*trend	-0.010**	0.004	-0.018**	0.006
agedif*ruedif	-0.002	0.001	-0.005*	0.003
Agedif	-0.001	0.002	-0.001	0.003
Constant	0.001	0.007	0.008	0.011
Adjusted R squared	0.663		0.277	
No. of observations	42		42	

Notes: Dependent variable is difference-in-difference hazard rate.
'agedif' is the difference in the mean age of the treatment and control groups.
'ruedif' is the difference in the unemployment rate pre- and post-policy.
Asterik denotes significant at 10% level. Double asterik denotes significant at 5% level.

6. A threat effect of MOI? Analysis using matching and difference-in-difference matching methods

6.1. Introduction

In this section we report findings from alternative approaches to estimating the 'threat' effect of MOI. These two approaches are 'matching' and 'difference-in-difference matching'. Each approach is implemented using payment recipients aged 23-24 years as a treatment group, and aged 25-26 years as a control group. Fundamentally, these approaches involve comparing payment outcomes for a treatment group of NSA/YA(o) recipients who are eligible for participation in MOI, and matched control group(s).

Application of matching methods provides a robustness check on results using duration modeling. Matching has the advantages by comparison with duration modeling of being non-parametric – not imposing on any specific functional form in the relation between covariates and the outcome variable; and making explicit the 'support' problem – that there may be no sufficiently comparable control group observations for some program participants (Smith, 2000, p.12). The main disadvantage of matching compared to duration modeling is that it is not possible to control for the effect of time-varying covariates.

The matching methodology compares outcomes for payment recipients who are eligible for and not eligible for participation in MOI in the period in which the MOI is in operation. The

difference-in-difference matching methodology compares the difference in outcomes for payment recipients eligible for MOI participation in pre and post MOI program periods with the difference in outcomes for payment recipients ineligible for MOI participation in pre and post MOI program periods. Each of these methods will be a valid estimator of the MOI effect under an alternative set of assumptions, and hence are subject to different potential sources of bias. The matching method will be valid where there are no unobservable differences between treatment and control groups that affect the outcome. Whereas the difference-in-difference matching method will be valid where outcomes for treatment and control groups evolve identically across time. The choice of optimal estimator for the MOI effect – matching or difference-in-difference matching - should depend on the relative size of potential bias. Our motivation for applying both estimators is that - in the absence of definite evidence on the size of each potential source of bias, or an alternative source of justification for either approach – this provides a robustness check.

6.2. Matching

Formally, the matching method estimates:

$$(7) \qquad \text{MOI Matching effect} = [1/n] \sum_{i \in \{D=1\}} [S_{1i} - \sum_{j \in \{D=0\}} w(i,j) S_{0j}]$$

where $D \in \{0,1\}$ is an indicator for being in the control or treatment groups; w(i,j) is the weight placed on the jth potential control group observation in constructing a comparison for the ith treatment group observation; S_{1i} and S_{0j} are the survival rates for the ith treatment and jth control group observations; and n equals the number of treatment observations.

The matching method therefore compares survival rates for each treatment group observation with a weighted average of control group observations, and the MOI effect is the average of these effects for treatment observations. Matching is implemented for payment recipients with a spell duration of at least 10 fortnights, and the outcome measure is whether a payment spell 'survives' to the 14^{th} fortnight. Hence the treatment effect of MOI that is estimated is the average effect of being eligible for participation in MOI for 23-24 year olds on having a payment spell that continues to the 14^{th} fortnight conditional on having a payment spell duration of at least 10 fortnights.

For the quasi-experimental matching method to be a valid estimator of the MOI 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 MOI and outcomes in the absence of the MOI (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.

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

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 being in the treatment group (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 is in the treatment group. Separate models are estimated for males and females. Covariates included in the model are – unemployment payment history (5 categories); whether the activity type is 'job search'; whether had participated in JSD previously; whether had positive earned income at payment spell commencement; whether had positive earned income at the 10^{th} fortnight; whether have child; country of birth category; indigenous status; housing type; ABS Labour Force Region; and calendar month commenced payment spell.

The unemployment payment history variable is defined over the twelve months prior to the commencement of the payment spell of each treatment or control group observation. The twelve month period is divided into four quarters, and for each quarter a {0,1} classification is made according to whether the individual was ever observed to be on unemployment payments in that period. 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).

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

From application of the balancing test preferred models that were estimated are reported in Appendix Table A3. The test was implemented by (i) dividing treatment and control group observations into 10 groups (to give an equal number of treatment observations in each group); (ii) performing a Hotelling test for the joint null hypothesis of equal means for all

covariates between treatment and control groups; and (iii) adjusting the number of groups or change functional form until it is not possible to reject the joint null for the Hotelling test.

Stage two of the PSM is to match treatment and control group observations. The main components of the method are:

(a) Use linear predicted score from PSM;

(b) Caliper method;

(c) Match each treatment observation with control observations in a 5 per cent confidence interval;

(d) Kernel weighting of control observations; and

(e) Re-sampling of control observations for different treatment observations.

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

Kernel weighting involves:

(8a) $w(i,j) = G^{ij} / [\sum_{j \in \{D=0\}} G^{ij}]$; and

(8b) $G^{ij} = G[(X_i\hat{\beta} - X_j\hat{\beta}) / a_{5\%}]$

where G^{ij} is the kernel for ith treatment and jth control observations for the male sample, $X_i\hat{\beta}$ and $X_j\hat{\beta}$ are linear predicted scores for the respective treatment and control observations in the male sample, and $a_{5\%}$ represents the use of a 5% confidence interval bandwidth around $X_i\hat{\beta}$. In this approach the biweight kernel is used.

To assess the quality of matching we compare the mean values of characteristics used in matching between treatment and matched control observations. (This is different to the balancing test applied in the first stage of the matching procedure. The comparison proposed here is directly between treatment observations and a kernel weighted average of the control observations to which they were matched.) From Appendix Table A4 it can be seen that no significant differences exist between characteristics of treatment and matched control groups. Hence, the choice of control observations has created a comparison group that is on average very similar to the set of treatment observations.

The common support assumption appears to be satisfied in this application. Appendix Figure A1 presents the linear predicted score from the PSM for treatment and control observations. It shows that there is a high degree of overlap between the distributions – although clearly the treatment observations are more concentrated at higher predicted scores. Using this matching algorithm, only 10 out of 11,552 treatment observations cannot be matched to a control group observation. The average number of times each control observation was used is 1,137, with a minimum of zero and maximum of 1,889. The average number of control observations matched to each treatment observation is 958, with a minimum of zero and maximum of 1,941.

6.3. Difference-in-difference matching

Formally, the difference-in-difference matching method estimates:

$$(9) \qquad \text{MO DID effect} = [1/n]\{ \sum_{i\in\{D_{t=1}=1\}} [S_{it_1} - \sum_{j\in\{D_{t=0}=1\}} w(i,j)S_{jt_0}] + [\sum_{j\in\{D_{t=1}=0\}} v(i,j)S_{jt_1} - \sum_{j\in\{D_{t=0}=0\}} x(i,j)S_{jt_0}] \}$$

where $D_{t=z} \in \{0,1\}$ is an indicator for being in the control or treatment groups in time period z (where z=1 is the post-MOI period, and z=0 is the pre-MOI period; w(i,j), v(i,j) and x(i,j) are the weights placed on the jth potential control group observation in constructing a comparison for the ith treatment group observation; S_{it_z} and S_{jt_z} are the survival rates for the ith treatment and jth control group observations in time period z; and n equals the number of treatment observations.

The difference-in-difference matching method estimates the MOI threat effect as the difference in survival rates between payment recipients eligible for MOI participation in pre and post MOI program periods compared to the difference in survival rates between the control group of payment recipients ineligible for MOI in pre and post MOI program periods. Matching is done three times using the group of payment recipients eligible for MOI participation in the post-MOI period as the 'base'. In each case matching is implemented for payment recipients with a spell duration of at least 10 fortnights, and the outcome measure is whether a payment spell 'survives' to the 14^{th} fortnight. Hence the treatment effect of MOI that is estimated is the same as for the matching method: the average effect of being eligible

for participation in MOI for 23-24 year olds having a payment that continues to the 14th fortnight conditional on having a payment spell duration of at least 10 fortnights.

For the difference-in-difference matching method to be a valid estimator of the MOI effect, it is sufficient that (Blundell and Costa Dias, 2000):

(a) Conditional Independence Assumption (CIA) - Conditional on a set of observable variables (X), participation in treatment is unrelated to the difference in pre and post-program 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.

The CIA for the difference-in-difference matching method requires that survival outcomes for the control group evolve from the pre-MOI to post-MOI period in the same way as would have occurred for the treatment group if they were not eligible for MOI. Once again, our main justifications for the CIA are the use of an adjacent age group as the control group, and that it is possible to undertake matching using a relatively rich set of covariates. Unfortunately, an absence of time-series data on rates of exit from unemployment payments prior to our sample period means we are not able to provide empirical evidence on the evolution of outcomes for the treatment and control groups in the absence of treatment. This issues can instead only be addressed indirectly by applying difference-in-difference matching to the two most recent pre-program periods. Primarily, our motivation for application of difference-in-difference matching is as a robustness check on results from matching – since we do not have any evidence on the size of potential bias using either approach, and the lack of any 'natural experiment' type justification for application of matching.

To implement the difference-in-difference matching method we use the same PSM approach as for the matching method. Stage 1 is to estimate a probit model and apply the balancing test to establish the functional form for the probit model. Stage 2 is to match treatment and control observations. This exercise is repeated three times –between payment recipients aged 23-24 years in the post-MOI period, and each of the three control groups: payment recipients aged 25-26 years in the post-MOI period; payment recipients aged 23-24 years in the pre-MOI period; and payment recipients aged 25-26 years in the pre-MOI period.

6.4. Results

Table 5 reports findings on estimates of the MOI effect from the alternative approaches. The main finding is that results on a 'threat' effect of MOI are highly consistent between the approaches. First, there is no significant MOI effect in the pre-MOI period for either males or females. Second, for males in the post-MOI period there is an apparent effect of MOI that declines across time, and in all approaches there is no significant effect for July to December 1999. Third, for females in the post-MOI period there is no apparent effect of MOI.

Table 5: Threat effect estimates (survival rate) using duration analysis, matching method and matching dif-in-dif by year and gender (survival at 14th fortnight, age 23-24 v. 25-26)

	Pre-program test		Post-program evaluation			
	Spell commenced in 1997		Spell commenced in 1998		Spell commenced in 1999	
	Jan-Jun	Jul-Dec	Jan-Jun	Jul-Dec	Jan-Jun	Jul-Dec
Males						
Hazard function	-0.011	0.027	-0.039	-0.058	-0.009	-0.026
dif-in-dif	(0.019)	(0.022)	(0.021)	(0.023)	(0.018)	(0.018)
Matching		-0.004	-0.049	-0.071	-0.053	-0.028
Method		(0.019)	(0.019)	(0.023)	(0.026)	(0.025)
Matching	-0.014	-0.009	-0.053	-0.062	-0.068	-0.023
dif-in-dif	(0.027)	(0.025)	(0.031)	(0.035)	(0.035)	(0.030)
Females						
Hazard function	-0.034	-0.006	-0.001	-0.030	0.068	-0.016
dif-in-dif	(0.026)	(0.029)	(0.027)	(0.034)	(0.027)	(0.027)
Matching		-0.044	-0.013	-0.030	0.001	-0.059
method		(0.027)	(0.032)	(0.035)	(0.039)	(0.039)
Matching	-0.029	-0.052	-0.034	0.026	0.017	0.001
dif-in-dif	(0.038)	(0.047)	(0.062)	(0.061)	(0.060)	(0.053)

Note:
1. Bootstrap standard errors are in parentheses. For the hazard function method the bootstrap standard errors are derived from 60 replications; for the matching method 400 replications; and for the difference-in-difference method 100 replications. In each case the size of replications were chosen such that standard errors were robust to increases in the number of replications and taken into account the computational time required.
2. Matching method compares treatment and control group in the evaluation years. For difference-in-difference methods, pre-program tests compares treatment and control groups in 1997 vs. 1996 and post program evaluation compares the two groups in the evaluation year vs. 1997.

7. The effect of participation in MOI

7.1. Introduction

This section examines the effect on payment recipients of participation in MOI. Matching and difference-in-difference matching methods are applied. For the matching approach the

sample used is payment recipients who commenced a payment spell in 1998 - that is, whose payment spell reaches 6 months duration during the first year of operation of MOI. The treatment group is payment recipients who participate in MOI, and the control group is payment recipients who had not commenced participation in MOI by specified fortnights. Estimates of the MOI effect are made for alternative treatment groups (23-24 years, and 18-24 years). For the difference-in-difference matching method the pre-program sample is payment spells that commence in 1997 - that is, payment spells that reach 6 months duration in the year prior to introduction of the MOI program, and the post-program period is payment spells that commence in 1998. The treatment group is payment recipients aged 23-24 years, and control group is payment recipients aged 25-26 years.

Participation in MOI can begin for an individual payment recipient at many different payment spell durations after 6 months; and occurs for different payment recipients throughout the sample period – This can be seen from Table 6. This potentially complicates the classification of payment spells as treatment or control observations. Our basic approach is to define: (a) Treatment group – NSA/YA(o) recipients with spells commencing in the specified sample period who commence MOI between fortnights 13 and 22 in their payment spells; and (b) Potential control group - NSA/YA(o) recipients with spells commencing in the specified sample period who have not commenced participation in MOI by the 13th fortnight of their payment spells. The reason for restricting attention to fortnights 13 to 22 is that it is only in those fortnights that there are sufficient commencements on MOI to enable the matching methods to be applied.

Table 6: Distribution of starting dates on MO – Payment spells commencing in 1997

Fortnight	*Frequency*	*Per cent*	*Cumulative Per cent*	*Fortnight*	*Frequency*	*Per cent*	*Cumulative Per cent*
13	246	5.49	5.49	22	122	2.72	83.13
14	606	13.53	19.02	23	77	1.72	84.84
15	754	16.83	35.85	24	75	1.67	86.52
16	686	15.31	51.16	25	59	1.32	87.83
17	467	10.42	61.58	26	55	1.23	89.06
18	312	6.96	68.55	27-39	348	7.78	96.84
19	217	4.84	73.39	40-52	120	2.68	99.52
20	183	4.08	77.48	52+	22	0.48	100
21	131	2.92	80.4				

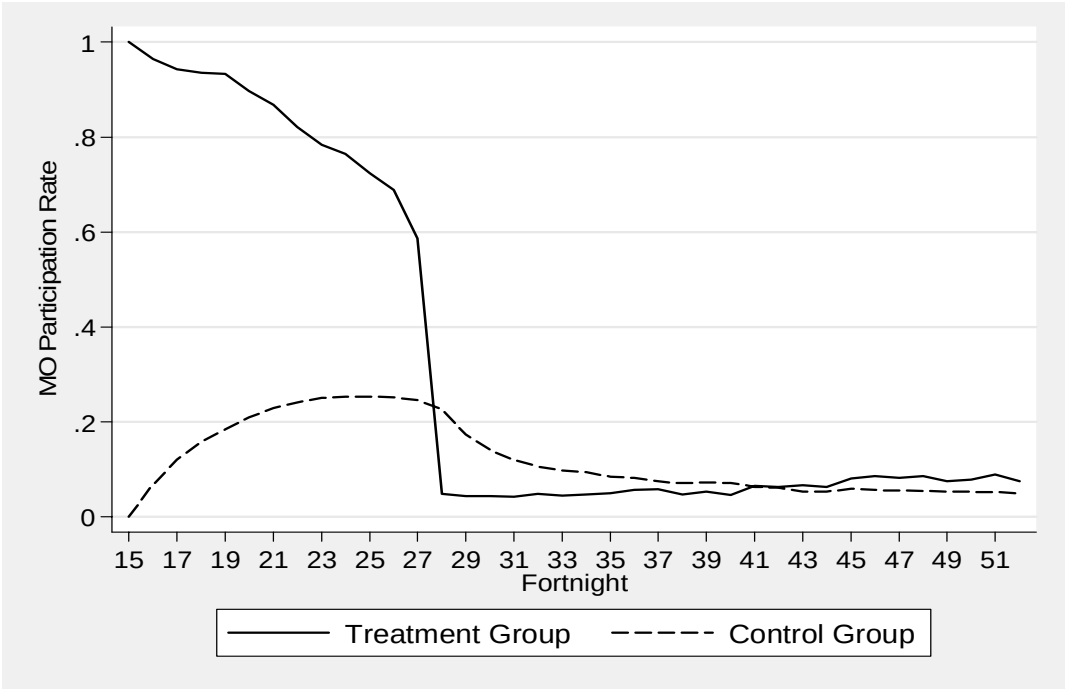
There are two main implications of this approach. First, over 83 per cent of NSA/YA(o) recipients who ever participate in MOI from our sample period are represented in the treatment group; however there is a proportion of NSA/YA(o) recipients in the potential control group who subsequently participate in MOI. Second, the approach does not address

the issue of type of participation. MOI participants in the treatment group are not distinguished on the basis of the type of activity they undertake. In order to make such a distinction, and to derive valid estimates of the effect of participation in a specific type of MOI activity, it is necessary that – conditional on the set of matching variables used – there is effectively random assignment between activities (Lechner, 2001, and Gerfin and Lechner, 2002). It would seem difficult to justify such an assumption – for example, payment recipients who already have part-time work prior to MOI would be very likely to choose that activity for their MOI; and Work for the Dole appears to have acted as the 'residual' activity for MOI participants who do not select another activity type.

The definition of treatment and control groups of course has direct consequences for the policy effect that is identified in this study. Estimates of the effect of MOI participation are the weighted average effect of commencing participation in MOI in a specific fortnight of a payment spell compared to not commencing participation in MOI in that fortnight averaged across fortnights 13 to 22.

Figure 2 provides representative information on the pattern of participation in the MOI for the treatment group who commence participation in the 15^{th} fortnight of their payment spells and corresponding control group. By definition, in the first fortnight participation by the treatment group is 100 per cent, and by the control group is zero per cent. The rate of participation for the treatment groups remains substantially above that for the control group for about 12 to 13 fortnights; after that time there is no difference between participation for the groups. During the initial period after the starting fortnight, the difference in rates of participation between treatment and controls groups is on average about 60 to 70 percentage points. 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 about six months.

Figure 2: Proportion of treatment and control observations participating in MO – By payment spell duration (fortnight) – Commence MO in 15th fortnight



A variety of outcome measures related to receipt of unemployment payments are used to estimate the MOI effect. MOI participation can commence up to three months after eligibility, and then must be completed in six months. Most MOI activities involve participation for six months; although there is some with shorter and longer time horizons (Table 1). Outcome measures have been chosen to attempt to capture short-run (impact) effects of the MOI, and possible longer run effects. One measure will be the effect of MOI on the incidence of exit from payments by 6 months and 9 months after MOI commencement. A second measure will be the effect of MOI on whether payment recipients are on payments at 6 months, 9 months, and 12 months after MOI commencement. The third measure applied is the effect of MOI on the number of fortnights on payments during the 6 months, 12 months, and 18 months after MOI commencement.

7.2. Implementation of matching and difference-in-difference matching

The formal definitions and implementation of the matching and difference-in-difference matching methods are the same as for estimation of the MOI threat effects. The motivation for application of the alternative methods – as a robustness check in the absence of information on the potential size of bias in estimates of the treatment effect using each approach – is also the same.

Estimates of the effect of participation in MOI are derived separately for payment recipients who commence MOI participation in each of the 13^{th} to 22^{nd} fortnights. For example, the matching approach is implemented having as a treatment group payment recipients who commence MOI in the fth fortnight, and as a control group payment recipients in the same age group who have not commenced MOI by that fortnight. A probit model is estimated for 'whether commence in MOI in fortnight f', and then matching treatment and control observations to derive the estimate of the effect of commencing MOI participation in for that fortnight. The aggregate effect of MOI participation is then the weighted average of these effects:

(10) MOI participation effect = $Y^f \cdot \text{Prob}(D^f=1|D=1)$

where Y^f is the estimated effect of MOI participation for payment recipients who commence MOI in fortnight f, and D^f and D are respectively indicators for whether a payment recipient commences on MOI in fortnight f and whether a payment recipient commences on MOI between fortnights 13 and 22. This follows the approach of Sianesi (2004) for estimating treatment effects where program participants commence treatment at different points in their payment spells.

Validity of the matching and difference-in-difference matching estimation approaches relies on the same respective CIA and common support assumptions as for the analysis of the MOI threat effect. The quality of matching – conditional on the set of matching variables – can be assessed by examining the mean values of characteristics used in matching for treatment and control observations. Appendix Table A6 shows this information for selected characteristics. Overall, the results suggest very strongly that the choice of control observations has created a comparison group that is on average highly similar to the set of treatment observations. For none of the matching variables – and for no individual fortnight or overall – is it the case that there is a significant difference between the mean values of those variables at the 5% level of significance.

The common support assumption is satisfied. Appendix Figure A2 presents the linear predicted score from the PSM for treatment and control observations. It is apparent that there is a high degree of overlap between the distributions – although clearly the treatment observations are more concentrated at higher predicted scores. All treatment group observations were used in the basic approach.

7.3. Results

The main results on the effect of participation in MOI are shown in Table 7. There is a quite strong message from the results. During the period where a payment recipient is participating in MOI activities, they are significantly less likely to exit payments than a comparable non-participant. For example, at six months after the commencement of participation in MOI, using as the treatment group payments recipients aged 23-24 years, participants are about 4 to 6 percentage points less likely to have exited payments than non-participants; and during that first six months MOI participants on average spend about 1 fortnight more on unemployment-related payments than non-participants. However, after the period of participation in MOI activities there is much less evidence of difference between MOI participants and non-participants. For example, at 12 months after the commencement of MOI participation the difference in the proportions of MOI participants and non-participants on payments is not significantly different from zero. Notably, the results for participants aged 23-24 years appear robust between the matching and difference-in-difference matching methods. Results for the treatment group of MOI participants aged 18-24 years are also similar in showing a significant negative effect of program participation on the likelihood of exiting payments during the period of participation, but that this effect reduces in impact after participation, and appears to have largely dissipated by 9 to 12 months after program commencement. (The main difference between the results for the different age groups is that the effect of time on payments during the first 18 months is estimated more precisely for the treatment group aged 18-24 years than 23-24 years.)

Figure 3 shows the difference in exit probabilities for MOI participants and control group using the matching approach for the sample of payment recipients aged 18-24 years from program commencement to 12 months after. This shows a similar pattern. For each group, for the first 2-3 months the probability of exit for MOI participants is below that for non-participants. However, after that time, at about 6 months after program commencement, this effect reverses and there is a higher probability of exit for MOI participants. By 12 months after program commencement there is little difference in the proportions of MOI participants and control group who have exited payments.

What these findings suggest is an attachment or lock-in effect of the MOI program. MOI participants are less likely to exit payments than matched non-participants during the period of participation; and therefore MOI participants spend a longer average time on payments. But once MOI participants exit the program, the effect of participation on exit from payments

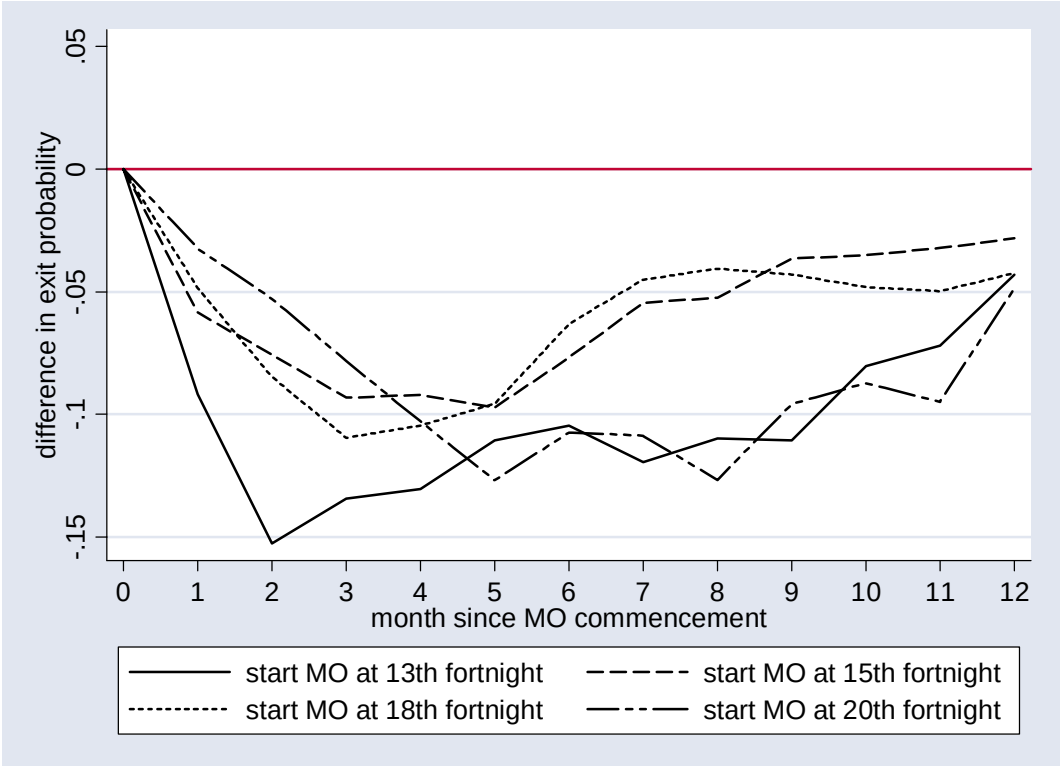
is fairly quickly undone – MOI participants and non-participants have similar rates of receipt of payments, and any difference in time on payments is primarily due to the effect of the phase of MOI participation. The existence of an attachment effect in programs such as MOI – as has been described above – is a phenomenon that is being increasingly recognised in the literature on evaluation of active labour market programs.

Table 7: Effects of MO – NSA/YA(o) recipients aged 18 to 24 years with at least one fortnight on MOI – Payment spells commence in 1997

	Matching method		**Matching dif-in-dif**
	18-24 years	*23-24 years*	*23-24 vs. 25-26*
% Off payments			
By 6 months	-0.063	-0.037	-0.058
	(0.012)	(0.019)	(0.034)
By 9 months	-0.039	-0.031	-0.076
	(0.011)	(0.018)	(0.033)
% On payments			
At 6 months	0.063	0.212	0.051
	(0.011)	(0.018)	(0.032)
At 12 months	0.005	-0.006	0.032
	(0.011)	(0.019)	(0.031)
Time on payments (Fortnights)			
First 6 months	0.928	0.751	0.744
	(0.092)	(0.150)	(0.278)
First 18 months	1.109	0.069	1.441
	(0.277)	(0.048)	(0.827)

Note: bootstrap standard errors are in parentheses.

Figure 3: MO Treatment Effect by Commencement Fortnight



Some further evidence relevant to evaluating existence of 'lock-in' effects is presented in Table 8. It shows that for MOI commencements that occur at later points in payment spell duration, there is a higher probability of assignment to the Work for the Dole program, and a lower probability of assignment to training, voluntary work, or part-time work. Duration of participation in Work for the Dole is generally for a longer period than the other options (see Table 1). This appears to be consistent with existence of 'lock-in' effects as it is evident from Figure 3 that MOI participants who commence in later fortnights of payment spell duration have longer periods before 'reversal' of the negative effect of MOI participation.

Table 8: Type of MO activity by commencement fortnight

MO commencement fortnight	WFD	Training	Voluntary work	Part-Time work combined	Part-Time work
13	10.13	18.99	7.17	11.39	52.32
14	5.18	19.03	9.18	12.19	54.42
15	7.14	18.46	15.23	10.11	49.06
16	12.5	16.96	16.22	12.05	42.26
17	21.35	17.86	14.6	7.41	38.78
18	26.86	13.59	15.53	11	33.01
19	32.86	14.08	15.49	7.51	30.05
20	37.02	12.71	17.68	8.84	23.76
21	52.67	13.74	7.63	6.11	19.85
22	55.37	9.09	7.44	6.61	21.49
Total	**17.63**	**16.81**	**13.46**	**10.15**	**41.95**

Table 9 shows results on the effect of MOI participation for treatment groups aged 18-24 years who commence on MOI in the 13th to 22nd fortnights of their payment spells. Generally, the results are fairly consistent across the starting fortnights. For example, for seven of the ten fortnights it is found that the proportion of MOI participants off payments by six months after the commencement of MOI is significantly less (at the 5% level) than for the comparable control group; but by 9 months the effect is only significant for three groups. While the data from Table 8 has suggested that the exact timing of the lock-in effect may vary by the fortnight at which MOI participation commenced, these results demonstrate that the existence of an attachment effect of MOI, and its un-doing following the end of MOI participation, is fairly uniform between payment recipients who commence in MOI at different points between the 13th to 22nd fortnights of their payment spells.

Table 9: Effects of MOI by fortnight in payment spell commence – NSA/YA(o) recipients aged 18 to 24 years with at least one fortnight on MO

	Differences in outcome:					
	% Off payments		**% On Payments**		**Time on payments (Fortnights)**	
	By 6 months	*By 9 months*	*At 6 months*	*At 12 months*	*First 6 months*	*First 18 months*
Fortnight						
13	-0.105**	-0.111**	0.105**	0.022	1.439**	2.532**
	(0.038)	(0.034)	(0.037)	(0.037)	(0.303)	(0.856)
14	-0.043	-0.020	0.058**	-0.021	0.601**	0.263
	(0.026)	(0.026)	(0.026)	(0.026)	(0.220)	(0.652)
15	-0.077**	-0.036*	0.088**	0.033	1.064**	1.667**
	(0.023)	(0.022)	(0.023)	(0.022)	(0.187)	(0.551)
16	-0.072**	-0.021	0.062**	-0.008	0.961**	0.646
	(0.022)	(0.021)	(0.023)	(0.022)	(0.185)	(0.558)
17	-0.056**	-0.048*	0.067**	0.001	0.833**	0.964
	(0.027)	(0.026)	(0.025)	(0.025)	(0.199)	(0.644)
18	-0.063**	-0.043	0.026	0.019	0.969**	1.274*
	(0.028)	(0.027)	(0.029)	(0.029)	(0.213)	(0.710)
19	0.014	-0.018	-0.003	-0.025	0.764**	0.062
	(0.036)	(0.036)	(0.037)	(0.037)	(0.262)	(0.878)
20	-0.107**	-0.096**	0.100**	0.020	0.967**	1.886**
	(0.039)	(0.040)	(0.036)	(0.039)	(0.298)	(0.959)
21	-0.094**	-0.053	0.094**	0.045	1.179**	3.071**
	(0.045)	(0.041)	(0.043)	(0.042)	(0.298)	(0.989)
22	0.002	0.003	-0.042	-0.041	0.730**	0.314
	(0.047)	(0.046)	(0.043)	(0.046)	(0.311)	(1.025)

Note: standard errors in parentheses.

8. Conclusion

This paper has examined the effects of the MOI program on income support outcomes for unemployment payment recipients aged 18 to 24 years. The main finding is that the program – during its initial phase of operation – appears to have had a limited effect. While there appears to have been a threat effect that did cause a small increase the rate of exit from payments for males in the first 12 to 18 months of operation of the program, this effect then seems to have disappeared. Analysis of the effect of MOI participation suggests a strong lock-in effect of the program, whereby MOI participants are significantly less likely to exit payments than matched non-participants during the period of participation, but once MOI participants exit the program, the effect of participation on exit from payments is fairly quickly undone.

Several further areas of research on the effect of the MOI program might be usefully undertaken. First, impacts of participation beyond two years after commencement might be considered, as some recent work does suggest that differences in outcomes for participants and non-participants continue to evolve for several years after participation (for example, Lechner et al., 2005). Second, the MOI participation effect estimated in this paper is an amalgam of the effect of participation in a range of types of programs, and it would be of interest to assess the relative effect of each type of program, and to consider whether assignment of young unemployed persons to types of programs has been optimal. However, to undertake this research would require that the CIA holds for assignment between different types of programs in the MOI (Gerfin and Lechner, 2002), and this seems problematic. A third fruitful area for study might be to use the MOI together with other international examples of labour market programs for young unemployed, to begin to explore the sources of heterogeneity in program impacts.

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Appendix

Table A1: Summary statistics

	1996				1997			
	18-24	23-24	25-26	25-31	18-24	23-24	25-26	25-31
time invariant:								
male	0.5866	0.6312	0.6761	0.7229	0.5829	0.6434	0.6686	0.7158
Australian born	0.8587	0.8379	0.8135	0.7757	0.8618	0.8391	0.8245	0.7905
ESC	0.0531	0.0597	0.0649	0.0829	0.0512	0.0558	0.0680	0.0803
NESC	0.0882	0.1024	0.1216	0.1414	0.0870	0.1051	0.1075	0.1292
ATSI	0.0345	0.0344	0.0317	0.0356	0.0340	0.0315	0.0333	0.0363
UE history:								
h0000	0.6008	0.5417	0.5616	0.5595	0.6246	0.5591	0.5601	0.5659
h0001	0.0723	0.0904	0.0799	0.0795	0.0702	0.0949	0.0916	0.0870
h0010	0.0147	0.0151	0.0140	0.0140	0.0127	0.0125	0.0149	0.0141
h0011	0.1037	0.1330	0.1276	0.1273	0.0793	0.0943	0.0990	0.0989
h0100	0.0253	0.0203	0.0193	0.0197	0.0270	0.0263	0.0250	0.0224
h0101	0.0058	0.0058	0.0046	0.0059	0.0063	0.0071	0.0078	0.0077
h0110	0.0335	0.0335	0.0351	0.0359	0.0310	0.0329	0.0309	0.0329
h0111	0.1028	0.1188	0.1129	0.1149	0.1026	0.1247	0.1207	0.1250
h1000	0.0028	0.0027	0.0011	0.0017	0.0028	0.0019	0.0029	0.0026
h1001	0.0005	0.0005	0.0011	0.0007	0.0005	0.0003	0.0014	0.0008
h1010	0.0001	0.0001	0.0000	0.0002	0.0001	0.0002	0.0003	0.0003
h1011	0.0006	0.0001	0.0003	0.0007	0.0006	0.0005	0.0005	0.0005
h1100	0.0139	0.0137	0.0132	0.0138	0.0167	0.0138	0.0158	0.0138
h1101	0.0039	0.0034	0.0043	0.0041	0.0030	0.0039	0.0045	0.0040
h1110	0.0095	0.0093	0.0118	0.0107	0.0097	0.0117	0.0095	0.0095
h1111	0.0098	0.0115	0.0133	0.0115	0.0130	0.0159	0.0151	0.0145
time variant (at spell commencement)								
part-time job	0.0868	0.0823	0.0787	0.0822	0.1346	0.1226	0.1136	0.1111
participated in JSD	0.0000	0.0000	0.0000	0.0000	0.3341	0.3458	0.3584	0.3559
Single	0.9268	0.8853	0.8289	0.7523	0.9285	0.8933	0.8334	0.7639
Married Partner NT IS	0.0261	0.0389	0.0544	0.0724	0.0263	0.0342	0.0503	0.0610
Married Partner on IS	0.0471	0.0758	0.1167	0.1753	0.0452	0.0725	0.1164	0.1751
have child	0.0210	0.0397	0.0748	0.1353	0.0210	0.0419	0.0763	0.1372
housing status:								
private rent	0.3349	0.4239	0.4435	0.4490	0.3247	0.4127	0.4420	0.4468
government rent	0.0083	0.0123	0.0181	0.0265	0.0076	0.0108	0.0173	0.0236
other rent	0.5388	0.4508	0.4134	0.3589	0.5903	0.5058	0.4441	0.3923
home owner	0.0084	0.0176	0.0400	0.0757	0.0075	0.0172	0.0347	0.0715
Purchasing home	0.0027	0.0052	0.0124	0.0189	0.0032	0.0076	0.0156	0.0251
other home owner	0.0016	0.0036	0.0049	0.0082	0.0010	0.0019	0.0045	0.0076
not stated	0.1053	0.0867	0.0676	0.0628	0.0657	0.0441	0.0418	0.0332
no. of obs.	27,851	7,742	6,521	17,902	24,143	6,469	5,767	15,923

Table A1: Summary statistics (continue)

	1998				1999			
	18-24	23-24	25-26	25-31	18-24	23-24	25-26	25-31
time invariant:								
Male	0.5662	0.6228	0.6664	0.7077	0.5694	0.6273	0.6764	0.7182
Australian born	0.8612	0.8378	0.8250	0.7943	0.8735	0.8357	0.8213	0.8033
ESC	0.0528	0.0621	0.0652	0.0776	0.0475	0.0593	0.0699	0.0766
NESC	0.0860	0.1001	0.1098	0.1282	0.0789	0.1050	0.1087	0.1201
ATSI	0.0329	0.0307	0.0359	0.0364	0.0368	0.0330	0.0386	0.0403
UE history:								
h0000	0.6774	0.6031	0.5809	0.5847	0.6460	0.5974	0.5971	0.5951
h0001	0.0659	0.0873	0.0974	0.0918	0.0712	0.0873	0.0844	0.0838
h0010	0.0101	0.0128	0.0122	0.0116	0.0144	0.0113	0.0119	0.0116
h0011	0.0622	0.0846	0.0859	0.0861	0.0669	0.0841	0.0816	0.0825
h0100	0.0253	0.0247	0.0231	0.0233	0.0279	0.0244	0.0209	0.0241
h0101	0.0046	0.0073	0.0078	0.0083	0.0062	0.0064	0.0057	0.0061
h0110	0.0289	0.0291	0.0318	0.0332	0.0339	0.0321	0.0307	0.0304
h0111	0.0811	0.1056	0.1114	0.1113	0.0902	0.1133	0.1233	0.1203
h1000	0.0040	0.0034	0.0044	0.0032	0.0036	0.0022	0.0037	0.0032
h1001	0.0006	0.0006	0.0002	0.0003	0.0005	0.0005	0.0004	0.0010
h1010	0.0000	0.0000	0.0004	0.0001	0.0003	0.0005	0.0000	0.0001
h1011	0.0004	0.0011	0.0009	0.0009	0.0004	0.0002	0.0014	0.0014
h1100	0.0162	0.0150	0.0137	0.0146	0.0150	0.0134	0.0135	0.0127
h1101	0.0030	0.0034	0.0046	0.0057	0.0029	0.0041	0.0045	0.0039
h1110	0.0094	0.0087	0.0109	0.0101	0.0096	0.0091	0.0086	0.0096
h1111	0.0106	0.0134	0.0144	0.0147	0.0111	0.0136	0.0125	0.0144
time variant (at spell commencement)								
part-time job	0.1228	0.1120	0.1013	0.0956	0.1183	0.1050	0.0834	0.0808
participated in JSD	0.6464	0.7002	0.6890	0.6680	0.6067	0.7612	0.7578	0.7375
Single	0.9299	0.8912	0.8353	0.7743	0.9333	0.8967	0.8569	0.7869
Mpnis	0.0261	0.0383	0.0560	0.0692	0.0237	0.0328	0.0462	0.0580
Mpis	0.0439	0.0705	0.1087	0.1565	0.0431	0.0705	0.0969	0.1551
have child	0.0193	0.0412	0.0747	0.1258	0.0178	0.0382	0.0720	0.1346
housing status:								
private rent	0.3052	0.4108	0.4448	0.4492	0.3362	0.4468	0.4763	0.4681
government rent	0.0062	0.0103	0.0148	0.0217	0.0064	0.0083	0.0125	0.0201
other rent	0.3628	0.3337	0.3101	0.2711	0.2423	0.2806	0.2778	0.2514
home owner	0.0065	0.0139	0.0307	0.0586	0.0047	0.0105	0.0215	0.0454
Purchasing home	0.0032	0.0082	0.0137	0.0303	0.0046	0.0110	0.0182	0.0408
other home owner	0.0008	0.0018	0.0044	0.0057	0.0004	0.0005	0.0027	0.0039
not stated	0.3153	0.2213	0.1815	0.1634	0.4056	0.2424	0.1911	0.1703
no. of obs.	24,668	6,195	5,411	15,510	25,155	5,817	4,892	14,707

Table A2: Coefficient estimates of hazard function using spells commenced between January and June 1997 and between January and June 1998 for individuals aged 23-26

	males		females			males		females	
	Coef.	Std. Er.	Coef.	Std. Er.		Coef.	Std. Er.	Coef.	Std. Er.
UE history:					start June	-0.064	0.046	-0.027	0.060
h0001	-0.053	0.045	-0.181	0.060	fortnight 1	-2.561	0.128	-3.062	0.195
h0010	0.215	0.114	-0.005	0.159	fortnight 2	-2.110	0.116	-2.013	0.145
h0011	-0.370	0.048	-0.380	0.068	fortnight 3	-1.912	0.113	-2.057	0.152
h0100	0.138	0.086	0.089	0.133	fortnight 4	-1.726	0.112	-1.848	0.149
h0101	-0.011	0.149	-0.416	0.233	fortnight 5	-1.750	0.117	-1.739	0.150
h0110	-0.191	0.081	0.001	0.125	fortnight 6	-1.934	0.127	-1.879	0.162
h0111	-0.378	0.046	-0.387	0.069	fortnight 7	-2.017	0.136	-2.471	0.207
h1000	-0.010	0.232	0.352	0.320	fortnight 8	-2.218	0.150	-2.249	0.196
h1001	0.990	0.456	-	-	fortnight 9	-2.394	0.165	-2.355	0.210
h1010	1.529	0.725	-	-	fortnight 10	-2.270	0.161	-2.119	0.198
h1011	0.667	0.454	-0.088	1.008	fortnight 11	-2.395	0.174	-2.249	0.215
h1100	0.055	0.106	0.097	0.148	fortnight 12-14	-2.345	0.122	-2.561	0.171
h1101	0.024	0.188	0.045	0.272	fortnight 15	-2.507	0.202	-2.244	0.237
h1110	-0.121	0.145	-0.553	0.211	fortnight 16	-2.605	0.215	-2.484	0.272
h1111	-0.209	0.107	-0.090	0.203	fortnight 17	-2.477	0.208	-2.727	0.307
part-time job(st)	0.354	0.049	0.391	0.050	fortnight 18	-2.624	0.227	-2.395	0.271
part-time job	-0.292	0.043	-0.601	0.048	fortnight 19	-3.191	0.299	-2.445	0.286
ESC	-0.153	0.058	0.088	0.069	fortnight 20	-2.743	0.248	-3.085	0.392
NESC	-0.332	0.048	-0.332	0.058	fortnight 21	-3.026	0.288	-2.604	0.319
ATSI	-0.382	0.080	-0.451	0.142	fortnight 22	-2.784	0.262	-2.531	0.319
partner on IS	0.584	0.071	0.380	0.075	fortnight 23	-3.220	0.326	-2.685	0.350
partner on NIS	-0.131	0.074	-0.265	0.080	fortnight 24	-3.299	0.343	-3.223	0.460
child	0.091	0.080	0.699	0.114	fortnight 25	-2.900	0.288	-3.446	0.511
Housing:					post*fortnt1	0.028	0.144	0.507	0.214
government rent	-0.384	0.120	0.094	0.177	post*fortnt2	-0.238	0.129	-0.158	0.156
other rent	0.087	0.031	0.167	0.040	post*fortnt3	-0.062	0.118	0.092	0.159
home owner	0.210	0.082	0.052	0.123	post*fortnt4	-0.321	0.122	-0.028	0.158
purcahsing home	0.148	0.124	-0.141	0.177	post*fortnt5	-0.124	0.125	-0.091	0.163
other home owner	0.456	0.203	0.022	0.340	post*fortnt6	-0.052	0.142	-0.101	0.187
not stated	0.134	0.044	0.142	0.058	post*fortnt7	-0.110	0.159	0.446	0.234
region dummies	Yes				post*fortnt8	-0.098	0.183	0.049	0.240
start February	0.046	0.041	-0.066	0.052	post*fortnt9	-0.370	0.223	-0.208	0.282
start March	-0.097	0.043	-0.084	0.056	post*fortnt10	-0.028	0.197	-0.357	0.272
start April	-0.195	0.047	-0.187	0.059	post*fortnt11	-0.040	0.219	-0.149	0.285
start May	-0.014	0.043	0.004	0.058	post*fortnt12-14	-0.046	0.132	0.146	0.191

Table A2(continued):

	males		females			males		females	
	Coef.	Std. Er.	Coef.	Std. Er.		Coef.	Std. Er.	Coef.	Std. Er.
post*fortnt15	0.085	0.257	0.136	0.302	treat*fortnt21	0.532	0.350	0.599	0.379
post*fortnt16	0.112	0.276	-0.440	0.418	treat*fortnt22	0.030	0.354	0.564	0.382
post*fortnt17	-0.012	0.275	0.378	0.388	treat*fortnt23	0.689	0.391	-0.010	0.487
post*fortnt18	-0.367	0.335	0.167	0.360	treat*fortnt24	0.698	0.412	0.903	0.540
post*fortnt19	0.530	0.365	0.329	0.371	treat*fortnt25	-0.553	0.469	0.718	0.627
post*fortnt20	0.142	0.325	0.385	0.519	post*treat*fortnt1	0.176	0.204	-0.306	0.275
post*fortnt21	0.643	0.347	0.310	0.428	post*treat*fortnt2	0.063	0.179	0.260	0.217
post*fortnt22	-0.481	0.417	0.335	0.426	post*treat*fortnt3	0.023	0.167	-0.049	0.209
post*fortnt23	0.105	0.447	0.585	0.450	post*treat*fortnt4	0.164	0.166	0.115	0.211
post*fortnt24	-0.007	0.487	0.588	0.607	post*treat*fortnt5	-0.148	0.173	-0.018	0.227
post*fortnt25	0.119	0.392	0.855	0.647	post*treat*fortnt6	-0.081	0.190	0.010	0.255
treat*fortnt1	-0.167	0.145	0.383	0.209	post*treat*fortnt7	0.380	0.224	-0.208	0.297
treat*fortnt2	-0.038	0.117	-0.264	0.149	post*treat*fortnt8	0.147	0.245	-0.109	0.320
treat*fortnt3	-0.103	0.114	0.141	0.148	post*treat*fortnt9	0.603	0.287	-0.195	0.370
treat*fortnt4	-0.017	0.109	-0.009	0.147	post*treat*fortnt10	-0.191	0.290	0.333	0.358
treat*fortnt5	0.079	0.115	-0.136	0.153	post*treat*fortnt11	0.169	0.287	-0.125	0.387
treat*fortnt6	0.215	0.131	-0.061	0.171	post*treat*fortnt 12-14	0.239	0.179	0.015	0.247
treat*fortnt7	-0.252	0.161	0.363	0.224	post*treat*fortnt15	0.024	0.333	0.060	0.412
treat*fortnt8	0.092	0.171	0.123	0.220	post*treat*fortnt16	-0.060	0.381	0.525	0.541
treat*fortnt9	0.045	0.196	0.259	0.235	post*treat*fortnt17	-0.287	0.413	-0.548	0.507
treat*fortnt10	-0.083	0.196	-0.049	0.233	post*treat*fortnt18	0.670	0.441	-0.150	0.532
treat*fortnt11	0.193	0.204	0.089	0.253	post*treat*fortnt19	-0.505	0.484	-0.817	0.539
treat*fortnt 12-14	0.039	0.127	0.252	0.177	post*treat*fortnt20	-0.004	0.444	-0.692	0.672
treat*fortnt15	0.398	0.237	0.016	0.293	post*treat*fortnt21	-0.802	0.482	-2.082	0.753
treat*fortnt16	0.171	0.269	-0.011	0.345	post*treat*fortnt22	0.414	0.566	-0.807	0.572
treat*fortnt17	-0.001	0.270	0.552	0.357	post*treat*fortnt23	-0.116	0.565	-1.591	0.813
treat*fortnt18	0.005	0.298	-0.180	0.366	post*treat*fortnt24	0.373	0.590	-0.933	0.765
treat*fortnt19	0.584	0.359	0.268	0.352	post*treat*fortnt25	0.629	0.623	-0.893	0.840
treat*fortnt20	0.237	0.315	0.683	0.463					

Males:
Log likelihood (-0.5*Deviance) = -19629.452
Cf. log likelihood for intercept-only model (Model 0) = -20256.941
Chi-squared statistic for Model (1) vs. Model (0) = 1254.9774 (d.f = 159)
Females:
Log likelihood (-0.5*Deviance) = -11477.645
Cf. log likelihood for intercept-only model (Model 0) = -11901.016
Chi-squared statistic for Model (1) vs. Model (0) = 846.74078 (d.f = 157)

Table A3: Coefficients of Propensity Score Estimation for samples at 10th Fortnight (treatment group=23-24 years old)

	males		females	
	coefficient	*Standard err.*	*coefficient*	*Standard err.*
Unemployment history: omit group=no history				
UE for 1-2 quarters & UE in the recent quarter	0.055	0.067	0.085	0.105
UE for 3 quarters & not UE in the recent quarter	-0.005	0.084	-0.014	0.148
UE for 1-2 quarters & not UE in the recent quarter	0.031	0.228	-0.053	0.344
UE for 3-4 quarters & UE in the recent quarter	-0.129	0.165	0.141	0.294
Activity type=job search	-0.018	0.129	0.209	0.167
Have done Job Seeker Diary before	0.085	0.068	0.054	0.104
Currently doing JSD	0.128	0.137	0.173	0.274
part-time job(at spell commencement)	0.044	0.105	0.090	0.129
part-time job	0.089	0.079	0.009	0.103
Have Children	-0.250	0.185	-0.554	0.390
ESC	-0.014	0.126	0.245	0.169
NESC	-0.024	0.100	0.035	0.141
ATSI	-0.116	0.155	0.131	0.290
partner on IS	-0.648	0.233	0.345	0.235
partner on NIS	-0.062	0.163	-0.243	0.178
Housing: omit group=private rent				
government rent	-0.105	0.223	-0.268	0.468
other rent	0.015	0.075	-0.067	0.125
home owner	-0.262	0.168	-0.248	0.256
not stated	0.161	0.075	0.304	0.111
Spell commenced in February	-0.187	0.086	0.161	0.125
Spell commenced in March	-0.073	0.089	-0.101	0.140
Spell commenced in April	-0.035	0.097	-0.061	0.142
Spell commenced in May	0.072	0.098	0.168	0.153
Spell commenced in June	-0.032	0.101	-0.250	0.164
LF region dummies				
Constant	0.315	0.237	-0.579	0.283
LR chi2(85)	108.29		108.94	
Pseudo R-sqared	0.0380		0.0818	
No of obs	2055		976	

Table A4: Matching quality: differences in mean characteristics of treatment and weighted control groups and the p-value from t-test of equal mean between the two groups

	Spell commenced Jan-Jun 1998				Spell commenced Jul-Dec 1999			
	Male		Female		male		female	
	difference	*P value*	*difference*	*P value*	*difference*	*P value*	*difference*	*P value*
No UE history	0.0033	0.8867	-0.0207	0.5392	-0.0127	0.6643	0.0208	0.6171
UE for 1-2 quarters & UE in the recent quarter	0.0082	0.6955	0.0126	0.6716	0.0192	0.4760	-0.0185	0.5997
UE for 3 quarters & not UE in the recent quarter	-0.0090	0.5882	0.0017	0.9345	-0.0062	0.7738	-0.0073	0.7982
UE for 1-2 quarters & not UE in the recent quarter	0.0000	0.9954	0.0018	0.8249	0.0024	0.7484	-0.0016	0.8623
UE for 3-4 quarters & UE in the recent quarter	-0.0024	0.7575	0.0045	0.6857	-0.0027	0.7997	0.0065	0.6481
Activity type=job search	0.0031	0.7674	0.0034	0.8432	0.0031	0.8646	-0.0044	0.8818
Have done Job Seeker Diary before	0.0156	0.4361	-0.0008	0.9781	0.0011	0.9610	-0.0070	0.8336
Currently doing JSD	0.0070	0.4879	0.0062	0.5784	-0.0116	0.5256	-0.0014	0.9537
part-time job(at spell commencement)	0.0048	0.7116	0.0099	0.6859	-0.0010	0.9442	0.0191	0.5266
part-time job	0.0084	0.6215	0.0044	0.8867	-0.0077	0.7188	0.0007	0.9854
Have Children	0.0040	0.6983	-0.0005	0.9248	-0.0018	0.8671	-0.0022	0.7870
ESC	0.0005	0.9615	0.0096	0.6046	-0.0003	0.9848	-0.0089	0.6593
NESC	0.0023	0.8741	0.0011	0.9641	0.0023	0.8898	-0.0072	0.8095
ATSI	0.0000	0.9972	0.0048	0.6616	0.0091	0.4609	-0.0017	0.8845
partner on IS	0.0021	0.5485	0.0095	0.5043	0.0017	0.8135	0.0056	0.7145
partner on NIS	0.0028	0.8212	-0.0033	0.8298	0.0039	0.7957	0.0019	0.9318
Housing: omit group=private rent	-0.0127	0.5780	-0.0280	0.4177	-0.0025	0.9306	-0.0232	0.5862
government rent	0.0019	0.7092	0.0003	0.9502	-0.0006	0.8912	0.0055	0.5987
other rent	-0.0111	0.5685	-0.0138	0.5901	-0.0067	0.8178	0.0076	0.8533
home owner	0.0016	0.7840	-0.0028	0.7826	-0.0002	0.9773	0.0002	0.9810
not stated	0.0203	0.3112	0.0443	0.1592	0.0101	0.3787	0.0100	0.6758

Table A5: Coefficients of Propensity Score Estimation for samples at 15th Fortnight

	Coef.	Std. Er.		Coef.	Std. Er.
1.1.1 Male	-1.792	1.420	Unemployment rate in LF statistical region	0.082	0.313
21-22 years old	0.218	0.078	(UE rate in LF statistical region)^2	-0.002	0.036
23-24 years old	0.066	0.094	(UE rate in LF statistical region)^3	-1.28 E-04	0.001
ESC	-0.085	0.109	Male*(21-22 years old)	-0.072	0.109
NESC	0.018	0.087	Male *(23-24 years old)	0.023	0.122
ATSI	-0.098	0.165	Male *(Partner on IS)	0.255	0.239
Partner on IS	-0.209	0.174	Male *(Partner on NIS)	0.533	0.451
Partner on NIS	-0.890	0.336	Male *uhs2	0.007	0.117
hs2	0.071	0.224	Male *uhs3	0.059	0.157
hs3	-0.070	0.064	Male *uhs4	0.433	0.376
hs4	-0.097	0.240	Male *uhs5	-0.142	0.341
hs5	0.006	0.053	Male *(Proportion with positive earnings)	-0.172	0.560
uhs2	-0.067	0.087	Male *(Proportion with positive earnings)^2	0.281	0.579
uhs3	-0.058	0.12	Male *(Average earnings)	0.0004	0.0003
uhs4	-0.271	0.317	Male *(Average earnings)^2	-6.82 E-08	6.88 E-08
uhs5	0.006	0.277	Male *(UE rate at start of spell)	0.396	0.389
Child	-0.041	0.208	Male *(UE rate at start of spell)^2	-0.041	0.039
Have done JSD	0.169	0.076	Male *(UE rate at start of spell)^3	0.001	0.001
Currently doing JSD	1.738	0.049	Male *(UE rate in LF statistical region)	0.179	0.435
Proportion with positive earnings	0.539	0.384	Male *(UE rate in LF statistical region)^2	-0.019	0.049
(Proportion with positive earnings)^2	0.040	0.379	Male *(UE rate in LF statistical region)^3	0.001	0.002
1.1.2 Average earnings	-0.0001	0.0002	constant	-3.191	0.969
1.1.3 (Average earnings)^2	3.52 E-08	5.33 E-08			
Had MO breach	-0.115	0.147	No. of obs.	10880	
No job search previous fortnight	0.147	0.09	Pseudo-R^2	0.3364	
Unemployment rate at start of spell	0.044	0.278	LR chi2(49)	1841.67	
(UE rate at start of spell)^2	0.003	0.028			
(UE rate at start of spell)^3	-2.82 E-04	8.99 E-04			

Table A6: Matching quality: differences in mean characteristics of treatment and weighted control groups and the p-value from t-test of equal mean between the two groups (treatment=commence MO at 15th fortnight)

Characteristics	*difference*	*P value*	Characteristics	*difference*	*P value*
No UE history	-0.0111	0.6058	Born in other English Speaking countries	-0.0037	0.6948
UE for 1-2 quarters & UE in the recent quarter	0.0049	0.7891	Born in non ESC	0.0009	0.9387
UE for 3 quarters & not UE in the recent quarter	0.0040	0.7644	ATSI	0.0004	0.9346
UE for 1-2 quarters & not UE in the recent quarter	0.0041	0.5137	Married partner on Income Support	0.0023	0.5558
UE for 3-4 quarters & UE in the recent quarter	-0.0019	0.7626	Married, partner not on IS	-0.0002	0.9816
Not on job search in previous fortnight	-0.0012	0.9246	private rent	-0.0066	0.7723
Proportion of fortnights with positive earnings	0.0293	0.0693	government rent	-0.0027	0.5406
Average earnings (average across fortnights with positive earnings)	20.3856	0.1642	other rent	0.0124	0.4772
Had Mo related breach	-0.0014	0.8362	home owner	0.0033	0.4682
Have done JSD	0.0014	0.8649	not stated	-0.0064	0.7734
Currently on JSD	0.0021	0.9027	Unemployment rate at spell commencement	0.0270	0.8066
Gender	-0.0066	0.7750	Unemployment rate in the current fortnight	-0.0269	0.7984
Have children	-0.0003	0.9709			
18-20 years old	-0.0218	0.3221			
21-22 years old	0.0175	0.4361			
23-24 years old	0.0043	0.8327			

Figure A1: Linear predicted score of MO participation – Treatment and control groups – 10th fortnight

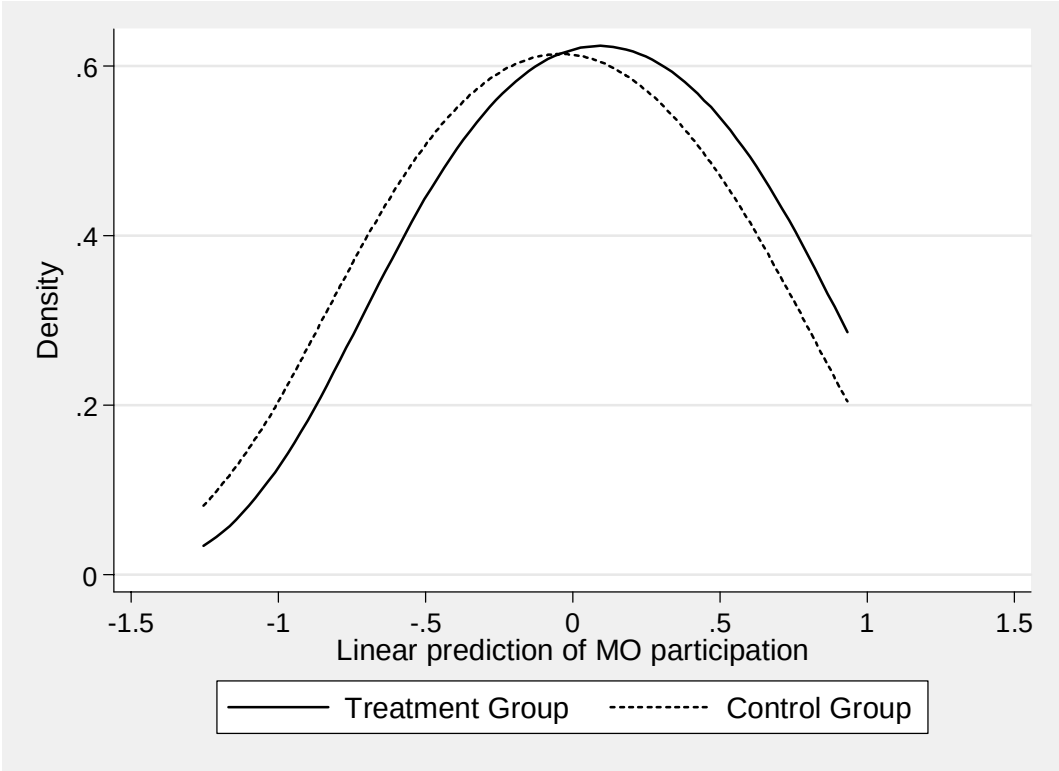

Figure A2: Linear predicted score of MO participation – Treatment and control groups – 15th fortnight